Official Transcript of Proceedings
NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Reactor Safeguards Subcommittee on Materials and Metallurgy
Docket Number:	(Not applicable)
Location:	Rockville, Maryland
Date:	February 5, 2003

Work Order No.: NRC-763 Pages 1-334

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NUCLEAR REGULATORY COMMISSION

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MEETING

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

SUBCOMMITTEE ON MATERIALS AND METALLURGY

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WEDNESDAY,

FEBRUARY 5, 2003

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. William Shack, Vice Chairman, presiding.

COMMITTEE MEMBERS:

WILLIAM J. SHACK, Vice Chairman
SANJOY BANERJEE, Consultant
MARIO V. BONACA, Member
F. PETER FORD, Member
THOMAS S. KRESS, Member
GRAHAM M. LEITCH, Member
VICTOR H. RANSOM, Member

STEPHEN L. ROSEN, Member
GRAHAM B. WALLIS, Member

ACRS STAFF PRESENT:

RAMIN ASSA
RICHARD P. SAVIO

ALSO PRESENT:

ALAN KOLACZKOWSKI
DAVID BESSETTE
ED HACKETT
MARK KIRK
MICHAEL MAYFIELD
JACK ROSENTHAL
NATHAN SIU

I-N-D-E-X

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

8:34 a.m.

DR. SHACK: This is the meeting of the ACRS Subcommittee on Materials and Metallurgy. I am William Shack, Vice Chairman of the Subcommittee. The ACRS members in attendance are Mario Bonaca, Peter Ford, Tom Kress, Graham Leitch, Steve Rosen and Graham Wallis and Vic Ransom.

Our Consultant, Mr. Sanjoy Banerjee, is also is attendance. The purpose of this meeting is to review Staff's draft report on the technical basis for revision of the pressurized thermal shock screening criteria in the PTS Rule 10 CFR 50.61.

The Subcommittee will gather information, analyze relevant issues and facts and formulate the proposed positions and actions as appropriate for deliberation by the full committee.

Since I am involved in NRC sponsored work at Argonne National Laboratory on the air oxidation of zirconium cladding, I will not participate in any deliberations relating to that work.

Dr. Kress will act as Subcommittee Chairman during these discussions, should they occur. Richard Savio is the designated federal official, and Ramin Assa is the cognizant ACRS Staff Engineer for this meeting.

The rules for participation in today's meetings have been announced as part of the notice of this meeting in the Federal Register on January 21st, 2003. A transcript of this meeting is being kept and will be made available as stated in Federal Register Notice.

It is requested that speakers first identify themselves, use one of the microphones, and speak with sufficient clarity and volume so that they can be readily heard. I would like to point out that copies of the staff's presentation are in the back of the room.

In addition, a few copies of the draft report are also available for reference in the back of the room. We have received no requests for time to make oral statements or written comments from members of the public regarding today's meeting.

We will now proceed with the meeting. I call upon Mr. Mike Mayfield, Director for the Division of Engineering Technology, Office of Nuclear Regulatory Research, for opening remarks.

MR. MAYFIELD: Good morning. Thank you, Dr. Shack. Let me start by apologizing to the committee and the audience. Our lead presenter is hung up in traffic and he hopes to be here, he said in five to ten minutes. Based on his ability to forecast schedules, I'm not optimistic.

(Laughter.)

MR. MAYFIELD: So, we'll see. Once he, the other dilemma is that he does have the computer that contains the only copy, electronic copy of the slides. So, what we've proposed to do --

DR. SHACK: Redundant back up systems here?

MR. MAYFIELD: Pardon me?

DR. SHACK: Defense-in-depth.

MR. MAYFIELD: We've apparently failed open here, yes. So what we would propose to do is do this the old fashioned way, as it was suggested earlier, and start with -- Nathan Siu has volunteered to step forward and start the presentation.

Once Dr. Kirk arrives, then we'll get the computer hooked up in short order and continue with the presentation. I don't think that based on the degree to which there has been an interdisciplinary approach and a number of staff members have been heavily involved, I don't think the committee will suffer for lack of technical content.

It's just a bit of irritation in the way we're going to have to present it. This meeting does represent a major milestone for us in, along the path we've had with the PTS Project.

This has been a major undertaking for us. All three technical divisions in research have been heavily involved. We've had the benefit of a number of meetings with this Subcommittee and the Thermal Hydraulics Committee.

So this has been something where we have benefitted from a number of interactions with you and we'd hope that your comments and concerns have been taken into account appropriately along the way.

And that you're going to be as pleased with the product that we have as we are. We have briefed up through senior management in the organization and they have expressed their general satisfaction with the project, but they are also keenly interested in what the committee may have to say.

So, with that, I would like to introduce Dr. Nathan Siu, and let him begin our presentation.

DR. SIU: Good morning. I'm Nathan Siu with the Office of Research PRA Branch. And I'm no Mark Kirk, but I'll try to step in and do this presentation. I've heard it a few times and hopefully

I'll do it some justice.

With me are Dave Bessette, Ed Hackett and Alan Kolaczkowski. What we're going to do in the morning is to talk about the approach that was taken in the re-evaluation project and then later on we'll get to the plant-specific results.

I guess I suggest to the Subcommittee that we can certainly be flexible in the agenda that you see posted. So if you'd like to take more timely, obviously, on the plant-specific results and less time on some of the later items in the agenda, that would be just fine.

And we'll just adjust our presentation accordingly. Okay. The first slide shows some of the principle team leaders here. Again, Mark and Ed on the Probabilistic Fracture Mechanics.

Roy Woods, sitting in the back, on the PRA. Donnie Whitehead from Sandia National Laboratories, Alan Kolaczkowski from SAIC. Also the PRA leads. Dave Bessette on Thermal Hydraulics.

We haven't listed University of Maryland contributors, with James Chang helping us on Thermal Hydraulic Uncertainty, for example, is also here in attendance. And so we have a number of folks who can answer questions as the need arises.

Our second slide shows, again, the proposed agenda, which you have in front of you. So, and the notion would be we'd provide an overview and background before the break and then get into plant-specific results after the break.

And then let the presentation go where it will at that point. And as you see we have a massive number of slides and so I'm sure we won't cover everything that's in those slides.

Our third slide shows the government and industry participation. This has been an activity that has been supported tremendously by industry. Specifically the MRP and EPRI. And you'll see a number of the organizations involved here.

We had to do plant-specific studies as part of this work, and without the cooperation of the utilities and other members in the industry we couldn't have gotten the job done.

We've also had very good reviews along the way of a number of the technical products and tools and interactions are still continuing in that area.

DR. WALLIS: This is very good, but I think when one reads the documents you put out, like the NUREG, it's clear that different bits were written by different people? Is this think working?

DR. SIU: Yes.

DR. WALLIS: And someone needs to put it all together so the whole story is perhaps clearer. That's an observation I have. And if you have too many cooks and not enough time to put together the main dish and explain it.

DR. SIU: Yes, thank you for the comment. And that's certainly an important point. We've tried to present an integrated approach, but clearly there's some places where the integration wasn't as good as in others.

MR. HACKETT: I think one of the points we'd add here, too, is the fact that in briefing this with the Office Director, there have been numerous opportunities for public interaction.

All the meetings with the groups that you see here. We just had one recently, for instance, just two days ago, where there was a public meeting with a lot of these entities here. But also the opportunity for public participation.

In practicality, there hasn't been a whole lot of interaction with other interested members of the public of late, but that said opportunity has been availed, you know, for at least the last two or three years now.

DR. FORD: On that point, public interaction, is it literally people off the street, or is it informed professors from fracture mechanics or whatever it might be?

MR. HACKETT: It actually, when we started this out was, I believe, April, 1999, is when we kicked the project off. And there was interest at that point from the types of news and press organizations that covered the NRC typically.

I don't believe there's typically been a whole lot of interest other than that. And I think, frankly, this topic, as technically complex as it is, I think some folks in that regard lost interest along the way.

So we haven't had that same level of participation. But we've gotten questions, you know, along those lines. So that, you know, that availability has been there.

DR. FORD: The reason for my comment relates to one of the comments that you had, Graham. In Thadani's covering letter for this NUREG document it intimates that the ACRS is the only Peer Reviewer. That cannot be the case, I hope.

MR. HACKETT: No, in fact that's not the case. We have a detailed Peer Review that's basically been engaged right now. We're hoping to complete that this year.

DR. FORD: But it hasn't happened yet. I mean Thadani says there has been a thorough Peer Review. And when you read his letter you find out, now who is this Reviewer? It's the ACRS. No, we are not Peer Reviewers of your report.

MR. HACKETT: Let me back up a bit. There have been a couple of things we've done, early on in the project, that as Dr. Wallis indicates, there needs to be more than that.

Early on in the project we engaged Dr. Tom Murley, he's a former Director of NRR. And Dr. Murley did a, I guess we'd call it, that's not a Peer Review either.

He did a technical and programmatic critique of what we were proposing at the time. He wrote a letter to the NRC, I think it was to Ashok, that was fairly complementary of the approach we were taking.

So that's just an element of the type of thing we've been doing throughout the project. There is the continual interaction with the committee, which we obviously appreciate, but does not substitute for a Peer Review.

But frankly we have gotten a lot of detailed comments from the committee and subcommittee and full committee that we have addressed and are continuing to address, so I think that's been very valuable.

But that is not the substitute for a detailed Peer Review. We have that activity engaged. It's not completed right now. We are expecting we will complete that in 2003. So that's where we are with that one.

DR. RANSOM: I'd like to add a little bit to Professor Wallis' comments on the documentation. I know that in the main document, I guess that you are going to present today, there was very little or no explanation of why the heat transfer coefficient and the downcomer is relatively unimportant to this analysis.

And you have to read this other report by University of Maryland to find out why that is. And I'm wondering what is the relationship between this report and, you know, the main NUREG? And I'm hoping that will be answered, I guess, today.

MR. HACKETT: Let's see if Dave can address that one.

DR. SIU: Do you want to get that now or

maybe we can address that later on in the discussion.

DR. RANSOM: Yeah, that's fine.

DR. SIU: Thank you for the comment. Mark Kirk is clearly here. He's setting up the computer, in body at least. So I guess I'd propose that we just wait for Mark to set up.

MR. ROSEN: You beamed him up, right?

(Laughter.)

DR. KIRK: I apologize for my lateness. One needs to allow a considerable margin in chaotic systems and I think we can all be glad that PTS isn't one of them.

DR. WALLIS: No, I think we have, now we have a data point on your appreciation of the need for conservatism.

(Laughter.)

MR. ROSEN: But it's only one data point, we can draw any line through that we choose.

DR. SHACK: We can get a full distribution.

DR. KRESS: You can draw a circle through there.

DR. WALLIS: No, one data point is enough to demolish a theory which claims to be correct. I think consistent with that theory. Well, if he claims to be absolutely correct.

DR. KRESS: Well, we could let Dave Bessette answer your question while we wait.

MR. BESSETTE: If you want. The main NUREG is not necessarily intended to be completely stand alone. But it will reference the University of Maryland report which itself will be a NUREG/CR, released as a NUREG/CR in the coming months.

DR. RANSOM: The strange thing is his question is answered in a couple pages in Appendix A of this report. And why that wasn't put into the introduction of the other, I, it's a real mystery.

DR. WALLIS: And the same thing is true of OSU work. I mean OSU has been working for two or three years on downcomer mixing and I don't think we've yet seen the final reports, so we don't really know the conclusions and the evidence.

And yet it is very important to this PTS work, it doesn't appear at all in this NUREG.

MR. BESSETTE: Yeah, well, it's a draft.

DR. WALLIS: So what do we conclude? That it wasn't, I don't know what to conclude.

MR. BESSETTE: The December NUREG is, of course, it's a draft and it still needs a little work.

DR. WALLIS: Well, are you now going to

put in a summary of the OSU work and it's conclusions and supply some evidence?

MR. BESSETTE: Yes.

DR. RANSOM: Well, are there results from that work that would change any of the conclusions, I guess, that are in this NUREG?

MR. BESSETTE: No, no. I think the, certainly the results of the OSU report are implicit in the NUREG.

DR. WALLIS: Either that or someone decided to ignore them.

DR. KRESS: Well, that work, as I remember, was taken just to confirm your assumption on the mixing. And pretty much did confirm it.

MR. BESSETTE: There was a couple of points to it. This one was to investigate phenomenon, mixing phenomenon. Second was to perform integral system experiments of PTS type thermal hydraulic transients to produce data.

It was something, you know, in previous, all the previous experimental programs we've had that the emphasis has been on core, ultimately on does the coring cover peak clad temperature and things like that.

It's the first time we tried to focus on the, our emphasis on downcomer characteristics. So that was the second main purpose of the OSU testing program, and it also, in producing this integral system data to provide some assessment data base specific to PTS for the computer codes, RELAP.

DR. KIRK: Without this, the briefing will end early. I have one of those five hour batteries that lasts for approximately two and a half. Not the quite the length for a transcontinental flight.

What slide are we on, Ed?

MR. HACKETT: Three, actually four. While Mark is still setting up, just to go through, I guess the Committee would have the draft NUREG by now.

DR. KIRK: One also begins to appreciate some of the advantages of so-called old technology. Okay, again I apologize for my lateness.

The objectives of the meeting are to review the draft NUREG that was issued at the end of last year from researched NRR. Detailed and technical basis that we've outlined in that NUREG that we believe provides a strong case to support rule making.

Discuss our ongoing activities, both in research and in NRR. Address concerns that you previously raised, and Ed is it correct to say that we are requesting a letter?

MR. HACKETT: That's correct.

DR. KIRK: Okay. We'll start at the end, so in case there's a fire drill, you know where we're going. And we will be working towards this at the end of the day. As a result of the, I guess it's about been three years of very concerted effort on the PTS re-evaluation, we believe we've provided the technical basis to recommend revision of the PTS Rule, mainly 10 CFR 50.61.

Two points to bring out from the work are that in plant-specific evaluations of two of the most embrittled plants in the fleet, including two of the most embrittled plants in the fleet, we find that we have through-wall cracking frequency at or below five times ten to the minus eight at the end of what would be currently anticipated as the license extension.

Another way to look at the current result is we examined what the through-wall cracking frequency is at our current RT_{PTS} screening limits and that works out to something on the order of one times ten to the minus eight.

And that can be compared with what we thought we'd been accepting, which is five times ten to the minus six. Obviously the plants are a lot safer than we previously believed them to be.

DR. WALLIS: Mark, in your figure 1.1, that's a plot of frequency versus surface temperature. And it says that 270 RT_{NDT}, which is a present screening criterion, the frequency is ten to the minus four.

DR. KIRK: That, yeah, I apologize for that graph. That's a misinterpretation of that plot. And what we should not have done, as you see the graph on the screen, is we shouldn't have put the two sigma margin on there.

Because once one adds the two sigma margin, then you have to change the X axis from being a mean to a mean plus two sigma. So the --

DR. WALLIS: It still seems the wrong way. I mean if it should be screened at two, now why should you allow people to go to 270?

DR. KIRK: Well, it's really adding something to the screening criteria and then also adding something to the way that it's evaluated.

DR. WALLIS: You add the same thing to both?

DR. KIRK: You basically add the same thing to both.

DR. WALLIS: It's extraordinarily confusing.

DR. KIRK: I would concur.

MR. MAYFIELD: This Mike Mayfield. Let's, we need to back up and provide a little history. When we did the original PTS Rule, there was a concern, as was noted.

The calculations were done based on mean surface temperature. And the intent was to then use that mean value as the point of comparison. However, the embrittlement correlations that were used, in Regulatory Guide 1.99, included a two sigma margin.

And there was some considerable interest, and at that point was viewed as a persuasive interest, to use the same embrittlement correlation methodology that people used when they were looking at setting their pressure temperature limits.

So that you didn't have two different schemes, two different methodologies that people had to make use of. So they took the 60 degree margin that was in Reg Guide 1.99, and they added it to the 210 degree mean value.

And said now we'll use that as the point of comparison. So there's one methodology for calculating embrittlement. So that was the history behind it. It's not that we're actually allowing people to run to a more embrittled state than reflected by the --

DR. WALLIS: That's the way it simple appears. I mean even if this figure, you can see 270, ten to the minus four.

MR. MAYFIELD: Well, we wouldn't allow anyone to go to 270 as a mean value, sir.

DR. WALLIS: I know, I don't care what you would allow. I mean the figure says it. So something --

MR. MAYFIELD: The figure does not say it, sir. The figure says we would, that on a mean surface temperature, not the, not the way we would calculate RT_{NDT}.

DR. WALLIS: But that's the problem is your RT, there is so many different RT_{NDT}'s that the reader can't figure out which one you're using.

DR. SHACK: If you look at their Figure 6.1, they plotted that graph the way they should have plotted this one with RT --

DR. WALLIS: I don't care about that. So, are you going to clear this off. Because --

DR. SHACK: Reg Guide 1.99 is the X axis.

DR. KIRK: Certainly one of the aims of the project and one of my personal aims is to make it a lot less confusing this time through.

DR. WALLIS: Because it looks to me as if you've gained a factor of ten to the fourth, by comparing this with what you're saying today. And that is an extraordinary achievement.

DR. KIRK: I guess I'll say one of our --

MR. MAYFIELD: I'm sorry, I can't, I can't let that go unrefuted. We do not allow plants to operate at that level on a mean surface temperature. I simply can't allow that to stand.

DR. WALLIS: What do you mean by mean surface temperature?

MR. MAYFIELD: It's the mean value of the surface embrittlement. The embrittlement at the vessel surface, as characterized by the reference nil ductility.

DR. WALLIS: Are there other places where it's higher?

MR. MAYFIELD: No, sir.

DR. WALLIS: Well, if it's the mean, there must be other places where it's higher and lower?

MR. MAYFIELD: Well, there is, if you went all the way around the surface you would find a variation in embrittlement. So, certainly, there will be places that it's higher. But there's no place that it goes up to a mean value of 270 degrees.

DR. WALLIS: So when you rewrite this report you will make it really clear which RT_{NDT} you're talking about and which T you're talking about, because there are all kinds of different temperatures.

MR. MAYFIELD: Yes.

DR. WALLIS: When you plot T minus RT_{NDT}, the reader has, this reader has a tremendously difficult time figuring out which one of the two you're talking about. Because there are different ways of defining both of them.

MR. HACKETT: There's that, I think it's obvious there's a fair bit of confusion over this issue and has been over the years. What one of our goals today will be to try to clarify, we'll try. We see how well we do by the end of the day.

DR. WALLIS: That's why you need a Peer Review.

MR. HACKETT: Well, that's at least one of the reasons. I would say there are many reasons. And one of the things that I was going to say, while we're on the subject, Mark will introduce this.

But, not to, you know, intentionally add further confusion, but we are introducing the concept of a weighted RT_{NDT} in this report, as you've probably seen. So it will shift yet again.

But we'll try to define that as best we can.

DR. KIRK: Which would perhaps be a conversation better left until later. But suffice it to say, when you get through all the analysis, obviously the first main bullet makes the point that doing a much more thorough analysis of PTS risk at our currently operating plants we find that the risk is much, much lower.

DR. WALLIS: I'm going to jump in again. When you write your overview in your introduction, it would help a great deal if you would explain how you managed to do this.

Now, when I read about floors, I flaws, I find very different assumptions about flaws that you made before. And it says in your report that you used a factor of 20 or 70. Well if it gains you 70, that's most of your factor of 100 that you've gained.

And it means that all of this, maybe for the thermal hydraulics you gain a factor of 1.2 or something, but what you assume about flaws is extraordinarily important in reaching this conclusion.

DR. KIRK: Yes, certainly it is.

DR. WALLIS: And then moving around the RT_{NDT} to be a best estimate rather than limiting and doing statistics on it and so on, probably gains you a factor of five or something, at most.

So it would clear if you could spell out what are the contributors to this change. And what confidence you have in the various elements in it.

DR. KIRK: We'll be going into that in detail today.

DR. WALLIS: Thank you.

DR. KIRK: And then the point of the second bullet is that as a consequence of the fact that the analysis suggests that the plants are much safer than we believe them to be, one could use this as a justification for a significant increase in the PTS screening criteria to put it on roughly the same basis as we currently use the RT_{PTS} metric.

The limit would be increasing by something between 80 and 110 degrees Fahrenheit, relative to the screening limits that are in 10 CFR 50.61. We should point out, although I think it's already become quite clear that this project is not yet over.

We both, ourselves and research, and our colleagues at NRR have several ongoing activities in research. We're completing our analysis of Calvert Cliffs Plant. We're looking into our current results in a lot more detail than we were able to do in the report that you have on your desks.

And looking at the steps we need to take to generalize these results to all of the operating PWRs. We're conducting a V&V of FAVOR. This, again, has already been discussed. We're convening an external Peer Review Panel to go over the project.

And we're also looking at the implications of these results on the operating limits, as spelled out in 10 CFR, Appendix G. We'll discuss that in a little bit greater detail later. But, suffice it to say, that having removed the conservatisms from the materials limits on an accident, we find now that the conservatism varied in Appendix G.

For example, the assumption of a quarter T flaw, ten percent safety factor on pressure would make the operational limits, in fact, more limiting than the accident limit. So there's something that needs to change there to.

At NRR, again, we of course passed them the NUREG on 12-31-02. They promised us comments back by the end of March and of course NRR management needs to make a decision as to whether or not it wishes to proceed with rule making.

DR. FORD: Mark, presumably, given the difference in timing there, that you haven't finished all the RES work, as given by the top bullet, is there enough flexibility in the system that you can take into account any modifications to the conclusions in the current NUREG document. Is that correct?

DR. KIRK: I believe so. I mean there is no, at this point, formal management commitment to proceed. However, discussions among the staff of RES and the staff of NRR, there's at least a working level understanding that their work could proceed in parallel with our finishing work, without tripping things up too much.

DR. FORD: The reason why I bring it up is, as you know, in license renewal discussions that we have with lots of plants, there are several plants approaching the 270 limit already.

DR. KIRK: Right.

DR. FORD: And we always question them about pencil sharpening and all this and what's the rationale behind that. And you get the feeling that everyone is saying, ah, but don't worry, this thing is going to solve it all. And we want to be sure this is on sound basis before we --

DR. KIRK: Yes, yes, exactly. And I think the, there is, of course, is this, you can see, if you turn around in the back of the room, there is of course a great interest in this result on the part of the industry.

However, from attending public ASTM meetings, I'm also aware that they're certainly not, and they can speak for themselves, but it's not my understanding that they're prepared to wait on this.

Certainly if plants are approaching 270 or 300 and they need to make business decisions to proceed, one could logically expect the business operators to pursue other alternatives.

MR. ROSEN: I'm interested in the last bullet on the slide, the decision to proceed with rule making. Will this depend on the weather or perhaps how one feels when they get up on a particular morning? I mean are there any criteria?

MR. HACKETT: Dr. Rosen, it is probably going to come down to, largely, a resource decision. We did, in addition to sending the paper to NRR, it was briefed through the Executive Director for Operations actually also just this week.

I think everyone things, technically, there is a rigorous basis that's been established to move ahead with this. It's probably going to boil down to, from the perspective, not for me to speak for NRR, but, you know, looking at from the Director of NRR's perspective, this would go into the bin with a lot of other things that NRR is pursuing.

And they are going to have to look at allocation of resources for, you know, what the Committee knows to be a two to three year process, at least, to go forward with this. So, I think that's the type of thing it will boil down to.

Where this ends up in NRR's prioritization scheme. And as Mark indicated, a lot of that depends on the interest of the utilities and the affected utilities. You know, discussing that or looking at, I think, I wasn't at the meeting, but there was some discussion, I heard, of potential for direct final rule making on this type of thing.

Which could be, or a petition for rule making that might come from the industry if it doesn't appear that that particular activity is going to get engaged on the, you know, the most optimal schedule.

But it's really, I think, what it will boil down to, in my opinion, at least, the resource decision.

MR. ROSEN: So, if it's a resource call, then I expect that the resource criteria, how you apply resources, there are criteria. And I would suggest that those are probably associated with the Commission's strategic goals.

MR. HACKETT: That's correct.

DR. KRESS: With respect to that other, many plants out there now that will be pretty soon approaching the current RT_{PTS} limit?

MR. HACKETT: Not soon. And the realization, I guess at this point, is that I suppose, and there are others here that can probably speak to this better than I could. I think the Palisades Plan is still technically the closest.

I believe, last I remember, that was 2011. So in terms of, that's close enough, in terms of if you're the Palisades Licensee and you're looking --

DR. KRESS: If you want a relicense.

MR. HACKETT: If you want to re-license that plant, I'm sure they're looking that far ahead and much further. So obviously the sooner the better with regard to this type of activity.

Even if this activity is going forward and the pace is not quite what a particular Licensee would like, that Licensee, of course, does have options to pursue that individually with NRR.

I think it would be, obviously, more desirable to have this thing, you know, further along, but that opportunity exists too.

DR. BONACA: At some point, one of the

major contributors, it seems to me, as we discussed in the previous presentation, is the elimination of secondary side cool downs as likely contributors.

And that really is because of confidence that you have in operator action. Now, during, you know, when I was reviewing the material I was trying to see a correlation between that assumption, which is so fundamental, and the precursors that you have on Figure 1.2.

At some point during your presentation I would like you to make some connection to that and tell me if anyone of those, because I didn't go back to the material and review it, was in fact a secondary side cool down.

And why should we have confidence that if any one of those were in fact secondary side cool downs they would not occur again.

DR. KIRK: In fact, and we'll get to this as we get into the detailed discussion, but the reason why secondary side cool downs have not shown up as being nearly as important as they were previously, is attributable to three reasons.

One of them being credit for operator action, however that's the ugly stepsister of the three reasons. That's the least important factor.

So, we'll, but I'll save that for later.

DR. BONACA: Yeah, I just, I was trying to understand, you know, reviewing the materials, Figure 1.2, how does it apply to the three reasons here, and realizing that there is no discussion of that anywhere.

DR. WALLIS: But if you're on Figure 1.2, I think you ought to explain what it has to do with PTS. Because there's no bridge between that and the rest of the analysis. And if you'd indicate what kind of challenges in terms of Ks were involved or something, that would be related to the other curves in the introduction.

But Figure 1.2 is just an indication that there have been transients with certain DT by DTs, and the reader doesn't know what this means in terms of its relationship to any criteria or anything.

DR. KIRK: Well, we can certainly make that connection.

DR. WALLIS: When you're talking about operator action, I notice that in your report that you stated that there had been a rigorous PRA analysis of operator action and rigorous PRA treatment of operator -- I'm not sure there is such a thing as a rigorous PRA treatment of operator action.

(Laughter.)

DR. KIRK: Alan, is that a rhetorical question or do you want to try it?

MR. KOLACZKOWSKI: Let's just say we'll address --

DR. WALLIS: It's a statement in your report.

MR. KOLACZKOWSKI: I understand. We'll address that at the appropriate point. And then the Committee can decide whether they think it was rigorous enough. How's that?

(Laughter.)

DR. SHACK: One of the other things, Mark, you know, everything deals with essentially the fabrication flaws and there's no statement about flaw growth in an even bounding sense.

It would seem to be, especially as I'm projecting lives out to 400 years to say something about the possibility of flaw growth.

DR. KIRK: Okay.

MR. MAYFIELD: This is Mayfield. Just to, that has been looked at, and it's certainly something we should have picked up in the report. I agree. And it's something we will put in.

DR. SHACK: Yeah, I don't think it's a

show stopper, but it's certainly something --

MR. MAYFIELD: Yeah, those kinds of calculations have been done in the past and for the size flaws you're talking about here and the range in stresses that the vessel sees, where you would get operation, you're going to operate a very long time before you'll substantively modify that flaw distribution.

DR. SHACK: At least based on embrittlement I can now operate a very long time.

MR. MAYFIELD: It's something we need to address in the report and will do so.

DR. KIRK: Okay, so in terms of the way this has been laid out, we're going to give some background on the current implementation of the PTS Rule. Although based on the comments that I've already received, I'm feeling that that background is inadequate, but we'll give it a shot.

And talk about the motivations for why we undertook this project in the first place. And then we'll go into what is essentially a verbal walk-through of the NUREG that you've been given.

Discuss the scope of the analysis, the plant-specific results. Talk about the reactor vessel failure frequency acceptance criteria, and discuss our conclusions regarding a proposed new PTS screening limit.

Oops, I'm going the wrong way. So for background, here's a graph I've truly come to despise, and it wasn't just recently. I would whole-heartedly agree with Dr. Wallis that RT_{NDT} is confusing.

And we can certainly take steps to try to alleviate that problem. But the graph, for what it's worth, is indeed the basis of the current screening criteria. The PRA calculations established a link between a mean surface RT_{NDT} meaning an RT_{NDT} accounting for the effects of embrittlement evaluated on the inner diameter of the vessel, using the peak fluence anywhere in the vessel.

And the PRA calculations establish a relationship between that, and at 210 degrees, a yearly through-wall cracking frequency of five times ten to the minus six. For reasons that Mike has already tried to explain and are probably too difficult to go into more detail on, a margin of 260 degrees was added to that and roughly -- 60, sorry.

Ah, 260, 60. Sixty degrees Fahrenheit was added to that and essentially that same margin is added in the assessment process. So while it is indeed confusing, it is also, in fact a wash.

DR. WALLIS: Which is the real, it's the RT_{NDT} that you talk about as being 270, really 210 plus a 60 degree --

DR. KIRK: That is correct.

DR. WALLIS: -- artificial addition? And is the RT_{NDT} you are talking about today, as a result of your far better analysis, is it plus 60 degrees or is it a real RT_{NDT} --

DR. KIRK: Absolutely not. It's a real RT_{NDT}.

DR. WALLIS: So I couldn't take this curve and superimpose it on your curve in your Chapter 4 or something where you show exactly the same thing with numbers like ten to the minus nine and ten to the minus four? I can't do that?

DR. KIRK: I, I, no, it would something akin to plotting a fruit bowl.

DR. WALLIS: Well, I think that's going to be very clear.

DR. KIRK: I agree, I agree. That point is well taken.

DR. SHACK: This, I mean, I always think of them in terms of real RT_{NDT} and regulatory RT_{NDT}. You know, there's the one I calculate out of Reg Guide 1.99 Rev. 2, and then there's the real world.

These are the real world numbers. The ones you are plotting in Chapter 4 are the real world numbers, too.

DR. KIRK: That's correct. And what we've, by going through the PRA frame work and the uncertainty analysis we have, again, like I said, one of the expressed aims of this project is to try to do this quote/unquote right and to get rid of a need for the very confusing margins.

DR. SHACK: When you get, the 60 degrees just relates the RT_{NDT} to the regulatory RT_{NDT}.

DR. WALLIS: But when you get to Appendix A, you begin to feel this is the real world. But there are other, the regulatory world is a sort of Alice in Wonderland world. Where you think you've got something, but it isn't that, it's something else defined some other way.

Let's get rid of all that in the future.

DR. KIRK: Works for me. So in the current rule, if anybody's regulatory RT_{NDT}, to borrow Dr. Shack's term, which I really like. If the regulatory RT_{NDT} is, seemed to approach the regulatory limit of 270 degrees Fahrenheit for axial welds, plates or forgings, or 300 degrees Fahrenheit for circumferential weld, the Licensee has to do something.

They either have to implement flux reduction, which decreases the efficiency of their plant but protects the beltline material from embrittling quite so fast. Or they need to perform a plant-specific PRA according to Reg Guide 1.154 to try to justify to NRR why operation in excess of regulatory limit is in fact a wise thing to do.

I've got three slides on motivations for revision. The words in the yellow box probably say far more than all the details I've put on this slide. Yankee Rowe in the early, or I should say late 1980's, found that it was approaching the regulatory limit at its anticipated EOL.

The Yankee Atomic Energy Company attempted to follow the provisions of Reg Guide 1.154 to build a case for operation. In excess of the limit, again, this is indeed a very long story which I think the Committee is probably, in general, more familiar than me.

Suffice it to say it didn't turn out so well, and the operating company made the business decision to shut down the plant in September of 1991. As a consequence of this, our Commission directed the staff to look into work necessary to revise the technical basis for both the regulatory guide and the associated rule.

You've seen this slide before. One of the things that the staff thought about, having gotten that directive is, gee, there are a lot of technical improvements that have been made in the past 20 years that suggests that the current rule is, indeed, conservative.

These improvements occur across the three major technical areas. Those being PRA, Thermal Hydraulics and Probabilistic Fracture Mechanics. We've gone through this slide before, but suffice it to say when one thinks and compares the type of analyses, the type of data and those sorts of things that were used in the original analysis and compares it with what we would do today, you indeed find things that would both reduce, you would believe if you did it right or to the best of your ability today, would reduce the calculated risk.

Those being represented by the green downward arrows. And indeed you'd find that there are some things that you feel you should include today that would in fact increase the risk, as represented by the red upward arrows.

Taking an example from PRA, previous

external precursor events were not considered. Mainly because the calculated probabilities of, yearly probabilities of vessel failure in the previous analysis in the minus six range when compared with what a scoping analysis would tell you would be an external event frequency.

You decide that the external event frequency is contributing at least two orders of magnitude less and so why bother including it. However, our numbers being, having a starting point of two orders of magnitude less, there's a necessity to look at external events and other things.

Having gone through these analyses, we're now in a position to put sizes to some of these arrows. Certainly some of these things matter more than others, but what we've tried to do, to the greatest extent practicable, is to take an even approach to this and include everything that we possibly could within, you know, within the necessary scope and resources.

This was brought up earlier, with Dr. Kress' question. Certainly some plants are close to the current screening criteria. This is a plot of how many degrees Fahrenheit the regulatory RT_{NDT} values are from the current regulatory limits, plotted versus the time that the plants come to end of license.

Certainly anybody that's close, within the red zone, and that's not an official term, the operator would worry that some change to their measured chemistry value, the next surveillance capsule, an alteration in their fluence calculation or methodology could take them from being slightly under the line to slightly over the line.

So anybody that gets anywhere close to the current regulatory limits is doing something to make sure that they stay away from them.

DR. WALLIS: Excuse me, you referenced chemistry. Is the chemistry of the water figured into this possibility of surface flaw development?

DR. KIRK: No.

DR. WALLIS: It's not?

DR. KIRK: No. There are people here that know a lot more about that than I, but I don't think that's a major factor.

MR. HACKETT: That gets back to Dr. Shack's previous question on, you know, whether or not there is potential for any type of flaw growth. Which, you know, over a very long period of time it would be prudent for us to go back and take a look.

You know, heretofore, has not proven to be an issue. But, no, other than that, that would be the only way the water chemistry would factor into it.

DR. WALLIS: So it's not figured into your analysis at all?

MR. HACKETT: No, it's not.

DR. FORD: You know, I agree with your assessment. I honestly don't see right now how environmental degradation in these particular plants could impact this over a reasonable time period. But what if you were wrong?

I mean so many times, I mean we've just seen this the other day. We've got a model uncertainty, a system model uncertainty. What would happen if? So, for instance, would your model be able to say, if I had a surface flaw, for whatever reasons, of say a quarter of an inch.

And we don't know how it got there, but it got there. How would that impact the results?

DR. KRESS: The standard answer to that is we use defense-in-depth, which involves the balance between CDF and containment failure. And they have a containment failure now calculated.

And it looks to me like it's sufficient defense-in-depth to deal with uncertainties like that. Which is something I see was lacking in the past in this area.

DR. FORD: My specific question to him was, how do you take into account that methodology?

MR. HACKETT: I guess there are a couple of ways that you could come at that. One is that you could say the fabrication flaw, density and distribution brackets that to some degree now.

Either analytically or experimentally we have seen flaws in that range. And the experimental part has shown that there is a lot of small flaws that are virtually inconsequential when it comes to PTS.

But they do, when you go beyond that and you're trying to address this analytically with codes like PRODIGAL, or you're just making, you know, statistical estimations of what might be there, those type of things do get factored in where you have maybe a quarter of an inch flaw or even a half an inch flaw that's going to show up there with some statistical distribution.

So to that extent, it's covered, but it's not assumed to have gotten there by any type of flaw growth mechanism. At least the Deputy Office Director for Research, Jack Strosnider, has asked us as part of one of our sensitivity studies, at least to address that type of thing.

Not specifically the flaw growth. Almost sort of regardless of how a flaw might get there, ask the question that Dr. Ford just asked. What if you missed it? You know, what if there is something you just miss?

Like analogous to the Davis-Besse situation. Previous to that we weren't anticipating you'd see degradation of that magnitude. So that potential always exists and we were going to try and come at it in this project through some sensitivity studies that, in that case, have yet to be performed.

DR. FORD: I was about to ask, when will that be done?

DR. KIRK: That was on the ongoing work slide.

DR. FORD: Oh, okay, I didn't see it.

DR. KRESS: With respect to this slide here, I'm sure it's plant-specific, but the question I have is is there a reasonable rule-of-thumb that I could use that says if I'm, say, 50 degrees or so many degrees away from the limit, how many years I have left?

DR. KIRK: About a degree Fahrenheit per year of operation. Once you're, with the proviso, once you're on the flat part of the embrittlement curve, which if you're close to the limit, you probably are about a degree Fahrenheit per year.

Between a degree Fahrenheit and degree centigrade per year to put an uncertainty bound on it.

DR. KRESS: Thank you.

DR. KIRK: Which is why a few degrees seems to be fought over so hotly, because it has very real economic impacts. So the scope of our analysis which, again, I believe the Committee is familiar with.

We selected four plants for very detailed, dare I say, PRA analysis. We've included one plant from each of the major PWR Manufacturers. Two plants were plants that were included in the study that established the current PTS Rule.

Those being Calvert Cliffs and Oconee. The other two plants in our study, Beaver Valley and Palisades, are two plants that are among the closest to the current PTS screening criteria, if not the closest.

MR. HACKETT: Let me add the caveat there, especially for the record, that that means close at EOL, not close right now.

DR. KIRK: Yes, yes, yeah. And there is a correction I failed to make. The word all should, of course, be in quotes because complete knowledge is, of course, never attainable.

MR. ROSEN: When you say close at EOL, for those plants, close at end of life of their current license life which is 40 years?

DR. KIRK: That's correct, that's correct. Both Beaver Valley and Palisades are within a degree Fahrenheit of the current screening limits at end of 40 years. And the last bullet just simply reflects the fact that we believe, and it remains for others, of course, to pass judgment, that the quality and the detail of the plant-specific analysis is indeed very comprehensive.

We'll go into a few details of the analysis approach. Our approach has been briefed to the Committee before in even greater detail, so we just wanted to hit the high points here.

The approach includes two main components, the first being plant through-wall crack frequency estimates. In constructing these estimates, we've used a frame work that was laid out by Nathan several years ago.

And it's important to point out that overlaid on this entire process we've addressed and quantified uncertainties as an integral part of the analysis process. That's quite a radical and I would personally say good departure from the past where uncertainties were buried with implicit conservatisms and were handled fairly non-uniformly without.

Here we tried to do a much better front-end job. The way the analysis process works, and of course there's many, many levels of detail below this which we won't touch on today.

But one starts with an events sequence analysis. That defines both the combination of things that can go wrong and the frequency with which they go wrong. The combination of things that go wrong are then fed into a thermal hydraulic analysis, conducted using the RELAP Code.

That estimates the temporal variation of pressure, temperature and heat transfer coefficient, which is fed through a probabilistic fracture mechanics analysis based on linear elastic fracture mechanics techniques performed using the FAVOR Code.

That combined with material property and prevention of embrittlement information, flaw information and fluence information, allows us to calculate the conditional probability with which a through-wall crack will occur.

That's conditioned, of course, on the fact that the sequence has occurred, so those conditional probabilities are multiplied by the sequence frequencies to obtain an estimate of the yearly through-wall cracking frequency.

DR. WALLIS: May I ask you how you handled uncertainties? Now in the PFM analysis in this NUREG we looked at, there's quite a long discussion of epistemic uncertainty, aleatory so how you handle that with PFM.

And it wasn't clear to me how you handle thermal hydraulics. Do you have a thermal hydraulic scenario? And then for that scenario you then do these uncertainties on PFM? Or do you have a the thermal hydraulic uncertainties also propagating through the PFM uncertainties? How do you handle that?

DR. KIRK: The, once the thermal hydraulic, once the pressure temperature and heat transfer coefficient variation with time gets to the PFM analysis, the PFM analysis treats it deterministically.

DR. WALLIS: It does? Okay.

DR. KIRK: Yes, that's correct. The uncertainty treatment on thermal hydraulics is effectively dealt with before that. And to give you the details I'd have to defer to either David or Alan.

DR. WALLIS: Okay, so it gets deterministic once it gets --

DR. KIRK: Once it gets to PFM it's deterministic, that's right.

DR. RANSOM: Mark, one question on the FAVOR Code. The second report indicates the FAVOR Code can't be used. And yet in this one says the FAVOR Code is a part of the analysis. So I'm wondering what is the status of that?

DR. KIRK: I'm confused. What's the second report?

DR. RANSOM: The second report is from University of Maryland by Chang, Alemenas and Mosleh.

MR. BESSETTE: I think that's in a time sequence. When we, two years, one year ago when we were doing a lot of the early uncertainty evaluation, TH uncertainty evaluation, the FAVOR Code wasn't final yet.

So a lot of that work was done prior to the release of FAVOR.

DR. RANSOM: Okay, this is a year old, I guess. Was that ever released or was it just a report to the NRC?

MR. BESSETTE: It's, well it's been in the works for about two years. The actual draft report --

DR. RANSOM: Okay, well FAVOR is working?

DR. KIRK: Yes. If FAVOR wasn't working, I wouldn't be sitting here.

DR. WALLIS: FAVOR sounds great, actually, from your, the NUREG that we reviewed.

DR. KIRK: FAVOR sounds great. Is that on the record?

DR. WALLIS: But we don't have, well, it sounds great, but we don't have, you need to fiddle the bass a little bit. There is this big whole about thermal hydraulic uncertainty which is not treated in this NUREG.

And then we get given these other reports, you know, uncertain age, and we don't quite know what to make of them. Are you going to put a proper treatment of thermal hydraulic uncertainty and then revise NUREG?

DR. KIRK: Yes.

DR. WALLIS: Okay, thank you.

DR. KIRK: Okay, so once you've figured out how often you think you're going to get a through-wall crack in your plant per year, you need to compare that with some metric of how frequently you would find that okay.

So we developed an acceptance criteria for through-wall cracking frequency consistent with current NRC policy and Commission guidance, specifically as expressed in Reg Guide 1.174.

And taking due account of the comments from this Committee and other areas, and Nathan will be discussing that in much greater detail later as we will be discussing the plant through-wall cracking frequency estimates.

And just notionally you can think of combining those two figures as shown on the lower graph computing through-wall cracking frequencies for different plant ages, and then using those data to discern a new screening limit.

And we can say upfront that we didn't go in with the a priori assumption that we would be using RT_{NDT}, it turns out that that looks like a reasonable thing to do. But that's certainly not the only way to do it.

So what we'll do now is go into some more details of each of the major parts of the analysis.

DR. WALLIS: Could you have used consistent acronyms throughout, so that when, the output of this box is RT_{NDT} or some, TW, no, it's TWC, FTWC or something?

DR. KIRK: Of what box, I'm sorry?

DR. WALLIS: The output of this whole thing on the left, the frequency of through-wall cracking?

DR. KIRK: Yes, yes.

DR. WALLIS: There's an acronym somewhere later in the report, although this part of the report talks about the CP. Are you going to make it clear which is the output of this process here?

MR. HACKETT: I think what we'll come to is, what you're looking at is what we're calling later on as RVFF, Reactor Vessel Failure Frequency.

DR. WALLIS: They are different things.

MR. HACKETT: You're right.

DR. WALLIS: Are you going to make it clear which is which and how they fit in and how they link to each other and so on?

MR. HACKETT: Yes, at least hopefully.

DR. WALLIS: Okay.

DR. FORD: Mark, could I just ask, since this is approaching the end, just to look further forward. The acceptance criteria, once it is decided upon, will be an absolute value. The other, the plant, will be plant-specific.

In the early round you talked about Yankee

Rowe being shut down because they couldn't use, couldn't usefully use the existing Reg Guide. Will this be user-friendly enough that the Licensees can use?

DR. KIRK: I'm not sure I want to speak for the Licensees on that one. But it certainly would be the intent to, you know, express what we've done in a way that people could understand it.

However, from a practical standpoint, one would need to ask the question of if we take, okay, right now let's say that we've got a plant within a degree of the screening criteria.

If indeed we do raise the screening criteria by something like 90 degrees Fahrenheit, is any plant likely to ever have to do a plant-specific analysis? Probably not.

DR. WALLIS: But if they did, would they have to do all the things that you were describing in your NUREG?

DR. KIRK: Yes.

DR. WALLIS: Well, how would they do that? Would they regenerate all your data and epistemic --

DR. KIRK: I think the parts that the plant would have to redo, and I, you know, encourage anyone to chime in with me. Certainly the fracture mechanics part is fairly generic. The material characterization is fairly generic.

The plants already have docketed embrittlement values and material composition values, so all that is essentially done. They have some estimates of fluence, so that's already done.

The specific parts that they would have to do is the front end. Is the, is the plant-specific PRA which indeed some plants have and some plants don't. And then they'd need to the thermal hydraulic analysis.

DR. KRESS: Your curve is based on fluence as it's associated with the four plants you --

DR. KIRK: That's correct.

DR. KRESS: Now, if a given plant says now I've got a fluence that's considerably lower than that, is there a real simple rule for them to say this, this means I can change my screening criteria by --

DR. KIRK: You wouldn't change the screening criteria. You'd simply --

DR. KRESS: I mean you'd change their --

DR. KIRK: Oh, yes, yes, yeah. The metric we get to in the end, the so-called weighted RT_{NDT} is, it looks a little ugly when you put it on the page, but it's nothing more than a simple algebraic formula.

DR. KRESS: It's linear in fluence.

DR. KIRK: Yeah, that uses material property information that's available, fluence information that's available and vessel geometric configuration information which is available.

DR. KRESS: So that's the reason you use that metric is cause it's relatively easy for the --

DR. KIRK: That's right.

DR. KRESS: -- utility to just plug in his case and get that number.

DR. KIRK: Yeah, that's correct.

DR. KRESS: And he doesn't have to go through all this stuff.

DR. KIRK: That would be the hope, yes.

DR. RANSOM: Incidentally, one thing I don't understand about this. You talk about the nil ductility temperature as an instantaneous value that you don't want to reach, i.e., or the temperature you don't want to go, I guess, below that, you know, in terms of chilling it down.

But yet, in terms of thermal stress in a vessel wall, it's clearly a time-dependent function. You know, it depends on the rate of which you achieve these temperatures. How is the rate actually factored into this?

DR. KIRK: The rate is factored in the FAVOR Code, if you want to think about it sort of at a very high level, FAVOR is doing two calculations. It's on the one hand calculating the applied driving force to fracture, which is mostly dependent upon the rate of cool down and the pressure.

And at the same time it's calculating the resistance of the material to that driving force or to that demand. And that's mostly dependent upon the temperature and the fluence and the embrittlement characteristics and then those two are compared.

The, in terms of the screening metric, something we'll get to, and this is sort of stealing a conclusion. In going through these calculations, of course, we've calculated the driving forces resulting from anticipated PTS sequences and put those through the analyses.

And what we find out, coming out of all three of these analyses, is that the, the level, even though these are plants made by different manufacturers, different times, if you get into the details, you look at them as being very different.

In fact, the level of demand relative to fracture toughness is fairly consistent from plant-to-plant. But, again, we'll go into that later.

DR. BANERJEE: I guess the tunnel hydraulic input is taken by the FAVOR Code and made into a stress of some sort?

DR. KIRK: That's correct. We take pressure, temperature and heat transfer coefficient all versus time and solve the conduction equation.

DR. BANERJEE: Right. Now is this done one dimensionally, multi-dimensionally?

DR. KIRK: One dimensionally.

DR. BANERJEE: So you don't take account of variations in the temperatures and pressures or whatever?

DR. KIRK: No, no. And we've --

DR. BANERJEE: And how accurate is that? What's the uncertainty in taking that into account? Not taking the multi-dimensional aspects into account.

DR. KIRK: Relative to the fully detailed analysis, at least from a fracture perspective, not much. Because the cracks tend to grow very long before they grow deep. And once you get a crack that's at least six times its depth, you may as well be doing a one dimensional analysis.

I know David has done, looked at the 3-D aspects of the thermal hydraulics.

MR. BESSETTE: So, yeah, of course that's one of the things --

DR. BANERJEE: How have you done that?

MR. BESSETTE: That's one of the things we were concerned about from the start, is whether this one dimensional analysis was adequate or not. What we did was a combination of looking at experiments and supplemented by some CFD analysis.

So we looked at the, of course that was one of the objectives of running the Oregon State Program was to provide additional integral system data on temperature distribution in a downcomer.

That was in the, during the ‘80's, there was a lot of work done at Creare in Finland and places, like by Theo also looking at downcomer mixing and kind of separate effects, salt water systems.

So we still had a concern or I'd say an interest in knowing that this uniform treatment of temperatures was adequate. And so, so like I say, we did additional experiments at Oregon State and CFD analysis.

And in addition to looking at other available data, like ROSA, where we do have an instrumented downcomer, and assured ourselves, let's say, that the temperature variations axial circumferential are with, let's say, ten degrees Fahrenheit.

DR. WALLIS: This is whereabouts?

MR. BESSETTE: In the, let's say, in the downcomer region.

DR. WALLIS: How far down the downcomer? I mean it evolves.

MR. BESSETTE: So, yeah, so this is, we're particularly interested in the downcomer region adjacent to the core, which is about, the top of the core is about five feet below the cold leg.

DR. BANERJEE: Are you going to discuss this uncertainty or is, what's going to happen? I don't see very much on this --

DR. WALLIS: There's nothing in this NUREG. It seems to me it ought to be in this NUREG. It's a big part of the whole picture, it ought to be there. Will it be there?

MR. BESSETTE: I'll add it.

DR. WALLIS: Will it be there? Will this NUREG be twice as fat, or will there be two or three NUREGs or what? You can't have this the final word on PTS without going thoroughly over these things which aren't in there?

MR. BESSETTE: I know, well, in the

thermal hydraulics area we have four NUREG/CRs that will be issued to support this NUREG.

DR. WALLIS: Well, they will be. But we can't sort of say we approve what you're doing until we also check that out, can we?

MR. HACKETT: This obviously needs to be addressed. It's a fundamental assumption. Terry Dickson is here, who is the author of the FAVOR Code. I think Terry would say if this assumption were grossly violated we couldn't use FAVOR the way it's currently configured.

So we do need to document that. I'm hoping it means that this NUREG won't be thicker. Like David indicates, I'm hoping it means that we can refer to another document that will cover that in detail, because we agree that has to be, that has to be documented.

DR. KRESS: In general the, where the water first comes in to the downcomer the thermal shock is worse but the embrittlement is a lot less. So those things offset each other until you get to the beltline, which is probably your worst condition.

So you're primarily interested in the thermal shock at the beltline?

MR. BESSETTE: That's correct.

DR. KRESS: Yeah, and the thermal hydraulics that were done, pretty much, you assume no mixing, I think, so that you're using the coldest temperature of the incoming. Don't you assume no mixing in your --

DR. BANERJEE: This is opposite.

DR. KRESS: Just well mixed.

DR. BANERJEE: Yeah, in fact, I think we need to see the uncertainty analysis because it's not a convincing story to say it is well mixed.

DR. KRESS: Yeah, and that's why the OSU tests are supposed to validate or confirm that assumption.

DR. BANERJEE: Both the multi-dimensional aspect and the well mixed assumption need to be examined.

DR. RANSOM: By well mixed I guess you mean node by node they're well mixed, right?

DR. BANERJEE: Well, there was, that's something that Dave can explain in more detail as to what the assumption is. So, but my impression was it was a well mixed downcomer.

DR. RANSOM: Well, unless they use one node for the entire downcomer, it would not be.

DR. WALLIS: Now you're talking about

computation. We're talking about reality.

DR. RANSOM: Well, reality --

DR. BANERJEE: Well, why don't we ask Dave what the assumption is.

MR. HACKETT: Either Dave or Jack Rosenthal would be able to --

DR. WALLIS: Maybe it's too much for today. We've got a lot to do today.

MR. HACKETT: There are a lot of things going on here, obviously, as Dr. Kress indicated. When the plume comes in, depending on the NSSS Vendor and how things are set up, not only do you have to get down to the beltline, but you have to get down to an embrittled beltline weld.

Which may or may not be in that vicinity of the coldest area of the plume. So there's an awful lot going on here, computationally. But we can get into that later. Maybe during part of David's presentation or take that as a take away.

DR. WALLIS: Yeah, I think we'll get to it when we get to David's presentation. He's going to be prepared

DR. KIRK: And David has as long as Alan talks to get prepared.

MR. KOLACZKOWSKI: I'm going to be brief.

DR. KIRK: What we wanted to do now is walk through the details of the plant through-wall cracking frequency estimates. And we're going to do that in the sequence of a discussion of the PRA analysis, followed by discussion of the thermal hydraulic analysis, followed by a discussion of the PFM analysis.

DR. RANSOM: Even on that previous slide, I noticed you've dropped the heat transfer coefficient already. You know, that that was never explained in the write up. You know, you're going to pressure and temperature versus time out of thermal hydraulics.

And indeed I understand now, after reading the other report, why you can do that.

DR. KIRK: Well, that, now you're perhaps reading too much into the graphic. And coming from a guy who loves solid mechanics and got a C minus in fluids, I just didn't include the heat transfer coefficient because I still don't understand the units.

But then again I talk in ksi square root inch and everybody thinks I'm weird. So, no, we do use the heat transfer coefficient. I apologize for leaving that off. Alan. Alan Kolaczkowski, who is our contractor in the, one of our contractors in the

PRA area was going to do the briefing on how we step through the PRA analysis.

MR. KOLACZKOWSKI: Okay, on this slide you see a blow up of the PRA portion. The Committee has seen some of this before so I'll try to go through it briefly and perhaps, I'm sure you'll slow me down if you have a question somewhere.

The left-hand side shows, again, the PRA part that eventually is providing both the sequence definitions, in terms of the overcooling transients that may present a PTS challenge that we need to model, both thermal hydraulically and then eventually in the fracture mechanics.

And, of course, the frequencies of those sequences which, again, a number that's going to be carried forward that ultimately is going to multiplied by the conditional probability of vessel failure for that scenario, to arrive at the through-wall crack frequency, which is a yearly number.

While I'll explain this as if it is a serial, done in serial fashion. Of course, in reality, as with any PRA project, you tend to iterate on these tasks. You go to Task 6 and then go back to Task 2, etcetera, etcetera.

But I'll try to explain it in a serial

fashion for clarity. Obviously, the first thing you've got to do is get a bunch of information. And here is just highlighted really the three major inputs that went into defining the scenarios that we had to worry about.

Obviously, first look at all the old information that was done before. At all the previous PTS analyses. Started with that as a baseline from which to then extend the current analysis.

We have done three specific plan analyses and are working on the Calvert Cliffs. Obviously in order to mode a plant-specific analysis, you've got to get information from the plant. And again, as highlighted here, just some of the major types of information that was gained on each plant in order to develop the models.

And then finally the last bullet, it didn't stop there. There were almost continuous, in fact, I don't know if any of the Licensees are here, but they would probably tell you that we called them too many times sometimes.

But there was continuous feedback of information going back and forth between the Licensees and us to make sure that the models had been developed appropriately and actually did represent the as-built plant condition.

In terms of the PRA model itself, looking at it from kind of two general perspectives first. You see in the top bullet, the initiators that we looked at involved all kinds. Primary system LOCAs, all types of transients which then have some subsequent fault, such as a stuck-open secondary relief valve or whatever as a result of the reactor trip, which would then induce the overcooling scenario.

So all types of transients were looked at, steam generated tube ruptures, steam line breaks and so on, on the secondary. Below that you see just sort of major classes of groupings of accidents that are included in the PRA models for the plants.

Noted here are overcooling events, both with either controlled RCS pressure, where RCS pressure remains high. Where RCS pressure perhaps initially drops and then we get a repressurization event.

Faults both in the RCS or the secondary or combinations. And lastly, we looked at this under both full power conditions, as if the trip occurred while the plant is normally operating at full power, as well as during hot zero power conditions.

Where you don't have the fission heat to act as somewhat of a suppressant in terms of controlling the down of the cool down event. So we looked at both hot power, excuse me, hot zero power and full power.

This is just an event tree format of really saying the same thing that the previous slide said. Across the top you see the major functions of interest that we're worried about that can affect the nature of the PTS challenge.

That is what is the status of the primary integrity? What is the status of the secondary pressure and secondary feed? And then what else is going on in the primary in terms of force flow versus natural circ, because that has something to do with the potential for stagnation, as well as what's going on with the pressure in the primary system.

And all this is meant to display here is just that we looked at all various combinations and interactions of those functions and what scenarios could cause those types of interactions to occur.

And ultimately pass that information on to the TH folks, etcetera, to model the plant thermal hydraulically for the various types of scenarios, and then again ultimately that was an input to the fracture mechanics folks.

DR. WALLIS: Are you going to talk about operator actions, are you?

MR. KOLACZKOWSKI: Yes, I am.

DR. WALLIS: Because what struck me was how many of these seemed to be influenced by operator actions.

MR. KOLACZKOWSKI: Yes, that is true, Dr. Wallis. Part of that rigor that we talked about earlier.

DR. WALLIS: Well, your rigor consists of considering the operator action. But how you treat them, I don't think there is a rigorous method. And you certainly admit that there.

MR. KOLACZKOWSKI: I guess, well, I'm about ready to talk about the operator action, so let me see if I answer your questions, and if not, then I'll try to be clearer.

That is part of the scope. I mean I think it is important to recognize that for some kinds of over cooling events, not in all cases, but in some kinds, the operator plays a very key role in the how severe the over cooling becomes.

And so clearly if we were going to do this correctly we had to consider what the operator may or may not do to either mitigate the event or perhaps even exacerbate the event.

And we've tried to do that in the PTS work. And so we've modeled, not only their successes, but errors of omission. And let me point out, again, adding to part of the rigor, if you will, we went to great lengths to think about things that the operator might do that would exacerbate the cooling situation.

And particularly looked at those things that were procedure-driven. Where there are places in the EOPs where, under certain conditions, because of course we were trying to make sure that we prevent under cooling events.

Where the operator will actually take actions that will, to some extent, exacerbate the cooling of the scenario. And so we wanted to make sure that those actions were included in the model.

MR. LEITCH: Did you reach these conclusions by observing operator actions in a simulator or just by looking at the EOPs and see where the likely errors of omission or commission could be, could occur.

MR. KOLACZKOWSKI: All of the above. In fact, I have a slide, which I'll get to, that will describe that a little further.

MR. LEITCH: Okay.

MR. KOLACZKOWSKI: But the short answer is all of the above.

MR. LEITCH: Okay.

MR. ROSEN: What does the parenthetical words, procedure-driven, mean under acts of commission?

MR. KOLACZKOWSKI: Well, just that while we did do some amount of searching for, shall we say, where the operator might just do something even though the procedure may not even suggest that a certain act be taken.

While we did do some searching for that and did include one or two others that I can think of, operator actions in the model that were of that type, we found enough of places in the procedures where it would direct the operator to enhance the cooling.

That clearly we wanted to make sure that those were included in the model and that's were the emphasis went. But we did try to think a little bit more about what else might the operator do in a realistic sense that maybe even isn't in the procedure, where they would enhance the cooling.

And we did come up with one or two events additional that are not necessarily in the procedures.

MR. ROSEN: Well, we'll come back to this point, but let me let you go further.

MR. KOLACZKOWSKI: Okay.

DR. BONACA: Did you, you focused on the three plants, of course, but did you look at these precursors I was talking about before? I mean there are precursors, particularly for B&W plants and also for Robinson's there are two that led to extreme cool downs. Did you look at those?

MR. KOLACZKOWSKI: Yes, we did. And we tried to look at the types of errors that operators have made in the real events, again, as a check to make sure are we including those types of acts in the model?

And so it was an input into deciding what ought to be modeled, yes.

DR. BONACA: Because, I mean, I know these plants had significant modifications because of those cool downs. And also clearly a big modification has been the EOPs which are system-oriented.

But we can't understand how they could be still defeated, for example, in the EOPs, to get back to transients that such as severe as this.

MR. KOLACZKOWSKI: Well, as I said, you don't even have to defeat the EOPs. You sometimes, in following the EOPs, you will enhance cooling which could, at least, be a potential challenge in PTS space.

DR. BONACA: The reason why I raise the issue of steam line break is because, you know, a break used to be the limiting transients before. And now they have disappeared from the horizon. We don't have them anymore.

And I, you know, when I saw the previous analysis, it was very strange to me. But you understand that that's really an area where we have to drill because the whole scenario has changed.

MR. KOLACZKOWSKI: I understand, and as Mark pointed out, the operator action credit is only one of three reasons why the secondary faults go away. And when we get to that point hopefully it will become clearer.

DR. BONACA: But it disappear as a steam line break is a big contributor I understand. I don't know what is the factor or contribution, but I believe it is a significant contribution in degrees, isn't it?

MR. KOLACZKOWSKI: In the early work, in the Oconee work, I think the main steam line breaks were close to 50 percent contributors and now they're more like five percent or less.

DR. BONACA: Yeah, yeah, so it's a big, big contributor. Okay.

MR. KOLACZKOWSKI: Yes. These are the classes, if you will, of human failures that we tried to consider. And what is indicated is the function of going and looking at those four functions on the event tree that you saw earlier.

The function which those actions most affect. Now actually some of these actions affect more than one function at a time, but we tried to, for category purposes, associate them with the function that they most affect.

And I guess the only point I want to make about this is that not only will you see so-called errors of omission in this list, but as I said, we tried to consider things that the operator might do in an act of commission which might worsen the over cooling scenario.

Just to take an example, if you look at the first column there, Primary Integrity. Not only do we look at things like where the operator would fail to isolate and isolable LOCA, which would be an error of omission, where the procedure says make sure you close off all isolable paths first in case indeed that's the source of the LOCA.

And if the operator failed to do so, the cooling would continue because the LOCA would continue. But we also looked at situations where, what would induce the operator to cause a LOCA and would again exacerbate the cooling situation.

And there are places in the procedures where operators do induce LOCAs under certain conditions. And we tried to make sure that that's included in the model.

MR. ROSEN: Now maybe this is the right time to ask my question about uncertainty and particularly the kind of uncertainty that has troubled this Committee most, which is model uncertainty.

And that goes to the question of what haven't you included in this which could dramatically change the PRA result feed into the thermal hydraulics result feed into the fracture mechanics results and lead to you an answer which isn't real.

An answer that says that pressurized thermal shock is very unlikely and therefore we can raise the criteria and let plants run longer than they would have otherwise been able to run. So you get to the wrong answer in the regulatory frame work if you get this problem wrong.

And where it could get wrong is right

here. Not including things that could lead to pressurized thermal shock, that operators do, could do. Now you've made a pass a that, clearly, by including acts of commission. And I applaud that.

But have you attempted to go beyond your statements of, that this is a rigorous analysis, grant that. But also say, but, we don't know that we've concluded all the model uncertainty.

In fact, it's unknowable. And so we need to do something with that knowledge, that we have an unknowable condition. We need to factor these results in some reasonable way. Do you understand my question?

MR. KOLACZKOWSKI: I think I do, Dr. Rosen. And let me try to answer it now and hopefully again with further slides in the presentation may it become clearer. But let me make, I guess, a couple of points.

First of all, we have done and are still doing, as you see some of the ongoing work, additional sensitivity analyses. Where we can do things like, well, what if we're wrong? Well, what if the operator error probability were one?

Would it make a difference? How much higher would the main steam line break scenario become, etcetera? And we're in the process of doing those kinds of things.

What we're finding so far is that we would have to be so grossly off, it seems. And I know it's hard to define what gross is. But we would have to be so grossly incorrect for the conclusions to change, that it almost seems inconceivable.

The other thing is if you look at what the dominant results are, you will see that LOCAs seems to dominate. In LOCAs, operators, for the most part, especially if the LOCA is much beyond, say, three or four inches in equivalent diameter size.

There's not much the operator can do anyway. Short of shutting off the HPI a la TMI, all they can do is let the event happen. The cool down is going to happen at whatever rate it's going to happen, which is largely defined by the break.

And the operator is essentially out of the picture. So, with the exception of, you know, recognizing that we have taken operator credit for the secondary faults, otherwise we said, there are other reasons why secondary faults are also not as important, that are thermal hydraulic-driven, etcetera.

That aside, if the LOCAs then really do

dominate, the operator is not that important part of the equation on how important these events are. So, I guess, I don't want to overemphasize the operator actions here.

Because if indeed if we're right and the LOCAs are the dominate types of over cooling scenarios to worry about, for the most part, especially in the larger size breaks, the operator is out of the equation anyway.

So it's not so important to completely try to quantify every little bit of the uncertainty. We'll try to, I'll try to show you what uncertainties we have addressed. You're really asking the age old question, how do you know you've been as complete as you can possible be?

Peer Review, discussions with Licensees, presentations in front of the ACRS. The subsequent Peer Review we're going to do. We're doing about everything we think we can do to say, have we addressed the issue sufficiently? Nathan?

DR. SIU: Yeah, I just wanted to add to that. Without overstating the, or over using the word rigor, we've tried to be systematic. And there is a systematic process that the team used to identify not only the human failure events in the model, but the conditions that would lead to reasonably high probabilities for those human failure events.

So it's not just a model that says this is the ATHEANA approach, which I think many on the Subcommittee have heard about. It's not just a matter of saying here's a human failure event and there's a random probability.

No, there's a search process that tries to identify what are the contextual factors that would tend to increase the likelihood of that event in the PTS example. And then, so it's that process that gives us some degree of confidence that we aren't missing things.

Now obviously you can't claim that you're perfect. But, again, I wouldn't necessarily claim rigor here. But it is a state-of-the-art or perhaps beyond state-of-the-art analysis.

Clearly, there are some places where human reliability analysis was weak. We've talked to the Committee before about our research program in this area and the area of quantification.

For example, it's not what you would, a process that you would say is rigorous, but it's systematic and it makes use of available information as best we can. And as Alan indicated, we do take input from observations of actual crews. We talk to trainers.

So it's not just the analysis team huddling together and dreaming up something.

MR. KOLACZKOWSKI: Let me just highlight. This is the first step of that systematic process. I mean if you decide, if you agree these are the functions of concern, our first question was rather than just sort of dreaming up, well, what could the operator do wrong?

We said how could the operator effect each of those functions. And this is the first step of that systematic process. Trying to decide how the operator can affect each function. And then from there, then going the next step and starting to derive the specific actions that could occur, that would then affect those functions and then include those in the models.

This is actually the first step of that --

DR. WALLIS: Then you'll get probability on those various actions.

MR. KOLACZKOWSKI: Yes.

DR. WALLIS: And that's where I think we're probably the weakest. Because you have to imagine what the person would do, and then you've got to put some number on it. And this is done, I understand, by expert elicitation?

MR. KOLACZKOWSKI: That is correct.

MR. ROSEN: And the expert elicitation in that case would be, for example, under primary integrity control, how likely is it that an operator will induce a LOCA by operating outside his procedure? Or by operating with inside his procedure, at a time that he should really do the things we're postulating him to do.

So I mean now you're presenting that to trained operators. And my guess, they'll underestimate it.

MR. KOLACZKOWSKI: Actually, interesting enough, and we'll get to it. But when we did the Palisades analysis we did a collaborative HRA effort. And actually the elicitation team was formed by a composite group of operators, EOP writers in the Licensee, as well as NRC contractors.

Interestingly enough, sometimes the Licensee people came up with higher failure probabilities than the NRC contractors did. And that was included.

MR. LEITCH: Have you considered the possibility that operator performance in the simulator may be considerably better than in the real world?

MR. KOLACZKOWSKI: I think we're all aware that, you know, whenever an operator is in a simulator they know, you know, way, way in the back of their mind somewhere, they know it's a simulation.

And you try to consider that. I, I don't know how else to address that.

MR. ROSEN: Well, and also this Committee has commented on the fact that the operating crews in simulators are the crew rather than the real case where the crew in the plant at 4:00 in morning on Saturday is two-thirds of the real crew and a third of make-up people.

People who are relieving someone else who is in the real crew but doesn't have to be here because he's on vacation of some other reason. So it's a fact of life that performance in the simulator, for several important reasons, is better, can be expected to be better than what we will see in the plants.

MR. KOLACZKOWSKI: All I can say is that in the elicitations, in all of the elicitations for all of the plants, we, when we posed the various questions of the probabilities we had to come up with, we tried to put uncertainties, of course, on the human error probabilities we were coming up with.

And we, when we asked the elicitated group to think about those probabilities, we asked them that at the high end they needed to think about things like what if this was at 4:00 in the morning on the worst day of days.

You know, you had other problems and nuisance alarms going on, etcetera, etcetera. And try to, as part of the elicitation process, capture not only, if you will, the nominal, normal state condition, but what are the extremes at the two ends.

When everything is going well, and when everything is going bad. And all I can say is many of our 95 percentile valves on our human error probabilities are numbers like .8 failure probability, .7 from the elicitation group.

We think we've captured that in our uncertainty on our HRA numbers. As best as the state-of-the-art allows.

MR. ROSEN: I hate to do this, but just to bore you just a tiny bit more.

MR. KOLACZKOWSKI: Sure.

MR. ROSEN: On the idea of getting numbers out of this group. What, did you attempt to anchor the group is some other actions that they know much better than the ones that you were questioning them on? You know, the anchor techniques that are in the literature.

MR. KOLACZKOWSKI: Yes, we did. In fact, and again, part of the ATHEANA work talks about G-cars, which are basically anchoring numbers. Trying to take operators and first explain to them actual events that have happened and how we assess, therefore, some probabilities associated with those acts to try to anchor the team, etcetera, before moving on.

And that was certainly part of the process.

MR. ROSEN: I'm not done on this subject.

MR. KOLACZKOWSKI: That's fine.

MR. ROSEN: But let's leave it there for now.

MR. KOLACZKOWSKI: Okay.

DR. BANERJEE: What impact did operator actions have on the RPT failure probabilities in that curve?

MR. KOLACZKOWSKI: Well, as I pointed out, if indeed we are correct that LOCAs dominate and particularly the, getting into the larger size LOCAs, then the operator really plays very little role at all.

Because, as I said, once the break occurs, the cool down is going to go at whatever rate it's going to go. The pressure is going to do whatever it's going to do. More than likely, especially after Three Mile Island, operators are not going to just go shut the HPI off.

So we're hitting the downcomer wall with cold water and it all becomes a T-H fracture mechanics game, and the operator is pretty much out of the picture.

The operator does provide some mitigating and/or exacerbating role in what happens to secondary faults, because that he has more control over. He can isolate a faulted steam generator.

He can close off an isolation valve on a stuck open atmospheric dump valve the ends the event. Or, he can open a valve, because he thinks it's the right thing to do. And we've included those kinds of situations in the model.

So he has much more affect on the secondary side. The primary side, there's really not much the operator can do, short of shutting off the HPI water and then it's not a PTS event it's a core melt event.

DR. BANERJEE: So the effect of operator actions doesn't shift that curve we saw some time ago which was done with this one code?

MR. KOLACZKOWSKI: That is correct. We would have to be vastly, vastly way off on our secondary effects in order that suddenly steam line breaks become super important and it raises the whole curve, etcetera and so forth. And I think that's very unlikely.

DR. SHACK: I'm going to suggest we take a break here. I sort of hoped we were going to get the PRA.

MR. KOLACZKOWSKI: So did I.

(Laughter.)

DR. SHACK: But I see that we're not going to do that in a reasonable time. So I'd like to take a break for 15 minutes and we'll come back in 15 minutes.

(Whereupon, the foregoing matter went off the record at 10:09 a.m. and went back on the record at 10:26 a.m.)

DR. SHACK: Let's go through the material in as much detail as we need to, because I think that's the most important thing. So I'm going to try to, I'll let that run. I want to protect the time that we've set aside to talk about the acceptance criteria, because I think that's another, and the containment-type issues.

What we may end up doing is short-changing the plant-specific results somewhat. Simply because there is not enough time. And that, and so I've sort of briefed the staff that that's the way we want to go.

But just remember, the longer we spend on the general material, the less time we're going to have to look at the plant-specific results, because at some time later in the day I'm just going to call an end to it and say we're going to go on to acceptance criteria.

Just so we can cover that rather important issue. Meanwhile, back at the PRA.

MR. HACKETT: Yes, so Bill, per the current schedule then, I think everyone has that on their cover sheet. We are probably going to get through the rest of Alan's presentation and then the discussion on thermal hydraulics and RELAP this morning.

And then you're welcome to weigh in afterwards when we go to the plant-specifics, we can limit that discussion as needed. Another thing I was going to say, during the caucus, some of the team was discussing, and maybe we didn't make this clear enough so I'll try to that now.

The focus, of course, of this project is on development of technical basis and technical basis evaluation. So I think Dr. Ford asked earlier and maybe there was some other discussion along the lines of is this, you know, are we so far down this path this is irreversible?

Are there things going on that we're not going to be able change? And of course the answer is no. There is, as everyone knows, or should know well around here, nothing happens real fast, particularly when you get to rule making.

So I think the question was asked can we engage, can NRR engage on rule making while we're finalizing some these technical aspects, and of course the answer is absolutely yes.

Now what if we're down the path at some point and we find what we think is a show stopper? Does that indicate that we can, you know, should we shift directions? And the answer to all of that is there's ample opportunity to do that.

When you get into rule making, as the

Committee knows, there is probably, particularly with an issue as complex as this, you're probably looking at about two to three years worth of rule making process.

From us drafting the rule to the internal consideration by Committees like ACRS, also CRGR, internally. Opportunity, at least twice, for public comments, detailed public comments, which the staff has to address.

So there will be numerous opportunities, as we go down this path in the future, to address some of the other concerns. But I just thought I'd state, for purposes of the record here and what we're trying to achieve, that this is still at a tech basis.

We still have to obtain agreement from NRR that they think we're there. We think we do have that. We obviously have to, hopefully, get some kind of consensus or agreement with ACRS and other bodies as to, you know, the merits to proceeding with this type of thing.

So, the bottom line is this is all very valuable to us and it's not a final product, but we're working towards that, obviously, as a goal and really appreciate the interactions with the Committee in that regard.

DR. KRESS: Since you already have a PTS rule in the books, and basically all you're doing is changing the screening criteria, shouldn't it be pretty easy to write a rule?

MR. HACKETT: I would, you know, I would hope so. Although, I guess, I know personally, I've been down this path a number of times and a number of the people in the room have too.

It invariably is a process that cuts both ways. And I think by intent it's not suppose to operate rapidly. It's suppose to give, for the NRC, for instance, suppose to give the public a chance to engage, to have people critique things.

So it would probably still be minimum a two-year process would be my best guess. Now, if there's a petition for rule making or potential for direct final rule making, I think that can be accelerated.

In practice, it still takes time. It's still, you know, probably more like 18 months than if everybody lines up in agreement with.

DR. KRESS: The only reason for urgency might be in some plant wants to come in for license renewal and this is an issue with them.

MR. HACKETT: Correct. With that, I'll

turn it back over to Alan.

MR. KOLACZKOWSKI: Okay, I guess continuing on. And the first two steps again was collect information. By the way, I want to point out -- could you go back to Step One, just for a moment.

Yeah. You'll notice that with regards to the human part, I just wanted to highlight a couple of bullets. Both the, we looked at the emergency and abnormal operating procedures.

We looked at training material. We talked to actual crew operators at the various plants, etcetera, to get a feeling for how sensitive or not they were to over cooling events. To what extent it's handled in their training.

How often they actually simulate over cooling events, etcetera. And then I also wanted to highlight the last bullet, observe simulator exercises. At each and every plant, all four of them, we simulated something like, it varied from plant-to-plant, but anywhere from two to four over cooling scenarios.

Some LOCAs, some secondary faults, etcetera. And observed how fast it took them to get through various steps in the procedures. When they, actually in one or two cases we found some places where procedures could actually be improved a little bit.

And the Licensees took actions, in fact, to do that. It might have been a minor point of confusion or whatever, but the point is we simulated quite a few number of scenarios and observed the crews working through those scenarios, so I just wanted to highlight that as well.

Now if we can go back to Step Three, which is where we were. Okay, so we've set the general scope of the model. The types of over cooling scenarios we want to include.

As I pointed out, the human does play a very important in some over cooling scenarios. We wanted to make sure that that aspect was also included in the model.

Now, I want to talk a little bit about the model constructions themselves. While they are all event tree,. fault tree-based, typical of PRA process, there are some differences among the models, and I wanted to point out what those differences are.

Not that the differences have any affect on necessarily the resolution of the answer or anything like that, but just that the construction process did differ a little bit and I just want to highlight what those differences are.

Okay, so first of all the Oconee model was the first one that we constructed. A couple of key aspects to recognize, this was one of the plants were the NRC contractors built the model by collecting information from the plant and then obviously having many phone calls and e-mails to deal with questions or issues as they might have come up.

The HRA, the Human Reliability Analysis, was initially performed by the NRC contractors. In other words, the expert elicitation panel was solely NRC contractors. But then that information was then reviewed by the Licensee.

And I'll point out a process when that was done. Also, additionally, the initiating event frequencies and the equipment failure data that are in the model are based on industry generic data.

That is they are not necessarily Oconee-specific initiating event frequencies or Oconee-specific failure probabilities of equipment. In that case we used actually generic data, trying to take data that was representative across the industry.

Because, again, ultimately we're trying to get an industry-wide solution to the PTS problem and not necessarily try to answer specifically what

Oconee's specific PTS risk is. But the human reliability analysis was based on Oconee's procedures, Oconee's training and so on and so forth.

When we were constructing the model, because it was the first one, we did not yet have preliminary thermal hydraulics of fracture mechanics information available to us. So, as a result, we didn't a priori screen out any times, any types of over cooling events in the PRA model.

Because we didn't know whether or not they could be screened out. So using the word all in quotes, the Oconee model is probably the most complete of all of them, relative to the over cooling scenarios that are included in the PRA model.

So, for instance, even if we had a secondary fault or just some small secondary valve opened up in the scenario that we were modeling, where later on we came to find out that that was a very unimportant scenario, it's included in the model because, again, we didn't have any preliminary information from the thermal hydraulics or the fracture mechanics that we could, with confidence, say well that's a scenario we don't need to model, we know it's not going to be important.

So the Oconee model includes,

quote/unquote, all the over cooling scenarios that we could think of. The Beaver Valley model is different. It was the second model that we developed, and just because of when it was occurring time-wise, again, this was a model that was built by the NRC contractors.

And then again reviewed with input from the Licensee. HRA was performed in a similar way. Same point made about the data. But, but that point, we had some preliminary information coming back on the results, the integrated results from the Oconee.

And we were learning that certain kinds of scenarios were likely to be unimportant. Couple that with the fact that by the time we were constructing the Beaver Valley model, we already had some 40 or so T rails run on Beaver Valley.

So we had some thermal hydraulic information and we had preliminary fracture mechanic information. We already knew that some scenarios were going to be relatively unimportant, from a through-wall crack frequency perspective.

So, as a result, we simplified the Beaver Valley model development and purposely did not model certain kinds of scenarios in the Beaver Valley model, because we had enough information from the TH and fracture mechanics, that we knew that those were going to be, if you will, non-challenging PTS events.

The next slide or two I think just illustrates some of the simplifications we made in the Beaver Valley model. I guess if the Committee has any specific questions, we can address those now or at a later point.

But I just wanted to highlight what some of the simplifications were. Next slide. And next slide. Palisades was the last model that we developed. And again, the Calvert Cliffs model is ongoing now and I will not address that, per se, unless there are questions related to it.

Because we're just starting in the Calvert Cliffs process. The Palisades was the last of the three that are in the report. This model was developed differently. In this case we, the Licensee was really, if you will, the keeper of the model.

Palisades, in their IPE and updated since model of the core damage frequency, had PTS scenarios already in the model. So, in this case, what we did was we took the Licensees model of the PTS scenarios.

We reviewed, the NRC contractors provided comments and input to the Licensee on other considerations that we thought they ought to include in their model. Palisades changed the model accordingly and then they ran the model.

We then reviewed the results and worked with them in making sure we interpreted the results correctly. So the point is here, rather than the contractors building the model, we started with a pre-existing model and modified it.

But the Licensee was, if you will, the keeper of the model. As I pointed out in this case, whereas in the other two plants you saw the NRC contractors did the HRA work initially and then it was reviewed and input was provided by the Licensee, in this case this was a collaborative effort.

We actually went up to Palisades, spent three or four days there, and as I pointed out, we got actual crew operators, trainers, one person was an EOP writer, along with NRC contractors and formed a team of about, I think was about six or seven people.

And we went through the HRA process together to come up with the failure probabilities that would be included in the model. So it was much more of a collaborative, hands on, working together kind of effort.

And as I pointed out, Dr. Rosen, it was interesting that sometimes the Licensees came up with higher failure probabilities than the NRC contractors did. Probably because they know the situation, they understand the situation better.

And they know where they could get fooled or where they might make mistakes. Okay, now we've got the models built. We did an initial quantification. We basically quantified all the action sequences.

Just to give you some feeling. In the Oconee model, the one that I said was the most complete from an overall number of scenario perspective, because we didn't a priori rule out any scenarios.

Donnie, what is there, 118,000 over cooling sequences, or something? Is that right?

DR. WALLIS: One hundred eighty-one thousand two hundred and fifty-eight.

MR. KOLACZKOWSKI: Thank you, Dr. Wallis.

(Laughter.)

MR. KOLACZKOWSKI: Again, the Beaver Valley model has much less sequences because we were able to do some simplification, as I pointed out. And the Palisades model is probably somewhere in between the two.

Now we cannot run 181,000 different TH scenarios. We'd still be here working on it. The

RELAP runs take a little bit more time than that. So clearly we had to do some binning.

And so what we did was we took like scenarios, similar in terms of what we expect their characteristics would be. Put those into bins and then those bins were what were actually analyzed by the TH folks, and then subsequently the fracture mechanics.

Let me point out this is a much, this is a very iterative process. We took an initial crack at the binning, got some results. That told us that either we binned in some cases too grossly, and some cases perhaps overly binned and we could combine some things.

So then we redid the binning process, if necessary, based on the PFM results, etcetera. So, again, while I'm explaining this as if it was a serial process, I want to point out it was actually quite iterative to make sure that the binning was of the proper resolution that we felt we needed to get the results.

MR. ROSEN: And I'm assuming the iteration went on at the PRA level too, between them. In other words, you learned something at Oconee that you applied at Palisades, and then you learned something at Beaver that you apply to Oconee.

MR. KOLACZKOWSKI: Absolutely, absolutely. And that started right at the beginning, at least looking at the old PTS stuff back done in the ‘80's. And then, and looking at things that, well, maybe in one study they did something that they didn't treat in another study, so we wanted to make sure, well let's make sure that we treat that in all the analyses, etcetera.

And it was a constant learning process.

DR. KRESS: Educate me a little bit on binning. When you take a thermal hydraulic sequence and you get some sort of severity criteria for that sequence, which may be the nature of the shock or the, and the pressure combined or something.

And you want to put a bunch of these sequences in a bin related to that severity, ah, severity range. Now, when you go to use that bin in your PRA, do you use the most severe one or do you use a mean or what do you use out of that bin?

MR. KOLACZKOWSKI: Okay, well first of all, let me indicate in a way, if I understand your point, in a way it's the other way around. The PRA is developing hundreds of thousands of sequences.

Now we need to take those and put those

into a, I think for Oconee we ended up doing something approaching 180 or so bins that we looked at. So we're taking 100,000 sequences and trying to put them into, roughly, a couple of hundred bins.

And what you do, effectively what we did was we, we first of all did some various types of, gross types of different types of scenarios. LOCAs of different sizes. Secondary faults with one valve open, with four valves open, etcetera.

And got at feeling, first of all, how much did the thermal hydraulics change under these various conditions? By that, and then run it through the fracture mechanics code, get some conditional probabilities of vessel failure. See how much those are changing.

Now you are beginning to learn where the sensitivities are. Where you need to bin very finely because whether you open one valve or two valves, seems to make a big difference on the thermal hydraulics, and/or therefore potentially makes a big difference in the CPF.

Versus other areas where you find out, gee, if I open up one valve or four valves, the thermal hydraulics hardly changes at all, so we can group all of those sequences, whether it be one valve, two valves, three valves or four valves.

Put them all into one bin, it's good enough. And so that's where the iteration comes in. I mean we tried some gross ones first, and then we learned from that. We began to recognize where we had to bin very finely.

Where we could continue to bin very grossly. Because the ultimate results just either were or were not very sensitive to the binning. And so --

DR. KRESS: Once you get a bin, do you have to select a representative set of thermal hydraulics for that bin?

MR. KOLACZKOWSKI: Yes.

DR. KRESS: My question now is how do you do that? There are some differences.

MR. KOLACZKOWSKI: The bin was actually run based on, for example, let me take the case where suppose, let's say, one to four valves does not make that much difference, okay?

What we would do is we would give the TH folks the scenario that they needed to actually run. The worst case, if you will. That is we would say, okay, then if it doesn't make that much difference, let's have them run the scenario as if four valves open and remain open.

And that's the scenario they're going to run and that's the scenario they ran.

MR. ROSEN: So what four valves are you talking about in that case?

MR. KOLACZKOWSKI: Four turbine bypass valves open versus one, for instance. Something like that. Or three ADVs versus one or three main steam safety reliefs versus one.

If it doesn't make that much difference, we had them run the worst case.

DR. KRESS: So binning, and I can conclude from what you say, is a source of conservatism, possibly?

MR. KOLACZKOWSKI: Yeah, where, where certainly once we created a bin to represent that bin we hopefully always tried to represent what we thought was the worst scenario that was still within that bin structure.

DR. KRESS: But then you're going to put an uncertainty band on that to do the uncertainty analysis?

MR. KOLACZKOWSKI: Well, there is an uncertainty about the frequency of that bin, as well --

DR. KRESS: That's on the frequency.

MR. KOLACZKOWSKI: Yeah, on the frequency of the bin. I'm not sure what you're --

DR. KRESS: Well, I was thinking about the thermal hydraulic uncertainty also on that.

MR. KOLACZKOWSKI: Oh, yes.

MR. ROSENTHAL: Let me, Jack Rosenthal, Safety Margins and Systems Analysis Branch and Research. In fact we ran over a hundred RELAP runs for each of the plants. I keep getting back to the fact that we start out at about 550 F and we end up at like two or 300 F and it takes about two hours to get there.

And so you, if you knew nothing but some basic mass and energy constraints and had a calculator, you would draw some sort of line, you know, between those points. And then in another one, and you know, it seems to me, relative to what we think we know about the total, how well we can do the predictions, we're slicing this pie rather fine.

So that I just wouldn't expect that the binning, within so many bins, that you're taking the worst within that bin, but there are so many of the bins that were really, that there's fine distinctions that have meaning.

MR. KOLACZKOWSKI: Let me point though, I mean we did a lot more binning in the early work. I mean the ‘80's work, if you look at how many bins they analyzed to then base their, the original PTS rule.

I mean they were looking at something approaching a dozen bins. We're looking at a hundred and something bins. And so from that perspective, we think we've removed some of the conservatisms.

As an example, in the early work where they might have said whether it was one turbine bypass valve versus four versus a main steamline break, we'll treat it all as if it's a main steamline break.

And therefore we're grossly over estimating the amount of the cooling you'd get with one or two stuck open turbine bypass valves. We've removed that conservatism by saying, well, there is a difference between a main steamline break and one stuck open TBV.

So we'll have a bin that represents one stuck open TBV and we'll have another bin that represent the main steamline break. Okay, I guess, moving on. So we had the bin and then eventually --

DR. BANERJEE: I have one question. Did you sort of make the bins which contributed to risk more fine than the ones that did not?

MR. KOLACZKOWSKI: The answer to that is yes. For example, as I said, LOCAs dominate. And originally we only started off with essentially three LOCAs in our model. A small LOCA, a medium LOCA and a large LOCA.

Small being something representative around two inches or so equivalent in diameter. Medium in the neighborhood of five or so, six inches in equivalent diameter. And then large being something like ten, 12, 14, all the way up to 22 inches actually we looked at.

When we recognized that those were going to dominate the PTS risk, we then took each one of those and further binned them into subsets, having to do with a number of variations that we treated in an uncertainty way, not only the size of the break but the amount of HPI flow.

What if it was 110 percent flow, what if it was only 80 percent flow, in terms of the cold water hitting the downcomer, and so on and so forth. So we binned those yet into further bins because we recognized we needed to be finer because this is where the dominate results were. So the answer is yes.

DR. WALLIS: Now what's happened as a result of your work, it seems to me, is that the order of things has been changed and turned upside down. Large LOCAs that previously were unimportant are now the dominate sequence and so on.

Is that because of something that's been changed in the PRA? Is it something that's been changed in the way --

MR. KOLACZKOWSKI: Go to the slide with the green and red arrows.

DR. WALLIS: -- I can't the materials makes any difference. I mean if it's a bigger challenge, then it's going to be a bigger challenge. And what you've done to refine the materials analysis isn't going to make any difference.

What is it that's turned, that's reversed the order of importance of these events?

MR. KOLACZKOWSKI: We showed you this slide earlier. I mean recognize, we're making a lot of changes from what was originally done in the early work, in the 1980's work.

DR. WALLIS: PFM doesn't do that does it? PFM doesn't change the order of importance of the scenarios?

DR. KIRK: But how the scenarios have been represented to the PFM can, and in terms of the contribution of medium to large break flow because, I think is part of the genesis of your comment.

DR. WALLIS: It leads to that, yes.

DR. KIRK: The fact of the matter is, is that they had been previously excluded a priori, and now they've been included. So --

DR. WALLIS: Had it been included before then the numbers would have been even bigger on that notorious Figure 1.1?

DR. KIRK: I don't wish to get back to 1.1, but yes, yes they would have. So that's why LOCAs are here, is they weren't included before. And when you look at the, when you look at the fracture driving force of the LOCAs relative to the secondary size breaks, relative to everything else, there's no question about it. They are the worst transient.

DR. SHACK: But it's not so much that they weren't included before, it's they are just more dominant because you've credited operator action which has essentially reduced the importance --

DR. KIRK: That, as well. That, as well.

DR. SHACK: I mean that would be the single biggest change, wouldn't it be?

MR. KOLACZKOWSKI: Well, let me point out in the early work, again, a priori, the larger size LOCAs were not even analyzed because at the time there was experimental evidence that they interpreted meant that you had to have considerable pressure for PTS to occur and therefore in large LOCAs, when, you know, very quickly get down to pressures of 100 pounds or whatever, the a priori were not analyzed based on that information.

We said, no, we're not going to start with that premise. We're going to assume that, we were getting evidence that was suggesting maybe the pressure was not as important as perhaps previously thought.

And so as a result we included medium and large break LOCAs in the analysis. They have been processed through the TH and the fracture mechanics and low and behold we're finding out that indeed the LOCAs and the larger size LOCAs are in fact a major contributor to PTS challenge.

So in that case they were a priori not analyzed.

MR. ROSEN: Even though the depressurized the primary system to a large degree?

MR. KOLACZKOWSKI: Even though they depressurized the primary system to a large.

DR. KIRK: And it's also, just as a side note here, but I think relevant to the discussion we're having, because we've, I mean the emergence of medium and large LOCAs as important contributors is, of course, a big change from the past, for reasons we're discussing that it was excluded.

One of the things that, one of the many things we've done to try to understand this is Terry Dickson went back in the Oak Ridge archives and dusted off a circa early 1990's version of a probabilistic fracture mechanics code of the genre that was used in the original assessments.

Put a large LOCA into it and found out that it's predicting the same thing as we've got now, that it's an important transient. So they weren't there before simply because they were excluded, and what Dr. Shack pointed out is also correct.

That previously other events, like secondary side faults, the severity of those is grossly over represented.

MR. ROSEN: So what we think we are at now is a pressurized thermal shock problem with a little P, big T.

MR. HACKETT: A bigger T than a P.

DR. BANERJEE: Is it mainly thermal stress now? Well, what is the driver?

MR. HACKETT: The results would indicate

that this is somewhat akin to where we were, I know we presented it to the Committee a number of years ago the analysis in the BWR world, where you just had the cold over pressure and thermal shock.

But that the thermal shock is, or the thermal piece is more dominant than the pressure piece, is what the results appear to be indicating.

DR. WALLIS: That figure you just eliminated is a very nice one, with the green and the red arrows. If you could put numbers on the range, it would be very revealing. I think you'd find, as I said before, that something like the flow, the change in the flow analysis had a tremendous amount of leverage.

The change in the treatment of TH always had a relatively small affect. And maybe, you know, if you had some numbers on here so we could see how important these things are, rather than just have green and red arrows.

MR. KOLACZKOWSKI: I guess I would just say that I know some of the ongoing work is attempting to do that. Some of it is hard to do. For instance, if you take the second bullet, more refined binning.

I mean to try to put a number on, well, they did ten bins, we did 150, what does that mean numerically? That's hard to come at. Qualitatively we know, we feel we've done a better job.

Because, as I said, we're not combining a one turbine bypass valve scenario in with a main steamline break. We've removed that conservatism. Now exactly what that means in a quantifiable way, is sometimes hard.

DR. SIU: And the short answer is yes. We are certainly going to be looking at trying to quantify that better. It's an important point.

DR. SHACK: I mean you want to quantify the ones where there is uncertainty. And more refined binning is good. You know, how good it is, is you know, now operator action credit, you know, flaws --

MR. KOLACZKOWSKI: Yes, agreed.

DR. SHACK: -- those are things with uncertainties and so when you take big credits for them you'd sort of like to know just how much credit you're really getting out of those things.

MR. KOLACZKOWSKI: Agreed. Okay, I think we're at Step Five, I believe. Okay, so we did some preliminary quantification, we do some binning. As I pointed out, it was really a rather iterative process.

But we did take a point in the process, once we had preliminary results available, that we went back to each of the Licensees and presented the results that we had in a preliminary, at this point in the process, and allowed, not only them, but ourselves to sort of stop, take a look at where we were and essentially ask ourselves where could we be wrong?

What else should we look at? Should the binning be changed? Do we have any inaccuracies in the model? Maybe a dependency we haven't treated right. Or maybe we grossly, in the Licensee's opinion, over estimated or under estimated an operator action credibility or whatever.

We gave them a chance to provide input to us. We actually got formal comments from the Licensees, and then responded to those comments accordingly. So we took a point in the process to stop and see where we were.

And, as I said, get the Licensees, as well as our own chance to take a look at where we were and whether we wanted to change anything. Models were changed. Values were changed as a result of this process. Next slide.

Then we did, based on the changes we made to the model, changes we made to the value. Now we're getting closer to the final results. I guess just a word, a little bit about the uncertainty from the PRA part is concerned.

And I know the Committee has seen a very similar slide to this before. If you think about each scenario, which is now, if you will, a TH bin, from the PRA perspective it's treated as an interaction really of three things.

You have some initiating event, and then you have some series of mitigating equipment successes and failures, like valves sticking open or not or whatever. And then the operator perhaps does or does not take certain actions.

From the PRA perspective, then,. the frequency of the scenario is treated as the frequency of the initiating event times the probability of the equipment response times the probability of the operator actions.

And each of those are treated essentially as a random event. So the model, in it's 181,000 different scenarios are describing the randomness of what can occur, in terms of what initiating event might occur, and then what subsequent equipment and operator responses might be.

So that's all captured in the model development. And that aleatory aspect, the randomness of what might occur and what could go wrong is really handled, not in a number sense, but is handled in the model by different scenarios in the model.

And that, hence, the reason why there is 181,000 different scenarios. For each scenario then you have to develop a frequency. And those frequencies are going to be summed together to represent the frequency of a bin.

Now we're dealing with epistemic uncertainties with regards to what is the actual frequency, are ability to best estimate what the frequency of that scenario is. And to capture those epistemic uncertainties, we put distributions on the frequency initiating event.

Distributions on the probability of the different equipment responses. Distributions through the elicitation process on the probabilities of the operator actions. And essentially propagate those through the entire model using Latin Hypercube sampling techniques to come up with the distribution, if you will, that's primarily capturing the epistemic uncertainties with regards to what is the frequency of each scenario.

DR. WALLIS: So when you hand something over to the next the stage, which is the fracture mechanics, you give them a whole set of these things with the uncertainty distribution on each one? That's a huge amount of information right there.

MR. KOLACZKOWSKI: That is correct. That is correct. Next slide.

DR. BANERJEE: How do you choose the distributions? Is there an empirical basis for this?

MR. KOLACZKOWSKI: Most of the distributions came like from the data source that we were using. Again, the first two plants, Oconee and Beaver Valley, as I pointed out, use a generic data.

That's largely from NUREG/CR-5750, work done by Idaho in which they developed not only mean estimates of things like initiating event frequencies and equipment failure and whatever, but also their estimates on what the distribution should be.

Where there ought to a be a beta distribution, a gamma, whatever. And what those distributions were like. And that information is what was used.

DR. SIU: Alan, if I can interject. It's not that the distributions were necessarily chosen, they are computed. They use the available experiential data, using an aging estimation process.

Now you do have to choose a prior

distribution. In general, they use non-informative prior distributions and then you simply update. And so it's an algebraic process at that point.

Now at some point you generally try to curve fit something that you can readily propagate through the model, but that's a very minor correction point.

DR. BANERJEE: How much data is there? I mean for certain things there might be quite a bit of data, but for others almost nothing, right? I mean there aren't many situations where you have operator actions under certain scenarios.

DR. SIU: That's right.

DR. BANERJEE: How do you choose those distributions?

DR. SIU: Let me distinguish between the two situations. In situations, obviously, where we have equipment failures and we can go through the process I talked about. In cases where you are doing a direct elicitation, now again, it's not a matter of choosing a distribution, per se.

You are asking the elicited experts what is the likelihood that this probability is in this range? So you can envision constructing a histogram, basically. And then you can rough that in a continuous curve to match that histogram or use the histogram directly depending on however you want to propagate that through the model.

So it's not a matter of choosing a particular functional form and saying this is the functional form a priori. You're trying to determine what is the experts belief as to the value of that variable.

What's the likelihood that that variable takes on that value in this range?

DR. BANERJEE: So the expert's opinion takes the place of data here?

DR. SIU: That's correct.

DR. BANERJEE: Okay. And then you do whatever it is to find --

DR. SIU: That's correct. That's right. The rest is, that's right. The rest is --

DR. BANERJEE: Is this procedure sort of laid out in this Idaho report? Or is this something that you've done with that data or expert's opinion in the report?

DR. SIU: Well, there are lots of, I'm not sure exactly the process Idaho used for things like the LOCA break frequencies, but in general the technology of expert elicitation, there are some

NUREG/CRs that we follow. One was written by Professor --

MR. HACKETT: I was going to add, Nathan, I think, just as an example, I think a point where your question is going is you look at large break LOCA. And, of course, there's not a lot of data on large break LOCA. In fact, there isn't any.

Which is a good thing. And so you're stuck in that kind of case with looking at things like expert elicitation or precursors that may have led to a large break LOCA under certain conditions.

And I think I can speak for just having read that portion, especially NUREG/CR-5750, does go into the assumptions that they made in that regard in pretty good detail.

But in some cases, obviously, the data is just not going to be there. You'd rather in every case in this project where you had the data, that's where you want to be. If you don't have the data, you're then looking at statistical methods for extrapolating or interpolating or you're looking at precursors.

Or you're looking at expert elicitation, sort of in a descending order as to, you know, where you'd like to be.

DR. BANERJEE: The main concern is, of course, that for the risk dominant sequences that you have here, they are very rare events. And there is not much data. And I don't know how you really establish, with any confidence, the frequency for these.

Or even for the initiating event, forget everything else.

MR. HACKETT: It's very difficult. When you look at the large break LOCA, that's just one element of this project. But there's also other efforts that the Research Office in NRR are pursuing.

They just had an expert elicitation panel convene this week to look at that particular issue for the reasons you cite. That's just a very difficult scenario.

DR. BANERJEE: Well, to take an example, who would have thought that these lines in the Japanese BWR and the, you know, expert opinion is not a great way to approach this maybe.

I don't know how you do it, but that nobody ever thought of these scenarios that actually occurred.

MR. HACKETT: Yeah, I think these are weaknesses that are inherent in that type of process.

And in that kind of case I think Dr. Rosen's question of earlier sort of, the fundamental situation of, you know, how well do you do with what you don't know?

And of course you do the best you can, is kind of what it comes down to. But in a number of areas the most stark example for the NRC recently, of course, is Davis-Besse.

And if we had all been sitting up here talking to the Committee two years ago and somebody were to have told myself or Dr. Shack or Ford that you were going to eat through a six inch reactor vessel head with boric acid corrosion, you probably would have been in denial over that.

The model, you know, would not have supported that type of view. So that's just fundamentally where you're going of up against the wall and you do the best you can.

DR. SIU: Ed, just to add one minor point here, again. We talk about point estimates sometimes and we treat the distribution as window dressing. But in the LOCA frequency estimate in particular, there are large uncertainties.

So what we're stating is our degree of confidence in the LOCA frequency with which we use in the analysis. And that frequency itself is a representation, as Alan indicated, of random process.

So you're not saying with certainty the event is going to occur, but here's a certain probability it would occur in a time interval. And we're highly uncertain about the governing parameter of that process.

So, again, it's a statement of the knowledge about that that we're trying to make. It's not that we're really confident that the LOCA is going to curve at a certain rate.

MR. ROSEN: I think the important point about operator action on a LOCA has been made. Which is the operators, in an expert elicitation, the operators drill LOCAs ad nauseam.

And the response that they're required to take is uniformly the same. Which is getting to E_{zero}, which is the, which basically confirmed the reactor has tripped, and allow the safety systems to do what they're designed to do.

Monitor what's going on. There is not a lot that they can do. So the issue is really all about initiating a frequency. Well, how often is this going to happen? And surely there, one doesn't know, fortunately, because it hasn't.

And, but as to what the operators would do if it did happen, I think we have a pretty good idea.

DR. WALLIS: Are you sure? Because one of the problems with, the key problem with TMI, one of them was that operators misdiagnosed what was happening.

MR. ROSEN: That wasn't a LOCA.

DR. WALLIS: No, but they could have a LOCA and for some reason that you don't know, they could think it's something else. And this could be because something else is happening in the plant that's distracting or confusing them or something.

MR. ROSEN: I grant that, yes.

DR. WALLIS: This also happened at TMI, several things went wrong simultaneously.

DR. SHACK: I think we're going to have to move on here. We don't want to miss the chance to get on the thermal hydraulics.

(Laughter.)

MR. KOLACZKOWSKI: We're almost there. We're almost there.

MR. ROSEN: But they've only done a hundred runs, so, here we've had 181,000 sequences.

DR. SHACK: I've got view graphs that don't mention Oregon State at all.

MR. KOLACZKOWSKI: Okay, so anyway, a word about the uncertainty analysis, again, a lot of the aleatory, the randomness part is really handled by the model construction.

Then we, to the best we can, put on distributions with regards to all the inputs to the sequences and then carry that through using Latin Hypercube sampling through the model.

Step Seven, then ultimately is really finalizing the results, doing all the final runs, etcetera. And the only thing I wanted to point out here is that, the point that was made earlier.

As we learned what was dominating, we went back and did even better, finer jobs, finer binning, whatever, on the stuff that was going to be important. And as part of that process, those aleatory -- oh, the slide before this one, I'm sorry.

Those aleatory uncertainties that were coming up to be particularly important, not only did we treat them in the model structure, but we also tried to quantify those aleatory uncertainties.

And I've just list some of the more important ones, where we actually tried to put numbers on things that were, quote, random originally as put in the model. And then we tried to associate a number with the probability of that randomness.

What's the chance a LOCA would be a, if you will, a small-small versus what's the chance it would be at the larger end of the small break spectrum. That kind of thing. Next slide.

And as it has already been pointed out by Dr. Wallis. Dr. Wallis, yes, we, basically what you get out of the end of this process is you have a bin that is represented by a series of TH curves, pressure temperature and heat transfer coefficient, primarily.

But also a lot of other information that tags along with that. And you have a frequency of that bin. And that frequency is described by a histogram that comes out of taking all the epistemic uncertainties, the distributions for all the inputs, propagating them through the model and getting an uncertainty on the frequency on the output.

And that was described in terms of quantiles. All that information goes into the fracture mechanics code, which ultimately is going to take this frequency information, which again is not just a mean or a point estimate, but actually a distribution.

Multiply it ultimately by a conditional probability of vessel failure, which is also going to be a distribution, to get a distribution out on the through-wall crack frequency.

DR. WALLIS: This is an amazing piece of work. Now are you going to expect all the Licensees to do the same thing?

MR. KOLACZKOWSKI: Well, I guess as was already pointed out, if the rule is going to be able to change as much as we think we might be able to, you may get to the point where no Licensee will ever be so, their vessels will not be challenged to the point they'll really have to do anything.

If it does not turn out that way and the Licensees would have to do some form of analysis, I'm sure that the NRC, whether it be at this rigor or some other, and I don't want to speak for the NRC.

But I imagine they'd say you've got to address uncertainty somehow.

MR. ROSEN: Have you made reactor vessels immortal?

(Laughter.)

MR. HACKETT: I wouldn't go that far. No, I don't think so.

MR. KOLACZKOWSKI: No. Maybe they're good to 60 years, I don't know if that means they are immortal.

DR. BANERJEE: Until a through-wall crack appears, I guess. Let me ask you, would this process you've taken a thermal hydraulics curve which you haven't moved up or down or put any uncertainties on.

The whole process of converting this into through-wall crack frequency or whatever is highly non-linear. Or apparently some of the correlations look very non-linear to me. So, does it make sense to do that?

I mean without actually putting the uncertainty on and propagating it through the non-linearity so you see whether it amplifies or decreases or whatever.

MR. KOLACZKOWSKI: Well, I guess if you're asking about the thermal hydraulic uncertainties, I'd rather wait for the next part.

DR. BANERJEE: No, no, I'm saying the process.

MR. KOLACZKOWSKI: Oh, the process.

DR. BANERJEE: I'm talking about the process right now. What in detail the thermal hydraulic uncertainties are is the second question. But not taking it into account, let's say here where there may be uncertainty of say 50 percent on the number and the time rate of change of temperature, maybe 100 percent. What effect does that have when you put it through this process?

MR. KOLACZKOWSKI: Can somebody help me, I'm still not understanding the question. I'm sorry.

DR. BANERJEE: Well, let's say that the temperature that you get out of this calculation and the rate of change of temperature has a large uncertainty on it, which is distributed anyway you like.

If you put this through this process, what happens? Then it will contribute to the ultimate uncertainty in the result, but will there be biases, for example, if you average it because it's a non-linear process?

So if you then average something and let's say the fluctuation, let's just take a number and you square it, the RMS is not zero, even though the fluctuation can be zero. So any non-linearity gives you this problem. So how do you handle that?

MR. KOLACZKOWSKI: Can somebody help answer that?

MR. HACKETT: Dave?

DR. SIU: Let me, before Dave starts, let me just take a crack at it because I'm not sure I exactly understand either. But maybe it's down to the time cut that you're taking here and --

DR. SHACK: I think the question is do really hand them three curves or do you hand them a good deal more information --

DR. SIU: Right, yeah, let me address that a bit because I had some hand in developing the uncertainty analysis approach. It's a very important question, and when we briefed the Committee and Subcommittee previously we said we were going to take a good whack at some of these uncertainties, but there were other things that we didn't think that we would be able to do.

Ideally, you'd like to hand a band of traces. Although, when you really try to do that maybe even the image of a band isn't a very good image, because the traces could develop quite differently depending on how you vary your parameters.

But if you just visualize a set of traces, yeah, you'd like to propagate that through. I think Alan's earlier slide indicated we did a little bit of that. We tried to identify what were key parameters and we then developed deterministic traces for those particular variations and assigned probabilities to those particular traces.

So we went a little bit deeper than the original bin definition and tried to create refined bins to accommodate that. But I don't think we have the full method here that says, yes, in principle we will take this band and propagate it all the way through.

Computationally, it would be a pretty extensive task, but of course, that's not reason not to do it. We just weren't able to in the scope of this project.

MR. KOLACZKOWSKI: Again, if the thermal hydraulic response for a scenario was, and again, it's hard to quantify, but noticeably different from some other bin we already had, one turbine bypass valve stuck open versus four.

If the rate of cooling and/or the final temperature we got to looked like it was starting to be ten, 15, 20 degrees difference or something like that, we said, well, let's don't keep these in one bin.

Let's create another bin. And so now we had a TH set of curves that represented the one TBV case, and a different set of TH curves that represented the four TBV case.

DR. WALLIS: I think that the uncertainty that my colleague may be referring to is not how many valves are stuck open, it's actually in the prediction of the RELAP itself.

MR. KOLACZKOWSKI: Right, I understand, yeah.

DR. SIU: The other thing I should of mentioned, obviously, again, we've pointed out before, we need to do some sensitivity analyses. We need to better understand the results of this integrated product that we're representing here.

So some of the things that we're talking about clearly need to be pursued in the coming months.

DR. BANERJEE: I wouldn't be so concerned if the conversion of this to the final result was a linear process that would average out. I don't know what the effect of the non-linearity is. That's what concerns me. That can give you a bias in the average.

DR. SIU: And that's where again, without necessarily carrying the full formalism of a quantified analysis, sensitivity analysis should give us some indication of the relative importance of that.

MR. BESSETTE: But the only way to really get information on that is to run the results through FAVOR. And that tells you, so you have, and every time you run FAVOR, you run it with some specific thermal hydraulic input, of course.

So, what you feed FAVOR as a series of

transients or a series of sensitivity studies of a given transient through FAVOR, and you see how much that effects, say, conditional probability of vessel failure.

DR. BANERJEE: That would be fine. I mean what you're suggesting is okay. I mean it would just parametrize the rate of change of temperature or temperature that you get out of these transients and feed it in.

And it's not a, not a thermal hydraulic calculation, it's just another FAVOR calculation.

MR. BESSETTE: That's right. So, in fact, we're in the process of doing, we haven't completed what we plan to do on that. We're planning to do a number, taking a specific transient and doing a number of perturbations on it, in order to understand better, you know, ten degrees at this point in time is big or small.

You've got to run these through FAVOR in order to answer that question.

DR. SHACK: I mean, I thought that's what Table 2.3 and 2.4 represented. We're doing that sort of thing. You're saying that you haven't done those runs yet.

MR. BESSETTE: Well, we have. See, we've

done thermal hydraulic sensitivity studies in the sense that we've got a given thermal hydraulic bin, let's say, as medium break LOCA.

And we vary, we go through a PIRT process to decide what do we think is the most important boundary condition, city analysis and physical modeling in an analysis. So we've done these sensitivity studies with RELAP so we feed, let's say, 30 RELAP calculations, that's 30 RELAP sensitivity studies of a given transient.

We feed that through FAVOR and we generate the distribution of conditional probability of vessel failure. So, but we're still doing this RELAP calculation.

DR. SHACK: Yeah, but I mean you assign probabilities to those. So in affect you do end up with a distribution. Now it maybe a crude distribution, but it at least begins to answer the question.

MR. BESSETTE: Correct.

DR. SHACK: So we've done that and we've got those results, but we want to go a little bit further. Well, I think you should at least take credit for doing that. That's all I really wanted to do.

DR. BANERJEE: That's what Dr. Siu was saying.

DR. SHACK: Yeah, but I just want to make sure that you've really done it.

MR. BESSETTE: Yeah, I thought that impacts were, something more was being -- we wanted --

DR. BANERJEE: I want something more --

MR. BESSETTE: You want something more --

DR. BANERJEE: But it's a beginning.

MR. BESSETTE: Yes.

MR. KOLACZKOWSKI: I think we're done with the PRA part.

MR. ROSEN: Well, I'm looking at the agenda and I guess we're through Roman one and two, which is the opening remarks and the introduction. I don't know where we are on this agenda now.

DR. SHACK: Oh, we have problems with the agenda, no question about it.

MR. HACKETT: We're most of the way through the background at this point.

DR. WALLIS: We're going to collapse four.

MR. HACKETT: At least we think we are. We were going to propose to collapse four and focus on five. And at this point we'll turn it over to David and Jack Rosenthal.

MR. ROSENTHAL: Yeah, let me just make an introductory remark for the benefit of all the people here. We did meet in December with the Thermal Hydraulic Subcommittee, all day.

And presented a lot of developmental assessment and took a lot of questions. And by the end of that day an independent observer would have been in dismay over what we, how well we were portraying what we knew.

So we did do a little bit of regrouping. One thing was that we had, as I say, just a large amount of developmental assessment which that Subcommittee was hearing for the first time. And we decided we need to write a separate report which people can really sit down and look at rather than just seeing, you know, 150 slides or something in the course of a day.

It's just an enormous amount of information. The second thing is, and it's just the way to go, we focused on where we had problems. Where things weren't, where we weren't predicting results well, rather than where we were.

That's the nature of the beast. And in fact in a trial run with the, with my contractors, I said, no, let's be very forthright and just show it all. But I think that we left the Committee with the wrong impression.

And, in fact, we can calculate downcomer temperatures rather well. And that's what Dave is going to present. So I just wanted to give that perspective.

DR. WALLIS: Jack, the minutes of that meeting show that we were reassured that everything would become clearer on February the 5th.

(Laughter.)

MR. BESSETTE: So I just wanted to, so we had this, one of the questions that was posed to us was how good is RELAP? And not only how good is RELAP by itself, but also the modeling approach which depends upon RELAP for predicting the temperature at a downcomer, which is potentially a multi-dimensional problem.

Which is where the question of plumes comes in. So we went through that on December 11th, including we spent a few hours on the Oregon State Program, and discussing the results and the CFD analysis that was associated with that.

And I think it was, at least for me it was fairly convincing combination of experimental results and analysis. So the, so I was going to go over here briefly --

DR. WALLIS: One of the things we learned, I remember, was there's not just the downcomer, it's actually, there's a lot of heating up of this cold ECC water before it even gets to the downcomer. And that made a big difference.

MR. BESSETTE: Yeah, that's right. It's, you might say it was surprising. Previous experimental programs didn't consider that aspect that you actually get a lot of mixing before the ECC water even gets into the cold leg curving in the injection line. It's almost an amazing amount of mixing.

DR. BANERJEE: But is that a peculiarity of the, well let's have a more generic issue here than that. There were certain things that were shown in the last meeting which we did not know was peculiar to the experiment that was done or was something that would happen in a full scale PWR.

And we suggested to you that you do some CFD runs to see whether that would actually occur in a full scale plant or not using the system that you had set up. Because this was all single face flow.

And the mixing that you saw. Was that done?

MR. BESSETTE: Let's say the work is in

progress. It's not done yet. We had to get additional funding out and graduate students have to be available. So between December 11th and now, no, it's not done yet. But we are working on it.

DR. BANERJEE: So we have no assurance that this is going to occur at full scale, the mixing and the injection line which gave you most of the credit.

MR. BESSETTE: Well, it's, except, well, it was a focus on, when we're getting ready to do Oregon State, it was one of the principle points of focus for the scaling. So there was, it was looked at.

The Froud number and the injection line and the Reynolds number were looked at. And so the injection, the size of the injection line and injection velocity was scaled accordingly. So that definitely was a point of focus on the scaling.

DR. BANERJEE: Well, maybe you should explain the point, which is that they got an enormous amount of credit, not for what was happening in the reactor, but in the injection line itself.

The high pressure flow was sucking in cold water from the cold leg and mixing it in the line. So what was coming out was sort of a mixed flow. That was the major source of mixing.

And because of that liquid coming in to the pipe into which the HPSI was going in was almost at the same temperature. So effectively the thermal shock problem went away in some way because of the mixing in the injection line itself.

MR. ROSEN: This is what, it could be very cold water going in?

DR. BANERJEE: It could be very cold water.

MR. ROSEN: And it mixes with the water in the, existing water in the injection line which could be, what? How hot?

MR. BESSETTE: So, yeah, they could be dealing with 60 degree Fahrenheit water coming in and mixing with, let's see, 400, 500 degree Fahrenheit water.

DR. BANERJEE: But if that 400 degree water doesn't, there's not an infinite amount of it. I mean if there's no flow coming in of new 400 degree water. A whole lot of things have to be right for this to work.

DR. WALLIS: Yeah, well the thing is that this has got to appear in some sort of -- I'm not sure we can go into all of it today. It's clearly got to be wrapped up somehow properly.

MR. BESSETTE: Yeah, so I hadn't planned to go through all that again today. But I just wanted to show you some selected assessment results from RELAP.

DR. WALLIS: This thing that bothered me about this thermal hydraulics is that the PRA guys used some hydraulic information and it seems to be somewhat of a moving reference point.

Because as OSU comes up with some new discovery of how things mix, there's a different thermal hydraulic condition which has got to be then used by the PRA people. And yet they're are already trying to make conclusions about plants based on the old models which OSU is showing they are no longer, not so good.

So are you really mature enough in your thermal hydraulic analysis to give them what they need?

MR. BESSETTE: Well, as I said, I mean, along those lines, this whole question of mixing in a sense, doesn't even arise with the risk dominate sequences, which are fairly significantly sized LOCAs.

For these LOCA sequences basically everything is at saturation. So we're not even concerned about the potential existence of plumes.

DR. BANERJEE: So, I mean what you're saying is the OSU experiments are totally unimportant because they don't address the risk dominate sequences. Is that what you're saying?

MR. BESSETTE: Well, the other way around, if we hadn't done them it would be certainly a whole other story. But as it turns out, yeah. It's, of course when we, three or four years ago, we didn't know large LOCAs were going to be risk dominate.

So we proceeded on the basis we had to understand mixing, we had to make sure we understood mixing well enough to ensure that the FAVOR approach was appropriate.

DR. BANERJEE: Well, I think that there is still the point that if the large scale plant is not well portrayed by the OSU experiments then, and because that is a possibility which maybe you can eliminate with some CFD calculations.

Then it could still be that the plume does not mix well in the large scale plant, and gives you very, very different temperature gradient than a well mixed assumption. So, I mean it's not completely closed, that hole.

MR. BESSETTE: Yeah, so in fact, so in

fact one of your suggestions at the December meeting was to do the full scale calculation where we take the existing Oregon State CFD model and multiply the diameter by seven and the length by four in order to get the full scale.

DR. WALLIS: Expecting to see the same answer.

MR. BESSETTE: You expect to see the same answer, almost by definition.

DR. WALLIS: That sounds trivial. It's like a homework problem, isn't it. Just multiply these variables by four and see what happens. Or ten, or whatever it is.

MR. BESSETTE: Yeah. But we're going to do it anyway, just in case.

MR. HACKETT: Dave, why don't you try and step through the slides and then we'll take any questions as we go and hopefully elaborate where needed.

MR. BESSETTE: Okay, so we use for all the PTS analysis we've used the latest version of RELAP 3.2.2 .gamma. It was released in June of ‘99. We used the following models. The Oconee model dates back to the original IPTS study and it's been updated periodically.

Palisades we utilized a model. We didn't have a RELAP model. We obtained a model that was developed originally by Siemens and we modified that.

For Beaver Valley we took the existing HP Robinson model, which again dates from the IPTS study. And Westinghouse revised, substantially revised that model to make it look like Beaver Valley rather than Robinson.

We added a two-dimensional downcomer and updated the treatment of boundary conditions and set points and operating procedures. So we reviewed the, we did, associated with the current effort, we did some assessment of RELAP for PTS applications.

We went over everything, basically everything we did at the December 11th meeting. We looked at a variety of separate affects for integral system tests. And I was going to show you today some of the integral system test results.

DR. WALLIS: We might be able to make some progress here. I mean we saw in December all the curves and RELAP predictions versus experiments of transients. Which were all very interesting, but didn't really address the question of what's the uncertainty as far as PTS is concerned.

And if you have a bottom line which says you've now evaluated the uncertainties, then we don't need to look at all the curves.

MR. BESSETTE: Well, so, yes, I'm only going to show you a few curves. The bottom line that you seek is only obtainable by feeding these results through FAVOR.

DR. WALLIS: FAVOR takes the results and calculates the uncertainties in the thermal hydraulics?

MR. BESSETTE: In a sense, yes. But what FAVOR does, it tells you if ten degrees is big or small. Or, you know, half a megapascal is a bigger small affect.

DR. WALLIS: It gives you the sensitivity.

MR. BESSETTE: It gives you the sensitivities. Now, so, for example, if you're predicting a 40 degree downcomer temperature you can be off by 100 degrees and it doesn't matter in terms of probability of failure.

But if you're at 200 degrees Fahrenheit, perhaps ten degrees is important. And you don't know that until you run the whole transient through FAVOR. So that, be that as it may, I wanted to give you some indication of stand alone, how well can RELAP predict pressure and temperature.

I don't think there's ever been much of a question about whether these codes can predict pressure. From the first time I saw a comparison in the ‘70's, it always does a good job on pressure.

The other key, one other key aspect to these codes doing things well, and it all goes along with predicting pressure. If you match, the first thing you have to do to get good agreement is to match your boundary conditions.

And they way you start by doing that is to try to get an accurate prediction of a break flow. And that, once you get that, you get the proper, essentially you get a very good agreement of system mass and energy. Next.

So I've picked out three integral system test results to show you. I didn't pick these out because they were the best ones, I just thought these would be the databases that exist for MIST and ROSA and so on.

These seemed to be the most appropriate tests to look at. This is a 4.4 inch break from MIST. MIST is an integral test facility configured to look like a B&W plant. On the left is a comparison of the RELAP predicted temperature and the experimental temperature.

And black is RELAP and the red is that data.

DR. RANSOM: Where is this temperature?

MR. BESSETTE: This is downcomer temperature.

DR. RANSOM: Where? Down the beltline?

MR. BESSETTE: It's in the beltline, yeah. Now, of course viewing the experimental facilities and so you always have limitations. MIST unidowncomer, external downcomer.

So in MIST you only expect to see basically a single temperature. But, in that sense, you know, you can see RELAP has done, what I consider, excellent job of --

DR. BANERJEE: What's the difference in the rate of change of temperature with time? Because that's one of the main concerns, right?

MR. BESSETTE: That's why, well, so that's, in fact, what I mean is that you have to look very carefully at these results in terms of are they important for vessel failure or not?

And the only way you can tell if a rate of change of temperature is important or not is when is it occurring?

DR. BANERJEE: So let's say you took the

data around 3,000 seconds at 2500. The experimental data shows you, and this is typical of many runs in ROSA IV. That the rate of change of temperature is much higher than predicted by RELAP.

In fact, if red is the experimental data. And I notice that in most of the other data that you've shown previously. So how important is the rate of change compared to getting the temperature roughly right?

MR. BESSETTE: Well, you have to know both. Because certainly the -- you have to know the absolute temperature, because that's giving you like the fracture toughness.

DR. BANERJEE: Right. And then it's the rate of change.

MR. BESSETTE: The rate of change is giving it a thermal --

DR. BANERJEE: In fact, we left a question open as to the trying to understand the relative importance of these and these transients. Because the rate of change was not well predicted.

MR. BESSETTE: I don't know if I'd go as far to say as not well predicted.

DR. BANERJEE: Well, it's a factor of two, sometimes three. I don't know what the number is.

DR. WALLIS: Well, it's a factor of about five or more if they are around 200 degrees Fahrenheit.

DR. BANERJEE: It may well not be important, but we need to know that.

MR. HACKETT: I think there are pieces here that are separable. It appears what David was prepared to do was to assess how well the code works in predicting temperature and rate of change of temperature versus an experiment.

And you're asking the much more difficult question. It's a real good question. Is what do those rate of changes, for instance, do when you get into the non-linearities and the FAVOR code.

And I think the short answer to that question is they could be significant. And I think we have more work to do in that area. We may not --

DR. WALLIS: We may not need non-linearities if it's rate of change of temperature that matters. I mean if it's bigger it may, non-linearity or not, it may produce a bigger thermal stress.

MR. HACKETT: Correct, correct.

DR. WALLIS: So that was the question we had in December. Was you can show us all these curves, but how do you extract from them what really is going to have an influence on the PTS answer.

MR. HACKETT: I think the only way you get there is exactly as David said. We need, we have some more work to do with FAVOR. An additional FAVOR runs in terms of some sensitivity studies.

I think Dr. Shack characterized it correctly is that we run some of these to the point that we have, you know, a certain level of confidence in what we've done to put forward a tech basis, but it's not to say we don't have more work to do.

And this is a very good case in point of where we've got some more work to do.

DR. BANERJEE: We came back in December and said that obviously there's going to be an uncertainty in the predictions with RELAP. This needs to be quantified and hopefully University of Maryland or some other organization was doing that, looking at the comparison systematically between RELAP and the experiments, quantifying the uncertainty, trying to understand what in fact that has on all these sort of results that are coming out.

Now, we haven't seen the uncertainty analysis yet. We were, in fact, one of the points we made is that we wanted to have that at this meeting.

DR. RANSOM: Well, one thing along that line I didn't understand, reading this document it seemed like often times you were feeding the FAVOR the average of wall temperature over maybe periods like 10,000 seconds.

And I don't quite understand. Maybe I misunderstood something, but it seemed like often times you were extracting out of the RELAP five runs an average wall temperature over a long period of time.

MR. BESSETTE: No, actually what we feed FAVOR are, or what we have fed FAVOR is points every 30 seconds. What that 10,000 second you are referring to is like a screening step that University of Maryland used in looking at the results.

DR. RANSOM: What, you go through a preliminary kind of screening and then --

MR. BESSETTE: Preliminary, yes.

DR. RANSOM: -- select the worst --

MR. BESSETTE: Yes, it was just used, so it was just used as a screening step. It was never fed into FAVOR. What we feed into FAVOR is 30 second intervals.

DR. WALLIS: Well, I think, David,

actually the picture is much better than it may appear from the questioning. I think probably there is a really good case that you have. It just needs to be presented in a more convincing way. That's all.

MR. HACKETT: I think, Dr. Wallis, we agree. I don't think we are prepared to go into that in detail today, obviously, in terms of FAVOR. In your proposal to maybe, you have been through these before with the comparisons, say with ROSA and MIST.

DR. WALLIS: They don't determine anything.

MR. HACKETT: Maybe we could just go to the PFM --

DR. WALLIS: I mean you see that there are curves and yes there are some wiggles are not explained, but we don't know what that means.

MR. HACKETT: We do not right now.

DR. WALLIS: And the problem with the NUREG is that at the end of Section 3.1 it says that assessment results confirm the applicability of RELAP V to analyze PTS transients. Well, yeah, that's okay.

And to establish the validity of uncertainty studies. Now there's no uncertainty study presented, so I don't know what that means. Because I don't, I don't know what's being established as valid because there is no uncertainty study in the report.

So, that's the basic problem we have, I think. And maybe you can clear that up?

MR. HACKETT: No, I think what that boils down to is a fairly major take away for us. And, as I stated earlier, this is by no means a final product at this point. And that was one of the, the sensitivity analyses that needs to be, that needs to be further explored and finalized.

So, what I would propose at this point, since we don't want to waste the Committee's time in that regard, these are results that have been shared --

DR. WALLIS: Well, maybe this is like Number 40 or something that's good. I mean it's talking about differences between RELAP V and experiment. What are the kinds of errors. That is actually, is that something that's new?

MR. BESSETTE: Yes, that's just something we did after the December 11th meeting.

DR. WALLIS: Does that help us then with this conversation?

MR. BESSETTE: To some extent. It's not, I would say again, it's not, it can't be the final word because the final word is only obtained after you run these results through FAVOR.

But what this says, though, is that, in terms of stand alone RELAP assessment, you get very good agreement between RELAP and the data for these principle parameters.

So in one case, for example, for ROSA AP-CL-09, you have a bias of zero with a substandard deviation. So you say --

DR. WALLIS: It could be zero even if you have a huge variation.

MR. BESSETTE: That's true. That's where the standard deviation comes in.

DR. RANSOM: Are these means over time? These are means over time?

MR. BESSETTE: This is over the time of the whole transient. So, basically what this says is I can't conceive of doing any better than this with a thermal hydraulic code.

DR. WALLIS: The question is, is it good enough?

DR. RANSOM: Well, I think, too, there may be confusion in the report between sensitivity and uncertainty. You know, I think you did some sensitivity studies to see how much variation you would expect in the parameters.

But that doesn't necessarily answer these questions with regard to uncertainty. That's more of a probabilistic question.

MR. BESSETTE: Well, so, you know, the final, when you get, when you see the final answers you get from FAVOR with the mean and some 95th percentile, those incorporate, quote, the thermal hydraulic uncertainties.

This thermal hydraulic uncertainties are in that uncertainty bin. How do we get these thermal hydraulic uncertainties is, like I said, we went through a PIRT process and we did ranging of the most important parameters and the physical models to generate discreet RELAP predictions which are then fed individually through FAVOR and generate a distribution of probability of vessel failure.

DR. RANSOM: By ranging, you mean that these were the ranges of uncertainty in those parameters?

MR. BESSETTE: Yes.

DR. WALLIS: Well, I think we may be giving you a difficult time about something which actually has very little influence on the final answer. But I don't know that.

MR. HACKETT: I think the questions that have been posed are fair and ones we have to pursue. And particularly with regard to these variations in rate of change in the temperature feeding into the FAVOR code.

That's a take away for us and we'd been working on that prior to this. But we need to come back to the Committee next time around, whenever that is, with, you know, a more definitive answer in that regard.

What I was going to propose is Mark just mentioned to me here, we have five or six more slides to go through on the overall process for probabilistic fracture mechanics, and then we might be at a good break point.

I'd propose that to the Chairman, if that's reasonable we'll proceed that way.

DR. SHACK: That's fine.

DR. BANERJEE: Can we also request a thermal hydraulic uncertainty analysis at some point? We did that before.

MR. HACKETT: Absolutely.

DR. RANSOM: Well, one thing that I'm --

MR. BESSETTE: It's difficult to tell you definitively about thermal hydraulic uncertainties in a stand alone basis. Because you can only tell if they're important, they are of relative important after you get the FAVOR output.

MR. HACKETT: Well, that's pretty much true of most every variable in the project.

DR. SHACK: I think what you need is a clearer explanation of how you've incorporated your thermal hydraulic uncertainties into the FAVOR analysis, because I think they are there.

MR. BESSETTE: They are there.

DR. SHACK: You're just not doing a very good job of making clear to us that they are.

MR. ROSENTHAL: In the sense that you've ranged variables within sequences and you've run hundreds of sequences.

DR. SHACK: What I think you need to do is to show that the ranging that you've done sort of covers, you know, we need to see some of those outputs to show that they would, they give you differences in slopes, differences in temperatures.

You've got some that, some of the ranging is sort of parametric things that just cover, but then you've got other things that cover model uncertainty. I think you have to show us just how much difference those have made.

DR. WALLIS: Maybe it's effect on K-applied and it's trivial.

DR. RANSOM: Well, one thing I think that, I know I was always fairly uncertain about before when I heard these results is the ability of, say, a code like RELAP 5 to predict the heat transfer coefficient.

I mean these are pretty hard things to predict very accurately, which presumably would affect the thermal transient. But the analysis like shown in this University of Maryland report, shows that the BL number is high enough that really the heat transfer coefficient is immaterial.

It's really the thermal diffusion in the wall that's important. And that takes a lot of the uncertainty out of the ability. And the only thing you really are left with is the pressure and temperature. And so I think you can capitalize on that.

DR. WALLIS: And you have to ask whether a very big temperature gradient for a relatively short time is going to be a big action grading a crack or not. Because that's the kind of thing that does happen when you compare RELAP with experiment.

DR. KIRK: Probabilistic fracture mechanics in six slides or so. Okay, all, we all know the PRA goes through TH and then comes into PFM. To expand PFM a little bit more in terms of what's inside the box, and again, of course in the report it goes into even greater detail.

The thermal hydraulic pressure and temperature and indeed heat transfer coefficient is passed in to what we've called an embrittlement and crack initiation model.

Other major inputs to that model include the flaw distribution, which describes the density of the flaws throughout the material. Their locations. Their orientation with respect to the vessel major axes, length, depth and so on.

DR. WALLIS: Are you going to talk about that today later?

DR. KIRK: In one slide.

DR. WALLIS: In one slide. Because that flow is a big actor and it's a big change from what you did before.

DR. KIRK: Yes, absolutely. And we can go into more details in one slide, certainly. Another input is the fluence and its variation around the vessel. And, of course, the material properties and composition information.

All of that goes into the crack initiation model and we predict out of that the conditional probability that a crack will initiate. It then goes into an arrest model and we perform a through-wall crack initiation run arrest, re-initiation re-arrest and so on, until either the crack stops through the end of the transient or we break the vessel.

That gives us a conditional probability through all cracking which, again, we just simply --

DR. WALLIS: How frequently does it stop in the middle of the wall?

DR. KIRK: Quite a bit.

DR. WALLIS: Quite a bit.

DR. KIRK: The separation between conditional probability of initiation and conditional probability of failure, order of merit is about an order of magnitude. So only about ten percent, and of course that varies transient by transient.

But only about, in bulk, only about ten percent of the cracks make it through.

DR. WALLIS: This may save you from some of the rapid, local transients. You may start a crack and then you just stop again.

DR. KIRK: Yes, yes.

DR. FORD: Mark, on that item, this was brought up at one of our earlier meetings. Do we have a good factual basis for the fluence attenuation through thickness of the wall that will impact on crack arrest?

DR. KIRK: The, actually I'm going to look straight at Stan Rosinski from EPRI, who is hiding from me now. Because Stan heard your comment at an earlier meeting and actually, recently, well recently, last summer EPRI published a very nice report on attenuation, it's influence on the embrittlement function and so on.

And I'll give you my short summary because I read it recently. Is that the attenuation function in Reg Guide 1.99, Rev. 2, while certainly I think we would all agree we would like to see a better physical and databases for it, is about the best we have right now.

And it's certainly not way out of bounds and I think is generally viewed as being conservative. And that review was conducted by Colin English of AEA. Who else was an author, Stan? Stan?

MR. ROSINSKI: Yes, this is Stan Rosinski from EPRI. Colin English was one of the main reviewers, but we also utilized information in that report that was performed by Ray Nicholson of the UK as well, from the Atomic Energy Authority.

DR. KIRK: The other thing to point out is that, so we've adopted, quite independently of the EPRI report, but we adopted the Reg Guide 1.99, Rev. 2, attenuation function. And I think if you ask me for a technical basis for choosing that, I'm going to reference the EPRI Report because it is indeed very good and I learned a lot.

I think the other thing is important in a PTS context to recognize that the flaws that get you are within ten percent of the inner diameter, within the first ten percent of the thickness.

And within that range, the attenuation function doesn't really make that big a contribution. However, if we get to ever discussing heat up and cool down limits in Appendix G, where you have to attenuate, or at least now notionally you attenuate to the quarter-T and three quarter-T, it makes a heck a lot of difference.

So, I think, it's certainly a factor. But in PTS, because of, because of where the flaws reside it's not as big a factor.

DR. FORD: Okay, so there are data to support whatever algorithm you have?

DR. KIRK: Yes.

DR. WALLIS: Now, Mark, can I ask you

about the stainless steel liner? Isn't there a stainless steel liner in these vessels?

DR. KIRK: That's correct.

DR. WALLIS: And all this discussion is about the vessel, the flaw distribution in the main steel of the vessel?

DR. KIRK: Yes.

DR. WALLIS: But in a transient, the stainless steel liner undergoes transients, does it crack?

DR. KIRK: The, okay, a couple of things to say. The stainless steel liner is included in our analysis in several senses. There is a residual stress distribution due to the weld overlay that's incorporated into our analysis.

There are stresses caused by the differential thermal expansion of the stainless steel relative to the ferritic steel that are also incorporate into our analysis. If a flaw is completely buried in the stainless steel, we don't calculate its influence --

DR. WALLIS: The stainless steel is bonded to the, weld to --

DR. KIRK: Weld overlay, yeah.

DR. WALLIS: Isn't there a source of flaws in that weld overlay?

DR. KIRK: Yes, indeed there is, and those are incorporated. Yes. The major contribution of the stainless steel is it's the only origin of surface cracks in our analyses. Because the flaw distribution work performed by PNNL showed that the only, well, they actually never really found a flaw that was all the way through.

They found, I think, one flaw that was 50 percent and one flaw that was 70 percent of the way through the stainless steel liner. And those were lack of inner run fusion between the weld beads.

And so, now here is, I'll reveal a buried conservatism in the analysis, to spite the fact that we haven't observed one, we took that as evidence that there is a non-negligible probability that you could get a lack of inner run fusion defect between two adjacent weld beads in the stainless steel cladding and that that could produce a surface-breaking defect in the vessel.

And those are indeed the only surface-breaking defects that are incorporated in it. Even though they are circumferential, where they are included they do make a small contribution to the conditional probability vessel failure on the order of five percent.

DR. WALLIS: Are you going to talk about the surface-breaking defect, the lack of surface-breaking defect from any other cause?

DR. KIRK: Yes, there's the, again, the work on flaw distribution found that there's no, well, first off, there's no empirical basis whatsoever for a surface-breaking defect. Nobody has found one.

Moreover, the work found that there was no physical basis for a surface-breaking defect save the lack of inner run fusion between --

DR. WALLIS: Is it because of the way the vessel is made, it only has flaws inside and not on the surface?

DR. KIRK: If they are on the surface of the ferritic steel, they will have been overlaid and therefore will now be buried --

DR. WALLIS: Or they've been removed in some way.

DR. KIRK: Yes.

DR. FORD: The point is, Mark, you just said you have in fact taken into account a surface-breaking defect.

DR. KIRK: Yes, yes indeed.

DR. FORD: And it happens to be from the

austenitic.

DR. KIRK: Yes.

DR. FORD: So, and it doesn't really matter whether it's in the austenitic or ferritic.

DR. KIRK: Well, the defect is assumed to fully penetrate the austenitic cladding and so its tip is in the ferritic material. And so it's treated as if it's in the ferritic steel.

DR. FORD: Okay, so you have done that?

DR. KIRK: Yeah, yeah.

DR. RANSOM: The experimental data that's used, that was taken at Oak Ridge on thermal stress and vessels, are those clad in the same way so they were typical of reactor wall?

DR. KIRK: I'm sorry, you've lost me. Could you repeat that?

DR. RANSOM: Well, the thick-walled vessel experiments that were made at Oak Ridge for thermal shock.

DR. KIRK: Right, right, yes.

DR. RANSOM: Were those, did they have typical clad walls like this vessel?

DR. KIRK: No, but our thermal stresses don't come from those analyses. Our thermal stresses are calculated from the thermal hydraulic and the conduction equation, yeah.

DR. RANSOM: Sure. But on the other hand, some of this nil ductility data came from those experiments, didn't it?

DR. KIRK: The NDT data comes from material-specific tests on each individual plant, and also laboratory experiments, yes. I'm afraid I'm not answering your question.

DR. WALLIS: It didn't come from Oak Ridge, the experiments. It comes from individual plant tests.

DR. KIRK: It comes from -- the data -- okay.

DR. RANSOM: Well, how were those vessel test used? Just to verify the models?

MR. HACKETT: It comes from, Mark is right. It comes from a variety of sources. When you're looking at in the, early on today we had the discussion about the regulatory application of this.

In regulatory sense, all of the plants have, by virtue of NRC's Generic Letter 92-01, have had to report their data that applies to this situation in terms of RT_{NDT}, fluence affects, limiting materials.

In addition to that, the NRC Research

Office, over many years past now, conducted confirmatory tests at Oak Ridge and other locations to say prototypically in a lab what would happen.

You know, I have this material, I applied this thermal shock to this scaled vessel, and what, how is, what sort of crack behavior or material behavior am I going to see. So they were intended to be confirmatory tests.

DR. KIRK: The, to answer the question you just asked, the vessel tests that were conducted at Oak Ridge were really used to validate that linear lasting fracture mechanics is an appropriate technology to apply to pressurized thermal shock situations. So a prototypical experiment.

DR. RANSOM: The type of flaw and things like that, that they, some of them I think they actually made flaws in the wall.

DR. KIRK: In all cases, yeah.

DR. RANSOM: But they may not have been typical of what you might find in a reactor?

DR. KIRK: No, those were laboratory generated flaws. The characterization of flaws that are typical of what you would find in a reactor came out of the flaw distribution work that was conducted at the Pacific Northwest National Lab where they, both non-destructive and destructively evaluated primarily welds, but also have done works on plates, forgings and the stainless steel liner that we were just talking about.

This is the summary slide on probabilistic fracture mechanics. And in particular we're focusing on the changes made in this analysis relative to the analysis that was used to establish the current rules on pressurized thermal shock.

I'll go through this and --

DR. WALLIS: Mark, I'm sorry, I've got to ask you about the presentation in this NUREG.

DR. KIRK: Yes.

DR. WALLIS: When you start reading and there's nothing about heat transfer, there's nothing about thermal transients and stress distribution in the wall. There's nothing about how thermal shock occurs.

And you never, you get the impression that you're never going to find out. And then you have to get to an obscure discussion in the middle of the discussion which is entitled Oak Ridge experiments to find out that, yes, someone does actually investigate crack driving forces and how it propagates through the wall.

So within the context of Oak Ridge experiments. Put that out front and say we really understand how cracks propagate and arrest. And give that theory some prominence in the report instead of hiding in this discussion of the Oak Ridge tests, which someone might just skip over.

DR. KIRK: Yes. Okay.

DR. WALLIS: I got much more reassured when I saw, yes, someone does understand these things.

DR. KIRK: And they actually were co-oped on the report. That must have been very reassuring. Again, here on the slide, and we've had full day discussions with this Committee on PFM, so I don't want to, unless you ask, revisit all that.

But I did want to focus on the major changes and then I've got a slide each on the ones that make the most difference. We'll start at the end with flaws, since we've been discussing that.

Our statistical distributions of flaws where we indeed do a count for our uncertainty or lack of complete knowledge in the flaw distribution. First off, it's based on significantly more data than was available before.

As we've already pointed, also, most, and by most I mean like 98 percent of the flaws are now embedded rather than surface flaws. And that's a major difference. However, there are many more flaws than there were before.

Our models now have a flaw density that is scaled to either the volume of the material or the area of the weld, as appropriate to the flaw type. And that results in somewhere on the order of two to six thousand flaws being simulated in each and every vessel.

That can be contrasted with the six flaws that were simulated in every vessel in the original PTS work.

DR. SHACK: Mark, do you know from a sensitivity study, just how much, you know, there's this quoted factor of 20 and 70 for the difference. How much of that is due to the fact that you don't have everything stuffed on the surface?

Is really the difference in the sizes less important than the fact that they're not surface-breaking anymore?

DR. KIRK: I'll ask Terry if he knows the answer to that question. My gut feel is yes, but I don't have a calculation to back that up.

MR. DICKSON: Terry Dickson, Oak Ridge National Laboratory. The simple answer is no. We, when I did that sensitivity analysis the, paper that you are referencing, I just bundled it together and did the analysis.

DR. SHACK: Everything this is in there.

MR. DICKSON: Yeah, yeah.

DR. SHACK: So you don't know independently --

MR. DICKSON: No.

DR. SHACK: -- how much is just due to the fact that they are not surface breaking any more.

MR. DICKSON: No, no. But my intuition would say that the surface breaking was the major, the dominant contributor. But I can't absolutely say for sure, because I didn't do the analysis.

DR. KIRK: Maybe there's another sensitivity study.

MR. DICKSON: There you go.

DR. KIRK: Certainly it would keep Mr. Strosnider happy.

DR. SHACK: Well, I think, in a sense, you know, there is less uncertainty in knowing that the flaws aren't all sitting on the surface than there is in the flaw size distribution.

DR. KIRK: That's right, that's right.

DR. SHACK: So if you could show that the location of the flaw really drives this all, then it's a warm feeling.

DR. KIRK: That's a good point. That's a good point. Also, one thing that, sub-bullet under flaws that isn't on the slide, but when we get to discussing embrittlement metrics will be very important, is the understanding both empirically and from an understanding of the physics of flaw formation, that the flaws, the big flaws here are of course the weld flaws.

The flaws associated with welds. And our inspection have revealed that most of those flaws, like on the order of 95 to 98 percent are fusion line flaws. And so that gives us a lot of information about the orientation of the flaws.

So axial welds may only have axial flaws. Circumferential welds may only have circumferential flaws. And as a preview, this is going to lead to a considerable diminution of the importance of the level of embrittlement of the circumferential weld, because it may only have circumferential flaws.

So that one piece of evidence, which again is empirical, but backed up very easily by an understanding of how flaws form in welds, is an extremely important insight.

DR. WALLIS: Mark, on flaws, I'm reading from your report. It says it was decided to adopt for further calculations flaw density space only on observations of the Shoreham vessel.

Now, I just wonder how typical a Shoreham vessel is. And vessels are made by different manufacturers, different welders actually weld these welds that are the source of many of the flaws.

DR. KIRK: Yes, yes. That's a very good point. The decision to adopt the flaw distribution from the Shoreham vessel as effectively the flaw distribution in every vessel was driven by the fact that we had basically two flaw distributions.

One from our Shoreham inspections, one from PV Ruff, and that the Shoreham was the worst of the two. It had, by and large, larger flaws and more of them. However, it's just a factual statement at this time.

We don't have a model that enables us to say how that would relate to any other vessel.

DR. WALLIS: But if flaws are caused by welding --

DR. KIRK: Yes.

DR. WALLIS: -- is welding really something, is that reproducible between one welder and another welder?

MR. HACKETT: A couple of comments we could make there. In the case of the large welds and the reactor vessels, probably the answer is yes. Particularly within the range of a manufacturer because these are automated processes.

In that case it would be submerged arc welding. Good and bad then, if something were to go wrong it would go wrong everywhere. But the good news is that it is a highly controlled process through nuclear fabrication QA.

And chances are, and everything we've seen says they are very well made. And to go beyond that, if you wanted to, again, this whole notion of where we have data and where we have to extrapolate, we do have a code, an expert code that comes to us from Rolls Royce in the UK called PRODIGAL.

That's basically a weld expert code. That if you're looking at I've got this particular weld process or I even have a welder laying it down a certain way and I want to see, in terms of a multi-pass weld, like goes into these vessels, what sort of defect distribution would I expect.

We do have a program that can predict those kinds of distributions. And we have run simulations with that code versus the data, and again, we get some pretty good comparisons. As Mark is indicating, the best data we have is from Shoreham.

But then of course we've had discussions with Jack Strosnider and others internally over how well that represents all vessels.

MR. ROSEN: The BWR vessel.

DR. KIRK: Exactly. So you do have, we've sampled a limited amount of welds. It's the best data that we have. There are obviously miles of welds probably that are in vessels in this country and worldwide.

So you're obviously, you know, having to adjust for that, you know, and you should do it in uncertainty space.

DR. WALLIS: Well, at least you know there is a variation because PV Ruff and Shoreham don't have the same distributions.

MR. HACKETT: Yes, right.

DR. KIRK: And they were in fact the same manufacturer.

DR. WALLIS: How big is that difference?

DR. KIRK: I'd have to go back to the data. I don't remember.

DR. WALLIS: Well, you're claiming one of them is typical of all and then you've got another one that's different. What should I conclude?

DR. KIRK: Well, the, the, maybe I've been a little cavalier in my statement. The, the, in some ways the distributional characteristics were established using both data sets, but the density, it was the density, I'm sorry, I misspoke.

DR. WALLIS: Yeah, the density is the one you relied on Shoreham for.

DR. KIRK: That's right.

DR. SHACK: Just another detail. Why don't the, the percentile, you have the Figure 2.18 where you have the small flaws and there's not a neat spread in the percentiles. The curves are actually different shapes as I go through.

You know, the other flaws, you know, when I go to the fifth percentile to the 95th, I get exactly what I think, you know. The flaws sort of go smoothly. And here the percentiles interchange the shapes. How did that come out?

DR. KIRK: I'll have to take a bye on that one, I don't know.

MR. HACKETT: I don't have a good answer to that either, Bill. We'll have to take that away and get back with you. One more comment I'd make just to the welding in general.

Of course, these types of realizations for fabricators and welding engineers have gone into this type of construction for a long time. So there is the realization in that case that in terms of welding, very often the worst case you get into is the root passes of welds.

And in a lot of cases in these vessel fabrication issues, the root passes are in the center of the wall. So they are in one of the more benign, it's not the case everywhere. But in a lot of cases the submerged arc welding is done such that the root pass is actually in the center of the vessel which is, vessel wall which is about one of the most benign places you're going to have it, you know, for this type of scenario.

DR. KIRK: And moreover it's ground out.

MR. HACKETT: That's right.

DR. KIRK: In areas other than flaws, in fluence we've used the calculational methodology expressed in our NUREG Guide. And the major change in our representation of fluence, relative to how we represented it before, is we recognized the spatial variation in fluence whereas previous analyses assumed that the maximum fluence existed throughout the vessel which is an obvious over conservatism.

In the area of toughness, we've made the bold leap to recognize that RT_{NDT} is a conservative representation of the index temperature, not the index temperature itself. And not a precise representation of toughness.

So we've statistically removed that conservative bias. We've also adopted a model describing the aleatory nature of toughness, uncertainty and both crack arrest and crack initiation.

Our embrittlement model is referenced to both toughness data and a physical understanding of the factors that cause embrittlement. So we've got a correlation with a much better empirical basis than before and some physical basis.

And also the slight bias, the slight differences between Sharpy shift and toughness shift have been eliminated, although that was not a major factor. Just to emphasize, you know, the question always comes back of how big are the green arrows?

And has been widely recognized, we don't have a complete answer on that, but I would like to point out that some of the arrows are bigger than others. And the one related to removal of the systematic conservative bias in RT_{NDT}, is indeed a pretty big arrow on the, on the graph on the bottom of this slide.

It quantifies that bias and shows that, in general, or on average I should say, RT_{NDT} is 65 degrees Fahrenheit higher than the true transition temperature. But that varies over quite a large range.

DR. WALLIS: Isn't that because T-zero is really a best estimate, as opposed to trying to understand how to correlate this toughness. I mean RT_{NDT} is an ASME conservative bounding sort of curve that's for design purposes. It's a different purpose altogether.

DR. KIRK: That's right. That's absolutely right.

DR. WALLIS: That doesn't come out in the introduction. And you want it to read that, and it says RT_{NDT} is a way to characterize toughness. It's not. It's really a way to conservatively describe toughness. It's quite different from trying to really predict what it is.

DR. KIRK: Yeah, yeah. But in fact, and you're right and that can be, can certainly be better described. But the difference here is more than just the difference between a bounding curve and a best estimate curve.

DR. WALLIS: You get a couple of these T-zeros and RT_{NDT}'s and all your criteria and methods to be based on an effective or modified or somehow done something with RT_{NDT}. And yet when it comes to the effect of radiation on embrittlement, in your Appendix, the effect is an effect on T-zero.

I don't understand how you translate the T-zero effect that you are predicting from embrittlement on to your RT_{NDT} frame work for analyzing common PTS. But that comes much later. But again --

DR. KIRK: Well, that comes from a --

DR. WALLIS: When you've got two different variables meaning different things but they are sort of correlated with each other.

DR. KIRK: Yeah, that's comes, the shift used in the RT_{NDT} model has always been the shift in the 30 foot pound sharpy transition.

DR. WALLIS: So that's the connection, that's the connection.

DR. KIRK: That's the connection.

DR. WALLIS: So you calculate your delta T-zero and then you get a delta TR-30.

DR. KIRK: That's correct.

DR. WALLIS: And then you go, that step is not, I think specifically brought out in that Appendix. It just says how you modified it to zero.

DR. KIRK: Okay.

DR. SHACK: Just a note on your presentation in Section 2314, you're very careful to put the epistemic air in the initial RD_{NDT}, but then the irradiation model is presented deterministically.

DR. KIRK: That's correct.

DR. WALLIS: I see, the irradiation was even more confusing because it says randomly select something and that's your best estimate. I couldn't quite understand that at all. How do we get these details to you? Do we send them our comments or what?

MR. HACKETT: That was one of the reasons for the request for the letter, not to over, put over much burden in Committee.

DR. WALLIS: A letter -- give you a hundred different comments on a report.

MR. HACKETT: We'd be happy to take those anyway you feel is most appropriate. In one-on-one sessions or anything.

DR. KIRK: E-mail, marked up copy.

DR. FORD: Mark, one of the questions that came out again in one of the earlier meetings was this question about the Eason correlation for the composition effects.

DR. KIRK: Yes.

DR. FORD: How happy do you feel about that? I mean if you have relationships where the correlation factor is pretty well zero, how do you, how do you put that into an uncertainty model.

DR. KIRK: I'm sorry, I was overwhelmed by your question about how I felt about it. So could try again and I'll try to recover.

DR. FORD: Well, the uncertainty that you have associated with the Eason correlation and the composition effects.

DR. KIRK: yes.

DR. FORD: How overwhelming are those on your end result? I get the feeling that it doesn't really matter too much. As scientists we can't really put too much faith in these correlations.

But in the end, is your answer, in the end it doesn't really matter?

DR. KIRK: Is, is, I'm sorry, is your question still, is your question, does the specifics of the embrittlement correlation matter much to the answer?

DR. FORD: Correct.

DR. KIRK: I don't think so, but I haven't proved that yet.

DR. FORD: Okay.

DR. KIRK: And the reason that I don't think so is that to get anywhere near, it might monkey around with the relationship between through-wall cracking frequency and RT_{NDT} whatever you want to call it, at lower levels of embrittlement when you're not on the flat part of the embrittlement curve.

But once you get up to any type of yearly frequency that anybody cares about, I would believe that the, the materials that are getting you and the cracks that are getting you are so embrittled that you can pick this correlation, you can pick the new ASTM correlation, and it's not going to make a huge difference.

DR. FORD: Okay.

MR. HACKETT: And I'll just add, that's not to say at all that there isn't, wasn't or isn't still significant controversy over the elements of that model. And I think our colleagues here from the industry would, you know, we could have a day-long session on that at least on the elements that go into that and their significance or lack of it.

DR. KIRK: Yeah. And to just be complete, so that Stan doesn't jump out of his skin, it should also be pointed out that while I've now, based on, in response to your questions, pooh-poohed the importance of either getting the attenuation function very precisely right, or getting the embrittlement correlation very precisely right in PTS.

You know, when screening at a yearly limit that's relevant to a regulatory agency. Both of those things are of the utmost importance when setting operational limits. And so when we, as we start looking at risk informing Appendix G, those are going to be very key issues.

And a good point from Dr. Wallis about comparing Rt_{NDT} to T-zero and one is a lower bound and one is a best estimate. So we can certainly tighten that up. Having said that, this correction represents at least an order of magnitude in the yearly through-wall cracking frequency.

The flaws themselves, we've already quoted the factor of 20 to 70. And there are many differences between the old Marshall flaw distribution and our current one. One thing, of course, is that our new distribution has many more flaws, but they are all smaller, they are mostly buried and that the weld flaws are along the fusion lines.

Those combine to make a very significant effect.

DR. WALLIS: This, to the uninitiated, looks impressive. I mean you've got probability, which is not all that small, having a ten percent wall flaw?

DR. KIRK: Yes.

DR. WALLIS: What do you mean by flaw there? It's a crack? It's an absence of bonding between --

DR. KIRK: Yeah, see, everything here has been modeled.

DR. WALLIS: I want to ask you what a crack is, because I once asked a Ph.D. student what a, in his final presentation, what a crack was, and he couldn't tell me. So, --

DR. KIRK: The absence of metal?

DR. WALLIS: No, no, defining what a real crack is, is not easy.

(Laughter.)

DR. KIRK: And anything else my mother told me not to say in public.

DR. WALLIS: What's in the flaw that there's nothing, there has got to be something in there. It says it's a space with nothing there?

MR. HACKETT: This is another one of those, this is probably another area of some buried conservatism and the fact, as Mark said, these are modeled as fracture mechanics sharp flaws.

DR. WALLIS: So ideally, they are the worst thing you could think of, or something?

MR. HACKETT: They would be, they would be, what they are is fatigue cracks in laboratory specimens. And so they are very sharp.

DR. WALLIS: So they have a leading edge which really accentuates the stress distribution around that.

MR. HACKETT: That's correct. When in all actuality, if they are weld flaws, they are very unlikely to look like that.

DR. WALLIS: And they don't run into other flaws or anything like that. Nothing gets complicated. You get the worst possible thing.

DR. KIRK: That's right.

DR. WALLIS: It's like a sword going through.

DR. KIRK: The conversion between the data that was taken and it's mathematical representation has been to assume that everything is, as Ed said, a fatigue crack or anatomically sharp crack which is, you know, clearly everything is not that and so there's, you know, there is a buried conservatism or a buried margin.

Having said that, you know, this improvement, again, is a significant factor in driving the through-wall cracking frequencies. This we've mentioned before and is indeed is something we haven't quantified, but you can see from the variation of fluence around the vessel, particularly azimuthally, that only very limited regions of the vessel experience the peak fluence where you would have the very high levels of embrittlement.

And if by, so by representing the vessel in a realistic way, we stay away from being so grossly conservative.

DR. WALLIS: And the thermal hydraulic analysis gets based on the fluid being well mixed by the time it gets to the 24 inches --

DR. KIRK: That's correct. That's correct. So we've got a, essentially a, well, I'm not sure how you do that. We have a fluence model that's 2-D planar, if you will. It wraps all the way around the vessel and gets attenuated through the vessel.

But that's combined with a 1-D TH model and a 1-D fracture mechanics model. Another, again, unquantified, but I feel very comfortable in saying major change from the past is previously we modeled the whole vessel as being made out of the most embrittled material.

Which, except in the case of Beaver Valley, is almost invariably a weld. And so in the past we represented the whole vessel as being made out of a material that in reality only represented about less than five percent of the vessels total --

DR. WALLIS: That does make a big difference.

DR. KIRK: Yeah. There, you know, in the list and even not on the list, there were many other changes in the fracture mechanics model, but I wanted to emphasize those because those are the, you know, those are the big arrows.

And the everything else is just being systematic about your process. So unless there are further questions --

DR. SHACK: It's time for lunch.

DR. KIRK: -- we can break for lunch.

MR. ROSEN: Let me ask one quick one. What's the big azimuthal variation of the fluence the result of?

DR. KIRK: That comes from the

differential and spacing of the fuel bundles relative to the, relative to the ID of the RPV. It's a checkerboard pattern. The fuel bundles are about like that and so at some places they might be only that far from the ID.

And in other places they might be that far. And you get an awful lot of attenuation of the neutron fluence through the water.

DR. RANSOM: What does this mean to these plants that have been upgraded by trying to flatten the flux profile, you know, throughout the core. I think we asked the question at that time and we were told that vessel cracking was not really an issue.

But fluence will be higher on the wall.

DR. KIRK: Yeah, and that would factor in, if somebody has done that, that would factor into their analysis and influence their surveillance program and so it would change the, quote/unquote, RT_{NDT} metric that they'd used to assess their vessel.

MR. BESSETTE: You know plants used to, they used to look for, try to get a fairly flat profile. If it have PTS importance, like 20 years, 15 years ago, they went to more of a peak profile. Now they may go back to a flatter again.

DR. SHACK: Okay, we'll come back at 1:25

then. And, Mark, one of the causalities might be the screening limit. It seems to me that's more speculative at this point, that's not really fundamental to the presentation.

DR. KIRK: Okay.

DR. SHACK: So, we'll probably have, we'll devote an hour to the plant-specific and I want to make sure we protect at least an hour to discuss the acceptance criteria and such. So we'll sort of run the individual analyses up until we have an hour left and then we'll go to the acceptance criteria.

DR. KIRK: Okay.

(Whereupon, the foregoing matter went off the record at 12:25 p.m. and went back on the record at 1:30 p.m.)

AFTERNOON SESSION

1:30 p.m.

DR. SHACK: It's time to come back into session.

DR. KIRK: We will try to present a somewhat abbreviated walk-through of our comments on plant-specific results. Now this might not quite track with what you've got in your slide packet.

To outline the discussion I will talk about, well maybe we won't. No, we won't talk about that. We won't talk about the plant-specific features and inputs, that's all detailed in the report.

We will discuss the estimated yearly through-wall cracking frequency in terms of both the values and the characteristics of the distributions of through-wall cracking frequency. We'll discuss both the transients and the material features that make up the dominant contributors to the through-wall cracking frequency.

And that will be the focus of Mark's in the next hour. This is the first presentation of the actual through-wall cracking frequency results. Just to orient everyone, we've tried to adopt a consistent format so that you don't have to keep reading the symbols from slide to slide.

Oconee will always be in blue, Beaver will always be in green and Palisades will always be in red. At the, during this phases of the presentation we're going to present all of the results regressed versus effect of full power years.

And defer discussion of RT_{NDT} since we've already acknowledged that RT_{NDT} is confusing until later in the presentation, if we get there. Suffice it to say, effect of full power years corresponds to how long the plant has been operating.

So longer operation, higher degrees of embrittlement. On the left-hand side of your screen you see one way of representing the distribution of the through-wall cracking frequencies

We've represented the fifth and 95th percentile, the median and means, with the means in the larger filled symbols. We've taken as our free variable in this analysis the years of operation in the plant.

And do to the low level or irradiation sensitivity of some of these materials, we've had to take the plants out to what I think everybody would agree to ridiculously long lifetime, in order to get mean through-wall cracking frequencies up in the E minus five, E minus six region.

Obviously, in principle, you can muck around with any of the variables in the analysis. For example, in the original PTS analysis a complete fictitious plan called H.B. Robinson Hypo was created by draining up very high copper numbers.

We felt it was less ambiguous just to increase the time variable. In any event, the main take away from this slide is that over any currently anticipated operational lifetime, the estimated through-wall cracking frequencies for these plants is very, very small.

At end of currently anticipated license extension or 60 years, the through-wall cracking frequency values range in the minus nine to minus eight region. And of course, as we've pointed and continue to point out, two of these plants, namely Beaver and Palisades, are among the most embrittled in current operation.

So at the end of any reasonably expected operating lifetime, we are way below the E minus five, E minus six type reactor vessel failure frequency criteria that have been considered.

I'd just like to take a moment to point out, on the left-hand side we showed the bounds of the distribution that we draw the mean or the median estimates from. I'd just like to take a moment to point out that these distributions have some characteristics that's common to all of our results.

Specifically the distributions of through-wall cracking frequency that come from propagating all of the uncertainties through the analysis. And this is now the amalgam of the PRA uncertainties, the thermal hydraulics uncertainties and the PFM uncertainties.

We get distributions that are both skewed and that most of the weight in the histogram is down at very low or in fact zero probabilities of failure, and they are very broad. Where greater than three orders of magnitude separate the fifth and 95th percentiles.

And the point that I would like the Committee to take away from this is these characteristics of the distribution, that they are skewed and broad, is not a mistake and not the consequence of any limited state of knowledge on the part of any of these models.

It's in fact a very natural consequence of the physics of cleavage fracture that results in absolute minima of K_{1c} and K_{1a}.. And so you've got, if you look at the distribution that's shown here in blue for Beaver Valley, 32 effective full power years.

And the bar on the graph that goes off the screen, which I realize is a little hard to read, but it represents that almost 80 percent of the simulations for Beaver, which is an embrittled plant, or currently thought to be an embrittled plant, at 32 effective full power years.

Almost 80 percent of the simulations result in absolutely zero probability of failure. Not a very small number with lots of leading zeros, but zero. And that's because the combination of the transient severity, the flaw size and the embrittlement wasn't enough to get the applied K above the minimum of the K_{1c} distribution.

And so there is just not, it's just simply not going to fail. As you increase the embrittlement in any of these plants, of course you get to the situation where the zero probability of failure goes away. But still the distribution is heavily skewed towards the low end.

DR. KRESS: You know what I'd take away from these curves?

DR. KIRK: What's that?

DR. KRESS: That I can quit worrying about PTS and we don't even need a rule or anything. Just

--

DR. KIRK: Can we end the briefing now?

DR. KRESS: Just forget about it.

(Laughter.)

DR. KIRK: Well, that's, it's much less, I mean obviously, as Dr. Shack pointed out, there is a need for the Committee to understand the procedure. But assuming the procedure is right, the consequence of the analysis, the PTS, is much less troubling than we thought it was.

So that's how all the distributions --

DR. SHACK: Until you get out to 200 years.

DR. KIRK: Yeah.

(Laughter.)

DR. KIRK: I'll be much older then. Also, one thing to just remember through the rest of the presentation is that because the distribution, or as a consequence of the fact that the distributions are this heavily skewed toward the low end, we've been plotting mean values, just as an order of merit.

However, in these distributions the mean in the 95th percentile approximately coincide. This slide speaks to what transients dominate through-wall cracking frequencies. And we've already sort of tipped our hand on this, in that for Westinghouse and CE Design Plants, LOCAs are the dominate contributor to risk.

In Beaver Valley, LOCAs are essentially everything. In Palisades they represent about 80 percent of the total through-wall cracking frequency. In B&W PWRs, due to the once-through stream generator design, we see that stuck open valves on the primary side are also dominate contributors to through-wall cracking frequency and in fact make up the bulk of the through-wall cracking frequency at low levels of embrittlement.

And as we discussed this morning, failures on the secondary side, including stuck open valves on the secondary side, like the stuck open atmospheric dump valve and certainly the main steam line break.

While they were dominate before, are not dominate now. And we'll now have a slide or two on each of these to explore the transient types in a little more detail. But, before we get there, this slide I call the Ashok slide because we made in response to a question asked us by Dr. Thadani.

And he said, well, that's great that the through-wall cracking frequencies are so low, but how is it made up. And of course, at least notionally, the through-wall cracking frequency is a product of how often things happen.

The initiating event frequency and the probability of failure occurring if the event initiates. And of course what one would like to see in an environment where you hedge your bets and don't want to believe entirely on any one thing.

As if there's some rough balance between the two. And when we look at the dominant classes of events and compare the initiating event frequency and the conditional probability of failure mean values, we find out that that's the case.

That for most of the dominant events, there's a rough balance and that these two figures are within an order to magnitude. So, it's not like we're getting low failure probabilities, it's not like was have extremely likely events, but our models predict that they don't matter.

Or the reverse. We've got extremely unlikely events, but if the event happens it's the end of the world. We do have a balance between these two figures.

Now getting back to the transients that dominate, as I already discussed, LOCAs are important in all three plants and dominate in the CE

Westinghouse-type plants. And, as we've said before, since these are the dominant contributors, therefore the dominant contributors to uncertainty in the total numbers, and so we discussed that a little bit.

There is at least three orders of magnitude uncertainty in these through-wall cracking frequencies, and in fact more orders of magnitude at lower embrittlements because at lower embrittlements you get many, many cases where you've got zero probability of failure.

At least two of those orders of magnitude come from the uncertainty in the LOCA frequencies, as we already discussed.

And the remainder to the uncertainty is largely attributable to the PFM on certain days, with about one order of magnitude for the flaw distribution and one order of magnitude for the RT_{NDT} bias adjustment that we discussed this morning.

And again, to reiterate what was discussed previously, especially for the medium to large break LOCAs, which are themselves dominating these contributors, operator actions do not really play a significant role.

There is not much an operator can do in response to a LOCA. This graph, I'll apologize to the non-fracture mechanists in the room because this is one of those inverse double normalized fracture geek plots.

The horizontal axis is the temperature at the crack tip normalized to the RT_{NDT} or to the index temperature. And I've turned the axis around intentionally, so you go from high temperatures on the left to low temperatures on the right, so as to make the X axis a quasi-time scale.

So you can think of time as at least approximately increasing as you move from left to right on the graph. The vertical axis is the ratio of the applied K to the minimum of the toughness distribution.

And what we've tried to do is, at least it's hard for me to look at probabilities of failure and gain a lot of insight. It was a lot o more instructive to look at just one crack, in all vessels, under equal embrittlement conditions and compare the dominant transients.

That's what this plot attempts to do for the LOCAs. And a couple of things to point out is first off, again, as we pointed out, until you get $K_{applied}$ above K_{1c} there is absolutely no probability of failure.

So you can basically ignore all of the parts of these plots that fall below unity on the Y axis. And the second thing to point out, as we discussed in detail at a briefing, I guess it was about this time last year, the conditional probability of initiation exactly and the conditional probability of failure, at least approximately, scales with just one point on each of these curves.

That being the maximum of the $K_{applied}$ to $K_{1c(min)}$. So it's the maximum on the graphs that are important, and the message that I'd like everybody to take away from this is looking at LOCAs, which are the dominant contributors to risk, and at least in two out of the three plants that we've looked at there's a remarkable similarity in the level of challenge produced to the vessel by LOCAs in the different plants.

There's not huge plant-to-plant dependencies that we're seeing in terms of fracture driving force. Moving on to the stuck open valves on the primary side that reclose later. Stuck open, these formed a contribution to the through-wall cracking frequency in all of the plants.

However, it was really an important contribution only in the B&W plant, and that occurred due to the greater tendency to decouple the reactor coolant system from the secondary due to the B&W steam generator design.

There are more uncertainties to deal with in this type of analysis. Specifically the degree of valve opening which was modeled in the PRA as a split fraction for valve openings of interest.

Of course, when the valve recloses is important because that's when you get your pressure spike. And that was modeled as, Alan, correct me if I'm wrong, after 3,000 seconds, 6,000 seconds or never.

And, of course, the operator actions in these type of scenarios do play a key role. Looking again at a comparison of, this is now a comparison of these type of transients. It came up as being risk dominant, which our definition is, contributes greater than one percent of the total through-wall cracking frequency.

A comparison of stuck open primary side valves that reclose later between the three plants. And again we see Oconee, the peaks in these transients for Oconee produces a little bit higher crack driving force than in Beaver and Palisades, but not a heck of a lot.

So again, there's a fair degree of similarity in the level of operational challenge between the three plants, as you see in the blocks in purple. I think it's purple. Well, blue on your screen.

In blue, there are some differences in the initiating event frequencies that are plant-specific and have been taken into account. And then the third one we wanted to point out is to discuss the non-dominance of the main steam line break transient or the secondary side transients in general.

Our analyses, as you see here, it's at best a five percent contributor and in often cases in less, and in most cases less. And in fact in Oconee they didn't even come up on radar at all.

So, since they were important before, the obvious question is why? And as I suggested before, there are really three reasons for this, and I'm going to try to go through them in rough rank order of importance.

The first is that in our analysis, and we've made points about this earlier, our binning has not been nearly as gross as in earlier work. In our current work we separate large breaks from small breaks, from different valve opening scenarios.

Whereas before everything might have been binned together with the main steam line break. And so grossly overestimate the significance of those transients. The second point, which I'll have a slide on in a moment, is just the point that these transients, if you compare them for the same crack, for the same embrittlement, are just simply not as severe as a LOCA.

They don't generate the high crack driving forces that the LOCAs do, which are dominant now because we've included them. And then the third thing is, yes, it's appropriate to admit that the credits that we've given for operator actions have helped to mitigate the severity of the secondary side events, because the operator does have influence over the degree of over cooling.

However, again, as Alan said before, we would have had to have been grossly wrong to turn these from five percent to 50 percent contributors. It has certainly been the feeling of the people that have conducted the analysis that if we, and this is again probably a ripe area for a formal sensitivity study, but that even if you assumed stupid operator actions, you wouldn't do more than double this contribution.

The next graph, yes, makes the point that even if the event occurs, the main steam line breaks are just simply not as severe as the LOCAs. Again, the thing to focus on in this graph are the peak values.

And this is, this has been done for same crack, same level of embrittlement. So it's a head-to-head comparison. And the main steam line breaks just don't get, don't generate the $K_{applied}$ values that the LOCAs do.

And the other thing, I think, and Alan can probably help me out with this, that's relevant to point out, is that the, there are, I think, four or five different curves on there on the main steam line break that represent different combinations of operator action, operator inaction, that we included in our analysis.

And you can see that all the curves essentially peak at about the same $K_{applied}$ so even that variation of operator action that we've included in our analysis is not making a significant difference in terms of the degree of challenge of the main steam line break.

And then, again, you've seen this type of presentation before. Just a comparison of the level of fracture driving force severity for the secondary side transients is relatively equal between the two plants.

They are peaking at fairly similar values. Moving on to materials considerations, we find that the plot is on the vertical axis, the percent contribution to yearly through-wall cracking frequency plotted versus the EFPY.

And we see that the axial cracks in axial welds are the things that dominant the through-wall cracking frequency. They are responsible for 90 percent or more of the through-wall cracking frequency.

And that means that the important material metric is, or I should say are, the material properties that could be associated with those cracks. So that's either going to be the RT_{NDT} of the axial weld or the RT_{NDT} of the plate, because those are the two materials that sit on either side of an axial crack and an axial weld.

Conversely, the circumferential cracks and circumferential welds play a very minor role. That would be the bottom half of this graph that I haven't shown. That they've never been responsible for more than ten percent of the through-wall cracking frequency.

So consequently, the properties of the circ welds or the forgings that the circ welds join, while they make limited contributions to the vessels resistance or perhaps lack of resistance to PTS, they are just not major players.

And then the third point is that the cracks in plates and forgings that are remote from the weld fusion lines, that are out in the bulk of the material, are just simply too small to play a role.

They have sizes that cap out around five percent of the through-wall dimension of the vessel, as opposed to 25 percent for the weld fusion line flaws. And those flaws are, those flaws subjected to these thermal hydraulic transients are just not big enough to generate any substantial crack driving force.

So these considerations, if we get to it, are going to be major factors in telling us how to construct a physically appropriate RT_{NDT} metric.

DR. SHACK: What happened to the rest of the Beaver for later in life? Why does it disappear at 100 years?

DR. KIRK: We didn't do an analysis beyond 100 years. At a, we stopped, obviously we had an inconsistent number of years. The consistent thing was we stopped running these analyses when we got total through-wall cracking in the E minus five, E minus six range.

And so no big surprise that you get there a lot sooner with Beaver and Palisades than you do with Oconee. So to summarize the findings of the plant-specific analyses. Again, the major take away is that the through-wall cracking frequency that occurs as a consequence of PTS, is low over any currently anticipated operating lifetime.

On the operational side, LOCAs and stuck open valves on the primary side dominant the PTS challenge. And breaks on the secondary side are insignificant contributors. And also, and this is an important point, holding all material factors constant, the operational challenge, in the way we modeled these plants, is reasonably consistent between the three plants.

Both measured in terms of the probability of the challenge occurring and the fracture challenge assuming, or the fracture probability assuming that challenge occurs.

From the materials side, the observation that nearly all of the weld flaws occur in the weld fusion line, the axial weld cracks therefore dominant the through-wall cracking frequency, so it's the properties that could be associated with axial weld cracks.

The axial weld toughness or the plate properties that are going to dominate the RT_{NDT} metric and circ welds make a minor contribution. So that's the really quick run through. If you have any questions, we can --

DR. FORD: Yes, could I come back to the materials composition. I noticed that on some of your initial slides, you were showing that Oconee was less susceptible, all other things being equal, in terms of operational changes.

It was more resistant, rather, than Beaver Valley and Palisades, which is the order you'd expect from the current way of doing it. Which is dominated by the materials influence inputs.

DR. KIRK: Yes.

DR. FORD: Do I take away that the materials composition effects are still an important part, but they are overlaid by these operational aspects, stuck open valves? Am I putting it clearly enough? I'm still worried about this materials composition.

DR. KIRK: I'd say it a little bit differently and see if you like this. Is that we've included the, of course we spent a considerable amount of time trying to find a way to get appropriate distributions for copper and nickel phosphorus and so on.

And the model we finally adopted was to use the values in the Arvid database, which have been docketed by the Licensees, as the mean values for all those distributions. And then we construct, and construct the distributions around them.

We constructed the distributions based on essentially all the data we could find on copper and nickel and phosphorus distribution in the literature, which included some detailed work that was done be EPRI years ago, some detail work that was done in Japan, and a number of other sources that don't come to mind right now.

But the level of material uncertainty that's been represented in these calculations has been drawn from essentially all available information on material availability in RPV steels. So I guess the way I would characterize it, is it's just not going to get any worse than that.

If any, if a specific plant were to come in, say Palisades, who spent a considerable amount of time measuring their material variability. They certainly have a greater state of knowledge regarding their material, their specific material, than was represented in these analyses because we use generic data and assume that the variability possible in any one weld was characteristic of the variability possible in all welds.

DR. FORD: Okay, let me just put it in another, replay back what I heard from you. What you're saying is don't get worried about the trendlines that are coming out of the Eason correlations. Forget those. If you just look at the worst, the worst it can affect you is not going to have any big affect on these results --

DR. KIRK: The worst, yeah. The worst that it could affect you is already in these results. So anything that's better would only tend to shrink the distributions, and well now, here's unsubstantiated sensitivity study opinion.

My guess is it's not going to influence them very much. Beaus I mean as materials people we look at distributions of copper and go, oh, my God. You know, that's really bad. And then Alan tells me, well, I've got a two order of magnitude certainty on how frequently this event occurs and all of a sudden I feel a lot better about what I know about copper.

DR. FORD: Okay. I have another question.

DR. KIRK: Okay.

DR. FORD: In your Executive Summary, you say that it's a blind statement, and without quoting it verbatim, it essentially says no PTS problem for all plants. I think you used all plants, all PWRs.

Based on the analysis for these three plants, you then go on in your main document here, the applicability of these analyses to all plants. You, is that the next --

DR. KIRK: Well, I wasn't planning on doing this is detail, but it's a question you asked.

DR. FORD: It is based solely on you look at the worst plants, five more extra plants and you say, well, what's different between those plants and these three plants and essentially there is nothing.

DR. KIRK: I'm thinking, I mean you're right, the statement in the Executive Summary was perhaps getting a bit ahead of ourselves in terms of, your know, rigorous drawing of conclusions from scientific information.

But I think the insights that have come

out as we've started to delve into this a little more, again getting back to those $K_{applied}$ parts, show that the, the level of operational challenge is remarkably consistent between the plants.

And what we've been able to do is to feedback our understandings about the level of challenge that these scenarios present and we fed that back to Alan and Donnie as they go forward to the other five plants to basically inquire, I mean do you, for example, do you have a LOCA that's going to be worse than this?

And since, I mean I think we need to do a little bit finer level thinking about the B&W plants because their operator actions are important. But it's quite frankly for me difficult to envision that, you know, an eight inch break in one plant is profoundly different than an eight inch break in another plant.

And so I just, that needs to be expressed better and more clearly, certainly. But it just doesn't seem, with LOCAs dominating the way they do, the plant-to-plant variability on the operational side, is going to be a significant factor.

DR. FORD: And then these other five plants, Fort Calhoun and the other four, they will be tackled in not quite maybe this rigor. I'm sorry I'm being --

DR. KIRK: No, that's fine. I was planning on omitting this, but we do appear to have time. The, what we've called the generalization step involves trying to take our insights from the three and a half plant analyses that we've done so far and then interrogate other plants to see if we expect them to be considerably worse.

And the strategy taken here was to take all the plants and rank them in terms of irradiation susceptibility. And specifically what that means is we took unirradiated RT_{NDT} , we added the Eason embrittlement shift at 32 EFPY.

We took out circ welds, based on the insight that circ welds don't contribute much, and then we ranked the plants from highest to lowest. And when we did that, Salem, in fact, came up as slightly more embrittled than Beaver Valley.

So basically what we did is we took the top five plants that we hadn't looked at and said, okay, these plants, based on our understanding, we believe to have the greatest level of materials challenge.

So now we want to go out operationally and see do they both have the highest materials challenge and somehow have a greater level of operational challenge than we had seen in these other three plants.

If we have both of those exacerbating factors, then we would conclude that, oh, well, perhaps there is something that we haven't, there is something that is outside of our current model that we haven't included that we need to.

If, however, we see that, you know, at the very least the highest five embrittlement plants that we haven't included have operational challenges that we believe to be equal to or less than what we've seen before, then we've reached the conclusion that, yes, these results should be applicable to remaining plants.

Not to represent them as a best estimate, but I think one would at least represent them as being of value. So that's something that's ongoing. Alan can talk to the status of that. We've drawn up a series of questions that is drawn out of our insights from what things are important and what things aren't important to basically ask that question.

To see if there's any operational challenge in any oaf these plants that is somehow more severe than something we haven't, than things that we've seen before.

MR. LEITCH: Is that effort in any way prioritized. I just noticed that I may not know the exact order in which plants are coming up for license renewal, but I think Fort Calhoun is quite soon.

DR. KIRK: Yes, it is.

MR. LEITCH: I think it's in-house at the moment and we're scheduled to review it in May or something like that.

DR. KIRK: Yes, Fort Calhoun has been in on a number of different occasions. The other ones, it's been prioritized only in the sense that those are the five that we picked that were the highest level of embrittlement.

We didn't pick it on the basis of who was coming up soonest. I don't know if there's any relationship there at all. If there aren't further questions on this part, we can go to the part on reactor vessel failure frequency.

DR. SHACK: Mark, just refresh my, if I go by initiation rather than through-wall crack, what do I, how much do I jump these curves?

DR. KIRK: It's about an order of magnitude.

DR. SHACK: It's about an order of magnitude.

DR. SIU: What I'm passing around is a segment of the action progression of entry which we're going to talk about in the discussion. And I'm passing it around just because I'm afraid the slides may not show very well.

And in your printed copy it almost certainly doesn't show because there is an animation and some of the blocks in the animation cover the actual tree. Given that we are actually ahead of schedule now, after that blinding presentation, we can just go ahead and take the hour? Okay.

Okay, I'm going to talk --

DR. SHACK: You could even cover the criterion.

DR. SIU: Yeah, actually I think that would be a good thing, quite honestly. I'm going to talk about the reactor vessel failure frequency criterion that we have done some analysis to establish what a reasonable value might be for that criterion.

We've tried to be a little bit careful and not express this as a risk acceptance criterion, because clearly we're not computing risk, although we're trying to inform the establishment of this criterion using discussions of risk.

And if that connection isn't clear later on, I'm sure we'll have questions on them. I'll point out a couple of things. This criterion plays a role in the current version of the rule in two ways.

First, it supports the establishment of embrittlement criteria, and those are the RT_{NDT} criteria that are currently in the rule. And furthermore, it provides an acceptance criterion in case a plant does an safety analysis and needs to compare, have a metric defining the level of PTS risk.

And the current value, as you know, is the five times ten to minus six per reactor year that's currently specified in Reg Guide 1.154. So there are two roles that this particular criterion plays.

What I'm going to report on is a limited scope activity that we've performed. And, just as a reminder, clearly the amount of time we are spending on this work is way out of proportion to the actual effort expended.

We spent a tremendous effort of looking at plant-specific, through-wall crack frequencies. What we're going to talk about here is very much a scoping study, just to get a sense of what an appropriate acceptance criterion could be.

And I'll get to the reason why in a little bit. Mark has already shown you this graphic that says how we might develop screening limits for embrittlement based on the establishment of an RVFF criterion.

Again, this is a notional slide. Our expectation is that the actual establishment of those limits would be done in a risk informed manner, and not a risk based manner. Nevertheless, of course, the risk information, again, informs that process.

And Mark is going to talk to how that risk information can be used, a little bit later. Okay. We covered some of these things already, I believe in the July briefing of the committee.

The activities were performed. Obviously, we had to identify options regarding criteria, and those were document in SECY-02-0092. We did perform a scoping study looking at the post-vessel accident progression.

It's largely a qualitative study, as you'll see. However, we did do some limited calculations, thermal hydraulic and structural, and Dave Bessette will talk a little bit to that.

We also reviewed the results of the pilot plant calculations to look at the energy of the system at the time of reactor pressure vessel failure. So we were trying to use these calculations to inform the judgments that underlie qualitative analysis.

We've, I mentioned the SECY paper already. We met with ACRS in July. We've had public meetings in October and just recently the end of January, talking about what we've done.

And the results, of course, are documented in Chapter 5 of the draft NUREG. I'll point out that the focus of this is on acceptability of certain levels of PTS risk. So although we acknowledge, as you've seen in the previous presentation that the PTS risk is probably very small, that particular fact didn't necessarily factor in very much with our effort.

Other than to say that we shouldn't spend a whole of time working real hard on the acceptance criterion issue. The principles that we applied in developing options. Again, we reported to the Committee on this back in July.

We wanted to be consistent with the intent of the original PTS rule. So the principles involved, keeping the risk associated with PTS at a low level, and keeping the relative contribution of PTS risk small compared to the risks associated with other sources.

We also, of course, wanted to bring in whatever thoughts had come about since the promulgation of the PTS rule in the ‘80's, with whatever risk informing issues have occurred since then.

Principally the Reg Guide 1.174 and Option 3 work. So we tried to make sure we were consistent with those, as we develop the options. These are the same options that we proposed to the Committee, so these were specifically in the SECY paper.

And Dr. Wallis isn't here, but the top, in terms of a definition of the reactor vessel failure frequency, we considered two options. The first one is essentially the through-wall crack, TWCF.

That's the current definition of reactor vessel failure frequency and so that was an actual option to consider. We did look at, very briefly, the issue or the possibility of adopting a definition based on the crack initiation frequency.

And I'll get you our conclusion on that in a second. We looked at three possible numerical limits for the acceptance value for RVFF. Those were the three that you see here.

DR. KRESS: I see only two there.

DR. SIU: I'm sorry?

DR. KRESS: I only see two there.

DR. SIU: No, the acceptance limits and numerical values?

DR. KRESS: I see, yes. Sorry. I was reading my slide there, I couldn't read it.

DR. SIU: Okay. And then of course in your letter back to us you suggested that there might be a fourth option, which is acceptance value significantly lower than the ten to the minus six.

So, getting to that point, after we met with the Committee, there were a number of discussions. Some naturally involved budget. And the decision was, and this is where the notion of the low PTS risk comes into play.

Expecting that the results were going to show that the risk was low, we decided not to spend a whole lot of effort on this particular task, the acceptance criterion tasks and spend most of our resources on making sure we had a good handle on the through-wall crack frequency for the pilot plants.

So, again, you'll see that we've done a scoping study and nothing more. And we're not pretending that this is a detailed analysis. We, of course, got the letter from ACRS indicating that we should base our considerations in terms of LERF.

That we should consider the possibility of something significantly larger than those underlying the current LERF criteria. And we could either start with a Level 3 PRA and work our way back to an acceptance criteria for reactor vessel failure or we should adopt a frequency-based approach just to assure that the frequency of failure of the vessel is very low.

In the letter you also expressed the expectation that the, whatever criterion we came up with would be significantly less than any of the options we proposed in the SECY.

I think the key point on this is in the quotation in the middle of the page. Whether air oxidation phenomena, and I would add large early release would be a likely outcome of a PTS event. And we've spent most of our time trying to investigate whether that's indeed the case.

Okay, just very quickly. On the first set of options regarding the definition of reactor vessel failure frequency, we stated in the SECY, I believe, the expectation that we'd come out with this conclusion and we still hold to it.

We believe that we should be defining

reactor vessel failure frequency in terms of TWCF. We believe that for two reasons. One, from a risk-informed standpoint TWCF is a more direct indicator of risk than in crack initiation frequency.

Now the counter argument to that, of course, might be, well, there are significant uncertainties in the prediction of crack arrest versus crack initiation. And I think our conclusion is that the current technology for predicting crack arrest is reasonably robust. And Mark will talk to that point.

DR. KIRK: Yes, I'd just like to make a few points on this slide. One is the graph that's already on the slide illustrates that when we compare K_{1a} data generated using ordinary laboratory experiments conducted as per ASTM standards, and that being just shown by the red data bounds.

Compare that with crack arrest data inferred from scaled vessel experiments, either the thermal shock experiments, the pressurized thermal shock experiments conducted at Oak Ridge and some of the experiments that have been conducted overseas.

We find both the same temperature dependency as well as the same distribution or similar distribution as is found in our laboratory experiments. So we've got a reasonable agreement, we feel, between specimen and structure data.

And also to point out that the uncertainty bounds shown there for K_{1a} are, if anything, a little narrower than the uncertainty bounds on, that are characteristic of K_{1c}. Let me find that on an empirical basis and also can anticipate it physically.

Also looking to how well we can predict the results of a run arrest event in a structure. We can reference back to, in a structure that we're interested in, we can reference back to the thermal shock experiments that were conducted at Oak Ridge, where we started with a thick wall cylinder and I forgot to show the holes.

But there is a hole in there that was heated up and then we filled it up with LN2, which of course generated a very severe thermal shock in the vessel. And after that a crack propagated from the ID out towards the OD.

And on the graph that's now on the screen, I've just shown the results of one of these experiments. Thermal shock experiment 5a. And shown hoe reasonable the prediction is. And the vertical axis was shown the percent of the vessel wall that was effectively cut by force excessive crack jumps.

And just make the point that using K_{1c} and

K_{1a} data within an LEFM model in a similar way to the way that FAVOR does the probabilistic calculations, we get a reasonable prediction of these experimental results.

DR. SIU: And that's all we have to say on the definition of reactor vessel failure frequency. So, if there are no other questions, I can go on. Okay, the rest of this discussion will be on the numerical criterion value.

And, again, we identified three options and really considered four, including the one suggested by the Committee. The key questions we were asking basically have to do with whether there is a margin between the occurrence of a through-wall crack and core damage.

If there is margin between the occurrence of the through-wall crack and a large early release. And should a large early release occur, associated with the PTS scenario, would the release characteristics of that be significantly different than what we consider risk significant events.

Our approach, we had identified a number of issues in SECY-02-0092. These were based on work done a little while ago by Idaho National Engineering Laboratory. We took, this was largely on the in-house reinvestigation of those issues, where we asked what do we know about the progression of events past the reactor vessel failure. And refine that list.

Just define. What are the things that we should be considering? We developed an accident progression event tree. This APET, as I'll refer to it, was not really intended to serve as a computation tool, although it can be used as such.

But really to identify issues. What's the progression of events. What's the context within which we should be evaluating the likelihood of events. So, in particular, you'll see in that APET, which we have a reduced version in the report.

What you would consider to be aleatory issues, such as the operation of containment spray, and you've also got epistemic issues, such as what's the force association with the crack opening.

Presumably, of course, in the latter case you could calculations to show what those forces are. We haven't done anything detailed along those lines but we've got some limited calculations to indicate what the forces might be.

We evaluated our current state of knowledge regarding these issues, focusing on the pilot plants that were addressed in the main study.

But we also took a quick look at some of the plants considered in the generalization portion and Mark had shown you that chart with the plants identified in color there.

And another important part of the context is whether the PTS changes that accident progression significantly. The point was to argue whether core damage or large early release could occur following a PTS event, but does it occur in a way and with likelihood significantly different than what you might find in other risk-significant accident scenarios.

DR. KRESS: What was your criteria for deciding whether or not to get a large scale air oxidation? Where does that show up on this event tree?

DR. SIU: Okay, well, I'll show you actually a the tail end here. This is the unreadable graphic, so don't bother. This is the one that is actually in the report. The next slide I'm just going to walk you through the top events in the event tree, so hopefully it will be a little bit more visible.

This, and then we'll have a similar animation for an event tree that shows the key sequences. A couple of things I want to point out with this event tree. First of all, the top events largely correspond to the issues that we, the technical issues that we'd identified.

And those technical issues are the ones that we've listed in the report. And it's a little bit different than the list of issues we had in SECY-02-0092. Another thing to note is that I've indicated here with yellow and red, two different classes of scenarios of interest.

The yellow scenarios are the ones where we thing that core damage is possible. Where large scale air oxidation is possible, and where the containment spray is operating therefore there could be a release but it wouldn't be a scrubbed release.

The red indicates the scenarios where containment spray is not operating, so you have the possibility of a large early release and large scale air oxidation for most of the scenarios that we looked at in the tree.

Large scale air oxidation and large early release are not synonymous, but for many of the scenarios the essentially occurred, we judged that they would occur at the same time or for the same scenario.

Another point I want to make here, we have ten scenarios, this tree has 200 scenarios in total.

Ten of those scenarios involve, what you would, involve the yellow kind of line. In other words, the scrubbed release.

And ten of them involve the red line, the unscrubbed release. Not all of them are equal in likelihood. In the report we identified the four scenarios we thought were the most important in terms of probability.

And I'll actually talk to those a little bit later in the presentation. Okay, this is the slightly blown up version of the tree. It reads a little bit better. Not perfectly, but again I'll just walk you through the tree.

First of all, of course, you start with PTS event. As Mark indicated, you can enter this tree with LOCA events. You can enter with stuck open relief valves that later reclose. So basically a low pressure event or a high pressure event.

But in both cases you'd be entering where the system has cooled somewhat, before you challenge the reactor vessel. And I'll talk to that a little bit later. The next branch deals with crack orientation. Whether the crack is axial or circumferential.

And as Mark indicated, again, there is

about a 90/10 split there. Ninety percent axial and ten percent circumferential. The next question we asked was how far does the crack extend.

We didn't do any new work ourselves, we referred back to an old Pacific Northwest Laboratory study on NUREG/CR-4483, I believe was the number. That's the one that we referred to in the report.

And some, that report documents an analysis that looked at the extension of cracks. And they considered whether the crack would extend to the circumferential welds, and I'm talking about the axial cracks, of course.

Whether it would go beyond the circumferential welds and whether it would turn the corner at a circumferential weld and continue on. And not so clear here, well, okay, I'll get to it a little bit later.

Clearly if the crack turns, if an axial crack turns the corner and continues, there is a possibility of arrest or continuation. And we had both of those possibilities in the tree. For circumferential welds, cracks, of course you still have the possibility of arrest or continuation.

So, again, these just identify the possibilities. We're not, in general, talking about likelihoods, yet. I'll talk to likelihoods a little bit later. There were certain hole sizes associated with these crack extensions and, again, we were relying on the old study to give us an indication there.

For the arrested cracks the size range was from zero to ten square inches. For cracks that would extend to the, beyond the circ welds, the range was from ten square inches to 1,000 square inches.

And we broke that up into two categories, a medium hole and large hole. And then we also allowed for a possibility of a catastrophic release and basically again the whole reactor vessel opening, should the crack turn the corner and go all the way around. So we did not discount that.

We didn't have, well, there are various opinions about the likelihood of that. We don't have an analysis to show us yet what would happen in that situation. We looked at blow down forces associated with these holes.

And, again, allowing for the possibility that the blow down forces are either roughly corresponding to design basis LOCA forces or even less, that's the upper branch. Or the possibility that the forces are significantly greater than design basis LOCA loads.

And that plays a significant role later on when we talk about dependencies. We asked the question as to where the containment is isolated. Clearly, if you have large forces on the piping, you might ask about, whether or not penetrations are affected. So we allow for that question here.

We ask if sprays are working. If the, if there's a large hole in the vessel, does the fuel get relocated outside of the vessel or does I t stay within the reactor pressure vessel, so that was a possibility that we asked about.

We asked if emergency core cooling continues to run. And we emphasize continues to run, because it was running prior to the reactor pressure vessel, or you wouldn't be in the PTS event.

And then we asked if the reactor cavity is flooded. Or is the cavity designed such that the water level coming out of the vessel would be expected to rise above the level of the fuel, which would be a cooling mechanism.

To answer your question, Dr. Kress, we looked at each of those scenarios and we decided, depending on whether ECCS was working and whether we had cooling, obviously, if you don't have cooling it would lead to core damage.

If you had large early release, we would consider that if containment in isolation was failed. And for air oxidation, we didn't think that that was possible or likely for some of the smaller holes in the reactor vessel.

And that's just based on considerations of the flow path for errors. But for the larger holes we didn't discount it. We simply said it could happen.

DR. KRESS: So the only containment failure you have is isolation, failure to isolate?

DR. SIU: That's the direct, that's right, that's the direct failure of containment. We have some calculations on pressurized to show why that's a reasonable thing. Yeah, that's basically what we did. Okay. All systems assessments, we were very concerned about dependencies between events here because that's what, dependencies between top events would lead you to any reasonable likelihood of the larger early release and so forth.

So we investigated whether there was characteristics of these scenarios that could lead to knock on affects. So we talked about plant systems. That refers to, for example the state of power at the time of the event.

And again, this is a situation where things are running prior to the reactor pressure vessel failure. This is very different than many of the severe accidents where station blackout is a major concern.

We asked questions whether the RPV, the reactor pressure vessel could move, given the forces on the vessel and given the time over which the forces would be operating. We asked questions about whether missiles from the failure of the reactor pressure vessel could to lead to failure of other systems, such as the containment spray.

And we also asked whether the fuel could be moved as a result of this kind of event. What we're going to talk about are some of the calculations that, again, inform the judgments that we made in the study.

I'll give an overview here and then I'll turn it over to Dave Bessette to talk about some of the TH calcs. But just to remind everybody what were the conditions at the time of the reactor pressure vessel failure.

And again, this is an analysis that assumes that the through-wall crack has occurred. And that's just, we're focusing on the conditional aspects of the scenario. First of all, power is available. We're not in a station blackout.

So systems that are not directly affected mechanically from the event, or affected say by other mechanisms, should work with high reliability. You were talking about independent hardware failures to lead to the loss of systems.

Now systems have been running at this time. So there, any probability that the failed to run would say that they would stop and the operators aren't able to restore the systems.

We're entering with LOCA events and stuck open safety relief valves.

In the LOCA events, of course, the reactor cooling system has been cooling and depressurizing for a while. In the case of the medium LOCA, the estimates for the time of failure of the reactor pressure vessel, and this is based on examination of the FAVOR calculations.

We're talking some 15 or 30 minutes after the initiation of the event. These times are indexed, by the way, to the 40 EFPY, effective full power year results. For large LOCA, things happen more quickly, of course, but still reactor pressure vessel failure occurs minutes after the occurrence of the LOCA.

And this has an effect on the thermal hydraulic state when we challenge the vessel. For the stuck open and safety relief valves, the system is at pressure, perhaps 2,400 PSI or thereabouts. But the pressure vessel failure is predicted to occur between 60 and 120 minutes after the trip.

So the system has been cooling for a while before the reactor pressure vessel is predicted to fail. With that, Dave is going to show some calculational results. Do you want to switch chairs?

MR. BESSETTE: What we did was to total up the primary system energy for all the PTS significant transients, that is to all the transients that contribute one percent or more to the total probability of failure.

So this is the plot for all the Oconee transients. If you remember, Oconee had a lot of contribution from events. There was a stuck open pressurizer safety valve that recloses. And most typically we took a reclosure time of 6,000 seconds.

The LOCA event that show up is this transient here. For LOCAs, the vessel failure time is typically about 1,000 seconds or thereabouts. Whereas the stuck open SRV cases typically fail around 7,000 seconds.

This is the initial primary system energy and power. And this dotted horizontal line if the energy of a primary system that was filled with 212 degree water. So basically this is a, you might say is a zero reference point for blow down potential.

So you can see when vessels fail for a LOCA-type event, basically there's no blow down potential. For the stuck open SRV cases it's perhaps, you're dealing with roughly, effectively one-third of the initial system energy.

This is the same plot for Palisades. These are the LOCAs and these stuck open SRVs, so you can have some idea of the blow down potential at the time the vessel fails.

DR. RANSOM: Is that based on the energy of the amount of the water still in the vessel?

MR. BESSETTE: This is so, these plots are the total primary system energy, includes both water and steam.

DR. KRESS: This is enthalpy.

MR. BESSETTE: Enthalpy, that's right, enthalpy.

DR. BANERJEE: Oh, it doesn't include the metal and fuel?

MR. BESSETTE: It does not include the

metal structures, no. Basically for a blow down, I mean there is some energy contribution from the metal, but in terms of blow down it doesn't, it's not a main contributor.

DR. BANERJEE: And the fuel?

MR. BESSETTE: The same thing with the fuel. The fuel is cold, by the way, fuel is the same temperature as the liquid. So in these vessel failure events, the fuel is passed about 300 F, with no stored energy.

We're not dealing with, there's not a difference. So when you have a large break, it occurs from here. And plus you have some additional, you have a significant energy input from the fuel from the stored energy.

These events, the fuel has, so to speak, no stored energy.

DR. BANERJEE: So zero time is vessel failure time?

MR. BESSETTE: Zero time here is the time of the initiating event. Now all these, these PTS events start with some sort of a LOCA. Let's say a four inch hot leg break or a safety valve sticking open.

Some time into the event is when the

vessel is predicted to break. So they say, some type, for a LOCA, the vessel is predicted to break at about 1,000 seconds. In these stuck open SRV cases it's dependent upon when we reclose the valve.

And typically we reclose it around 6,000 seconds. It takes another 1,000 seconds for the system to refuel and pressurize, so the failure occurs about 7.000 seconds.

In fact, these numbers are the calculated failure times here, by FAVOR.

DR. KRESS: The main point is that containments are designed to withstand LOCAs.

MR. BESSETTE: That's correct.

DR. KRESS: So if you have a LOCA is not going to fail the containment, unless you have other things going on.

MR. BESSETTE: That's correct. I'll show you the containment pressure plots for these two. Containment is designed to take this amount of energy, plus the, like core stored energy and instantaneously dump that into the containment.

And finally, this is the same plot for Palisades. Palisades is dominated by LOCAs, so we're dealing with vessel failures around here.

DR. KRESS: Yeah, we were concerned that

the blow down forces on the vessel might fail containment.

MR. BESSETTE: So we have, I have some indication on what kind of pressure differentials that they generate. We did calculations with three left. We used the Calvert Cliffs model, which was similar to, Calvert Cliffs is similar to Palisades.

We used Calvert Cliffs because we had an existing containment model for that plant. With two representative transients, the four-inch surge line break and a stuck open pressurizer safety valve that recloses at 6,000 seconds.

We looked at two vessel failure modes, an axial break at 12 square feet, that's a one foot by 12 foot break. And then a full 360 degree circumferential break on the vessel. With three break opening times, ten milliseconds, a tenth of a second and one second, this is, let's say, the fastest conceivable break time for the vessel.

And this perhaps, who knows exactly. This may be more representative. The, let's say the vessel break opening time is important because very fast breaks you can heave these subcooled pressurization waves going through the fluid.

DR. SIU: Excuse me, just for a second.

I forgot to point out, by the way, that the viewgraphs are this handout here. So this is a substitute for the packet, that segment in the package that you have.

DR. KRESS: So, containments are designed to stem double ended rupture of the largest pipe. And how does that 12 foot square compared to that.

MR. BESSETTE: A large cold led break is about six or seven square feet. So it's about half of the size.

DR. KRESS: So you're actually --

MR. BESSETTE: We're in the ball park.

DR. KRESS: You're in the ball park but you're subjecting the containment for a little more than normally it's designed for.

So it's a little bigger break occurring at lower system energy.

DR. KRESS: Oh, yeah, it's a lower energy, that's right.

MR. BESSETTE: This shows you where we located these breaks in the RELAP model. This is the circumferential break. This is the core region here, so its, we've located the break near the bottom of the core.

The break extended across six RELAP nodes, so you get junctions above and below, it says 12 junctions. This is the axial break. This extends 12 feet in the region, again, adjacent to the core.

So we got from the bottom in the downcomer, from the bottom of the core to the top of the core.

DR. KRESS: Now, you're using RELAP to calculate the blow down rate, is that what you're using RELAP for?

MR. BESSETTE: Yes, so we used RELAP for the blow down. We used RELAP for the entire transient starting time zero. We go through the initiating event which is four inch LOCA or the stuck open SRV.

And we initiate the vessel break at a, let's say at predetermined points in time. We put a flag, let's say, and RELAP opened the vessel break.

DR. KRESS: So it's still coming out at choke flow?

MR. BESSETTE: Yes, yes.

DR. RANSOM: You're doing this for a consequence analysis, is that right? I mean these are highly improbable events apparently.

MR. BESSETTE: Well, that's right. But we wanted to get some idea of the, let's say the pressure forces within the vessel and the containment pressurization.

DR. KRESS: But we asked them what the probability was of containment failure given this event.

DR. BANERJEE: And this doesn't take, the thing doesn't open up and throw missiles and things all over the place, nothing like that?

MR. BESSETTE: Well, that was one of the questions. How large are these, these blow down forces. And from what we can see so far, there's no, we're not filling the core barrel or we're not breaking up fuel assemblies, that sort of thing.

We're not generating ex-vessel missiles.

DR. BANERJEE: So this practice grows and stops. It doesn't sort of unravel the whole thing?

MR. BESSETTE: Well, that's the question too. We looked at both cases. We looked at cases where what possibility it is, it starts and it grows, let's say, the length of the weld, which is perhaps eight feet or so.

And it stops at the end of that particular plate weld. The other possibility is that it goes to that point and then it continues around a vessel, 360.

DR. BANERJEE: Is there sort of evidence of that. Because BSF, which is a company that did some vessel tests where they cracked open a vessel like this and it sort of just unwound and boom you had a really -- there's a lot of this documented.

Now I don't know if this is a muck thicker vessel or what it is, but these things sort of, there is evidence that they just come apart.

MR. BESSETTE: Well, yes, one of the candidate cases we looked at is there vessel was in two pieces circumferentially.

DR. SIU: The PNNL Study we talked about a little earlier in the presentation, certainly they did some analytical calculations to look at the progression of the crack. How far it would extend, whether it would turn.

They didn't calculate where the crack would arrest, but they also, in later parts of that report looked at missile generation. Talked about failure of vessels under pressure and what kind of missiles could be generated from that. And I'll talk to that a little bit later.

MR. BESSETTE: So these are the primary system conditions taken at the time that we failed the vessel. So for a four inch break-to-break, the vessel break time was 2,400 second.

The primary system pressure was 200 psi. The downcomer temperature was 250 degrees and that was at saturation and that's the corresponding FOP. Stuck open SRV case, we failed the vessel.

This was, let's say we imposed this time since we're not dealing with a FAVOR generated time in this case. Pressure was, we failed the vessel when we reached the safety valve set point at 2,400 psi.

Downcomer temperature was 355 in this case, F. This is somewhat higher because in Calvert Cliffs, even with the stuck open SRVs, we can't get cold enough in the downcomers we do, let's in Oconee where this transient shows up as being more significant.

And then for comparison, we did a large cold leg break LOCA. This initiates at time zero, initial system conditions.

MR. LEITCH: In the second case there, what are we assuming, the vessel, that their stuck open relief valve opens at time zero. And then at 82.30 seconds is when the vessel fails?

MR. BESSETTE: That's right. We opened at time zero, we closed at 6,000 seconds or 100 minutes.

MR. LEITCH: Okay.

MR. BESSETTE: And then --

MR. LEITCH: It recloses.

MR. BESSETTE: -- it took another 2,200

seconds in the primary system to completely refill. And once we lifted the safety valve casing, we broke the vessel.

DR. RANSOM: Now when you say you broke the vessel, do you mean you exceeded one of the these fractured criterias?

MR. BESSETTE: Well, this case, since this is a scoping study, which this is not, these calculations would not tie directly to FAVOR. We broke the vessel at this particular time. I can say this was tied, we tied this to the time when the primary system went water --

DR. RANSOM: So this kind of scenario would assume something more than the normal pressure, PTS type of transient that would rupture a vessel.

MR. BESSETTE: Yeah, but basically, these two, these two transients are quite representative of the risk dominant sequences. And we've got the, most, about two-thirds of our risk dominant sequences are the LOCAs. Most of the rest are these stuck open SRV cases.

DR. RANSOM: What's the probability of either one of those occurring?

MR. BESSETTE: Overall, yes.

DR. SIU: Again, what we were trying to do in this part of the study is talk about what ‘s acceptable as opposed to what we would achieve. And part of this discussion is to argue that there's margin between the occurrence of a PTS induced reactor pressure vessel failure and large early release.

So we're trying to get a sense of what are the forces involved, because if there are large forces involved, we might have to argue that the mitigating systems, such as containment spray or ECCFs in recirc mode are effected by the occurrence of the PTS event.

Therefore, there might not be much margin. If we can demonstrate that the forces are low, there's little dependence between the occurrence of the event and the failure of these systems and therefore there is probabilistic margin. And that's the essence of the argument that we're trying to present.

DR. BANERJEE: You're doing a consequence model here. Pure consequence. There's no risk, probability aspect.

DR. SIU: It's conditional, that's right. Exactly.

MR. BESSETTE: These are some of the results calculated for Calvert Cliffs by RELAP. We have, again, the three transients to be calculated to four inch surge line breaks and stuck open SRV.

And for reference the design basis accident LOCA. Here we're looking at two vessel break opening times, ten milliseconds and one second. We looked at axial and axial vessel breaks and circumferential vessel breaks.

And these are the peak differential pressures as calculated by RELAP. And one of the things, of course, is that these peak pressures are highly dependent on this vessel break opening time.

The slower the, you go from ten milliseconds down to one second. These peak pressures drop considerably in most cases. And the other thing about this is that I'm showing, these of course are peak pressures.

For these ten millisecond cases, these are, you know, you might say of sonic nature. So their durations, these peaks are very sharp. The durations are on the order of ten milliseconds. So that's kind of an impulse load.

And you can see these duration times, roughly speaking, are in this column. This basically gives the message that these pressures, these peak loads drop considerably with longer opening times.

And for these really fast break opening times, they are very short duration. But you can see, generally speaking, they are comparable to or much less than a design basis large break LOCA.

The vendors typically will analyze large break LOCAs for these conditions very quick, almost say instantaneous break openings. This is the calculated containment pressures from these events.

So on the bottom here, this is at the time of the vessel break. For comparison, this is the large, cold leg break design basis accident. This is the containment pressure. This is additional pressures, 15 psi and roughly atmospheric.

These four inch break LOCAs, since the LOCA has been in progress, you're starting from a slightly elevated containment pressure when the vessel breaks. And you can see the relative pressure rise.

You recall that there is very low system energy in these four inch break cases when the vessel fails, so you get only about a 3 psi, 4 psi pressurize.

DR. KRESS: Where did you get that initial pressure from?

MR. BESSETTE: This pressure here? We calculated this whole primary system containment.

DR. KRESS: Oh, you used RELAP as a containment model.

MR. BESSETTE: We used RELAP as a containment model.

DR. KRESS: Okay, thank you.

MR. BESSETTE: And these are the stuck open SRV cases. The pressurize is about 10 psi, compared with the cold leg break of about --

DR. KRESS: So this is RELAP as a containment model using one node in containment?

MR. BESSETTE: No, this is about containment, you can, you can --

MR. LOTT: They have about 15 nodes.

MR. BESSETTE: Yeah. You can nodalize, you can have some flexibility in terms of how you nodalize containment with RELAP. It's not like containment where you have a single node.

DR. KRESS: How do they compare the containment?

MR. BESSETTE: To contain?

DR. KRESS: Yes.

MR. BESSETTE: We don't have a comparison here for contain, but we've looked at RELAP with containment modeling versus other calculations. We did that for AP 600, and it's in the, it's in the right ball park.

DR. KRESS: The 36, how does that compare with the design pressure?

MR. BESSETTE: Design pressure is about, it's about 45 psi.

MR. LOTT: Tom, Norm Lott. It didn't you have all the containment features in it. All would have included fan coolers, but there were no fan coolers. But it does have a spray cooling unit and it has, dumping all the energy from these, both the transient and from the less than zero is the PTS transient dumps energy in as well. And then after the vessel break, you've got the vessel break energy. And I think that's the main thing that Dave is trying to show here.

That if you don't have a very energetic system, it doesn't pressurize and contain it very much.

DR. KRESS: Yeah, I think that we recognized that. Our concern was whether you've got a hole in the bottom of the side of those things and you've got a momentum forces tending to move the vessel and the penetration on the hot leg or the cold get going through the containment, would that, you know, contain it, I think was one of our concerns.

MR. BESSETTE: Yeah, we looked at this momentum flux aspects, you know, jet reaction force and that sort of thing. We don't have, we're still working on some of those things.

So it doesn't look like the, again, it doesn't look like the reaction forces you get from a vessel break or any worse certainly than a cold leg break.

DR. SIU: The other thing that's, again, worth pointing out, Dave had the right-hand column showing the duration of the pressure pulse. And it's very short. There's no time.

DR. KRESS: That's an impulse.

DR. SIU: Tens of milliseconds and this thing is over.

MR. HACKETT: Dave, this result, too, is large dry, right? This is showing Calvert Cliffs? It's specific to that type of containment?

MR. BESSETTE: Yes, Calvert Cliffs.

DR. KRESS: Would there be any special considerations for ice to the condenser containments. Would the steam go where it's suppose to go in those?

MR. BESSETTE: I mean off hand I can't think of any particular reason why things should be much different. Certainly the primary system energies are going to be the same. So the blow down potential is going to be the same.

DR. KRESS: The primary system is all the same, right.

MR. BESSETTE: So the enthalpy discharge when the vessel fails is going to be the same. And also, the rate at which this energy gets discharged in the containment is essentially so fast that whatever containment heat sinks are there --

DR. KRESS: Don't come into play much.

MR. BESSETTE: -- really don't come into play.

DR. SIU: So far you haven't seen any probabilities associated with these. What we were trying to do is establish a sense of the conditions that the containment would see and what the reactor pressure vessel would see.

And actually what you've seen is material that we've generated since, or finalized, I should say, since the writing of the report. So these arguments were not factored into the report, and so it's an additional conservatism, I think, on the results that we're going to talk about in a second.

This is a diagram here, again, it's in your hand out. It's not in the report, per se. It just is another slice at that 200 sequence event tree. APET, it shows the four scenarios that we identified in the report as being of potential interest.

I couldn't give you the numbers off hand, but it doesn't really matter. I'll walk you through some, just as an example. I passed around a hand out with some colors on it showing three different kinds of scenarios.

This is, again, basically that same picture blown up a little bit, but with some of the scenarios highlighted. The red scenarios, again, are those that lead to the unscrubbed large early release.

Blue scenarios that lead to a scrubbed release. And the pink scenario is something rather more benign. It could lead to the scrubbed release, but the probability should be significantly lower as I'll talk to you in a second.

So I'll try to talk about all three as I walk through the tree. Okay, so again, we enter with a PTS event. Crack orientation, as I indicated already, we think roughly a 90/10 split based on the plant-specific calculations to date.

Based on the PNNL work, NUREG/CR-4483, there are, there is a distribution of probability across the different crack extension possibilities. Remember the top branch associated with the crack arrest at the circ weld.

The next branch was crack progressions beyond the circumferential weld. And the bottom branch on the axial crack leads to a circumferential crack. This is the one where the crack turns the corner and continues.

And here on this tree you'll see that I've indicated both the arrest and the propagation possibilities for the case where the crack turns the corner. It's not that we're going to say that these numbers are hard and fast.

The PNNL report actually shows that there is significant variation across the three plants that they looked at when they did the calculations. It's just to indicate that there is some distribution and we didn't take any credit or significant credit for the fact that this particular branch might be, let's say, along the 45 percent line as opposed to 15 percent line.

We just didn't bother with that. But if one were to pursue this in more detail, obviously, that would be a potential place to look at. The hole sizes we looked at we associated deterministically with the different crack propagation possibilities.

So, again, the bottom,. let me focus on, I don't want to blind anybody. Okay. It's on already. Can you hear me? Okay. So, here we have the crack. This is an axial crack that initiates the circumferential crack.

And the crack progress and it's arrested. And then again we have the case also where the crack continues. And we didn't assign a split fraction associated with that. If the crack is arrested, there is a possibility of a moderate size hole, which turns out to have relatively low consequences. Or a larger hole.

This was the 100 to the 1,000 square inch hole opening. Following that, depending if the forces are roughly design basis or significantly greater design basis, that's the branch in here. And that's what Dave was just talking to you.

We did not, at the time of the report, we had a suspicion that the tree should go up in this direction, we didn't have a basis for that. Now I think we have a stronger basis for saying this branch seems to be rather low likelihood.

So again, the thermal hydraulic calculations to date would indicate we would probably head up the upper branch. But these two branches are branches that we've identified in the report as being potentially significant.

If the forces are a roughly designed basis, then the question of containment isolation, this question is really a question of independent failure at this point. If the forces are beyond design basis, then obviously there's a potential for dependence, that's the concern that you raised.

And so we allow for that. The containment spray, and this is probably the crux of the argument. If we had to boil it down to one slide, this would be it. We look for mechanisms by which we could fail containment spray due to this particular scenario.

We looked at the possibility of missiles and we looked at the energies associated with potential missiles and whether they could penetration the biological shield around the reactor pressure vessel and basically get to the containment spray lines which are running up the inside wall of the containment, and just did not see that that was happening.

There was just, the penetrating capability of these missiles, even if you assumed optimal shapes and assumed hardening, just the forces aren't there. So that tells us that the sprays are independent.

Now there is one potential fly in the ointment and that has to do with some blockage. We assumed that sump blockage, or programmatically you said sump blockage is an issue being addressed in the GSI-191. And we were very explicit about that in the report.

And presuming that that issue is addressed, then containment spray is indeed independent and the reliability is the reliability of a multi-trained system that should be ten to the minus two or even significantly less than that. Unreliability should be less than ten to the minus two.

DR. KRESS: Let's hope that that sump blockage is resolved before you actually get to a pressurized thermal shock effective full power year of 40 years.

DR. BANERJEE: But the issue of sump blockage would come from the insulation on breaking apart.

DR. SIU: That's right. Remember, we've entered this perhaps with a large LOCA. So you've got the same sump blockage issues, potential sump blockage issues. Recirculation generally we would predict to occur after the reactor vessel fails.

So any additional debris or stuff coming out might add to that problem. But there's already a problem independent of the PTS.

DR. BANERJEE: Right. So you either spray or you don't depending on sump blockage at that point.

DR. SIU: That's right, that's right. Okay, now would this be a road block in case the issue is not resolved? No, I think that, but then you'd have to purse the other lines of argument that Dave has already indicated.

The energy available and what that, whether, for example, it would lead to consequential failure of containment.

DR. KRESS: So with the sprays already you have a ten to the minus two.

DR. SIU: That's exactly the point, yes. We would, arguing independence based on the consideration of the causal mechanisms. Fuel location, I won't get into. Again with the low energies involved, you wouldn't expect.

In fact, we did a preliminary analysis looking at the core barrel distortion associated with some of the pressure differentials that Dave calculated. It showed relatively small strains and it's not a surprising result.

DR. KRESS: You know, for the large breaks, where you pretty much assume it goes to power oxidation event, because you dump the water out pretty fast. I recognize that the blow down will fail containment and then you've got a lot more energy coming out of air oxidation.

And maybe a lot of hydrogen. Does that worry you about the independence of the sprays?

DR. SIU: Well, this will get to an issue of timing which we clearly didn't address. The, thinking in terms of a large early release, when something has to occur within four hours or four hours or less.

DR. KRESS: You might have a, the early part of the large --

DR. SIU: Reactor pressure vessel failure, as we said, for the pressurized scenarios you're talking maybe 60, 120 minutes down the road from the initiating event. The LOCA events it does occur more quickly.

DR. KRESS: That kind of impacts on my issue that I think I've about got the Committee convinced is right, that we shouldn't just focus on large early release. There ought to be some considerations of late containment failure also.

DR. SIU: Yeah.

DR. KRESS: You know, pretty soon I'll get them on my side.

MR. BESSETTE: Of course you actually need steam oxidation to get a lot of hydrogen.

DR. KRESS: You need steam to get the hydrogen.

MR. ROSENTHAL: If it's an axial crack then we would, then for some of the cases they, even though the cracks there, you have like a wheel well effect, you're still pumping a lot of water.

And so you have water in the bottom of thing and so you'd melt the core in a steam environment. If you go down this ten percent probability path where the axial crack comes to the circumferential weld and then unzips around and the bottom head falls off, now you've got clearly an oxidizing environment.

And it's correspondingly lower probability and you still ask are sprays running to scrub. So we've tried to reason our way through it.

DR. SIU: Just to finish the tree off here, again, if the forces are roughly design basis then we wouldn't expect a knock on effect on to ECCS and, by virtue of pulling pipes. And so again you would get some high reliability out of that operation.

We did say well it's potentially dependent failure here, not knowing at the time of the report what the forces were. We also pointed out the possibility of cavity cooling. And for some of these plants you would expect, indeed, the water level to rise above the top of the fuel and to cool the fuel that way.

And so you shouldn't get core damage, let alone in a large early release there are other plans for which you can't count on that. You have some water in the cavity, but not enough to assure that the core remains intact.

Okay, so, as Dave pointed out, we believe that the accident energetics are more benign than many of the scenarios that we've already analyzed. We believe containment pressurization is likely to be less than what you would get from a design basis LOCA.

We, Dave showed you the delta ps associated with the cases that we analyzed. And so we think that it's likely, obviously this is not a full proof, we haven't looked at all the various possibilities, but it's likely that the blow down forces are likely to be on the same order of magnitude as the design basis LOCA or even less.

And again, point out that the time over which these forces are acting is very, very short. We actually think the containment spray failure probability might decrease for these events are compared to the risk significant events because you're not in a station black out situation.

So you're largely talking a hardware failure or possibly operator error. We talked about the likelihood of fuel cooling being dependent on reactor cavity design. And of course the point that GSI-191 is the issue addressing the sump blockage.

DR. WALLIS: I wasn't here for this, but if you unzip a reactor and it's got 2000 psi in it, would you apply 2000 psi to the whole --

DR. BANERJEE: It's down in there.

DR. WALLIS: If you split it in half, half goes up, half goes down?

DR. BANERJEE: It's down in pressure when it splits.

DR. WALLIS: I know the pressure goes down, but initially the pressure is very high. So the initial force is bigger than large break LOCA. It doesn't last very long.

MR. BESSETTE: Yes, well, if you look at the situation, you know, those events that have a stuck open SRV that closes, you are in need of a 2,400 psi. But that pressure is saying, it's not a thermal pressure, you've got a lot of cold water that's been pressurized by a pump.

So you're just dealing largely with the compressibility of water which is --

DR. WALLIS: Okay, so it goes away very quickly. It's not like steam.

MR. BESSETTE: That's right, it's not like hot 2400 psi water.

DR. SIU: Just on the separate hand out, viewgraphs 20 and 21, we had some of the calculational results. Okay, where are we in terms of conclusions.

The, we believe that the conditional probability of early fuel damage, and this is really the core damage question, would be extremely small for plants where you would get the flooding, but it's non-negligible for the plants, you could have fuel damage for plants where you're not going to get the flooding.

And this is absent any real, you know, phenomenological analysis. This is just based on rough consideration.

DR. KRESS: When you non-negligible, it still could be pretty small.

DR. SIU: It could be. Again, we did not do any calculations at this point. You'd have to look at --

DR. KRESS: I believe the reliability of sprays is at least less than .01.

DR. SIU: No, yeah, but I'm talking fuel damage in the first bullet.

DR. KRESS: Oh, oh.

DR. SIU: The second point is the sprays, right. That we believe regardless of the cavity design, the conditional probability of the early containment failure and a large early release would be very small, very small in that I've used that terminology saying less than .01.

However, should a large early release occur, we haven't done anything to show that large scale air oxidation will not occur also.

You'll see, if you were given the full event tree, which you weren't, you would see in that that most of the sequences involved large early release. Also we would say would involve large scale air oxidation. So they are, the conditions would lead to both.

DR. KRESS: And those sequences normally aren't the dominate PTS sequences, I thought I heard.

DR. SIU: Well, those sequences would, these are all, the APET is tied to the dominant PTS sequences. We don't think any of those sequences are likely. Conditional on the occurrence of the PTS induced reactor pressure vessel failure.

So, the implications for the reactor vessel failure frequency criterion, we think that the ten to the minus six value is consistent with the philosophy of the original PTS rule. It's consistent with the guidance you've given us in your July letter and with the safety goal policy statement.

We think it's consistent with the philosophy of the rule because basically we have this low conditional probability of large early release, given the occurrence of a PTS induced reactor vessel failure.

So that would ensure your low level of risk. I mean if you were just to take numbers literally, say, ten to the minus two times the ten to minus six, that gets you to ten to the minus eight. And that's extremely low.

And obviously for similar reasons, the relative contribution to total risk would be small because this would be a virtually negligible contributor. Ten to the minus six is indeed more limiting than what you might use otherwise in terms of core damage frequency.

And this was the point that you raised earlier and thought that we should look at something that was based on LERF considerations and not core damage frequency consideration
If you were just looking at core damage you would pick something like ten to the minus five.

We think that this is consistent or even conservative with respect to the quantitative health objectives, both in terms of prompt early fatalities and in terms of latent fatalities, because again if you equate the ten to the minus six with core damage, I think we would be right there.

So that's why we, in the report, we stated that we think that we can support a ten to the minus six per reactor year acceptance criterion. Again, as I indicated in the beginning, our expectation is that embrittlement limits would be set in a risk informed manner, so what we're talking about here is an important input to that process but it's not the only input.

And that's just basically the same thing I've just said. So, I think we're at the end of the hour.

DR. WALLIS: Now we were told this morning that the predicted frequency is actually much less than that.

DR. SIU: That's correct. These are acceptance criteria. This says what we are willing --

DR. WALLIS: So, I was thinking, what would be the effect then if you have a frequency which is far less than that, then would this lead the Licensees to say, now, we're no longer going to be limited by this, can we change something about how we operate our design.

Is that, is there something like that likely to happen.

MR. ROSENTHAL: Yes.

DR. WALLIS: And what sort of things would be likely.

MR. ROSENTHAL: Well, I think we told you earlier that we would expect that Licensees, these places were originally designed with flat core power distributions and high, and hence higher fluence in the vessel walls.

They'll want to regain some of that margin because it limits them with respect to the TCT and things like that. So they'll flat, and also fuel economy. So they'll go back to, to some degree, to flatter power distributions and higher fluences. But I think that we've addressed that.

DR. KRESS: And eventually it might even lead to a second license extension.

MR. HACKETT: It could be. So flatter power gives you more margin to LOCA and DNB.

DR. WALLIS: It is the immortal vessel.

DR. SIU: Well, recognizing of course that PTS is one class of scenarios and Mark talked earlier about some other considerations that would have to be thought about before we make these changes.

MR. ROSENTHAL: I also suspect I'm talking about less than factors of two on an issue with multiple orders of magnitude of certainty.

DR. SIU: Questions?

DR. KRESS: I think it's pretty clear what they did.

DR. SHACK: I mean you would come back to essentially your start up shut down would then be your limiting vessel operation and however you decide to change that, in all likelihood it would still end up being probably the controlling thing on the vessel.

DR. KIRK: Yeah, well the only reason why, at this stage, the start up shut down would be more limiting is having done this analysis where we've made our best effort to be realistic.

And when you consider that Appendix G

includes many of the same varied conservatisms or greater than we started with here, then they've not been done on a consistent basis.

But certainly you know, given the difficulty that a significant LOCA has in breaking the vessel, it's very difficult for me to envision that a controlled heat up and cool down, even done, you know, as aggressively as you would want to from an operational perspective, is going to be of any significant challenge whatsoever.

DR. KRESS: If I may ask you a strange question. When we talk about safety goals, prompt fatality safety goals, it was said because the way it was there were considerations that have at least 100 plants out there operating for about 40 years at that level of safety.

It kind of was that consideration. Now you've got one plant that you're talking about that's already used up all of its life and it's only honorable to set of sequences a short time. So the question is why isn't reasonable to think the safety goals is the right value to use here when, it's all right, I think you're all right with the safety goal, but was that even at in your thinking?

DR. SIU: No. Yeah, we actually, the

question came up in our recent public meeting from a somewhat different angle. That, whether the fact that the plants are only approaching this level or risk toward the very end of their life, whether that makes a difference.

Clearly it could. Safety goals, as I understand them, regardless of how they were derived, are stated in sort of an instantaneous frequency terms and that's kind of where we are.

DR. SHACK: And where we should stay.

DR. WALLIS: I wasn't around, sorry. Did you talk about the long term cooling or the long term situation at this station after it's had such an event?

DR. SIU: No, we were focused largely on the large early release issue.

DR. WALLIS: Yeah, I know that's the way that this Agency thinks. But I think the public might be concerned about something with was not clueable in the long run.

DR. KRESS: See, I have one convert already.

DR. SIU: But I guess again if you equate, even, and I think we've shown because of independence of various systems, that the occurrence of the PTS event does not equate, it's not equivalent to the occurrence of core damage. There is some margin. Certainly for some scenarios.

But even if you were to equate it to core damage, setting the limit at ten to the minus six per year for that should addressed that concern. And you're saying you just, I mean this is the point, I guess, Dr. Powers was making, there's very, very low likelihood this event is going to occur.

So low that it's in the thinking behind Reg Guide 1.174 and the definition of what small means. It's small, almost you can't measure it.

DR. SHACK: Well, I suggest we take a 15 minute break at this point and we can come back to discuss this proposed screening criteria.

DR. KRESS: Thank you, Nathan, that pretty well answered my questions on this.

DR. SIU: Thank you.

(Whereupon, the foregoing matter went off the record at 3:20 p.m. and went back on the record at 3:37 p.m.)

DR. SHACK: Back into session.

DR. KIRK: Okay. This is the discussion of the considerations regarding a new proposal on a materials based PTS screening limit. I've added a few slides here to try to make the points more clearly.

I'll start by reviewing some operational challenge considerations, discuss some materials considerations and then lay out the characteristics one would like to see in a physically motivated embrittlement metric, and then show you how the heck we got to RT_{NDT*}.

And I should point out this is simply one possibility among many, but we think it has some desirable features. Operationally you've seen the graphs on this slide before in our discussion of the plant-specific results.

But the point I'd like to reiterate is what's shown in yellow that all materials factors held equal, the severity of PTS challenge is remarkably similar between the plant study. And the frequency of challenge is also fairly similar but with some greater plant dependencies.

The reason for pointing this out is this observation leads us to at least one metric of success on our embrittlement metric that we shouldn't be really expecting to see much separation between the plants if we get the embrittlement metric right.

From a materials viewpoint, again, this is a repeat, but we'll reiterate the axial weld flaws and the material properties that can be associated with axial weld flaws are what's driving the through-wall cracking frequency.

So to set up, what do we want to see an embrittlement metric? Well, certainly, what we'd like to see is, again, shown in yellow. We'd like there to be a causal relationship between the embrittlement metric and through-wall cracking frequency.

Or, as my ten year old would say, you want to blame the right person for the failure. Don't go picking on me, it was my little brother that broke the vase. So given that principle, the axial weld and plate property should dominate the embrittlement metric because those are the properties that can be associated.

DR. KRESS: Is that because there are so many more axial welds than there are circumferential welds?

DR. KIRK: No, no. It's because the axial flaw orientation produces a higher crack driving force, than the circumferential flaw. And also --

DR. KRESS: Yeah it would with the thermal shock.

DR. KIRK: Right. And also of particular

importance that higher driving force perpetuates much deeper into the vessel wall. The circumferential crack are much more likely to arrest. However, it is possible to get circumferentially oriented cracks that can fail the vessel.

So they do play a minor role. A third important point is of course that the relevant fluence has to be that where the flaws are. So the relevant fluence is that along the welds and that the large regions of plate and forging remote from the welds really don't count for much.

So these are some slides that I inserted, that since we have time, I thought we could step through.

DR. BANERJEE: Could we have copies of these?

DR. KIRK: Yes, absolutely. I thought we could step through these to go from an embrittlement metric of the type that we've got now to the one that we're proposing, so you can sort of see the thought process rather than just be confronted with a screen of algebra.

First off, there will be no margins here. So we're just not going to go there again. It was too painful the first time. So all RT_{NDTs} that you'll see plotted here would reflect an unirradiated RT_{NDT} plus an RT_{NDT} shift appropriate for the irradiation conditions of interest.

So right now the way we evaluate a vessel, setting aside the margin part, is we characterize the vessel as having the maximum RT_{NDT}, wherever it is in the vessel evaluated at the maximum fluence.

DR. WALLIS: So this RT_{NDT} here you're plotting is something that comes from the ASME formalism for evaluating and it doesn't come from anything you've corrected for your, the epistemic thing, it doesn't come from anything that gets you to the mean instead of the extreme. This is the traditional ASME RT_{NDT}?

DR. KIRK: Yes, yes. And the reason why we're using that is not because the traditional ASME RT_{NDT} has any desirable features except the one desirable feature it does have is that we've established and docketed a value for each and every material in each and every plant.

DR. WALLIS: And people know how to measure it.

DR. KIRK: And that's about the only thing it's got going for it.

DR. WALLIS: Isn't it also true that

people know how to measure it, it's sort of traditional they know how to get it.

DR. KIRK: Yes, that's correct.

DR. SHACK: Wait, let me, let me, so you're not correcting for the 65 degree bias?

DR. KIRK: Yes and no.

DR. WALLIS: Oh, well, you can't have it both ways.

DR. KIRK: Yes, I can have it both ways. The correction for the 65 degree bias is inherent to these values. Because these have been calculated by FAVOR. However, there is no correction for, this is the straight ASME RT_{NDT} here. So we're just using that value, but these values have all the biases and aleatory and epistemic, that's all been accounted for.

DR. WALLIS: That went into the calculation.

DR. SHACK: The semi-regulatory RT_{NDT} .

(Laughter.)

DR. KIRK: Yes, that went into the TWCF.

DR. WALLIS: It didn't go into the RT_{NDT}.

DR. KIRK: Yes.

MR. ROSEN: Semi-log.

DR. KIRK: You can tell it's getting late in the day. Okay, so what's on the horizontal axis is the ASME RT_{NDT} plus the Sharpy shift, Sharpy shift evaluated from the Eason formula. No margin, no nothing. So all the information that was needed to calculate this is in the ASME RT_{NDT} method, Eason embrittlement formula and copper, nickel and phosphorous values in the Arvid database.

And what you come up with is a significant separation between the plants and in particular, you know this is, or one would expect that this wouldn't relate things terribly well because, for example, in Oconee the maximum RT_{NDT} is in the circ weld.

And we've already told you that the circ weld doesn't contribute much. So, in the context of my sons, I'm blaming the circ weld for breaking the vase, but actually it was axial weld that did it. So one.

DR. BANERJEE: Excuse me, what is the physical reason for the separation?

DR. KIRK: There is none. It's the wrong metric here. That's what we're trying to get to.

DR. BANERJEE: Oh, it's still the wrong metric? Oh, okay.

DR. KIRK: Yes. I'm working you to, remember I started here and said that a physical appropriate metric would have all the, there would be a causal relationship between the thing we're plotting on the X axis and the result on the Y axis.

The problem with the first iteration, which is very much akin to what we do now, is that causal relationship is broken because we just pick the maximum RT_{NDT} in the vessel and that might be a circ weld, and we know that circ welds aren't major contributors.

So at the first step, if we just take that out and say, okay, well, --

DR. WALLIS: Wait a minute. This RT_{NDT} is a function of, it's different for different welds on different parts.

DR. KIRK: Sure.

DR. WALLIS: I thought you got it from a Sharpy test. You do a Sharpy test of a weld?

MR. HACKETT: That's a way of getting it. There are a number of ways if you go through, as you're indicating ASME has methodology for getting at RT_{NDT} , and you can get it through measuring Sharpies, through drop weight NDT tests.

There are other forms of estimation, but yes it will work for different welds. It will vary upon conditions. The fundamental problem we're up against here, I just thought I'd mention it to see what it's worth.

Is we're trying to regulate to fracture toughness, which s the meaningful parameter here. The problem is these plants were all licensed before fracture mechanics, frankly, was all that much developed.

So we're sort of back fitting a science on something that wasn't ready for it and never will be. You know, in the case of the plants that are out there. So, as Mark is saying, you're trying to use a fairly imperfect estimator or index of the material toughness in RT_{NDT} to try to get to a fracture toughness or sort of more what is truth.

And it's got all the warts that you're seeing here and that's why it's so confusing.

DR. BANERJEE: These are measured RT_{NDT} at the inside of the vessel wall, I mean from specimens.

DR. KIRK: No, no. Let's be clear. What's going into all these, anything down here is the unirradiated, the RT_{NDT} measured before anything started, plus the Sharpy shift or the RT_{NDT} shift if you will, evaluated based on an embrittlement trend curve correlation evaluated using copper, nickel and phosphorous values that have been docketed by the plants as being representative of their materials.

DR. WALLIS: It's got the irradiation shift which is in your Appendix as a shift NT_{zero} .

DR. KIRK: That's right. No, it's got the irradiation shift that's in the Appendix is a shift in Sharpy that's in the --

DR. WALLIS: Delta T-30, then.

DR. KIRK: Delta T-30, yes.

MR. HACKETT: And then a further clarification on the unirradiated RT_{NDT} as you're indicating in some cases their measured values. In other cases they're not. And that's as defined in our 10 CFR 50.60, 50.61, as to what you can and can't do there.

DR. KIRK: All the complexities and the different ways, and indeed I would agree with anybody that says that are current RT_{NDT} methodology is confusing. But all the complexities and the different ways of getting RT_{NDT} and Sharpy shifts and so on have been incorporated in the FAVOR methodology and so are reflected in the vertical axis values.

What we're simply trying to do is find a meaningful yet easy to evaluate based on available data parameter on the X axis to use.

DR. KRESS: That has a one-to-one correspondence for all plants for that side over there.

DR. KIRK: That would be the hope and the reason for putting --

DR. KRESS: Or as close to it as you can get.

DR. KIRK: The reason for putting up the previous slide was simply to suggest that the fracture mechanics tells us that if you had, if you had these three different vessels and held the embrittlement equal and put one flaw in them of the same size, the level of challenge of these various dominant transients is not grossly different from the different plants.

DR. KRESS: And that would be what would separate them.

DR. KIRK: That's right, that's right.

DR. WALLIS: Maybe your final report you'll have RT_{NDT} with some superscript or something which says ASME or regulatory or best estimate or whatever, so we know which one you're talking about.

DR. KRESS: When you get ready to make the rule, you won't even have that other stuff in there.

DR. KIRK: Yeah, certainly we could do a lot better on nomenclature. I'd be the first to agree with that.

DR. BANERJEE: So are those lines then a function of fluence or did you just pick it at some point in time now.

DR. KIRK: Yes, they are a function of fluence. These are evaluated per example for --

DR. BANERJEE: At what fluence are they evaluated, those curves?

DR. KIRK: These are evaluated at the peak fluence in the particular material region --

DR. KRESS: For that plant.

DR. KIRK: -- for that plant. Well, no, because you've got different --

MR. HACKETT: At a particular time.

DR. KIRK: -- at a particular operating lifetime. So right now the formalism that you go through in 10 CFR 50.61, is you look at all the different plates, welds, forgings in your plant, you find the peak fluence within that geometric region and you evaluate the Sharpy shift based on your copper, nickel and phosphorus values at that peak fluence.

Then you find the highest value of all your different welds, plates and forgings, and that's what Mr. Mitchell will be forced to evaluate your plant based on. And so this is --

DR. WALLIS: That's your X axis.

DR. KIRK: That's the X axis. So this is the parallel to the current regulation. But pointing out that at least in one case, for Oconee, we're plotting the results from a circ weld, and we know from doing the FAVOR analysis that the circ weld hardly contributed at all through all cracking frequencies.

So again we're posing a causal relationship where one doesn't exist.

DR. WALLIS: The most striking thing is the yellow, the Palisades is about two orders of magnitude above Beaver.

DR. KIRK: I would caution you not to interpret this, because that separation is not real.

DR. WALLIS: We have to interpret it if you show it to us.

(Laughter.)

DR. KIRK: Well, then I'll take it out. Really we know that in Oconee, as in all the plants, it was the properties associated with the axial cracks. So it's either the higher of the axial weld properties or the fake properties that are controlling the through-wall cracking frequency.

So when we take out the Oconee circumferential weld, which was there, and plot the

Oconee axial weld, which is there, now Oconee and Palisades, which are both axial welds, are correlated reasonably well.

The flyer down here is Beaver. Now when we came up with this result, Denny Weakland, who is the Chief Metallurgist at the Beaver Valley plant, was extremely happy because all of a sudden his plant, which is within fractional degrees of the PTS screening criteria was somehow far less embrittled than Oconee which is so far down that nobody at Oconee really cares much about this.

And that was all terribly surprising. But, as I pointed out earlier, the problem with this procedure is that the current procedure, you find the peak fluence anywhere in your material region and you combine that with the copper, nickel, phosphorus and evaluate your embrittlement shift.

The problem, the reason this didn't work so well for Beaver, is Beaver, with the help of Westinghouse, has intentionally placed their fluence peaks way out in the middle of the plate. Not at the weld, where the cracks are.

So that where the cracks are is actually in a fluence trough.

DR. WALLIS: It sounds like a good design.

DR. KIRK: It's a very good design. So if you evaluate now the Sharpy shift for Beaver Valley with the appropriate fluence, that being at the axial weld, you find that it now agrees fairly well with the other results.

However, it should also be noted that we've said and we keep driving home and you're probably sick of hearing it, like most things I say. That the axial flaws and the axial welds are important.

Well, in Beaver Valley and Oconee, there are two axial welds. In Palisades, there are three axial welds. So, again, all other things being equal, Palisades has half again more axial welds and half again as more axial flaws as Beaver Valley and Oconee.

So if you normalize out the weld length effect, you get a slightly better correlation.

DR. WALLIS: You seem to be struggling to get us back as close as possible to the 270 to 300 degree range.

DR. KIRK: But it's a different number.

MR. HACKETT: And he'll never be able to explain that.

(Laughter.)

DR. KIRK: Yes, I will.

DR. SHACK: It's amazing how well the Reg Guide PTS does to correlate the data.

DR. WALLIS: Although it's the wrong one.

DR. KIRK: And that's got the margin in it and you know your colleague will never accept that.

DR. SHACK: I only look at the data.

DR. KIRK: Yeah, yeah. So what we came to, that was the thought process. But what we came up with as a weld length weighted embrittlement metric is illustrated on the screen here. And I'll, if you're interested, I'll try to step through this.

It includes to waiting factors and two weld length weighted reference temperatures. One weighting factor is for the plate and axial weld properties and it ranges anywhere from 90 to 97 percent contribution, which is consistent with our results.

And then you've got a reference temperature for plate and axial welds which depends upon the most embrittled of the two materials.

MR. HACKETT: I think you may have out done Nathan in powerpoint.

DR. KIRK: We're dueling, but he makes movies, so he beat me. The length of the weld and the max fluence along the weld. Then there's a weighting factor for plates, forgings and circ welds, which is good for anywhere between three and ten percent depending upon the number of circ welds, of course, the most embrittled material on either side and the max fluence along the weld.

Now, I have to admit that I was truly appalled that about five cells on a spreadsheet, which is really nothing more than a weighted average, turned into this much algebra when I laid it out, but that's how it turned.

DR. WALLIS: There was something I didn't understand in your report and that is that subscript U in parentheses.

DR. KIRK: Unirradiated.

DR. WALLIS: That's unirradiated? I thought it was something to do with uncertainty.

DR. KIRK: Certainly not, no, no.

(Laughter.)

DR. KIRK: No. Okay.

DR. WALLIS: It's not described, it's not defined, and I looked for it and I couldn't find it.

DR. SHACK: It's defined in the Appendix. Well, it's not, it appears.

DR. KIRK: This does my heart good that clearly people have read this report. And you've been sleeping well, I'm sure.

DR. WALLIS: Do you really want to know?

(Laughter.)

DR. KIRK: No, too much information. So when you put that, now if you use the RT_{NDT*} metric or the weld length weighted formula from the previous page, this is the relationship you get between the mean through-wall cracking frequency and RT_{NDT*} .

So taking the reactor vessel failure frequency criterion of one times ten to the minus six, one comes out with a 290 degree Fahrenheit RT_{NDT*} screening limit.

However, I should point out, as is probably obvious, that RT_{NDT*} is not the same as RT_{PTS} . First off, it doesn't have that blasted margin term which is good for at least 60 degrees. And when you do just a simple correlation, and it obviously various with fluence and a whole host of other things. But as an order of merit RT_{NDT*} is about 90 degrees Fahrenheit less than RT_{PTS} .

So at 290, RT_{NDT*} screening limit turns into approximately a 380 degree Fahrenheit RT_{PTS} screening limit. Or approximately an 80 to 110 degree Fahrenheit increase over the current screening limit is possible and still stay below one times ten to the minus six.

One other thing to point out is that as you saw in the earlier graphs when we were plotting versus effective full power years, in order to get results up in the E minus five, E minus six range, we had to go to what I think everybody would agree is absurdly long operating times.

And that all the results at reasonable operating lifetimes are considerably below the acceptance criterion limit. A couple of other points to make. One is that as we've discussed earlier, because these distributions are so skewed, the mean through-wall cracking frequency corresponds roughly to the 95th percentile through-wall cracking frequency.

And this next slide, I'm not sure if I see him, was motivated by a comment that Mark Cunningham made the other day about, you know, could we think of this in terms of a margin.

And he suggested plotting the, plotting where the median correlation would be drawn. So I, I didn't have time to go back to all the spreadsheets, but I sketched it on there that at the highest levels of embrittlement we looked at, there's approximately a one order, the median is about one order of magnitude down from the mean.

DR. KRESS: They only like to use a median if you don't really believe the tales. So here you're saying we believe the tales. So we don't, it's not a real margin.

DR. WALLIS: You wouldn't want to use the median anyway, would you?

DR. KIRK: No, I'm not suggesting to use the median. I'm just suggesting that there is a, if there is a significant different in either temperature or probability space.

DR. KRESS: Yeah, but it's not really a margin. So I would be careful about calling it that.

DR. WALLIS: Stay away from the word margin.

DR. KIRK: I, based on my experience today, I would agree.

DR. KRESS: And besides you don't need it.

DR. KIRK: And speaking of margins, and why we shouldn't use them, margin on RT_{NDT*} would be neither appropriate nor necessary and I came up with this slide far before I heard of Dr. Wallis' comments.

And this gets back to what I mentioned to Dr. Ford earlier. That buried in the guts of the FAVOR calculation we've reflected the maximum material uncertainties in FAVOR, because we've used generic data to derive these uncertainties.

And they've been explicitly accounted for. So any plant state of knowledge has to be better than we've simulated here. And also, if it hasn't already become clear, I would like to point out that this particular limit pertains only to one particular pathway of getting to this new proposed RT_{NDT} metric.

It's based on a measured unirradiated value and copper, nickel and phosphorus plugged into a particular embrittlement shift. There are certainly many other ways that at least in current practice the licensees will evaluate RT_{PTS} and --

DR. WALLIS: Tell me about that measured value. I'm not an expert on Sharpy and all this history of RT_{NDT} . But it looks from the data and I may refer to Chapter 1, I think it's Figure 1.3, it looks as if there are a lot of scatter on the curves looks not to be all the same shape and all that.

When you do these tests, are they repeatable.

DR. KIRK: I'm sorry, 1.3 is --

DR. WALLIS: Well, I mean, it's K versus RT_{NDT} for different steels. The EPRI data. How repeatable are these tests that give you this RT_{NDT} and what's the uncertainty in the test itself.

We seem to be treating this RT_{NDT} ASME as if it were something that was really known.

DR. KIRK: The unvarnished answer is it's not very repeatable at all. However, that uncertainty has been represented in the calculation.'

DR. WALLIS: Yeah, but when you plot something like RT_{NDT} on a graph, that is something which itself is very uncertain, isn't it?

DR. KIRK: That's correct. But the way RT_{NDT} has been designed, it's virtually impossible to underestimate it. Everything that you do in going through the, everything that you are forced to do by the ASME procedure, forces you to, if anything, overestimate the value.

DR. WALLIS: And that gets you to that Curve A in Appendix, way off to the side.

DR. KIRK: Yeah, yeah.

DR. KRESS: Are you going to sell this weighted thing to ASME and get them to change their ---

DR. KIRK: If we have enough time. Maybe.

DR. KRESS: It's not surprising that that weighted thing gives you a better correlation because it's based on your calculations, frequencies or contributions.

DR. KIRK: It's based on an understanding of what counts.

DR. WALLIS: So the Licensee has, this RT_{NDT} that the Licensee calculates, is that calculated by a formula giving all this chemical composition. Or is it calculated from tests on samples that are pulled out of the reactor.

DR. KIRK: Currently the answer is both. By the current regulation you are allowed to do both.

DR. WALLIS: And they have to be compatible or what? And how do you resolve, if you get different answers from each one.

MR. HACKETT: It all goes, it's all documented in 10 CFR. And also in the --

DR. WALLIS: All of the mystery there.

MR. HACKETT: Yeah, in the regulatory guide. But as Mark says you can come at a number of ways. The idea being that if you have data, you have hopefully somewhat greater certainty over what the actual property is.

But they also allow you to estimate if you don't have data, and they that's where you get into adding margins to hopefully address --

DR. WALLIS: That's what worried me is that, you know, everything is hung on this RT_{NDT} .

You've done a great job of dealing with all these things, but I'm not quite sure how accurately the Licensee can estimate from these samples or whatever.

DR. KIRK: I think that we're not asking the Licensee to do anything really different than they've done before. And again the reason that it works in this case is that RT_{NDT} as measured, it has to be a bounding property. There is no way to do otherwise.

DR. KRESS: I think if you give 20 people the input that goes into calculating that from a given plant, which they just gather, they'd all calculate the same number.

DR. KIRK: Yeah, yeah, given the input.

DR. KRESS: Given the input, it's only, it's just the input that's the problem.

DR. WALLIS: The input, if it's a bound, because bounding means you have to have enough points to determine what's bounding. And it may be that some erratic point pushes the bound out.

DR. KRESS: Well, if they have to measure their copper and --

DR. WALLIS: But they don't have very many samples in the reactor. They are using experimental data.

DR. KRESS: Then they have to assume they got a certain amount in there and that's where the conservatism comes in.

DR. BANERJEE: Can they measure these things based on the surveillance samples in the reactor. I mean which are actually being exposed to fluence and all this stuff.

MR. HACKETT: Again, unfortunately the answer depends, depends on whether they have the limiting material in their surveillance program for that reactor. Or are they relying on, let's give an example.

In the case of the B&W plants, they have an integrated surveillance program where you may use Oconee's results to predict Three Mile Islands irradiation damage. But you have to argue some kind of equivalency of the irradiation environment.

So the answer there also is a mixed bag.

DR. BANERJEE: Presumably the fluence can be pretty accurately calculated.

MR. HACKETT: Presumably.

DR. BANERJEE: Presumably. There's another question I have. The mean TWCF, that you have there, that's a function of a whole lot of things. And it's sort of surprising that all these things collapse so well because that suggests that the sequences which are sort of risk dominate plus the transients plus all things are very similar between these plants. Essentially there is not too much difference between them.

DR. KIRK: And that was the slide that I tried to emphasize at the beginning of this development is, yes, it seems to be surprisingly so that the, between these plants the level of challenge if you will is indeed remarkably similar.

MR. ROSEN: And that's because it's dominated by LOCAs and LOCAs are primary system phenomenas that are relatively the same in BWRs. Even once-through steam generator PWRs and recirculating steam generators PWRs are not affected because the primary systems are pretty much the same even though the steam generators are different and behave differently.

You're looking at what happens when you punch a hole in the reactor system. And that's the same in a PWR. They both start out at 2,200 psi roughly and depressurize and there you are.

Operators go, oh, no, my gosh, keep your hands off, make sure the reactor scrammed and that's it.

DR. KIRK: Yup, that's correct.

DR. KRESS: And they use generic frequencies for their bin.

MR. ROSEN: So it's not a surprise.

DR. WALLIS: Now to get back on this, I'm sorry to keep on this. You did a beautiful analysis of epistemic RT_{NDT} and I thought what you were doing there was you were looking at taking this ASME RT_{NDT} how well does it correlate real toughness data.

And how well does the theory represent this real toughness data. That was what was your epistemic analysis. And that still assumes that one has a very good way of knowing what that ASME RT_{NDT} is.

DR. KIRK: No, actually it doesn't. Those ASME RT_{NDT} values, I mean the distribution that we showed before is that they are on average about 60 degrees too high.

DR. WALLIS: That's why you have this epistemic and --

DR. KIRK: That's right, that's right.

DR. WALLIS: That's if you want to get toughness results out of it.

DR. KIRK: That's right.

DR. WALLIS: But it may well be that some plants don't do a very good job of analyzing their samples. And that's not in there, is it?

DR. KIRK: Of analyzing the RT_{NDT} samples? The more careless somebody is doing an RT_{NDT} test, the more conservative it becomes.

DR. WALLIS: That doesn't make sense.

DR. KIRK: Because if you, okay, if I'm, when you do, when you test for RT_{NDT} you have to take these specimens that have a brittle weld bead on them and a notch and you have to go until you establish a break/no break condition.

DR. WALLIS: You either bust them or you stretch them.

DR. KIRK: Well, actually you have to just simply establish a no break condition.

So if I want to do that with a minimum of samples, I pick a high temperature, I slam the hammer down and I decide it hasn't broken.

That doesn't mean that the real temperature between break and no break might be 100 degrees Fahrenheit lower. I can always overestimate RT_{NDT} , I can't under estimate it by the way you go through the procedure.

So if I want to be, if I wanted to be very precise, I'd get a whole bunch of specimens and very carefully bracket the break/no break temperature. But all the ASME MB-2331 requires me to do is demonstrate no break performance.

DR. WALLIS: So long as it hasn't broken.

DR. KIRK: Yeah. So if I've only got two specimens to do that with, and I want to establish a code value, I'm going to guess high.

DR. WALLIS: Well, I guess I'm saying is that there's got to be quality control in the way it's tested and all that kind of stuff as well.

DR. KRESS: That's pretty standard.

DR. WALLIS: So standard that you have no doubts at all about that.

DR. KIRK: Yeah, the way the tests are conducted is indeed standardized and controlled by ASTM. The procedure you go through, if you will, to discern RT_{NDT} based on $ASTME208_{NDT}$ data and ASTME 23 Sharpy data is not very well specified. But, and this is the only good but, the way it's not well specified is that it forces you to overestimate the value.

MR. ROSEN: Now help me with my understanding of how to use this chart. If I'm in Oconee, Beaver or Palisades, I'm right on the 290 degree screening limit. Is that right?

DR. KIRK: Only if you operate your

reactor until about the time that warp technology is invented.

DR. SHACK: When you replace it with your fusion plan.

MR. ROSEN: Why is that? I guess I must have missed that part of the discussion.

DR. WALLIS: Where are they now on this curve?

DR. KIRK: The now on the curve, everybody now is in the yellow oval.

DR. FORD: Even below it.

MR. ROSEN: Everybody.

DR. WALLIS: Well, they slide off the curve as they go on.

DR. KIRK: Yes, so time increases this way. And for Palisades that was a 500 year analysis. For Oconee, that was a 1,000 years. And for Beaver that was 100.

MR. ROSEN: Okay, because of the two orders magnitude. So you're saying that a clean plant now, low fluence, good materials is going to be off the bottom of that thing.

DR. KIRK: Yeah, because these were two of the --

DR. SHACK: The difference is really

materials, not, I mean they're all going to have roughly the same fluence per years of operations, but the materials respond very differently.

MR. BESSETTE: Some plants have neutron belts, neutrons pads.

DR. SHACK: Yes, but I think that's small compared to the material difference.

DR. WALLIS: So for Oconee to get up to one to the minus six, it would be several thousand years?

DR. KIRK: Yes, a thousand.

DR. WALLIS: So it's not just 60 to 80, it's thousands of years.

MR. ROSEN: I don't its turbine will last that long.

(Everyone talking amongst themselves.)

DR. FORD: Mark, could I ask. Up until the time you showed us these graphs, I was absolutely with you.

(Laughter.)

DR. FORD: And I can understand why you're going the way you are. But you're making one big assumption. The assumption is that there is one unique curve, that one that you've shown there, which normalizes all plant.

And that's an assumption that I haven't heard questioned physically. And then the second one you've gone into a bit of a g-ray pokery about a whole lot of different equations with ten percent and circumferential.

And I can understand where they came from, but I don't understand why they are on those specific algorithms that you've put down on this slide here. Now I don't doubt, the derivation of those long equations were being driven by the fact that you want there be one curve.

And I just feel uncomfortable because I don't understand some of those physics.

DR. KIRK: Actually the thought process here, I mean, honestly, the idea was what's shown on the screen now. Was simply to say, okay, let's lay the blame for through-wall cracking frequency on what's to blame.

So, let's not say that circ welds contribute a lot. Let's take account of differences in weld length. Let's get the fluence right. So all these things were done, and I shot myself in the foot by not presenting this in time sequence.

All these things were done and we got to, now I can't go fast through this damn thing.

DR. WALLIS: How did you pick 90 and 97?

DR. KIRK: The weighting factors were motivated simply by the results that we have so far. I mean those are, you know, honestly, just pulled straight out of the results. It was only after this that we looked at it and said, wow, that's good.

And it's only after that that we've come to the, by looking at, by fixing they crack, fixing the level of embrittlement and looking at the $K_{applied}$ to the dominant trends and said, oh well, you know, we didn't start the a priori assumption that they should line up.

We said let's construct a physically appropriate metric. We got to this and said, oh, is that, was that fortuitous or is there a reason for that. And then looked at the $K_{applied}$ trends and said, okay, yeah, they seem to be somewhere.

And again, as Dr. Rosen said, you probably don't need to look at the $K_{applied}$ once you've reached the realization that you say it's LOCA dominated and a fixed size hole in plants of this design is a fixed size hole, and it's going to do about the same thing.

So, no, it wasn't driven by the notion that they had to line up. It was driven by the notions that whatever we plot on the X axis should have a relationship, should be what's causing from a material standpoint, through-wall cracking frequency.

And then if you do your best to doing a materials normalization and there is still a difference, well then that must be an operational difference.

DR. WALLIS: This is truly remarkable. Because if you look at some of your figures, like Figure 5.4, which is the shift of topness transition temperatures during irradiation, there is an enormous amount of scatter on that figure.

There are datapoints all over the place and then there's a curve through it that you are using and yet somehow, despite all this tremendous amount of scatter and what you're working with, everything comes together in one curve. It's really remarkable.

DR. BANERJEE: Is that an upper bound?

DR. KIRK: Yeah, that's the mean.

DR. BANERJEE: Is that the mean or the upper bound?

DR. KIRK: It's both.

DR. BANERJEE: You can't put uncertainties on it.

DR. KIRK: If you remember the distributions, they were so highly skewed that the mean and the 95th percentile were about the same. And the, so, well, recognizing that one should not call a median a margin and I'm not going there again.

Okay, there is some rough feel for how this scatters out. And in fact once you get down to, once you get to down to the lifetimes where plants are, it's sometimes not even possible to define it. Well, the median is zero.

DR. KRESS: And that's when that scatter that --

DR. SHACK: Well, there's plenty of scatter.

DR. KRESS: Yeah, there's plenty of it, isn't there.

DR. WALLIS: Well, it sort of concerns me that there was a lot of scatter in the data and it seems to me rather unusual that you can define a limit or whatever or a conservative value, whatever you want to call it, so well.

DR. KRESS: Well, you are actually plotting something against itself, basically.

DR. WALLIS: You are?

DR. KRESS: Basically. Almost, because when you calculate this mean over here you've got the fluence effects in it, while the fluence effects are also in this. And we just got through saying that the thermal hydraulic affects were about all the same and that these are all LOCAs.

So it's not surprising to me that these line up, because it almost is like plotting something against itself. You're saying the RT, that this defined basis down here is a good representation of the --

DR. SHACK: No, I mean fluence affects K material. It has nothing to do with $K_{applied}$, they are really independent kinds of quantities. So fluence has a big affect on K material, it has zippo affect on $K_{applied}$ which is all a matter of how big a hole I punch.

DR. WALLIS: I think you need to retract what you said, because there is absolutely no way whatsoever plotting it very well against itself.

DR. KRESS: Maybe so.

DR. BANERJEE: Well, RT* has fluence built into it right now, right? It's almost linear with fluence. Roughly, if you look at the 97 percent weight and go back to the equation, it's almost linear with fluence, right?

DR. KIRK: I wish I could go to the end of that. The fluence is in --

DR. BANERJEE: Where is the fluence?

DR. WALLIS: None of these are measures of the probability of fluence.

DR. BANERJEE: What is FAW?

DR. KIRK: The fluence --

DR. BANERJEE: What is that?

DR. KIRK: That's the fluence, but the fluence affects this, the highly non-linear action.

DR. BANERJEE: Oh, is that a function?

DR. KIRK: That's a function.

DR. BANERJEE: Okay, it's just the way you wrote it, it looked like a -- so it's non-linear but it's a function of fluence anyway.

DR. KIRK: Yes.

DR. BANERJEE: So you have RT* as a function of fluence and certainly the abscissa and the ordinate are both functions of fluence.

DR. KRESS: Yeah, and that's what I was saying.

DR. BANERJEE: If you take the fluence out, you get something interesting now.

DR. KRESS: Yeah, you would.

DR. BANERJEE: Right. That would be a real measure.

DR. WALLIS: Well, that's the time. That's the time. As time goes on, you move off the curve. You've got to have that. It's still not plotting some variable against itself. Both variables are functions of time, yes.

DR. KRESS: Both variables are --

DR. WALLIS: They are functions of time, they are not plotted against themselves.

DR. BANERJEE: The thermal hydraulic uncertainties are not in there yet.

MR. HACKETT: I think we're going to agree with Dr. Shack, to some extent. Maybe not to the extent the Committee is looking to see, but we need to articulate that better.

DR. SHACK: You're going to explain it someday.

MR. HACKETT: Some day.

DR. BANERJEE: What you have to explain is that you don't, you are not bias towards only the low rates of whatever.

MR. ROSEN: Now nuclear safety is a zero sum game. I mean there is only so much resources and attention people can put here. If they're putting attention on this then they are not putting it on something else that may be even more important.

MR. HACKETT: That was indeed one of the motivations, you know, Mark mentioned a few when we started this morning, but, and I don't think this has been, we looked at the frequency of these a while back, but the challenges to the LTOPP systems, for instance, low temperature over pressure protection systems for the PWRs.

If you tighten this too much, and then you might get challenges that are acting adverse to safety that are challenging the LTOPP system. So that's exactly right.

MR. ROSEN: Well, I was going to, well, your question about, something about rule making here. It seems to me it's kind of like what we were saying yesterday. When are we going, this was on another different generic safety issue. Okay, let's get on with it. You know, this, that one happened to be significant.

This one you're saying, and I think convincingly, it's okay, we treated this conservatively for a couple of decades, maybe more than a couple, because we really didn't understand it.

But now that we have a better handle on it, we need to back off some.

MR. HACKETT: That's fundamentally the RES recommendation in the paper that went over from Ashok Thadani to Sam Collins. So what it's going to come down to, of course, first off NRR needs to review the draft and comment on the draft that's been sent over, and we've got at least another month of that.

And then there's probably more significantly, as you're talking about in terms of rule making, is prioritization within NRR over where does this fit in the scheme of things that NRR is working on in a world of limited resources, is kind of what it comes down to.

And we've inherited an awful lot of, we've all, at the NRC, inherited an awful lot of take aways from the Davis-Besse activity that are going to be keeping several of the offices pretty occupied in a priority sense. It remains to be seen where this will fall in.

MR. ROSEN: We don't run the Agency, all we can say is, on this subject, we make a, I draw a conclusion.

DR. WALLIS: I think with the next slide you're going to say get on with the rule making, aren't you?

MR. ROSEN: Yes.

DR. WALLIS: Well, I'd like to go back to that slide. I think that you've got to be very careful here. The second bullet there is not the way to put it. Because your current limit appears, as you have explained to me many times, it's a different -- so what you need to say, this limit is equivalent to an increase in the current limits on RT_{PTS} by 100 degrees or something.

It's not as, because if you add 100 degrees to 270, you don't get 290. So it's obviously, you've got to make a distinction somehow. It's not 100 degrees higher than 270, is it?

DR. KIRK: No, I understand.

DR. FORD: Could you not also put a second bullet after the first prime bullet, saying that you have reasonable, it's a reasonable conclusion to say that this applies to all PWRs.

DR. KIRK: I was thinking of putting that in, but that hadn't been vetted through management, so I decided not to.

DR. FORD: But surely that's an important conclusion.

DR. KIRK: No, that's an important conclusion and that's getting into the ongoing activities. And that's, that's the topic of our ongoing work that, at least I'll just say I personally am beginning to believe that, you know, that bullet should be added.

But we need to go through that in a little more detail, I think, before we get to that point.

DR. KRESS: Does that rest on the research and on the weighting factors being the same for all plants.

DR. KIRK: I think it rests more on the examination of the operational challenge.

DR. KRESS: I agree.

DR. KIRK: But again, I mean the weighting, I can't, the reason why the weighting factors are the way they are is just simply that axial welds or axial flaws are far more challenging than circumferential flaws.

That's not going to change on a vessel-specific basis, and we've done three plant-specific analyses, we're going to do another one. We came up with something like a 90/10 split.

I find it difficult to envision that any plant-specific features is going to change that radically because the flaw sizes are all going to be the same. The orientations are going to be the same.

DR. KRESS: So then the only other variable in this is the thermal hydraulics. Because you're taking care of fluence and material properties.

DR. WALLIS: Which probably is a much more certain science than materials.

(Laughter.)

DR. KIRK: No comment.

MR. BESSETTE: Like the point was made that the plants have similar, let's say, prior to volume, stored energy in the primary system is similar across all the plants. So like it has been said, a four inch break in Plant A is going to look like a four inch break in Plant B and C and D and so on.

DR. KRESS: So it looks like you have a good basis for saying this, generalizable to all plants.

MR. BESSETTE: I believe so.

DR. WALLIS: It seems to me the most important thing here is to get a very good external Peer Review, so you really pick up things where if something is misunderstood or misstated or something.

And I think you need to put in an activity here which is the best way to present this material. No, seriously, I think this is a very important thing. I hope you do proceed with rule making. I think it can make a big difference to the plants and it can make a big difference to the industry.

It can reassure the public about a matter which could be of some concern. And you have to really express it in a way which is as believable as possible.

DR. SHACK: Yes, leave a lot of our your point.

DR. FORD: Could you go back to the previous one. Could you not also put in the first major bullet, it could be a second major bullet. There's an argument for having acceptance criteria for the order one times ten minus six.

DR. KIRK: Yes, that should be there. That should be there.

DR. FORD: Because that would then lead into your screening limit.

DR. KIRK: Yes.

DR. FORD: And for me personally, I can follow why you say there should be an appreciable increase in the RT value to an RT*, but I'm still mulling over the 80 to 110, the rationale for that.

DR. KIRK: Yeah.

MR. HACKETT: Why don't we go to that last slide again. I think what I'll do is just say it, at least I see three take aways and we can talk about this. What we hoped to have left you with as a result of the meeting today is, and it's probably pretty obvious that there's a draft technical basis that's documented and forwarded for your comments.

Work is still ongoing. However, we consider that we do have a tech basis that indicates initiation of rule making in the burden reduction area. So that's those three things, I guess, is what we're seeing.

And then we're also looking at requesting the letter and I guess maybe some discussion now of form or content for what you'd like us to do tomorrow with the reprise to the full Committee.

I guess going in the proposal is we did a briefing just two days ago, I guess it was, for the EDO, that's a much, much condensed version of what we've been through today.

DR. SHACK: How many slides?

DR. KIRK: Sixteen.

MR. HACKETT: And we would probably propose to try and run through that tomorrow for the full Committee. That's, I have not looked at the agenda for tomorrow.

MR. ROSEN: It looks like too many to me.

MR. HACKETT: We can take that down a peg.

MR. ROSEN: If I were you, if were trying to make this case, I'd bring in all the studies and stack them up in hard copy over there. And then I put all the presentations in a pile next to it, which would be a foot high.

The stack of studies would be four feet high. And then this would be a foot high. And then I'd have one piece of paper, one viewgraph that I'd put up and I'd say, here's the answer. It's really backed up by all this stuff, but you don't need to trouble yourself.

(Laughter.)

MR. HACKETT: We could go back to the Bob Hardies' slide that said, let's say, PTS transients don't occur on vessels that are tougher than we give credit for and flaws that don't exist in welds.

(Laughter.)

MR. ROSEN: Yeah, well I think that's where we started in our briefing, right?

MR. HACKETT: We've used that slide before.

MR. ROSEN: You certainly got our attention. And really, the bottom line, that's really, if the President wanted to know what's this all about --

MR. HACKETT: That's probably what we'd say.

DR. WALLIS: I'm wondering about what

we're going to do for a letter, though. Because it seems to me that in essence this is, looks like a very significant piece of work and looks as if it should go to rule making.

But there are obviously things that we could bring up. But I don't want the letter to, although there are things to bring up, I didn't want to have to bring up too many things, because I we know you're going to fix them.

But it's still a bit premature to sign off and say the case finally has been made for rule making.

MR. HACKETT: And I don't think that's necessary, either. Maybe something along the lines of what Dr. Shack was suggesting. A letter from the committee that's more of a high level document. Maybe going into a few specifics.

And then maybe use pursuing with the committee other mechanisms of dealing with individual comments that may be many through e-mails or meetings, whatever you feel is most appropriate.

DR. FORD: But this is not the last time we're going to hear about this.

MR. HACKETT: No, that's the other point to emphasize, when Nathan and I were talking earlier, to come back to some other comments I was going to make here. But this again, just to re-emphasize, this is a technical basis development.

And then I guess we even have to get into defining what that is and isn't. And it is not rule making, number one. We're not going to be so presumptive. None of us here work for NRR.

NRR, that's NRR's activity. They will engage that if they feel it's justified and consistent with resources and other demands on NRR. So it's absolutely not that, not that level. Where we are on tech basis is we think we have a good solid draft case to be made and that's why we're here with this document.

This document obviously needs work. I think that's one of my, I've got many, I've got at least two pages of notes here in terms of take aways and very sensitive to comments Dr. Wallis has made.

We can do a lot better in presenting this, I don't think there's any question. Probably both in terms of the document itself and in terms of these presentations and trying to get it more in a plain language sense.

Particularly with regard to RT_{NDT}, I think that's a definite take away. So I think that's where we are in terms of, in terms of tech basis. But absolutely, there's going to be much more time going forward from here in which the committee can engage and which we are going to be taking on a lot of other comments from other stakeholders.

MR. ROSEN: But, Ed, all of that is about process, getting towards the rule making and ultimately into one. But what I'm worried about and I want to be sure to hear your answer is are any of the technical activities that you still have in front of you likely to change this result.

MR. HACKETT: We think not. Not to say we couldn't be wrong, but we have kind of wrung these things out, you know, for the most part over a couple of years.

DR. WALLIS: What about the loose ends? I understand this is a draft report from OSU. Now it hasn't been reviewed and may need some changes. You can't really refer to some key part of that work until that work has been finalized.

And we've got this new Maryland report on uncertainty which I understand is a year or two old and says things that are no longer valid. When is that going to come to maturity so that you can really rely on it.

And you've got these various cornerstones of your case and it seems to me that two or three of them aren't there yet.

MR. HACKETT: I think this is, the only answer I guess I could, you know, forward to that would be that this has ever been a dynamic project. And I think there are always going to be pieces that are evolving as we go forward.

And it's like the problem we had with the embrittlement correlation, at some point you had to kind of freeze things and move forward and get on with some kind of standardization activity, like with ASME or rule making in this case.

Because I think that's a really good point. And I think it will always be the case, you know, in this area particularly. So we'll just have to, you know, at some point we cut off the sensitivity studies and other aspects of uncertainty analyses and say we think we've gotten far enough for now and then maybe several years from now we're back with removal of the rule, you know, if that seems to be warranted at some point.

But I think that's, you know, it's going to end up being a step-wise process.

MR. BESSETTE: So you can see like the

first sample up there is Calvert Cliffs, which is a fourth plant that we're going through, similar to the three plants we showed you today.

MR. HACKETT: I think another thing I'll mention in closing here, I took a lot of notes and I won't go through all of those. But one, or one or two that stuck with me, in particular, Dr. Banerjee and Dr. Wallis raised the issue in particular with regard to the thermal hydraulics.

And I think there, I think the team is sensitive to the rate of change uncertainty and how that propagates through the rate of change in temperature uncertainty and how we're capturing that and how that propagates into FAVOR.

That's a definite take away that, you know, we need to be very sensitive to. I think there's the whole issue, and I think Dr. Rosen mentioned this in terms of just overall in this project model uncertainty.

That's something that you look at. I've spent the last, you know, the better part of the last year doing Davis-Besse things in terms of lessons learned. And you look at the model uncertainties that were there, for instance, in terms of corrosion and corrosion rates.

You don't ever want to be so arrogant in this thing that you think you captured all of that. There is always going to be model uncertainties and they could end you up on the other side of the range real quick if a couple of key things are out of whack.

And so we're very sensitive to that and we've been trying to come at this the whole time with a real questioning attitude in that regard, but you know you still have got to keep pushing at that all the time.

And another one I'll just mention in closing here to is the notion of flaw growth for the long term. We have not considered that. If we are going to get out significantly into license renewal periods, it may be a reason to revisit that at some point.

But right now we're not dealing with that, so that's another take away there. And at this point I guess I'd ask Nathan, too, to see if there was, is there any part of the summary that I've missed here that you wanted to highlight?

DR. SIU: No, I think you've covered. Basically, again, there's a process that we're going through and this report represents, obviously, a key milestone in that process.

MR. HACKETT: Otherwise, definitely I would like to thank you for spending yet another day with us on this topic which is never easy. And we've always gotten valuable comments from the Committee and take aways that I think have made for a better product.

And, you know, one of our major challenges continuing is to try and do a better job of communicating this both orally and in writing. So that's a major take away for us. But, thanks for listening.

DR. SHACK: Anymore comments or questions from the Committee.

MR. LEITCH: I had a couple of things, Bill. One, I was wondering in the review of the emergency operating procedures and recognizing that the issues here are relatively insensitive to operator actions, I agree, but I'm wondering if there were any insights that you gained as a result of looking at those emergency operating procedures that should be communicated to the industry.

MR. HACKETT: It looks like the right man is coming to the mic.

MR. KOLACZKOWSKI: Well, again, we can't pretend that we've reviewed everybody's EOPs. On the other hand, having said that, we have to recognize that when the EOPs did change after Three Mile Island, etcetera, etcetera, you know, as we're all aware pretty much the owners groups got together and developed, if you will, the initial set of the procedures and then the plants have pretty much just, you know, use those as models and change them to the extent necessary to perhaps reflect their specific setpoints and things of that nature.

As part of this generalization tasks, that we're in the middle of., one of the things we are doing is looking at some of the other procedures of those other five plants to indeed convince ourselves that the procedures are in fact similar and so on and so forth.

And so far that is the case. Now, so having said all that, in the ones that we have reviewed, I think I indicated at one point in my presentation that for one or two the plants we did find a few places in the procedures, as they were written, where a slight modification, let's say, would be clearer as to a particular operator action and when they should or should not do something.

And the Licensees, upon seeing that, took it upon themselves to say, yeah, I'm going to make a change here and make this clearer. Again, I don't want to overemphasize that, it wasn't like it was a major impact.

But something that, this could be a little clearer, let's change the order of this or something like that. We have not found anything that is so, say, blatantly of a concern that we feel like, gosh, we've got to raise this to the industry, this is clearly a big issue that needs to be addressed.

Little minor things now and then, yes, we have come across.

MR. LEITCH: But those minor changes, as I understand it, were only in the three plants that were studied.

MR. KOLACZKOWSKI: That is correct.

MR. LEITCH: But I guess what I hear you saying is they are not of such a magnitude that they ought to be communicated to the rest of the industry.

MR. KOLACZKOWSKI: That is correct.

MR. LEITCH: Another question I had was we have some plants coming down, you know, for license renewal and quite a few of them are in the pipeline. And I guess the timing of this thing, as I see it, is that some plants that are, what we might call more embrittled plants, could be coming on our plate here for license renewal decisions before this PTS rule is changed.

And I guess that just presents an obvious problem. I don't know exactly how we deal with that issue.

MR. HACKETT: I think a couple of things we could mention in that regard and it might be that some of the industry folks may want to comment also but I think you're absolutely right because they have to look very far downstream just in terms of the economics.

And if you're, you know, part of the Board of Directors of a nuclear plant and you're thinking am I going to apply for a license renewal and come document and argue that with the NRC, you probably don't want to go in with your vessel in question.

So, you know, that's going to back you up many years. I think the good news in that regard is that I think this has been perceived in a very positive way by the industry, this project, regardless of the exact status it's at right now in terms of proceeding to rule making.

And that I think it would be fair to say hopefully from the industry perception that it would be looked upon as on a success path if they were to have to come in, in a preemptive way to try and argue this.

But I think it's a very, it's an excellent point. You know when you look at Palisades as being the closest and they're 2011, and you know we're 2003, that's really not a whole lot of time, you know, when you start to get to, and I don't particularly know what decisions that plant has made with regard to license renewal. But it, that would obviously be a factor for them.

MR. LEITCH: You know, well Fort Calhoun is very close. I mean it's within months we're going to have to make decision in that regard.

MR. ROSEN: Along those lines, can I ask a question about the present use of this future technology. I mean what if a plant had an overcooling event with some pressurization and the ROP was looking at it. What would you tell the Senior Resident and the SRA and the Resident Inspectors. I mean could they be thinking about this? Or is this still future tense.

MR. HACKETT: No, I think this is, I guess again a couple of ways of looking at that. I guess maybe I need to back up and ask for clarification in terms of if you are looking at if you had an overcooling event, did you for instance potentially propagate a fabrication flaw and you just didn't realize it. Was that sort of where you were heading?

MR. ROSEN: Yeah, I was thinking of a plant that actually had some kind of an event like this and the ROP made it red and really it isn't because we know they got a heck of a lot more margin than they really would calculate under 50.61.

MR. HACKETT: Yeah, that's a real good point. I can't say I have thought about that myself, but somewhat analogous to the Davis-Besse situation in that you are now down into having to argue a significance determination that would be probably pretty tricky.

DR. SHACK: You'd come in and say I'm below RT_{PTS}, failure frequency is less than five, ten to the minus six, good bye.

MR. ROSEN: It's really below five times ten to the minus nine.

DR. SHACK: Yeah, but it's good enough.

MR. ROSEN: All right, it was just a thought in terms of what could come across our plates.

MR. HACKETT: I would think the most significant thing you'd want to do, first off you'd have to be in a plant where all these things line up.

And then you'd probably have a nervous regulator that you had a severe overcooling and maybe it wasn't one of the more embrittled plants.

Then you might want that plant to have to come and tell you to a very high degree of certainty I don't have any flaws in those welds. Or have them go look at those welds really hard, just in case you got a propagation and it didn't go through the wall, but maybe now you've got a vessel with, you know, a large crack in it somewhere.

At least you'd be, somewhere in the back of your mind you'd be worrying about that. I don't know how much worry you would assign to it, but it is an interesting point.

DR. SHACK: Anybody have any particular suggestions for the presentation tomorrow?

DR. KRESS: Well, I don't know what their 16 slides look like, but that sounds like a good idea.

DR. SHACK: Let them pick their 16 slides.'

MR. ROSEN: It's going to go where it's going to go anyway, but that's the measure of the uncertainty when you're dealing with ACRS views of what presentations ought to be.

DR. FORD: But do I take those 16 slides

cover, for instance, the latest results on Palisades and Beaver Valley.

MR. HACKETT: Yes, they do.

DR. FORD: And it touches on the generic nature of your findings?

MR. HACKETT: Yes.

DR. FORD: And then, so those are the main conclusions, that's the basic message to go on to the rule making. And if you've got the scoping studies to look at the screen and acceptance criteria.

MR. HACKETT: That's correct. And in fact --

DR. FORD: These are not absolute, these are just ideas which will be then developed.

MR. HACKETT: Right, which was the whole, the objective of the presentation with the EDO was in fact Dr. Travers had not been briefed on this before. And it was really to update him on what we'd been doing and to make him aware that we feel that this is potentially ready for rule making.

And I think he came away with the same kind of conclusion that you folks have reached.

DR. FORD: And so it's not data the full committee has in front of it to make a decision.

MR. HACKETT: Right, right.

DR. KRESS: I think Dr. Powers will be interested in your containment and entry and the acceptance criteria. Is that part of the slides you have, the 16?

MR. ROSENTHAL: The acceptance criteria, but not the, none of the containment stuff. Because, you know, I'll repeat it again. We see this as a PTS rule and then in response to your questions we, I think we did some organized thinking and a little bit of code running, but we still see it as, to answer questions and to make us smarter.

But we see it as a PTS rule. So we didn't even bring it up the other day.

DR. KRESS: Well, I think Dr. Powers might be interested.

DR. SHACK: It will come up.

(Laughter.)

DR. KRESS: Yeah, that's my point, it will probably come up, and I would be prepared to address it.

DR. SHACK: I guess I don't understand that argument. I mean your acceptance criteria has to be based on something. It has to be based on those arguments. You can't just say it's a PTS rule. You know, we have no frequency criterion.

DR. KRESS: Acceptance criteria is part of the rule.

DR. SHACK: Acceptance criterion is part of the rule. And the logic by which you get to it is intrinsic to the rule. Now whether you have enough time is another question, but you better at least be prepared to start down that path. Any other comments?

MR. LEITCH: I would just like to say I really appreciate the presentations of the entire team today. I mean I think it's really been very, very helpful to me, personally outstanding.

In seeing the way the PRA, the thermal hydraulics work and the probabilistic fracture mechanics kind of dovetail to work through this whole process I think is very good.

And to me personally it was very helpful. I've been pretty quiet, but I've been doing a lot of listening and it's really, like I say, it's really been very helpful to me and I appreciate the efforts of the whole team to pull this presentation together.

DR. KRESS: I second that. It was outstanding. Especially the work from Oak Ridge.

(Laughter.)

MR. HACKETT: We've already told Terry he can't retire.

DR. SHACK: If there are no further comments, I think we can adjourn for the day. And again, I'll add my words of appreciation for a very well done presentation. The document needs some work but you're getting there.

MR. HACKETT: Thanks, Bill.

(Whereupon, the foregoing matter was concluded at 4:49 p.m.)