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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
(ACRS)

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JOINT MEETING OF THE SUBCOMMITTEES ON
MATERIALS AND METALLURGY,
THERMAL-HYDRAULIC PHENOMENA,
RELIABILITY AND PROBABILISTIC RISK ASSESSMENT

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TUESDAY,
NOVEMBER 30, 2003

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ROCKVILLE, MARYLAND

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

<u>COMMITTEE MEMBERS PRESENT</u>:

WILLIAM J. SHACK, Chairman
RICHARD S. DENNING, Member

COMMITTEE MEMBERS PRESENT (Continued):

MARIO V. BONACA, Member
PETER FORD, Member
THOMAS S. KRESS, Member
VICTOR H. RANSOM, Member
STEPHEN L. ROSEN, Member
JOHN D. SIEBER, Member
GRAHAM B. WALLIS, Member

ACRS STAFF PRESENT:

HOSSEIN NOURBAKHSH
CAYATANO SANTOS

ALSO PRESENT:

BILL ARCIERI, ISL
DAVID E. BESSETTE, RES
MARK EricksonKIRK, RES
ALLEN HISER, RES
MIKE JUNGE, RES
MICHAEL MAYFIELD, RES
NATHAN SIU, RES
DONNIE WHITEHEAD, Sandia

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

(8:35 a.m.)

CHAIRMAN SHACK: The meeting will come to order.

This is a joint meeting of the ACRS Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena, and on Reliability and Probabilistic Risk Assessment.

I am William Shack, Chairman of this meeting. Members in attendance are Mario Bonaca, Rich Denning, Peter Ford, Tom Kress, , Victor Ransom, Steve Rosen, Jack Sieber, and Graham Wallis.

The purpose of this meeting is to discuss the technical basis for potential revision of the PTS screening criteria in the PTS rule, 10 CFR 50.61. The Joint subcommittees will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee.

Dr. Hossein Nourbakhsh is the designated federal official for this meeting.

Also Mr. Tani Santos, ACRS staff, is in attendance to provide technical support.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on November 2nd, 2004.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so they can be readily heard.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's meeting.

We'll now proceed with the meeting, and I'll call Mike Mayfield, who is here to begin.

MR. MAYFIELD: Just in time.

CHAIRMAN SHACK: Just in time, right.

MR. MAYFIELD: Well, good morning. This is, I think, the beginning of what we hope will be sort of the last series of briefings on this program. We have enjoyed good interactions with the committee over the course of this.

As some of you know, we got into this stemming from largely the Yankee Rowe review and the Commission's direction to go fix our regulatory guidance, but the more we looked at the guidance the more convinced we became that wasn't going to do it alone, that we needed to go back and take a more fundamental look at the technical basis behind the rule.

We have had the benefit of good cooperation from the industry, and I'm glad to see they're well represented here today. This has been a collaborative program in virtually every sense of the word. So it has been a multi-year success fat, not that there haven't been bumps along the way, but it has been a very rewarding effort, I think, for everybody that has been involved.

Our goal for this is to finalize our documentation and formally transmit it from Research to NRR. The documentation provides the technical basis for a rule change to 10 CFR 50.61. We're hoping to do that on or before December 31st.

I figure Mark is going to have a long New Year's Eve, but we've gotten Carl to commit to signing this thing out, assuming we're done.

I am told that NRR has budgeted for rulemaking, assuming that that's the decision that ultimately is made by the senior management. So that is a hurdle I am told that the regulatory staff has gotten around.

We have interacted with the committee a number of times, and that's been very useful to us.

We've talked a good bit about whether at the end of this meeting with ACRS we would like a letter from you or not. I think that we would like a letter to sort of bring an end to where the committee has been and your thoughts and views on the work that's done and whether it's adequate to support the objective.

One of the things that we had committed to you at, I think, the last time we met was that we would provide a number of reports, one of them being a summary report on the bases for some of the thermal hydraulics work. That report is notably missing.

However, we've provided the detailed reports over a period of time, and there's a fairly lengthy presentation that Dave Bessette is going to make that I think will lay out and connect the bits and pieces of information so that hopefully you will see how it all connects because it's not intuitively obvious to just look at the detailed reports, how the bits and pieces fit together.

So in the absence of that summary report at least for this meeting, we hope that David is going to be able to lead you through the thicket.

We are still committed to publishing that report, and that will be available by the same time we would send forward the technical basis summary to NRR.

DR. WALLIS: Mike, I'm just a little puzzled here. You want a letter from us before we see this report?

MR. MAYFIELD: No, all of the detailed information is available, and there will be nothing new in that report. The only thing that report is --

DR. WALLIS: But I have trouble finding it because it's scattered around.

MR. MAYFIELD: Well, that's what I was saying, and hopefully with David's presentation that will connect the bits and pieces and show you how they fit together. That's what we're trying to do with this presentation.

DR. WALLIS: We won't see a document that pulls it all together before we write a letter.

MR. MAYFIELD: That's correct.

DR. WALLIS: I think that's a pity, but maybe --

CHAIRMAN SHACK: Well, he's asking that. We don't --

DR. WALLIS: Maybe David can do it.

CHAIRMAN SHACK: -- have to do it.

MR. MAYFIELD: David has got a pretty good challenge, and Jack Rosenthal is here. So if David should fail, we'll drag Jack up front, and you can throw any number of things at him.

DR. WALLIS: It's just that a written report is something solid to review, and an oral testimony is not quite the same thing.

MR. MAYFIELD: We agree, and it had been our full intent to have that report to you with the rest of the documentation. It didn't happen. As much as we wanted it to, the fact is it didn't happen.

If that becomes an obstacle to the committee writing a report, then I guess the only thing we can do is come back to you after the first of the year. That would not be our first choice, but if that becomes an obstacle to completing a letter from the committee, then that's a commitment we'd have to make.

DR. BONACA: My main concern would be I believe in that last letter we wrote, the only concern left was with documentation, and there was a debate within the committee on whether it was just documentation or lack of documentation was evidencing something else.

So some of us on the fence were looking for documentation so we could make the judgment, and that's why I -- anyway, hopefully we'll hear enough to be able to comment now.

MR. MAYFIELD: I hope so.

CHAIRMAN SHACK: And we've just received the peer review comments also.

MR. MAYFIELD: We just received the peer review comments. There's a reason that you just got them, is we just got them. We had been hoping to have those a bit sooner, but the one thing with peer reviewers, and to a degree it's the same thing you get with the committee, is you ask for what you would like to have and then you take what you get, and we had hoped to have the peer reviewer comments much sooner so that we could digest them and make a better presentation of what their findings are for this meeting.

They just didn't all get in to support that. So we apologize, but you got them -- we got them what, finally all yesterday? And you got them --

MR. EricksonKIRK: They're still smoking.

MR. MAYFIELD: -- within hours of when we got them.

So there may be some surprises for us still imbedded, although Mark tells me he's read all of them now.

With that, I would turn it over to Mark to begin the presentation.

MR. EricksonKIRK: Okay. Thank you.

On your agenda, we're now on Item 3, Project Overview.

My name is Mark EricksonKirk. I work in the Materials Engineering Branch. Listed on the title slide are the names of people who you will see up here presenting in the next two days. Donnie Whitehead, Nathan Siu, and Mike Junge will be presenting regarding the probabilistic risk assessment and human factors aspects, and Dave Bessette and Bill Arcieri will be presenting regarding the thermal-hydraulic aspects of this work.

In terms of what I'm going to talk about in the next 30 minutes, I'm going to give you a bit of background on the project because the last time we briefed you was two years ago, and also for the benefit of those in the audience who aren't familiar with where we've been, talk a little bit about what the current PTS regulations are and what our motivations are for developing the technical basis to potentially revise the rule, then give you an overview of the project, including an overview of our current results and bottom line recommendation to hopefully excite you so much that you'll stay awake for the next day and a half.

CHAIRMAN SHACK: We've already found your first typo.

MR. EricksonKIRK: Where?

CHAIRMAN SHACK: "Guiding principals."

MR. EricksonKIRK: Oh, fine.

DR. WALLIS: That's all the way through your report, you mix up the spelling of those two.

MR. EricksonKIRK: I have to confess I went into engineering because I thought there wouldn't be a lot of writing, and, boy, have I been disappointed.

And then we're going to tell you what we're going to tell you.

To be fair, the list of co-conspirators on the title slide is but a small percentage of the total population of people both in those organizations and other organizations that have participated in this project.

We started in 1999 and since then have enjoyed the support of a large number of people from a large number of organizations, both in the NRC contractor base and also in the industry working under the auspices of the EPRI materials reliability project, and just suffice it to say without the full participation of this complete group of folks, we couldn't have gotten to where we are.

DR. KRESS: Does that UT-Battelle symbol have anything to do with Dolly Parton?

MR. EricksonKIRK: I'll refrain from comment. Okay. It's going downhill quick.

In terms of where we've been, from 1999 to December 2002, we developed our models and our uncertainty process. We performed initial analyses of Oconee, Beaver Valley, and Palisades, and we issued a draft report the title of which and the ADAMS ML number is shown on your slide.

We briefed this committee on that report in February 2003, and since then that report was also reviewed by NRR, by the industry again working under the auspices of NEI and EPRI, and by our external review panel.

We got a lot of comments back both on the details of the model and also on the details of the documentation which said, "Please do your best to make this a bit clearer." So we've tried to both improve the models where possible, correct the errors where they've been identified and subsequently found, and also improve the documentation.

This figure which appears in Chapter 4 of NUREG 1806 outlines the total documentation structure, and those of you who have a copy of the report, it's probably easier to read on paper, but we have a number of different reports in the form of NUREGS, NUREG CRs, and public documents posted into the ADAMS system, to detail the models that we've used, the validation of those models and our calculational procedures, and each of the three major technical areas: probabilistic fracture mechanics, thermal hydraulics, and probabilistic risk assessment.

And we also have detailed presentation of the results also summarized in a series of reports, and while I'm on this slide, just to be clear, Dr. Shack was telling me before the meeting that the committee has not yet received NUREG 1807 and NUREG 1808, the probabilistic fracture mechanics procedure and sensitivity studies reports.

Are there any other reports that you know of now that are missing?

We have those, by the way. It was an oversight that they were not distributed to you almost a month ago.

Well, just suffice it to say all of these reports exist except the one that Mike mentioned at the current time. All of them exist except for NUREG 1809, which is still being prepared.

So if you're missing any of the other documents, it's a clerical error on our part for which we apologize, and we can get them to you forthwith.

The provisions of the current PTS rule, 10 CFR 50.61, is that licensees are required to monitor the condition of their vessel, the vessel steel, using a transition fracture toughness reference temperature called RTndt, and an estimate of that and the effect of irradiation and uncertainties on that metric is obtained through an Appendix H surveillance program.

DR. WALLIS: what is this strange curve that you're showing here?

MR. EricksonKIRK: That's meant to represent the fracture toughness, the variation, and initiation fracture toughness.

DR. WALLIS: Off the reactor wall of the weld or --

MR. EricksonKIRK: Of the reactor vessel steel.

DR. WALLIS: Reactor vessel steel.

MR. EricksonKIRK: And what the cartoon shows is that the RTndt temperature, which is estimated per the procedure in 10 CFR 50.61, indexed the position of the initiation fracture toughness curve, and as you'll see later in this presentation, indeed, of the arrest fracture toughness curve and of the upper shelf fracture toughness curve.

So placing an upper limit on RTndt essentially places a limit on how far we allow the fracture toughness, on how low we allow the fracture toughness to get.

DR. WALLIS: So these evolving curves, as the reactor gets older they move to the right?

MR. EricksonKIRK: They move to the right, yes. And placing a limit on RTndt essentially says how far right the curves can go.

And so in our current regulations those limits are established as 350 degrees Fahrenheit for a circumferential weld or 270 degrees Fahrenheit for any other material, and I should emphasize that that's the screening limit. That means that in our current regulations, the belief is that once a vessel material exceeds that limit, the probability of developing a through wall crack is exceeded five times ten to the minus six events per year, and the licensee is then required to do something else to demonstrate to NRR that the vessel is safe for operations.

That something else could be either something physical, like reducing the flux loading to the vessel wall, which many licensees have done, or annealing, which no licensees have chosen to do, or they can attempt to analyze their way out of the situation much as we've done here by performing a plant specific PRA.

Everybody on this committee, I think, has seen this slide before. One of our motivations for undertaking this project was that since the time that the 300 and 270 degree Fahrenheit limits were established nearly two decades ago, technical improvements in understanding, in data, and physical modeling and so on have improved in all three of the major technical areas, and by and large, the bulk take-away is that by and large those improvements in understanding, if incorporated into an integrated calculational model, would tend to drive the estimated through wall cracking frequencies down. That's indicated by the green arrows.

Certainly we also want to point out that there are other improvements in understanding or improvements in our methodology of doing things that would tend to drive the through wall cracking frequencies up, and it has been our aim in this project to incorporate the current best state of knowledge, best state of understanding and to incorporate all of these effect into an improved calculational model.

Regulatory motivations for rule revision, one is that the current rule is believed to produce unnecessary burden on the licensees, specifically the 300 and 270 degree limits. When we started this project, they were believed to be far more conservative than they actually needed to be to maintain safety and to maintain the risk of vessel failure below the five times ten to the minus six metric.

Maintenance of the plant vessel wall below those RTndt limits doesn't necessarily increase overall plant safety because you may be focusing resources on something that doesn't really matter and thereby taking away resources from something that truly does matter.

And also, these limits can create an artificial impediment to license renewal because in the license renewal application, the licensees have to demonstrate each and every time that they stay below these limits, whereas, we believe we could do something on a generic basis to essentially lift the limits on all plants and make the license renewal process both easier and more rigorous for our colleagues in NRR to undergo.

So just diagrammatically how we assess PTS risk in a calculation is we start off with PRA, and PRA tells us how often PTS initiators might occur. Those initiating event sequences are then passed to thermal hydraulics, which tell us what would happen inside the vessel as a result, how pressure temperature and heat transfer coefficient would vary inside the vessel with time.

We then use probabilistic fracture mechanics to estimate the response of the vessel, whether a crack starts at all from a preexisting defect and whether that crack will propagate all of the way through the vessel.

The probabilistic fracture mechanics is then used to estimate whether the vessel fails or not. Obviously if it doesn't fail, that's a good thing. If it does fail, it could potentially lead to core damage or a large early release, which of course begs the question as to what is a tolerable frequency for those events.

So that in a nutshell are the various things that had to be considered to get to revision of the 270 in --

DR. WALLIS: The vessel, is there any question about core damage?

MR. EricksonKIRK: I don't believe so, but I'll defer that to my colleagues.

MR. BESSETTE: It depends on the size of the failure. I mean, a vessel failure, even a large vessel failure is not much bigger than a cold leg break, but it depends on the elevation of the failure in terms of how much water you can keep in the core.

DR. WALLIS: Well, so by vessel fails, you don't mean it falls apart. You mean it actually just --

CHAIRMAN SHACK: Through wall crack.

DR. WALLIS: -- develops a hole?

MR. EricksonKIRK: It develops a through wall crack which could be a leaker.

DR. WALLIS: I see.

MR. EricksonKIRK: So a little bit more formally, and this figure does appear in the report, this is how we structured our analysis which is essentially the same things you saw before. We perform a PRA event sequence analysis, and that both defines what could go wrong and the frequency with which we estimate those things to go wrong. Thermal hydraulics estimates pressure temperature and heat transfer coefficient. That's past probabilistic fracture mechanics, which combined with knowledge of the vessel material, fluence and flaws gives us a conditional probability of through wall cracking.

That's multiplied by the frequency with which bad things happen to estimate the yearly frequency that we might develop a through wall crack in the vessel.

We perform those analyses for various vessels at various levels of irradiation embrittlement and then at least conceptually use that variation shown by the dashed green line, along with an acceptance criteria for through wall cracking frequency that's been established consistent with current Commission guidance to get a screening limit.

We then also have looked at the characteristics of the types of transients that dominate the failure frequencies and the characteristics of the plants that produce those types of transients to give us some insight as to the general applicability of that screening limit to all operating PWRs.

As the committee is, I think, familiar with, one of the guiding principles of this project has been a very systematic and, we hope, thorough treatment of uncertainties, and there are certainly sitting around the table folks who are much better experts on the words "aleatory" and "epistemic" than I. So I won't go into that because I'll probably trip up.

MR. SIEBER: He's not here yet.

MR. EricksonKIRK: Oh, okay. Good.

But from my point of view as a practicing engineer, I think the process that we've gone through is good because being very systematic, it has made the uncertainties visible, and once you make something visible, then there's a certain obligation to treat it, and I think it improves the overall comprehensiveness of the model.

DR. WALLIS: Mark, in the document which you reviewed I think it's two years ago, it was a big, fat thing.

MR. EricksonKIRK: Yeah.

DR. WALLIS: There were lots of very useful plots where you actually plotted data, and we could see the uncertainty. The new document doesn't have that. So in order to find out what it's really based on, you have to go somewhere else, and I found that rather difficult.

MR. EricksonKIRK: You'll find that in the supporting documents that somehow erroneous you just received.

DR. WALLIS: But the final document looks so great because you don't have these plots which we had before, but the data were all over the place, and someone was saying you can do something with that, which is useful.

So I had some trouble with that. Maybe I'd just like to see the evidence somewhere in the final report so that we know what kind of a beast we're dealing with.

MR. EricksonKIRK: I think the plots you were referring to were, of course, the materials and fracture mechanics plots. Those were taken out of the top report and put into the detailed report on fracture mechanics, which again unfortunately didn't get delivered to you even though it was available. So there has not been an attempt to obscure that, but just to put it into --

DR. WALLIS: Oh, no, I don't think that you're obscuring, but it would have helped in our understanding of how you treated the uncertainty, which is a key thing you're doing here. If we could have looked, again, at that and seen what the nature of this uncertainty was.

MR. EricksonKIRK: Yeah, the best way I can say it is that we made the decision to take the details of the process, which means all the detailed model development and justification and the uncertainty treatment, and to put that in three supporting reports, one on PFM procedures, one on --

DR. WALLIS: Which we didn't get.

MR. EricksonKIRK: -- TH procedures, which unfortunately you did not get.

DR. WALLIS: So how are we going to get a good feeling that this is all technically justified?

MR. EricksonKIRK: Is Dr. Shack going to bail me out on this one?

(Laughter.)

MR. EricksonKIRK: It would be only fair to give you time to read that report, in my opinion.

CHAIRMAN SHACK: It's not clear that you're going to get your letter this time I guess is the answer.

MR. EricksonKIRK: That's perfectly fine.

No, you certainly should go through those detailed reports because it's in there, and what's the saying? The devil is in the details, and the details are in those reports, and I would personally find it gratifying if somebody read them. I spent a lot of my life on it.

So, no, they are there, and I apologize if it was in any way even unintentionally obscured.

The scope of the plant specific analyses we performed is we did detailed analyses of the Palisades, Beaver Valley, and Oconee plants. In picking these, we have one from each of the three major PWR manufacturers.

One plant, namely, Oconee, was used in the original PTS study, and the other two plants, Palisades and Beaver Valley, are among those that are the closest to the current PTS screening criteria.

So when you talk about PTS in current regulatory space, almost invariably you have great interest in and discussion of both Palisades and Beaver Valley. So we thought it important to incorporate those.

And not, incidentally, I should add that these management of these three plants felt it was in their best business interest to participate.

So now I'm going to get on to results, where I'm sure we'll have -- well, this is a preview of things to come, and so if you don't see supporting details, it's because I'm trying to get through this in ten minutes.

Looking at the material factors controlling vessel failure and what the cartoon attempts to show is the big block with the lines on it is a schematic roll-out of the inside of a reactor pressure vessel. So pretend you're standing inside, slit it, and then unwrap it flat, and so that shows at least schematically the locations of circumferential welds and axial welds, and then the sort of transparent thing is the austenitic stainless steel cladding, which of course goes over top.

And then the red squiggly lines show the azimuthal and axial variations.

DR. WALLIS: Now, is that to scale so that it means that it means that the fluence is four times or something?

MR. EricksonKIRK: Yes, that is correct.

And that, of course, depends upon the specific core geometry, but that's typical.

DR. WALLIS: So you just rotate the core occasionally, huh?

MR. EricksonKIRK: Well, actually, no, no. You shouldn't because it's good to have -- you can think of how you're going to bring the fracture --

MR. SIEBER: She can't hear you.

MR. EricksonKIRK: I'm sorry. Each of the areas of low fluence you should view as not being a bad thing, but a strip of very tough material --

DR. WALLIS: Do the cracks only go 90 degrees and then they stop?

MR. EricksonKIRK: Yeah. That in the very unlikely event that a circumferential crack actually made its way through the wall, it would be encountering tough material on both sides and then stop.

So, no, I don't think you should rotate the core.

MR. ROSEN: It would also be bad for the attached coolant lines to do that.

MR. EricksonKIRK: As you can tell, I'm not an operational guy. He's sitting in the back.

MR. SIEBER: Yeah, you rotate the core and not the vessel.

(Laughter.)

MR. EricksonKIRK: Okay. So it is perhaps self-evident, but the distribution of flaws and also, therefore, of -- well, not there, but the distribution of flaws varies widely through the vessel. Welds have different sorts of flaws and plates. Cladding has different sorts of flaws and so on, and of course, the toughness varies through the vessel both because these different regions, plate, weld and so on have different chemistries and, therefore, different irradiation sensitivities.

DR. WALLIS: Cladding is all welds, isn't it?

MR. EricksonKIRK: The cladding is all austenitic weld, yes. So the cladding is a factor in this analysis not because it can lead to brittle fracture, which of course because it's stainless steel it can't, but because it introduces a full population that pokes its nose sometimes into the ferritic material and can therefore initiate.

So for reasons, again, the details we'll go into later; axial flaws are much more damaging than circumferential flaws, and obviously large flaws are worse than small flaws. So flaws that are larger than the rest and oriented axially and located at high fluence locations are, of course, the most damaging.

DR. WALLIS: And on the surface.

MR. EricksonKIRK: On the surface, but we don't have too many surface flaws in this analysis because there's not a physical reason for them to be there, but, yes, surface flaws are, of course, more damaging than imbedded.

So what we find out in the materials analysis is the vessel failure is controlled mostly by the axial flaws, and larger axial flaws being worse than smaller axial flaws. It's the axial flaws along the axial weld fusion lines that contribute the lion's share to the through wall cracking frequency.

And so it is, therefore, the properties that could be associated with those flaws, namely, the properties of the adjacent plate or the properties of the weld that to a large extent control the vessel failure probability.

DR. WALLIS: And these welds are located relative to the cold legs in some way as well, is it not? I don't know where the cold legs come in.

MR. EricksonKIRK: The cold legs are up here.

DR. WALLIS: If there are plumes, then I don't know where the plumes are relative to these flaws -- these welds.

MR. EricksonKIRK: That's right. Well, Dave will be talking about plumes later, and I think --

DR. WALLIS: -- relative to the welds?

MR. EricksonKIRK: I'm sorry?

DR. WALLIS: Do the plumes bathe the welds or are they in between the welds?

MR. EricksonKIRK: They could be either, and I'm not sure they're preferentially located, but, Dave, do you want to say something?

MR. BESSETTE: Well, most plants the welds don't fall underneath cold legs, but there may be some which do. I haven't really been able to find that information, exactly which is which, but I know that in most plants welds are not underneath the cold legs.

MR. EricksonKIRK: It's certainly knowable, but for plumes you shouldn't be so concerned about the axial flaws. You should be concerned about the circumferential flaws because the plume, if it contributes anything, it contributes an increased opening force to flaws that are located circumferentially, not axially.

DR. BONACA: Would you give me a sense of how many axial welds there may be? I mean --

MR. EricksonKIRK: You either have the plate segments are either 120 degrees or 180 degrees, most commonly 120. So you'll normally have three around, sometimes two.

DR. BONACA: But none of them has one? I thought the C process as the one of bending the material.

MR. EricksonKIRK: I'm not familiar with it, but I'm not sure I'd rule it out. Again, that's information we can get you, and certainly less welds would mean less flaws, and that's better. The plants we've analyzed, Beaver has 180 degree plate segments, and Palisades and Oconee have 120 degree plate segments.

Again, for reasons we'll go into, the circumferential cracks don't have the through wall crack driving force that you can get in axial cracks, and so the embrittlement properties of the circ. welds and the forgings are of little consequence to the vessel failure probability.

DR. WALLIS: Why did plumes not contribute to axial flaws?

MR. EricksonKIRK: Because they don't produce an opening stress perpendicular to the axial flaw.

CHAIRMAN SHACK: Yeah, but you're a through wall crack guy. For an initiation guy if I have a plume, I get a big surface stress. I can at least initiate a crack.

MR. EricksonKIRK: Yes. Well, perhaps we'll defer. I would like to defer discussion of plumes until David has a chance to convince you that plumes don't exist and then you won't ask me any tough questions.

So, now, looking at the contributions of these different flaw populations to through wall cracking frequency, on this plot you have three different grafts with reference temperatures at the bottom. Forgive my use of degrees ranking.

Reference temperature for the axial welds on the far left side; reference temperature for the plates; and reference temperature for the circ. welds. We'll go into a detailed discussion later of where these reference temperatures come from, but I think that the easiest way to say it right now is these reference temperatures represent the toughness of the material at the location of a flaw.

So the reference temperature for the axial welds is taken along the axial weld fusion line. The reference temperature for the circ. welds is taken at the circ. weld fusion line. Of course, the position of maximum fluence because that happens somewhere along the circ. weld, and the reference temperature of the plate is also calculated at the maximum fluence because --

DR. WALLIS: Well, RT is a material property. It has nothing to do with temperature.

MR. EricksonKIRK: No.

DR. WALLIS: It's not a material. It's a material property.

MR. EricksonKIRK: It's a material property expressed as a temperature. If you remember the schematic you asked about, the reference temperature tells you how embrittled the material is. If you want degrees Fahrenheit, what is it? Subtract 430.

MR. ROSEN: Now, what sort of uncertainty is there on, for instance, the point on the axial weld chart? Take the upper point for Palisades, for example. It just shows the one point.

MR. EricksonKIRK: that's right.

MR. ROSEN: That's the RT axial weld and ET for --

MR. EricksonKIRK: Well, which -- would you like me to do uncertainty vertical or uncertainty horizontal?

MR. ROSEN: Well, certainty is either way, but --

MR. EricksonKIRK: Well, the uncertainty vertical is these are mean through wall cracking frequencies, which is we'll go into detail, correspond to the 90th percentile or higher.

So all of the through wall cracking frequencies calculated relative to this analysis, 90 percent of them are down here. So I would treat those as upper bound points for through wall cracking frequency. In terms of horizontal uncertainty, I think the thing to keep in mind is we can talk about uncertainty and we can certainly share your uncertainty in index temperature placement, but this is an attempt to characterize a vessel using three reference temperatures, and you can certainly appreciate going back to the last slide, that forgetting about uncertainty, just looking at deterministic variation, you have toughness that varies point-wise through the thickness of the vessel, around and up and down.

CHAIRMAN SHACK: But when you show it to us, won't you have built all of the certainty into the vertical uncertainty because that's really your nominal temperature there and all of the uncertainties you've sort of built into the fracture mechanics calculation, haven't you?

MR. EricksonKIRK: I'm sorry. Say that again.

CHAIRMAN SHACK: When you say 90th percentile, that's really the 90th percentile against the nominal RTAW.

MR. EricksonKIRK: Yes.

CHAIRMAN SHACK: So there's no uncertainty in that horizontal term.

MR. EricksonKIRK: That's right. That's a nominal value that's calculated to represent a particular plant, and you'll see as we go on that those values are then used to establish a screening criteria.

MR. ROSEN: Doesn't that surprise you, given that data represents all of that in three different plants, that it all falls so closely along the line?

MR. EricksonKIRK: Not a bit and I'll show you why.

MR. ROSEN: Okay.

DR. WALLIS: Now, let's get this clear again. This RT is not a temperature. It's --

MR. EricksonKIRK: No, it is.

DR. WALLIS: It's not really a material property. It's what is calculated from an equation really, ASME's or somebody's equation.

MR. EricksonKIRK: No, it's not an ASME question.

DR. WALLIS: But it's calculated from something. So it's a nominal value. It doesn't tell you what the toughness of the steel is in the plant.

MR. EricksonKIRK: No, it most certainly

does.

DR. WALLIS: No, it doesn't. There's a tremendous scatter if we plot these data on a plot like this. There's a tremendous amount of scatter as I remember.

So your RT you're using is some kind of calculated thing, which is deterministic, and then the scatter appears somewhere else. We can't scatter on that horizontal axis you have because RT is calculated in a deterministic way.

MR. EricksonKIRK: Yes.

DR. WALLIS: But if we look at different steels on a plot like this, the curves are all over the place.

MR. EricksonKIRK: That's right.

DR. WALLIS: So you say what's the real RT for a steel with a lot of uncertainty.

MR. EricksonKIRK: No, the uncertainty that you're talking about is the fracture toughness in the --

DR. WALLIS: It's for uncertainty in the RT. We take different steels as you did in your earlier report and plot them like this. You've got a lot of different curves.

MR. EricksonKIRK: That's right, and what you find out is again, as shown in the schematic, radiation is shifting the curve that way, but if you test enough of a material, you will converge in on -- you know, if I take this plate, if the conference table was a plate and I chopped it up into 1,000 specimens, you'd see that there's one reference temperature for that, and that the uncertainty in RTndt is a testing uncertainty, but that given enough testing, you can resolve out.

But what you're finding is the uncertainty in the actual toughness itself and so what we do is we use the reference temperature as a metric of irradiation damage.

DR. WALLIS: Well, this is probably where you have to go back to the technical details which you can't go into today and which we don't have, but I guess the RT you showed in the other curves where everything came together nicely --

MR. EricksonKIRK: Yes.

DR. WALLIS: -- the calculated value doesn't claim to be sort of the mean value of a prediction for a plant. It's actually a calculated value from something that's deterministic?

MR. EricksonKIRK: The RTs that were shown in the other plot are calculated based on the mean chemistry properties of the welds, plates, for forgings in the vessel that are in the RVID database. They're calculated based on the fluence at the flaw locations, which is also in the RVID database, and on the length of the welds.

DR. WALLIS: And they are the lower bound of a whole mess of data that's scattered all over the place?

MR. EricksonKIRK: No. They're the values that are in the database that are taken to be mean values, but if you recall, I think we're focusing on the wrong axis because it doesn't matter if we're using a mean value or a lower bound or an upper bound. What you want to know is irrespective of the procedure I give you for calculating RT whatever, what you want to know is that at that RT value, whatever it is and however I got it, that most of the failures are down here and a few of the failures are up there.

And that's, indeed, the case. So hopefully this will --

CHAIRMAN SHACK: In fact, I mean, you want something that you can calculate.

MR. EricksonKIRK: Yes.

CHAIRMAN SHACK: You have to have something that is deterministic in this plot, you know, and then you want to have the scatter going up and down this way and bound that.

MR. EricksonKIRK: Yeah. If you will, the analysis results here is the vertical location, and we were using mean values, that because of the distribution shape represent 90th percentiles or higher, and then the horizontal values, as Dr. Shack pointed out, I think, more eloquently than myself, are values that you can calculate for each plant using only the information that we have available.

CHAIRMAN SHACK: You know, you've done through wall cracking frequency, and I noticed none of your peer reviewers gagged over that. You know, but don't the Europeans still basically look at this problem as an initiation problem?

MR. EricksonKIRK: They do, yes. They do look at this as an initiation problem. I think that was a deference for whom they were reviewing. I don't think any of our European friends necessarily advocated through wall cracking frequency, but just to expand on this because I know you've asked me this before, if one -- and I'll just say "if" -- if one wanted to move to an initiation based criteria, not only would the numbers change, but what's important would change because for reasons that we'll go into in the details, while circumferential flaws find it very difficult to propagate all the way through the vessel, the probability of initiating the circumferential flaw is, if anything, equal to or greater than initiating an axial flaw.

So if one were to go to an initiation based criteria, you'd find the properties of the circumferential welds and the forgings becoming important again, and they're not now.

But, no, to address Dr. Rosen's question, I don't find this at all surprising, and I guess you'll have to accept that on faith and hopefully I can build the faith over the next day, but what we find is that the transients that contribute to these failures are pretty similar from plant to plant, and the frequency with which they occur are pretty similar from plant to plant, and the material metrics that we're using here are estimated at the location where the flaws are, as opposed to being some conservative bound that's inconsistent from plant to plant.

So, no, I don't find this type of agreement in any way surprising.

CHAIRMAN SHACK: If you have material that's embrittled to the same site and you hit it just as hard, it's not going to matter whether the plant --

MR. EricksonKIRK: The same thing is going to happen each and every time.

One thing I'd like to take away from this plot is the relative contributions of axial weld flaws, plate flaws, and circ. weld flaws. Axial weld flaws at a fixed level of embrittlement contribute 100 times more to the through wall cracking frequency than plate flaws. The reason for that difference is that plate flaws tend to be smaller, but they're still axially oriented.

And then circ. weld flaws, again, at the same level of embrittlement are, again, 50 times less. So circ. weld flaws can in rare cases of high embrittlement go through, but essentially for a through wall cracking frequency criteria, they're nonplayers.

Looking at similar plots, but now dividing things up into contributions of different transient classes, we see a similar good agreement or I should perhaps say reasonable agreement between the plants. Primary site pipe breaks where the through wall cracking frequencies are dominated by medium and large break LOCAs; primary site stuck open valves and main steamline breaks, all are reasonably consistent from plant to plant, and again, the reason for that is --

I don't have the words here that I'm searching for -- is that let's take an example of a large diameter pipe break, eight or 16 inches.

At that point, the cooling of the water inside the vessel from the depressurization is so fast that the steel wall can't keep up. It's a conduction limited situation, and so the rate and magnitude of thermal stress development in the wall is controlled only by the thermal conductivity properties of the steel, which since it's a physical property and not a mechanical property are very consistent from material to material.

DR. WALLIS: Not the surface. The surface gets chills. The actual surface layer gets chilled.

MR. EricksonKIRK: Yes.

DR. WALLIS: It's very important whether or not there are flaws at that surface, isn't it? I mean, the penetration of the thermal wave is going to affect flaws which are in the material, but the surface is under very high stress, isn't it?

MR. EricksonKIRK: That's right.

DR. WALLIS: That variable surface layer. So it depends a lot on whether or not there are flaws near the surface?

MR. EricksonKIRK: That's right, and there are flaws near the surface. I mean, the probability of getting an embedded flaw in the vessel is, from our inspections performed at PNNL, is equal as you go through the vessel thickness.

DR. WALLIS: Well, you're saying that the wall doesn't -- I agree that the wall doesn't cool down, but the surface has cooled down to the vessel.

MR. EricksonKIRK: Yeah. Well, I mean, obviously it's a continuous process, but the point I was trying to bring out is that the transients that are producing the single transients or classes of transients that are producing the largest contributions to the through wall cracking frequencies are transients where by and large the details of the transient don't matter. They're the larger breaks whereas let's take an alternative example. If it was smaller breaks that are controlling, then the time at which certain pumps come on would be important, where you're getting your injection water from would be important, all of these little minute, plate specific details would become important.

But the things that are driving most of these through wall cracking frequencies are transients or transient classes that are fairly consistent from plant to plant, and that's responsible for the -- that and the fact that we're using consistent material metrics that represent the toughness at the flaw locations -- is responsible for the good agreement that you're seeing.

CHAIRMAN SHACK: Why do I get the cross-over between the stuck open valve and the pipe break?

MR. EricksonKIRK: Because it would appear that at lower levels of -- okay. Certainly what you see -- let's talk about the primary site pipe breaks. You get a very high thermal stress in a pipe break, but I won't say no because that's an old wives' tale, but much lower pressure stresses. So it's very --

DR. WALLIS: So it's a reclosing of the valve.

MR. EricksonKIRK: It's the reclosing of the valve. It's very easy for a thermally dominated transient to initiate a crack, but to push it all the way through, you have to have a vessel that's pretty brittle.

So you get high initiations from LOCAs at all embrittlement levels, but it's only when you crack up the embrittlement level that they can go all the way through, whereas the primary site pipe break, as Dr. Wallis just pointed out, has that nasty repressurization sometimes later on which, if a crack has started, it will fail, and that's a big difference between these two types of transients.

A medium to large break LOCA, if a crack is initiated only between one and ten and one and 100 of those cracks will eventually go through wall almost irrespective of embrittlement level, whereas with the primary site with a stuck open valve that later recloses, it's the pressure stress that's failing it, and so if it initiates it, it will certainly fail.

DR. WALLIS: This is one stuck open valve. Does two stuck open valves, you couldn't quite seal the bottom line for that in your --

MR. EricksonKIRK: Two stuck open valves contributes somewhat more -- well, it contributes -- hold on.

Holding all other factors constant and just comparing one stuck open valve with two stuck open valves, two stuck open valves is a little bit more severe because since you've doubled the valve opening area, you've increased the cooling rate, you've dropped the minimum temperature somewhat, and so at the time of valve reclosure when you get that sudden pressure stress, you've got a little bit higher thermal stress and a little bit lower toughness. So you get a little bit more through wall cracking frequency.

But the thing that makes two stuck open valves not be a dominant contributed to the through wall cracking frequency is the weighting by the initiating event frequency because it's so much less likely to have two than one, and once you get up to three, forget it.

MR. ROSEN: And also you have to consider that both stuck open valves reclose.

MR. EricksonKIRK: Yes, both stuck open valves have to -- well, no. Okay. I'm winging it now because I haven't actually looked at this plot, but the thing that makes two worse than one, one reclosing is enough to produce the complete return to full system pressure, assuming the operator doesn't throttle in a timely fashion.

But if you've got two stuck open, you've got twice the water going in. So you've got twice the cooling rate.

MR. ROSEN: I understand that, but I'm thinking about what happens at the end of the transient. One recloses or both reclose? Is there a difference in --

MR. EricksonKIRK: Yeah, once you --

MR. ROSEN: There certainly is a

probabilistic difference in both reclosing.

MR. BESSETTE: Yes, necessary to have two of them stick open and two of them reclose, yeah, if that's what you're saying. So in a probability sense --

MR. ROSEN: Just not thinking about the frequency of both reclosing at essentially the same time.

MR. BESSETTE: Yeah, yeah.

MR. ROSEN: I mean, clearly that's not going to happen with a frequency of --

DR. WALLIS: Unless they're the kind of valve that has a block valve or something in series and the operator could shut them both.

MR. ROSEN: Well, yeah. Manual action could do that, but not --

DR. WALLIS: Anyway, it's the frequency that makes it unimportant, the initiating frequency.

MR. EricksonKIRK: Okay. I'm going to move boldly on because we're running behind.

Just some observations on the transient classes of control failure. Secondary side breaks are much less damaging than primary side breaks, the major reason being not because the cooling rate is any different, but because the main steamline breaks you've got a multi-square foot opening. That cools down every bit as fast as a big pipe break. The major difference is and the dominant factor controlling the through wall cracking frequencies is that the minimum temperature doesn't get so low.

When a secondary side break occurs, the lowest temperature the primary can get is to the boiling point of water at the pressure of the break. So 212 for a break outside of containment, about 40 degrees higher for a break inside of containment.

So since the temperature is higher, the toughness is higher, and you just don't get that big a contribution.

Overall, and my PRA colleagues will go into details on this, we have credited operator action throughout this analysis, and I know that's been a concern that, you know, we might be developing a rule that's based on credits for operator action.

However, when you get to the end of the day and you look at the transients that are contributing the most to the through wall cracking frequency, you find that the operator action credits really haven't had a very big influence on those frequencies.

Certainly for the primary side pipe breaks there were no operator action credits at all because the operator can't do anything.

DR. WALLIS: Well, you can turn off the coolant injection and stop the thermal shock.

MR. EricksonKIRK: Well, you could, but then you'd melt and --

DR. WALLIS: That's right.

MR. EricksonKIRK: -- presumably procedures would prohibit that.

For stuck open valves, operator action credits are important. However, we have found that the operator has to act very, very rapidly in order to prevent the repressurization, and he can only successfully prevent repressurization when initiation has been at hot-zero power. So the net effect of the operator action credit has been very small in the end result.

And also, and again, this is all summary. So we're going to go into the details. We believe that with only a few caveats our findings should be applicable to PWRs, in general -- I've said a lot of this before -- because the transients that contribute to most of the through wall cracking frequency have a approximately equal occurrence rate and approximately equal severity across plants.

Operator actions have only a small influence on the final calculated through wall cracking frequencies for the transients that are important.

Similarity in PWR designs plays a big part. We have similar diameters, similar system pressures, similar thicknesses and so on, and also as we'll go into, there are a number of conservatisms that have been left in the model.

DR. BONACA: The question I have was on the issue of steamline break versus LOCA, and you already went through this before. But this steamline break was the limiting transient before, used to be.

MR. EricksonKIRK: That's only because large break LOCAs weren't analyzed.

DR. BONACA: Ah.

MR. EricksonKIRK: Yeah. In the old analysis -- and Mike can correct me if I'm remembering my plants wrong -- but I believe it was Oconee for which the main steamline break was dominant transient. It was the dominant transient only because large break LOCAs weren't analyzed and stuck open valves weren't analyzed.

DR. BONACA: Well, but they assume that the feedwater would keep running.

MR. EricksonKIRK: And they made a very conservative treatment of both, what happened, and also the frequency with which it occurred.

DR. BONACA: Which is an incredible thing, that the operators would not stop it, but wouldn't the operator be significant action?

I'm just, I guess --

MR. EricksonKIRK: For the steamline break, again, well, we can do all of the presentation now.

DR. BONACA: No, no, no.

MR. EricksonKIRK: A steamline -- well, if a steamline break breaks, it breaks within the first ten or 15 minutes, long before operator action is likely because the thing that produces the high stresses in a steamline break is that rapid cool down, and if you can survive that, you're okay.

DR. BONACA: We'll see when we get there.

MR. EricksonKIRK: I'm not sure how much detail we want to go into on these type of plots because clearly, the committee is looking for more details, but what we're proposing as a revision to the PTS screening limit is a multi-parameter approach where you calculate a reference temperature for your flaws in your axial welds, a reference temperature for your flaws in your plates and a reference temperature for your flaws in your circ. welds, and this can all be done based on information that's available now to the licensees and is in the RVID database.

And based on that, based on those metrics, you can place a point which represents a plant in a space, say -- let's just look at plate welded plants -- of the axial weld reference temperature and the plate reference temperature, and then this is a failure probability space where the further you get from the origin, the higher your failure probability becomes.

And using a limit on failure probability, one times ten to the minus six, you can construct a locus where if the plant assessment point is inside the locus, you're at a lower failure probability, and if it's outside, you've passed your limit and you need to do something else.

So that's going to be where we're heading, but also by means of summary, suffice it to say that at both end of license and even end of license extension none of these assessment points and what you see on here are assessment points for all the PWRs that are currently licensed to operate by the NRC; none of them are anywhere close to the limits that are calculated by this procedure.

CHAIRMAN SHACK: Now, those temperatures that you're showing us there don't have the margin terms, do they?

MR. EricksonKIRK: No, they do not have the margin terms.

CHAIRMAN SHACK: But you're arguing that you don't need those margin terms because you've built that uncertainty into your bounding envelope.

MR. EricksonKIRK: Because we've built the uncertainty into the bounding envelope and because of the conservatisms; that the conservatisms left in the model far outweigh the nonconservatisms left in the model.

The point I'd like to make here is just in terms of this graph, and you can kind of discern it from the graph that was on the previous page. This is a histogram of an estimate of through wall cracking frequency for all the PWRs that are currently licensed to operate by the Nuclear Regulatory Commission. We showed distribution for forged vessels and for plate vessels, and you can see that even the worst plate vessel doesn't have a through wall cracking frequency estimated at EOL that exceeds ten to the minus seven, and by and large the average value is much, much lower.

But to get to Dr. Shack's comment, certainly currently a margin term is assigned to -- is used in our current assessment procedure to attempt to account for unknowns and uncertainties that weren't considered in the process that generated the 270 and 300 degree limits, and that's certainly an appropriate reason to use a margin term, is to account for things that we believe to be outside of your analysis.

Certainly we believe we've tried to do a much more comprehensive job in setting these bounds, but also in the process of building any model, you never have perfect knowledge,a nd so there are always judgments that you have to make along the way, and so at the end in assessing this type of screening procedure and whether you believe that an additional margin needs to be attached or not, to kind of put it in perspective, I think it's appropriate to look at the residual conservatisms in the model and the residual non-conservatisms in the model.

DR. WALLIS: This is where it would be useful for us to look at the actual technical reports.

MR. EricksonKIRK: That's right.

DR. WALLIS: If we look at, say, the model of RT shift due to embrittlement, I remember there was a lot of stuff in your technical details which was interesting on that subject --

MR. EricksonKIRK: That's right.

DR. WALLIS: -- in the previous report, and I didn't find any of it this time.

MR. EricksonKIRK: In the mysterious missing 18 minutes of report, yes. And we'll be discussing these over the next few days, but certainly it's at least my personal view -- I think it's a view that's held by most of the staff -- that both the number of conservatisms in the model and their magnitude far outweighs the non-conservatisms that are left.

So I personally would be pretty comfortable with using these risk based limits and the proposed calculational procedures to get plant specific points without having to add an additional margin term because --

DR. WALLIS: Why is the heat transfer model non-conservative? Actually for the worst case it doesn't matter anyway, does it?

MR. EricksonKIRK: For the worst case it doesn't matter anyway. Dave can go into detail. The placement of any one of these words on either side is obviously a matter of judgment. So this is biased by the person that made the slide.

However, in Chapter 9, the use of the heat transfer model that was proposed by Professor Catton, I think, showed a factor of three increase in through wall cracking frequency relative to the one that we're using for the 12 dominant transients in Palisades.

So that was my basis of putting it there. As you all know, I'm not a heat transfer expert. So if you folks decide it belongs over there or to be completely scrubbed, I'd be happy to make that modification.

MR. SIEBER: Do we have this slide in our package?

MR. EricksonKIRK: No, you don't.

MR. SIEBER: Could you provide us with a copy?

MR. EricksonKIRK: Yes, we will. I'll have to get together with Dr. Shack to find out exactly what's missing and we'll provide you with a complete finalized set.

I guess this was the most major modification, and the reason being is we got Dr. Murley's comments yesterday, and one of his comments was he said, "I see your nice list of conservatisms. To be fair, guys, you really need to have a list of non-conservatisms, too, because I know they're in there."

And so we've gone through and tried to do our best job at listing or at providing a balanced view.

MR. ROSEN: Go back to the slide that Murley commented on and let me torture you some more on that, but only in the stuff above where he commented.

MR. EricksonKIRK: Okay.

MR. ROSEN: Well, now, you see, that's different from what I have in my package.

MR. EricksonKIRK: What's that?

MR. ROSEN: I was going to ask about in my package it says -- it's the third bullet that says the results are not much different at the end of the license renewal period, and I assume that's referring to this chart on the right.

MR. EricksonKIRK: That's right.

MR. ROSEN: Which, by the way is at EOL 32 effective pull power years.

MR. EricksonKIRK: That's right.

MR. ROSEN: Which is not the license renewal period, which is why they made that comment on the earlier version of the slide.

MR. EricksonKIRK: Okay.

MR. ROSEN: Now, moving ten to 20 degrees Fahrenheit closer to the screening limits and EOL, I guess, is what I was seeking, to get a sense in the slide package that was handed out, the statement that their results are not much different isn't particularly helpful, I mean, at the end of the license renewal period because this committee spent so much of its time on license renewal.

MR. EricksonKIRK: Right..

MR. ROSEN: What happens to these through wall cracking frequencies? What happens to the bulk of these plants when you go out to 60 years?

MR. EricksonKIRK: Yeah. If you look in, and I can pull it up on the screen, but if you have the summary report, if you got to -- there's a histogram of that in Chapter 11, of the summary report, and if I can look at it, I can describe it to you.

CHAIRMAN SHACK: You go to your scatterplot and just move the points ten or 20 degrees over, and they're not going to move very far.

MR. EricksonKIRK: In other words, you don't get --

MR. ROSEN: But characterize it in words.

Mark, work with me on this one. Just look at the slide on the upper right-hand, what you're showing now, on through wall cracking frequency. What happens to the bulk of those plants? Do they move half an order of magnitude or less than half an order of magnitude?

MR. EricksonKIRK: About half.

MR. ROSEN: About half?

MR. EricksonKIRK: About half.

MR. BISHOP: If you go back to your Slide 14, Mark, you've got a lot of the through wall cracking, which is the reverse of Part A, and Part A is one of the ten or 20 degrees, and you get back for the worst axial flaws.

MR. SIEBER: Could you use the microphone, please?

MR. EricksonKIRK: I'm sorry, Bruce. Fourteen?

MR. BISHOP: That right there. You can just see ten or 20 degrees. Those degrees are --

MR. SIEBER: You have to use the microphone.

MR. EricksonKIRK: Okay. What Bruce Bishop from Westinghouse is pointing out is that actually the slopes on these lines are all very close to each other. So if you look at changing 20 degrees on any one of these lines, you're looking at increasing the through wall cracking frequency by half an order of magnitude or less.

MR. ROSEN: Okay. That's very helpful.

MR. EricksonKIRK: And, indeed, that's what you'd expect because you're getting out, you're using up the embrittlement in the vessel. It's starting to plateau. It's not getting much worse.

MR. ROSEN: So, now, let's extrapolate. If you wanted to go 100 years for the plant or 500 years --

MR. EricksonKIRK: Or perhaps 1,000.

MR. ROSEN: -- you're saying at some point it's just not going to change anymore. The vessel is not going to become limited because of physical --

MR. EricksonKIRK: Well, from a materials viewpoint you reach a physical limit on embrittlement where it's just not going to get any worse.

Now, whether the driving force is low enough to keep you from failure, that's another issue.

MR. ROSEN: But the vessel material just gets as bad as it's going to get, and that's all it is.

MR. EricksonKIRK: That's right.

MR. HISER: Hold on one second. This is Allen Hiser from the Engineering Branch of Research.

You've got to watch out because our understanding of fluence effects on embrittlement, there's after a certain level of fluence, we don't know what happens outside of those. There may be there are postulates of additional embrittlement phases and mechanisms that kick in. So we need to stay in the box, if you will, with the data that we have before we extrapolate too far.

MR. ROSEN: I wasn't really advocating a 1,000 year plan.

MR. HISER: I'm not sure that 100 gets us there either.

MR. EricksonKIRK: Okay. Just one more slide. Since we're already behind schedule, so for the remainder of the briefing, we've structured the briefing to parallel the summary report which you have received, fortunately. So the next thing we're going to go through are our fundamental assumptions which you'll find in Section 3.3.

We'll then go on to address significant changes that we've made in our models since we last briefed you, and in some cases talk about significant peer reviewers' comments and, of course, changes in our models.

That will take us up to lunchtime, and then after lunch we'll be briefing you on our baseline calculations which are in Chapter 8, generalization to all plants, and Chapter 9, reactor vessel failure frequency acceptance criteria, and Chapter 10, Chapter 11 on PTS screening criteria, and then a summary.

And then tomorrow morning we'll go into a more detailed discussion of the peer reviewers' comments. And at least on some of the slides you'll see indices to sections, figures, chapters in your in your detailed reports so that you can see where we're getting the information from.

That's all I have on this section unless there are any more questions.

(No response.)

MR. EricksonKIRK: In that case I'll ask Donnie Whitehead to join me up front. Donnie is from Sandia National Laboratories an has performed a probabilistic risk assessment.

MR. WHITEHEAD: Good morning. My name is Donnie Whitehead, and I'll be making a presentation on at least the PRA/HRA aspects of this analysis.

The first topic that we want to cover this morning has to deal with basically the fundamental assumptions that have been made as it relates to the PRA aspect of the project, and basically there's two types of assumptions that we've made.

If you will, the typical type assumptions that are always generally made within the PRA work, things like, you know, the example given here, in the actual plant system configuration is represented by the as-built, as-operated information that's documented.

What I'd like to concentrate more so this morning though is on the assumptions that we've made specifically for the PTS analysis, and those basically can be categorized into seven different sets of information.

The first one is Project Execution, and basically by that I mean just what kind of lessons did we learn ad we went through our analyses. The first plant that we dealt with was the Oconee plant, and the analysis that was done for that plant was a very detailed exhaustive analysis where we look at basically all types of initiating events. We look at all types of system and equipment response and try to identify, you know, any possible combination of equipment failures and/or successes that might lead to conditions that would produce thermal stress in the reactor vessel, ultimately leading to failure from PTS events.

We then used the information that we learned from the Oconee analysis to modify what we did for the two subsequent analyses, both the Beaver Valley and the Palisades analyses, and so basically we used information that we learned like what thing were showing up to be important, what things were showing up to be not important to modify the rest of the analyses as a means of saving resources for the project.

The next issue that we dealt with has to do with initiating events. There are basically two types of initiating events that we didn't look at or actually didn't analyze. We did look at them, but we screened them from our analysis.

The first one is basically the anticipated transient without SCRAM EVENTS. We eliminated that type of event because typically these generally begin with severe under cooling. In essence, there's actually too much power for the cooling that you have, and so we used that plus the frequency that typically occurs with these events to eliminate them from further analysis.

The other initiating event that we removed from the detailed analysis was interfacing system loss of coolant accidents. While we recognized that these could involve over cooling from the start of the event, it was also recognized that significant ISLOCAs often fail or are assumed to fail the various mitigating equipment in the PRAs, which ultimately would lead to an under cooling event rather than an over cooling event.

So we used that argument to eliminate them from our detailed analysis.

One other thing that we did was we had to deal with the fact that we're looking at both at power and hot-zero power initiators. We decided that the best approach for that was to look to see basically what fraction of time plants are at hot-zero power as opposed to being at power operation, and to look to see if there were any evidence associated with an increase initiating event frequency for various types of initiators depending upon whether you were at power or whether you were at hot-zero power.

And what we found was that the only type of initiating events that were typically more prone to occur to occur at hot-zero power than at full power were those involving reactor or turbine trips.

And what we did was look at the

information and made an estimate that, you know, about a factor of ten increase in those types of frequencies would bound the information that we were seeing.

And so what we did was we multiplied the fraction of time that plants are typically at hot zero power by this factor of ten, and resulted in a multiplier of .2 for an initiators that initiate at hot-zero power and involve either reactor or turbine trips.

MR. ROSEN: Donnie, let me ask you about your definition of hot zero power.

MR. WHITEHEAD: Yes.

MR. ROSEN: Is that a critical condition or is it just normal operating pressure and temperature and not critical?

MR. WHITEHEAD: It would be normal operating temperature and pressure and basically not critical. Zero --

MR. ROSEN: Okay. This is Mode 3 basically?

MR. WHITEHEAD: Yes, basically.

MR. ROSEN: Rather than Mode 2 because Mod 2 you're in a very, very short time.

MR. WHITEHEAD: Yes, that is correct, yes.

MR. ROSEN: And then Mode 3, it's possible a plant might linger in Mode 3. Point, oh, two is the number you're using.

MR. WHITEHEAD: That's correct.. That was based upon the information that we had for the typical type of outage that plants might be in.

MR. ROSEN: So that's like seven days, as long as, right?

MR. WHITEHEAD: Something like that, yes.

MR. ROSEN: That's probably conservative, too.

MR. WHITEHEAD: Actually we found that the real number that we actually looked at is somewhere around one and a half to one and three quarters percent. Here's one of the areas that Mark would talk about where we have, you know, essentially some small conservatism built in. Instead of calling it, you know, one and a half percent, we just simply rounded that to two percent.

MR. ROSEN: Well, you're effectively saying the plant is going to stay at normal operating pressure at temperature during any given year for seven days, and I think that's conservative. I don't think plants will do that unless some very unusual circumstance.

A more typical number might be in the

hours range really, and some years they won't be in it at all.

MR. WHITEHEAD: That's correct. I mean, this is based on, you know, looking at multiple refueling type outages and things like that, and so, you know, again, this is an area where we would expect there to be some conservatism in, but again, it's an assumption that doesn't significantly or does not affect the overall conclusion that we've been able to reach, that is, that, you know, there appears to be sufficient room to warrant maybe a modification to the PTS rule.

In the area of scenario development, there were a couple of things that we want to talk about. As Mark has alluded to there were some of the classes of initiating events where we basically did not take any credit for any type of operator actions or anything like that. These consist mainly of the large break and medium break LOCAs.

They were basically just the initiating event frequency, and that was then passed to the thermal hydraulics people with the appropriate break sizes, break size spectrums that we looked at for the various breaks.

The reasons being is that at this point in time if you have a medium or large break LOCA there is really nothing that the operators can do other than, as someone else pointed out, turning off the injection equipment that will affect the outcome of the scenario, and so basically we just simply assumed that equipment would respond as appropriate, and so therefore, we didn't really take any credit for some, you know, small, .99 multiplier that you might use to reduce the frequency for high pressure and low pressure systems' injection failures.

Another issue that we dealt with was the status of pressure operator relief valves and the SRVs on the pressurizer. We assumed that the failure of these types of valves or the demand for these types of valves would be unimportant for small LOCA scenarios. The basic reason for that is if you have a LOCA event occurring, you're going to have a pressure drop within the system, and, therefore, this should preclude the demand for the opening of any primary side PORV or SRV.

And then the third bullet basically says that there are some things that we just simply didn't include in the models because they didn't really have any impact or had very little impact on PTS risk, and those were things like pressurizer sprays and heaters.

Continuing with scenario development, we -- and this again goes to one of the points I made for the large break LOCA and medium break LOCA -- is that we simply assume the function for certain SSCs, for certain scenarios. We assume that the accumulators would object if conditions warranted their injection.

We did not include the failure probability associated with the check valves failing to open. So, I mean, instead of multiplying something by .999 that the injection valves would not open, we just simply assumed that they would do so. You know, very small conservatisms, but we wanted to point those out.

Another issue that we dealt with was the importance of when operator actions occur or when a piece of equipment changes state due to various issues associated with PTS. We looked at a limited set of important operator actions, for example here, we have operator fails to throttle high pressure injection, and equipment state changes, stuck open, pressurizer safety relief valves, that either remain open or that subsequently reclose.

We included those into our analysis.

Things that had long-term effects on

scenarios we typically tended to not include those into our analyses, such as heating and ventilation failures were ignored because typically those failures show up long term several hours into various types of scenarios, and that time frame is such that any PTS issue would long be decided and the failure of those types of systems would just simply not be important.

There were a few cases where we used engineering judgment to determine failure probabilities for various SSCs. Typically we tried to be conservative when we had to make these estimates.

An example that I've already given is the fraction of time associated with being in not-zero power condition. We used the value of two percent, where in reality the data that we were looking at showed something on the order of maybe one and a half percent.

But there were a few other cases where we had to use that information.

Human reliability analysis. We had two types of human actions that we looked to. These were the pre-initiator human failure events. For the Beaver Valley and Oconee model, we did not include these explicitly within our model. They were assumed to be in the industry-wide data that was used to model system unavailabilities.

The Palisades model is different. The approach there was different in the sense that this was an existing utility model that was modified to address various PTS issues that we had identified, and basically we just simply left as is any of the human failure events that they had in their model because most of these were events that simply wouldn't have any real impact on what we were doing, and we felt that there was no real need to examine those or to make modifications to them in detail.

Now, for the time at which operators performed the actions on the, if you will, post initiator actions, we typically look at, at least for the ones that were important, we looked at a spread of operator actions, that is, the earliest time at which an operator action could occur and the latest time at which an operator action could occur that might possibly have some impact on the PTS progression of the event itself.

And we would then sometimes choose an intermediate value, one in between those two, just to see if something in between might have some impact.

Another issue was what do we do with the human actions when we're at hot shutdown or hot-zero power. The human reliability analysis that was done is one that's typically based upon the ATHENA approach, and using the ATHENA approach, we did find that there were some cases where it might be that because of what was going on in hot shutdown and so forth, that the human error probabilities could increase somewhat. And so we did account for that.

In the PTS bin development, obviously as you're aware of, you know, we would have --

DR. BONACA: Excuse me.

MR. WHITEHEAD: Sure, yes.

DR. BONACA: The human reliability analysis, you didn't mention any operator actions during secondary site events for breaks.

MR. WHITEHEAD: Yes, we did include those. Typically those would have been things like the operators controlling the steaming from the bad generator, making sure that either feedwater or auxiliary feedwater level was controlled.

DR. BONACA: So you did include that?

MR. WHITEHEAD: Yes, we did include those. Those types of actions were included, yes.

In the bin development, there were large numbers of potential PTS scenarios that were actually generated for the Oconee analysis, and smaller numbers for the Beaver Valley and the Palisades analysis as we became smarter and, you know, had a better understanding of what was potentially important.

What we were faced with was obviously there's no way that we could have done thermal hydraulic calculations for the literally tens of thousands of individual scenarios, and so what we were faced with was trying to bend the scenarios into a more limited number of calculation or bins that we could actually then pass to the thermal hydraulics people for calculations.

And basically what we did was if we as the PRA analyst judged that a scenario's response would be similar to existing TH calculations that we already had, then we would bin that into the existing calculation. If we judged that a scenario's response could be significantly different than what we had as existing calculations, then we requested new TH calculations and we created new bins.

So obviously, there's judgment associated with this and, you know, it was a process of identifying what we believed to be, you know, scenarios that could fit into things that we already had, the various types of calculations that we had already done, thermal hydraulically, and also then looking to see whether or not we -- you know, if the scenario development was sufficient different that we needed to see what would happen, you know, if we did a new TH calculation.

And that was a matter of give and take on the PRA people wanting, you know, typically to do all of the calculations and the thermal hydraulic people saying that, you know, we can do only a certain number of calculations.

MR. ROSEN: Well, you're implying that there was a give-and-take. That means you met with the thermal hydraulic people and --

MR. WHITEHEAD: Yes, yes.

MR. ROSEN: -- discussed these scenarios.

MR. WHITEHEAD: Yes.

MR. SIEBER: Now, you know, in the presentation you indicate all of this spinning, and the reason I keep asking questions about the secondary side break is really for B&W plants. I mean, there is a significant difference between a steamline break in a B&W plant and a steamline break in a C plant where you have a huge inventory of water.

In a B&W type of plant you have, like Oconee, you have essentially no inventory in the steam generator. So you're feeding steam water and flashing and cooling down at much faster rates so that the intervention of an operator is much more important at some point to stop the cool-down.

So I'm having a hard time in seeing the generalization of the treatment for all of these types of plants when I see such a significant difference between, on one hand, Beaver Valley and the Palisades and, on the other, the Oconee plant.

MR. WHITEHEAD: Okay.

DR. BONACA: But you deal with that issue.

MR. WHITEHEAD: I think we'll talk about that in the generalization issue, but let me just add that what you pointed out is absolutely correct, and that is actually reflected in some of the human error probabilities that were assigned to the same type of action depending upon whether it was at, say, Oconee rather than Beaver Valley. Because at Oconee the operators are much more sensitive to what happens on the secondary side than necessarily is the case at the other plants with the larger inventories in the steam generators because they know that there's time available for them to respond.

So those types of issues and conditions were considered, looked at, and incorporated into the analysis.

DR. BONACA: Yes, because, again, you know, the elimination of secondary side as consideration is acceptable to me. I mean, it's obvious for the Westinghouse and C type of steam generator, but the burden, it's higher in eliminating those scenarios from the B&W type plants.

MR. WHITEHEAD: Yes, but even --

DR. BONACA: Because you have to assume, you know, and I believe it's possible and we discussed it a long time ago, regarding the effectiveness of the operator to follow procedures and to isolate and to terminate the event.

But that is why it was such a limiting event for BRW plants when it was originally analyzed, because they assume continuous feeding of water and all, but as an intervention.

MR. WHITEHEAD: Right, and as we're all aware, assuming that the operators will do absolutely nothing is not necessarily the best course of action to take.

MR. SIEBER: How many bins did you end up with?

MR. WHITEHEAD: Typically we ended up with, let's see, you know, in the tens of bins. Oconee, I'm trying to remember off the top of my head.

We had, you know, 40 or 50 bins.

MR. SIEBER: And each one represents a different thermal hydraulic analysis?

MR. WHITEHEAD: Yes, it represents a thermal hydraulic analysis that we, both the PRA and the thermal hydraulics people believe was sufficiently different enough that it warranted its own bin, yes.

MR. SIEBER: Okay, and the bins were different depending on the manufacturer of the plant?

MR. WHITEHEAD: There could be some differences in the bin, though typically there tended to be quite a bit of overlap because the response of the plant would be the same.

For example, the bins that dealt with LOCAs, the medium break LOCAs and the large break LOCAs, I think in each plant we had three medium break LOCA bins and one large break LOCA bin because the thermal hydraulic response could be characterized by, you know, that set of bins both for the medium and the large break LOCA.

And so you know, we ended up with essentially the same number of bins, though there could be some small variation in break size and/or equipment response depending upon what was particularly important at one plant versus another.

MR. SIEBER: Yeah, and the ultimate result was a cool-down curve for each bin?

MR. WHITEHEAD: That is correct. Both a minimum downcomer temperature, the pressure plot, and the heat transfer coefficient plot.

MR. SIEBER: Okay. Thank you.

MR. WHITEHEAD: Yes. And let's see. The way the bin development process occurred was we, as the analyst, looked at minimum downcomer temperature as our primary means of making a determination as to whether or not we needed a new bin or not, and if the minimum downcomer temperatures were approximately the same, then we typically tried to fit the scenarios into the ones that had the higher pressure.

So, I mean, given the same minimum downcomer temperature profile, we then looked to see what kind of variations we were seeing in pressure response and, you know, as long as the pressures response was not substantial, then we typically tried to pick the one that had the highest.

Obviously if the pressure responses were vastly different, then that was one of the keys that we had to go and request, you know, additional information, different calculations for the expected equipment response, the expected temperature, pressure response for the various sets of operating conditions, equipment failures, successes, operator successes, failures.

So I mean, you know, basically we looked at temperature first and then as a deciding factor, we looked at pressure response.

I believe that is mine. Any other questions?

MR. SIEBER: Thank you.

CHAIRMAN SHACK: All right. Forty minutes behind already. I'd like to propose we take a break for ten minutes and then we'll come back.

(Whereupon, the foregoing matter went off the record at 10:11 a.m. and went back on the record at 10:27 a.m.)

CHAIRMAN SHACK: We can hear about plumes finally.

MEMBER SIEBER: There aren't any. Thank goodness.

(Laughter.)

MR. BESSETTE: Yes, there aren't any.

CHAIRMAN SHACK: And if they are, they don't make any difference anyway.

MR. BESSETTE: Yes. And if they are -- if there aren't any, and if they were they wouldn't make any difference.

(Laughter.)

I'm going to talk about the basic assumptions in the thermal hydraulics analysis, and it's -- first, it's that we've done an adequate number of calculations to resolve the accident space or the spectrum of accidents.

And we have a corresponding level of detail between the thermal hydraulic calculations and the PRA bins, and that RELAP5, which was the basis for all of the analysis, is able to adequately predict downcomer temperature, pressure, and heat transfer coefficient, and that multi-dimensional effects, in particular in the cold leg and downcomer, are adequately represented by RELAP.

I shouldn't say adequately represented, but are not significant to the answer.

MEMBER RANSOM: What about the heat transfer coefficient? Because isn't it what really governs the thermal stress in the wall?

MR. BESSETTE: Well, it's really the heat flux.

MEMBER RANSOM: Well, the heat flux, right.

MR. BESSETTE: And which is a combination of temperature -- fluid temperature and heat transfer coefficient.

MEMBER RANSOM: Right.

MR. BESSETTE: Our starting premise, which has held true throughout the analysis, was that you have these three factors. The most important is temperature and pressure and heat transfer coefficient. So it's not that heat transfer coefficient is inconsequential. Effects can be seen in any results, but that -- we understand the magnitude of these effects, and we've looked at these effects.

MEMBER RANSOM: One thing that I don't recall is why you're able to make these other plots with RTndt as the governing parameter, as far as the material. But then, you know, to relate that to the stress in the wall, which is -- I guess there's an assumed pressure, but also the cue is the other factor, like you mentioned.

MR. BESSETTE: Well, as you know, you have to do -- let's say your thermal hydraulic boundary conditions have to be, in effect, individually deterministic, because it's the whole temperature history or the whole heat flux as a function of time that gives you the temperature distribution in the vessel wall.

MEMBER RANSOM: But the previous plots we saw are sort of generalizations of a lot of transients, and apparently there must be some of these effects that are common.

MR. BESSETTE: I think -- you know, I think one thing we can say is we've covered such a spectrum of transients that we've covered all -- all possibilities that can happen.

MEMBER RANSOM: Okay.

MR. BESSETTE: I wanted to show the PRT that we -- we based -- in effect we based our work on to illustrate a point. First of all, we did a PIRT to try to identify the dominant features of the plant design and the physical models in RELAP.

And this is color-coded, so that the green are items that form part of the RELAP input deck or the RELAP plant model that was used in the analysis. And the blue are the physical models in RELAP, and the red is a combination of boundary condition and physical modeling.

And the interesting thing about when you do this PIRT is that most of the important features of the analysis relate to the input deck, how the plant is modeled. And as well as how the plant is modeled, it's the actual event sequence, the initiating event, and things like tripping the reactor coolant pumps, and so on, and operator actions.

So when -- in the previous slide when we talk about plant behaviors resolved adequately, what we try to do is take these -- this PIRT, and since so many of these things are actually a definition of the event sequence, it is to evaluate these features by an adequate number of individual RELAP calculations.

So, for example, for break location, we looked at breaks in the hot leg and cold leg, the break -- main steam line -- main steam line breaks can be either upstream or downstream of main steam isolation valve.

This is an important aspect, because a break downstream of the valve or outside a containment, reactor coolant pumps don't trip, whereas if the steam line breaks inside containment it generates an isolation signal which would result in a trip of the reactor coolant pumps.

For example -- and this was discussed a little bit earlier -- we did a large number of calculations on hot, full power, repeated them at hot zero power, to look at the effect of decay heat. The pressurizer -- class of events of pressurizer SRV stuck open, which we closed. We basically -- we looked at -- broke that down into they reclose at 3,000 seconds, 6,000 seconds, or never.

And, in addition, in response to a request from Dr. Murley, we did a more complete spectrum of reclosure times to characterize a whole range of possibilities.

And as Donnie was saying, like operator actions, we looked at variations in the timing of HPI throttling, the feedwater isolation, to cover basically the spectrum of possibilities.

And this is a continuation of the PIRT. Again, you can see that most of the features are boundary conditions. We did do sensitivity studies on the wall heat conduction, which I'll talk about today or tomorrow.

This we can't represent in RELAP -- ECC-RCS mixing in the cold legs and downcomer. But we looked quite a bit at experimental data. This look at the effects of thermal stratification in the cold leg and temperature distribution and downcomer we feel -- we have a story on that, which we'll tell you --

MEMBER WALLIS: Well, doesn't RELAP just bring everything to equilibrium in a node? It doesn't have two different temperatures and things. It just brings everything to a --

MR. BESSETTE: That's right. This is a single fluid temperature, a single liquid temperature and a single vapor temperature.

MEMBER WALLIS: So they're not necessarily the same.

MR. BESSETTE: They're not necessarily the same. But you only have one liquid temperature.

MEMBER WALLIS: One liquid temperature.

MR. BESSETTE: Yes. So there's no possibility of representing thermal stratification in the cold leg.

MEMBER WALLIS: There's no possibility of a plume.

MR. BESSETTE: There's no possibility, really, of --

MEMBER ROSEN: Which is plumes are important.

MR. BESSETTE: Yes. So that's why we spent a fair amount of time worrying about do plumes exist, and how large are they.

MEMBER WALLIS: Well, you have these wonderful pictures where you have red dye plumed, which are really spectacular, obviously are there.

MR. BESSETTE: Well, actually, I guess you might say --

MEMBER WALLIS: When you do thermal hydraulic, you do the thermal study, and they don't seem to be there. They're there when you visualize them, but they're not there when you --

MR. BESSETTE: Yes, but I think the thermocouple is more accurate than the eye.

So this speaks to item 1, whether we have adequate resolution of plant behavior. And when we looked at the results, we see that the range of thermal hydraulic conditions in a given bin, as finely as we discretized plant behavior, is large compared to the uncertainty --

MEMBER WALLIS: I was a bit surprised by this factor of 10 range in break size within a bin. The break size doesn't make that much difference, then, so you can bin it?

MR. BESSETTE: Well, I'll get to that. We break -- first of all, we take LOCAs and we break them down into four, say, "uber bins," you know, a small break, medium break, large break, and very small break.

MEMBER WALLIS: That's your factor of 10 range.

MR. BESSETTE: So when I speak of a factor of 10 range in a bin, I'm talking about this "uber bin." And then, we further break down this uber bin into -- I call them sub-bins or bins. So we discretize, let's say, small break LOCAs into five RELAP calculations, and intermediate breaks into three RELAP calculations, and large breaks into one.

And we feel that this is about as finely as it makes sense to break these bins down, because of the -- how accurately you can define the frequency of a small break LOCA. And you can't -- if you have a small break LOCA classified as a break 1.54 inches, it's hard to say, "Well, within that total frequency, this is how the frequencies of a 2-inch break, 2.5-inch," and so on. So I don't think that the PRA knowledge exists to break these bins any finer than we did.

As Donnie said, there was a close relationship between the PRA bin process and the thermal hydraulic uncertainty analysis where we met periodically and had a lot of discussions on what calculations to run.

And in our uncertainty analysis, we looked at both the -- in RELAP space can be broken down into a code input deck, which is defining the boundary condition to the thermal hydraulic problem, and the physical models and numerical solution methods in RELAP itself.

CHAIRMAN SHACK: But does your first bullet imply that you're telling me that the second sub-bullet in your last bullet really is sort of encompassed by the first bullet? Is that the implication?

MR. BESSETTE: Yes. I'm trying to say -- from this bullet, I'm trying to say that this uncertainty range you get from here is small compared to this uncertainty range.

CHAIRMAN SHACK: So you're really only going to sample from the code input model.

MR. BESSETTE: Well, we tried to cover all the bases.

CHAIRMAN SHACK: Oh, you did.

MR. BESSETTE: Yes. In our uncertainty analysis.

DR. NOURBAKHSH: Can you tell -- being that it has the characteristics of plume is more important -- for example, if other loops are -- you have fluid in other loops, there is more possibility of breakage. So have you made a bin that characterized to maximum potential for a strong plume? Then, based on the frequency, we can --

MR. BESSETTE: Yes. Well, it's probably -- I should that defer that to the plume discussion, but --

MEMBER WALLIS: Essentially, I think we're learning that RELAP is surprisingly absolutely accurate compared with all these other variations.

MR. BESSETTE: Yes. Actually, I'm going to get to that in a second. As you say, RELAP is amazingly accurate. This comes from a RELAP agnostic or a CODAC agnostic.

I was surprised when I saw these results. We looked at -- in support of this study, we did 12 integral system test, assessment cases, and we chose sequences or event sequences from ROSA, ROSA-IV, ROSA/AP600, APEX, LOFT, and MIST.

Now, these -- ROSA, APEX, LOFT -- are basically configured to Westinghouse CE designs, and MIST was modeled according to a B&W design.

And we did do some statistical comparisons, just summarizing the assessment results here. And where I use 12 tests, on the average RELAP is within four degrees of the experimental data. And the -- when you talk about an average of a standard deviation, it works out to -- the typical standard deviation is about 10 degrees K.

MEMBER WALLIS: Is this in the final report, this table?

MR. BESSETTE: I'm not sure if it got in there or not.

DR. NOURBAKHSH: You discuss qualitative, Chapter 6 maybe.

MEMBER WALLIS: Because in the final report there's all kinds of comparisons with -- between RELAP and all sorts of experiments. And it didn't seem to be pulled together into where they gave me some sort of a metric on how well RELAP is doing. This seems to be doing that.

MR. BESSETTE: That was the intent, yes.

CHAIRMAN SHACK: Yes. The four-degree number is quoted everywhere.

(Laughter.)

MR. BESSETTE: Well, I guess the bottom line might have been, but --

CHAIRMAN SHACK: Yes. That you see everywhere.

MR. BESSETTE: So that -- this, to me, was amazing when I saw it.

MEMBER DENNING: Help us a little more in the interpretation of this in terms of, is this -- if you look at the temperature transients, is this the maximum difference, or what is -- what is the left-hand column, and then what's the right-hand column on the standard deviation?

MR. BESSETTE: We have maximum and minimum differences, which I didn't present here. This is an average difference over the course of the experiment. So if the experiment runs for 3,000 seconds --

MEMBER WALLIS: That's the average. Because some of these experimental -- it's in Chapter 6 of the final report. There are some really big spikes in the RELAP model, which obviously aren't shown here.

MR. BESSETTE: Yes. Well, the standard deviation is going to capture the -- I mean, you can get a small average by being above half the time or below half the time.

MEMBER WALLIS: That's that an average is.

MR. BESSETTE: Yes.

MEMBER WALLIS: Almost.

MR. BESSETTE: But standard deviation will -- captures how -- in general, how far off are you.

MEMBER WALLIS: But the actual -- the worst deviation may be 100.

MR. BESSETTE: Well --

CHAIRMAN SHACK: Yes.

MR. BESSETTE: -- yes. So this is one sigma, so you --

MEMBER WALLIS: Right. So that is pretty big there, isn't it?

MR. BESSETTE: For this one, within -- at the two signal level, it means 90-some percent of the time you're within 50 degrees K of the experiment.

CHAIRMAN SHACK: Were your time chops of the downcomer temperature sort of calibrated with the penetration depth of the wall? I mean, so that any spike within this thing that I missed really wouldn't affect the overall temperature transient very much?

MR. BESSETTE: Well, most of these comparisons are fairly -- these are very fine temperature fluctuations, like on the order of one second, don't penetrate sufficiently to --

CHAIRMAN SHACK: Right.

MR. BESSETTE: -- to be a factor. You have to stop worrying about temperature fluctuations of the order of 10 or a couple of tens of seconds.

CHAIRMAN SHACK: Well, that's my question. Is this -- were these histories that you derived these from fine enough to capture all of that? I mean, you didn't do it every second, but did you do it frequently enough to capture everything that would be of interest to the wall?

MR. BESSETTE: Well, I think the way we did it -- you know, it -- typically, in the experiments you have recording frequencies of about 1 Hertz or so. And so we would have done it on that frequency.

MEMBER WALLIS: But if you look at the ROSA data, the biggest numbers you get in a standard deviation there are for ROSA. ROSA data showed a downward spike in the temperature in the data. So there's something real there in terms of a quenching of the wall in ROSA.

MR. BESSETTE: Well, it -- yes, see, some of these experiments, in particular ROSA, include these like bifurcations -- bifurcating events, which is like the opening of the automatic depressurization system. And so if RELAP -- and the timing of the opening of the ADS is key to the level in the core makeup tank.

And so if you're off a little bit on timing, you'll get a big error in your calculation. And also, you have -- you know, an opening of ADS valve causes a dramatic change in the event sequence, where you can get sudden changes in temperature.

MEMBER RANSOM: Are these data all for

prototypical initial temperatures and injection temperatures?

MR. BESSETTE: Pretty much. LOFT, MIST, and ROSA start from prototypic initial conditions. APEX is somewhat reduced. It starts at about 400 degrees Fahrenheit instead of 550.

MEMBER RANSOM: Well, wouldn't it be better to use a non-dimensional temperature and make a comparison on that basis rather than absolute temperatures?

MR. BESSETTE: In the end, yes. But since, you know, I considered APEX was sufficiently close to these others or that -- it really wasn't worth the additional complication or simplification, particularly when you look at it.

MEMBER WALLIS: Well, also, what was missing from the discussion in Chapter 6 was there are all kinds of data shown. There's MIT pressurizer and Semi-Scale, UPTF, and so what does this have to do with the scenarios of real interest for PTS?

MR. BESSETTE: That's one of the missing links.

MEMBER WALLIS: It is.

MR. BESSETTE: The separate effects cases were chosen to explore what we felt were the most significant physical modeling features.

MEMBER WALLIS: Well, we know that RELAP does a pretty good job on lots of things. The real question is: how good is it for the kinds of scenarios which are most important for PTS?

MR. BESSETTE: Yes.

MEMBER WALLIS: It's not clear that this kind of a matrix or table covers that at all. Are these LOFT tests relevant at all to PTS?

MR. BESSETTE: Well, that's why this particular list was chosen from the --

MEMBER WALLIS: Because it's not relevant?

MR. BESSETTE: No, to be of most relevance. These were chosen as representative scenarios --

MEMBER WALLIS: Can there be some output in the report, this connection between these scenarios and the PTS scenarios?

MR. BESSETTE: It can be in it. It will be.

MEMBER WALLIS: But the MIT pressurizer test has nothing to do with PTS.

MR. BESSETTE: Well, it does -- it does in the sense that you have this class of events that involve repressurization. And what you want to know

-- is RELAP doing a reasonable job under repressurization conditions? Which is what the MIT pressurizer test gives you.

MEMBER WALLIS: So could this be spelled out in the final report? This is the question we're asking, and this is the sort of degree of effect that we need in order to answer this question, and, yes, we've got it, or whatever?

MEMBER SIEBER: Yes. But this is just a demonstration that RELAP5 can model certain transients.

MEMBER WALLIS: Oh, yes.

MEMBER SIEBER: Okay. Well, it's nice to put it down in your report.

MEMBER WALLIS: It may model 99 percent of all of these transients, but the one which is most critical for PTS, it may not model well at all.

MEMBER SIEBER: Yes, and you may not be able to determine it from the series of tests.

MEMBER WALLIS: Unless they cover somehow the typical scenario that leads to a PTS.

MEMBER SIEBER: Well, one would hope there's some continuity from one test to another.

MEMBER DENNING: What about scaling questions here, too? Most of these are clearly much smaller than the real system, which would affect things like plumes and stuff like that. Is there some discussion of that?

MR. BESSETTE: Well, I'll get into that. I think the most important scaling factor in terms of these integral system tests from the perspective of PTS is a power-to-volume scaling. And that was one of the basic principles used in all of these facilities. This power-to-volume scaling gives you the right energy inventory behavior.

MEMBER SIEBER: Okay.

MEMBER WALLIS: Does that necessarily model how far a plume penetrates?

MR. BESSETTE: No, that's a separate issue. And there you have to look at all of the available data, and I'll get into that later.

MEMBER WALLIS: You'll get into --

MR. BESSETTE: It's probably best to --

MEMBER WALLIS: Is there a theory of plumes which is used, or is it just looking at data?

MR. BESSETTE: Well, we started off looking at the theory of plumes and then decided that what we were dealing with was not decay of plumes. It was something quite different.

MEMBER WALLIS: Are you going to get into that?

MR. BESSETTE: Yes. Okay. So this is a similar result with the same set of experiments, now looking at the pressure statistics. And, again, the comparison is -- absolute comparison is quite good within RELAP -- is within 10 psi of the data, which is --

MEMBER WALLIS: Just follows the whole system pressure, doesn't it?

MR. BESSETTE: Yes, within -- it's an absolute comparison. So within the context of system pressure it's -- the difference is trivial.

DR. NOURBAKHSH: UPTF is here as far as pressure constant, but for temperature you didn't show it -- the previous slide. UPTF is missing as far as temperature.

MR. BESSETTE: Yes. This is --

DR. NOURBAKHSH: UPTF is relevant to --

MR. BESSETTE: This UPTF test is a condensation test. I don't know really -- it was -- it was intended to be run as kind of a steady-state, but it ended up being a -- kind of a transient. But basically what we're looking for is to try to see if -- how well RELAP was doing, but condensation during ECC injection gives us an important factor in determining downcomer temperature.

So the bottom line is RELAP compared well to the experiments, and basically the reasons are that pressure and temperature are global parameters representing basically the energy of the reactor coolant system. And RELAP5 -- the code itself is based on conservation of mass and energy, solution to the conservation equations. And that what this says is that you can look upon your reactor coolant system as a control volume problem.

MEMBER WALLIS: There's no momentum in there. When you start putting momentum flux in the downcomer, you get weird and wonderful behavior.

MR. BESSETTE: Yes. So far we're only talking about conservation of mass and energy. We'll get to momentum later.

And so, basically, as a basic thermal hydraulic control volume problem, it's characterized by its initial condition and then its boundary conditions. And the point I made before is that integral system test facilities are directly instructive, because they're based on power-to-volume scaling.

Now we get to the heat transfer coefficient, and the issue here of course was the possible underprediction in RELAP since it did not model buoyancy opposed mixed convection conditions that you get in a downcomer, which is based -- you have an annulus with heated walls on both sides and a colder fluid moving downward past the heated walls.

And in those conditions, you expect an enhancement to heat transfer -- to, let's say, the heat transfer you get from an ordinary forced convection model, which is what RELAP had. The base case RELAP includes Dittus-Boelter for turbulent forced convention and Churchill-Chu for free convection.

MEMBER WALLIS: I would think that you might get a stagnation point where the hot plume rises up the wall and the cold fluid comes down, and at some point they balance each other and the fluid comes off the wall.

MR. BESSETTE: You get these instabilities, yes.

MEMBER WALLIS: Well, there might be some region where those aren't --

MR. BESSETTE: I think what you find is that --

MEMBER WALLIS: -- neither natural convection nor forced convection is happening. One is actually stopping the other.

MR. BESSETTE: Yes. I think basically the down flow wins out over these boundary jets.

MEMBER RANSOM: Well, one thing I would think you'd want to try to quantify to some degree would be local effects. You know, the RELAP5 models are basically fully developed heat transfer coefficient models for both natural convection and forced convection.

And I guess you'd worry that you might somewhere have an interaction between two flows into the downcomer that may create a local scrubbing effect and higher turbulence and higher heat transfer coefficient. And I'm wondering how big that variation might be, or maybe we'll see that you've taken that into account some way.

And most of the experiments that you show, of course, they don't measure enough heat transfer information to ever reveal these kinds of things.

MR. BESSETTE: Yes. Well, I think the first -- of course, the first thing is you wanted to know if we got the average temperature right, which I think we can --

MEMBER RANSOM: Right.

MR. BESSETTE: -- we've demonstrated that we did. And then, the second thing then is to know whether -- how non-uniform are the conditions in a downcomer?

MEMBER RANSOM: Right.

MR. BESSETTE: So this is what's -- the basic models in RELAP that get applied to the downcomer during these PTS transients are a combination of Dittus-Boelter and Churchill-Chu, and RELAP takes the -- calculates heat transfer both ways and takes the higher of the two.

So under natural circulation or flow stagnation conditions, Churchill-Chu gives a higher value of heat transfer than Dittus-Boelter, and so that's what gets applied. We had this -- of course, the suggestion was that we -- of course, that we ought to look at mixed convection, and so we implemented -- what we did is we implemented the Petukhov -- test my pronunciation -- Gnielinski -- Gnielinski, is that right?

MEMBER WALLIS: And what is this for? This is for mixed --

MR. BESSETTE: This is -- so Petukhov-Gnielinski is pretty similar to Dittus-Boelter. It has some slight corrections on it, but we did hand calculations and we did calculations as implemented in

RELAP. And it gives results pretty close to Dittus-Boelter over the range of --

MEMBER WALLIS: So Churchill-Chu is for flow going up the wall, and Dittus-Boelter is for flow coming down the wall. It seems to me rather strange that you don't try to model what really happens by using fluent or something, where the flow is coming down on the outside but maybe going up near the wall.

MR. BESSETTE: Yes. But Churchill-Chu actually seems to be surprisingly -- well, actually, it's fairly --

MEMBER WALLIS: These are then compared with APEX or something, are they?

MR. BESSETTE: Well, what we did is we compared it against -- what we did is we compared it to the -- what Swanson and Catton did, you might know why we did this particular comparison -- was they ran some experiments back in the late '80s and looked at annular geometry. And they suggested that the use of the multiplier, rather than doing a free convection type of correlation, they -- they suggested using a multiplier on Petukhov, which is this equation here.

So we implemented a combination, and they related it to a multiplier. Their multiplier is -- so this term here is this one here. And so this is their multiplier, and we implemented this in RELAP, and we did a number of calculations.

MEMBER KRESS: These heat transfer coefficients are assumed to be, in effect, 360 degrees around the vessel, right?

MR. BESSETTE: Yes.

MEMBER KRESS: And to be substantially more important at the midline, the baseline, or the midpoint of the vessel where the wells are, where the high fluence is.

MR. BESSETTE: Yes. I mean, we're really only worried about the region of the vessel adjacent to the core.

MEMBER KRESS: Which is almost a region of well-developed flow prior to the L over D annulus. I'm trying to get to a state where I can say, okay, it's a well-developed flow --

MR. BESSETTE: Oh, I see.

MEMBER KRESS: -- and you're being a bit conservative, because you're applying it only around the vessel.

MR. BESSETTE: Yes. Well, I think -- well, I'll get to that. I think -- I don't know if we -- if we ever -- at what point we get the fully developed flow at the downcomer. In fact, I think the flow is sufficiently complex where it -- and varying with time, but fully developed is an approximation.

MEMBER KRESS: But there are a lot of L over D's.

MR. BESSETTE: Yes. Oh, yes. This -- in terms of the -- that this -- well, in other words, whether we have enough to get the fully developed flow -- it's certainly several L over D at least.

MEMBER DENNING: I think the problem with that argument, Tom, is that we don't know what's going around azimuthal perhaps.

MEMBER WALLIS: It's a very short L over D azimuthally. It's going around. It's very squat. So it's never fully developed azimuthally.

MR. BESSETTE: Well, going around -- actually, you could probably get more L over D's going around --

MEMBER WALLIS: Are you going to tell us you get stratification, is that what's going to make everything uniform in the downcomer?

MR. BESSETTE: That we don't get stratification.

MEMBER WALLIS: Don't get stratification.

MR. BESSETTE: That we have fairly uniform downcomer temperatures.

MEMBER WALLIS: Now, aren't these heat transfer coefficients so big that it doesn't matter anyway?

MEMBER KRESS: Yes. It's the penetration in the wall that governs that seems to --

MR. BESSETTE: Well, that's one of the issues we looked at, because, of course, going back to 1980 or so, people have looked at the BO number in this situation and decided that is conduction control. But along the way we've gotten the results that popped up which show some sensitivity to heat transfer coefficient, more than you might expect when you look at the BO number.

And so the reason for that was sort of what was coming up a little bit earlier, is that the flaws when you do the FAVOR analysis or the analysis that was done in the 1980s -- I forget the name of the fracture code then -- the flaws that cause the vessel to fail are located near the inner surface, in the first inch or less.

And so when you do a BO number analysis, of course, you have to choose a length term when you do the BO number analysis. And if you choose one inch, let's say, or -- instead of the whole vessel wall thickness, you get a much different result which shows that you're no longer conduction controlled.

So we had this -- we had -- we're dealing with a potential non-conservatism in the heat transfer.

MEMBER WALLIS: Are you going to explain why you have now a good heat transfer coefficient rather than just the fact that there are four theories?

(Laughter.)

MR. BESSETTE: Well, you know, as I said, by itself Petukhov-Gnielinski gives results that are similar to Dittus-Boelter. And references I've looked at say that for the conditions for which they are developed they have accuracy, good accuracy, and --

MEMBER WALLIS: Petukhov is a Russian reference? It doesn't have any kind of NRC quality control or anything, and yet you believe it?

MR. BESSETTE: Well, I mean, it's -- there's been comparisons with data that showed good agreement, and 90 percent of that data is within plus or minus 20 percent.

CHAIRMAN SHACK: So they both agree when they're tested under the appropriate conditions, but, again, are the conditions which you need here.

MR. BESSETTE: Well, that's where Swanson and Catton come in, because they -- they developed their correlation based on the experiments they ran, which were the appropriate conditions. And so they apply a multiplier to -- to Petukhov, and which is what we used.

MEMBER WALLIS: Is your Petukhov right? It looks very, very strange. Is the number proportional to the Reynolds number? Is that -- I guess it could be, because of the CF over 2. I guess it would --

MR. BESSETTE: It's basically the same formulation. It's just a little bit added term. They're all --

MEMBER RANSOM: Well, they still have a friction coefficient apparently. I don't know if you can have applied friction correlation or --

MR. BESSETTE: Well, it's based on -- yes, well, you have to calculate the Reynolds number with RELAP.

MEMBER RANSOM: But then you have to get an actual C sub F.

MR. BESSETTE: Oh. Yes, that's calculated through RELAP.

MEMBER DENNING: Does the fact that the correction factor makes the difference which is -- has a Grashoff number in it, it implies that there is some sort of recirculation that's going on in that annulus that's of significance, a natural convection-driven circulation added on to the general downflow?

MR. BESSETTE: Well, I think it's a little bit -- it deals with it more locally than that. It deals with the -- the fact that you have these wall boundaries, these buoyant wall boundaries, which are counter to the predominant flow, which was downwards, and that increases the -- basically, the turbulence, the local turbulence, and, therefore, it gives you more heat transfer. On top of that you may have large-scale flows, too.

MEMBER RANSOM: That's kind of a strange correlation, though. It has the Grashoff number times the Reynolds number. If you had stagnant flow, there would be no natural convection, which is counter to intuition.

MR. BESSETTE: Well, there's kind of a Grashoff over Reynolds squared that -- basis that Catton used as kind of determining what -- how much of your total behavior is, you know, buoyancy controlled versus bulk flow controlled.

MEMBER WALLIS: Well, Petukhov just looks like a Reynolds analogy. That's all it is. Why don't we move on.

MR. BESSETTE: What we did, we applied this new heat transfer model to -- based on Palisades. We chose the 12 risk-dominant transients for Palisades, and we ran sensitivity studies with the default heat transfer, which is Dittus-Boelter, Churchill-Chu, and with Petukhov -- I call it the Petukhov-Catton model.

And then, in addition, we applied on top of that to cover residual uncertainty -- well, we applied multipliers of .7 and 1.3 to the values obtained using Petukhov-Catton.

MEMBER WALLIS: But this Petukhov is for flow in a pipe, isn't it?

MR. BESSETTE: Yes.

MEMBER WALLIS: So what has it got to do with the downcomer?

MR. BESSETTE: Well, that's the Swanson-Catton. I mean, the Swanson-Catton correlation was determined from the --

MEMBER WALLIS: That's the only one that's related to downcomers, right?

MR. BESSETTE: Yes, determined from the downcomer experiments they ran. So it's an enhancement over pipe flow.

These are the 12 cases -- I told you we ran 12 -- the 12 Palisades risk-dominant sequences, and these are the 12 cases that we ran. There was a range of --

MEMBER WALLIS: Did you check -- did you run them to -- use them to predict some APEX results or something? Why did you sort of validate the method?

MR. BESSETTE: Validate the models, do you mean, the heat transfer models or --

MEMBER WALLIS: Well, you ran RELAP and all these things. Didn't you run them against some experiment at APEX or something to see which of these things you show on slide 13 worked? Or you just ran them?

MR. BESSETTE: Well, they both work in this. I mean, the reason we know they work is that we -- we know that in terms of the fluid temperature, the heat transfer from the wall to the fluid does not have a strong effect.

MEMBER WALLIS: But the Reynolds number is just the flow rate averaged over the whole downcomer, is that what it's based on, the velocity?

MR. BESSETTE: It is determined by a velocity and the hydraulic diameter.

MEMBER WALLIS: So it's a mean velocity over the whole downcomer.

MR. BESSETTE: Well, it's determined in each node, but --

MEMBER WALLIS: But it's a one-dimensional node.

MR. BESSETTE: Yes. But you do -- you still have a hydraulic diameter of RELAP.

MEMBER RANSOM: How was the downcomer modeled for these transients, just one single pipe?

MR. BESSETTE: No. It's six channels and about 10 axial elevations.

MEMBER RANSOM: Were they cross-linked, then?

MR. BESSETTE: Yes, it's a --

MEMBER WALLIS: So it's a 2D model.

MEMBER RANSOM: So it does give you sort of a 2D --

MEMBER WALLIS: It's a 2D model? I couldn't figure out from the report whether you had a 2 or 1D model of the downcomer. Sometimes it seems to be 1; sometimes the other.

MR. BESSETTE: Well, we did those kind of sensitivities, too.

Bill, I can't -- I'm not entirely sure.

Did we use a 1D downcomer for this, or 2D? 2D. So we used a 2D model, base -- the basic model. But as you can see, we ran a range of these. These 12 dominant cases in Palisades include a number of different sequences -- the stuck-open valves on the secondary side, stuck-open valve on the primary side, main steam line break, and a spectrum of LOCAs.

MEMBER WALLIS: You did them all with these different models?

MR. BESSETTE: Yes. We did them all with the different models.

We checked Petukhov-Gnielinski -- or I'll call it Petukhov-Catton for simplicity -- against the heat transfer predicted by the base case RELAP. And overall it increases heat transfer by about 20 percent, heat transfer coefficient by about 20 percent.

So we checked that both through some spot checks, hand calculations, but also as implemented in RELAP.

MEMBER WALLIS: Suppose the heat transfer coefficient is infinite. What does it do?

MR. BESSETTE: Eventually -- well, it has -- of course, like I say, it has some effect on the probability of vessel failure. The probability of vessel failure -- the tendency is to go up as heat transfer increases.

MEMBER WALLIS: Well, it must level off at some point.

MR. BESSETTE: You reach an asymptotic limit, and we looked at that in the past. Eventually, you reach an asymptotic limit.

MEMBER WALLIS: You need to convince us that you're close enough to that already, and you're not going to be too concerned about the heat transfer coefficient.

MR. BESSETTE: What we can do is show you the sensitivity.

So Petukhov-Catton, we've got an increase in CPF by a factor of 3.2 over base case RELAP.

MEMBER WALLIS: Are you going to tell us what the heat transfer coefficient is typically?

MR. BESSETTE: Well, of course it has a range. It starts off at about 25- to 30,000 watts per square meter degrees C when the pumps are on. And then, under natural circulation it drops down to about in the range of 2,500 or so watts per meter degrees C. And then, under flow and stagnation conditions it's in the range of 1,000 to 2,500.

So this gives you an idea of the -- and

then, on top of that, we applied factors of .7 to 1.3 on heat transfer, and we got changes in CPF of .3 and 2, respectively.

MEMBER WALLIS: You used those multipliers because you had some idea that that's how accurate it is?

MR. BESSETTE: Yes. I mean, based --

MEMBER WALLIS: You could have applied numbers -- factors of .5, whatever.

MR. BESSETTE: Well, looking in the literature, a number like 1.2 or 20 percent uncertainty is -- is what's often quoted.

MEMBER WALLIS: Well, that's for when you've got a lot of data, like pipes. And for downcomers you've got very little data.

MR. BESSETTE: Yes. So we used -- instead of 20 percent, we used 30 percent.

So this is -- the first bullet here is temperature and pressure are determined from conservation of mass and energy, and these are global parameters.

Even under flow stagnation conditions, there's still a fair amount of flow present in the system. It just means you no longer have loop flow, but you still have flows driven by the break, by ECC injection, by in-vessel natural circulation processes where you've got mixing occurring at the downcomer, and so these -- the fact that you still have these -- a lot of flows being driven by natural processes precludes pronounced variations in temperature and --

MEMBER WALLIS: You don't get any boiling on the surface of the downcomer?

MR. BESSETTE: Yes.

MEMBER WALLIS: You do.

MR. BESSETTE: We do. Well, we know what RELAP tells us, because these -- like Dittus-Boelter, and so on, they're for -- they're not -- they're for convection processes, not like nuclear boiling processes. So we checked that for these various transients, and typically you find we're in convection rather than boiling in a downcomer.

MEMBER WALLIS: Do you sometimes get it?

MR. BESSETTE: Say again?

MEMBER WALLIS: Do you sometimes get boiling, or you don't?

MR. BESSETTE: Yes. Sometimes we'll get to saturation or nuclear boiling in the downcomer.

MEMBER WALLIS: Then the heat transfer coefficient goes up a lot?

MR. BESSETTE: It goes up a lot, and

you're using a different correlation.

MEMBER WALLIS: Right.

MR. BESSETTE: You no longer have this uncertainty or this proposed uncertainty about mixed convection versus free convection.

MEMBER RANSOM: Are those cases generally when the pressure is dropped, I assume?

MR. BESSETTE: Yes. You tend to see it more for larger break LOCAs when the whole system pressure and energy are coming down so fast. You tend to stay closer to --

MEMBER WALLIS: Don't you get some subcooled boiling then?

MR. BESSETTE: You can get subcooled boiling sometimes, yes.

MEMBER WALLIS: I would think the worst case would be when you get the pressure going -- shooting down, pouring this cold water, and you get subcooled boiling, which quenches the wall like throwing a piece of hot steel into -- quenching an ingot or something. You actually get boiling on the surface of it.

MEMBER SIEBER: Right.

MEMBER WALLIS: It's the worst case, isn't it?

MEMBER SIEBER: Yes.

MEMBER RANSOM: It's worse from the thermal stress point of view. But by that time, the pressure has dropped, so presumably --

MEMBER WALLIS: Yes, but that's the worst case is when you have the big break and you have the -- essentially the thermal stresses dominating, because the temperature differences are so big.

MR. BESSETTE: Well, I mean, I guess it's -- are you speaking now of like a bubble growth and collapse on the wall or --

MEMBER WALLIS: I just want to see that you've covered the water found, that your analysis includes the cases where there is boiling, and that your RELAP runs put in boiling when there should be boiling and calculate a reasonable heat transfer coefficient. That's all I'm trying to find out.

MR. BESSETTE: Yes. Well, that's -- I don't -- nobody -- I think a couple of factors come into that, of course. You have to know if RELAP is correctly the right bulk fluid conditions and if has the right -- it's one thing to say it has the right subcooled boiling model, which I don't think is in question, but also, is it invoked at the right time? Which is, I think, the more basic question.

MEMBER WALLIS: So RELAP does have these boiling models in it, it has criteria for when boiling happens and when it doesn't.

MR. BESSETTE: Yes. It has -- it has models for the entire, you know, heat transfer regimes from -- you know, everything. It covers -- it has models for the whole spectrum of heat transfer regimes.

MEMBER RANSOM: Saturated boiling and subcooled boiling. I'm sure it covers that entirely.

MR. BESSETTE: It has distinct models for subcooled boiling versus saturated boiling.

MEMBER WALLIS: Well, did any of these experiments that you cited earlier with your table -- was the boiling in any of those experiments?

MR. BESSETTE: There probably was. I didn't look at it in that much detail.

So now that -- item 3 is adequacy of a 1D code for modeling potentially non-uniform fluid temperatures. And what we see in all of the experiments that they showed earlier is that there are large temperature gradients in the cold leg, but there's little temperature variation in the downcomer. And this is from looking at UPTF, LOFT, ROSA, and APEX, the same list of experiments I showed earlier.

So I'll cover these in turn. We looked at -- there's one mixing test run in UPTF, and that was Test 1. And actually -- this actually comprised five individual experiments.

So UPTF is a full-scale test. In this test, they put -- injected HPI water into one of the four cold legs, and the system, the cold -- the rest of the system was filled with stagnant hot water.

Now, UPTF doesn't have all of the steam generators and all of that, but it had the vessel and the cold legs and the hot legs.

Initial system temperature was, you can see here, 456 K, which is 360 F, and it was at a pressure of 260 psi. And the injection was in the cold leg, too, and the injection temperature was 90 degrees Fahrenheit, so you had a delta T of 270 degrees.

They covered the range of injection rates that you might expect from HBI and accumulator. What I'm going to show is one case.

This is -- let's see, showing data from three locations in the downcomer, in the upper downcomer. This is the -- away from the -- this is in the downcomer away from the cold leg that had injection, and this is in the upper downcomer immediately below the cold leg that had the ECC injection.

And these are the RELAP calculations for this experiment at two -- two locations. You know, they had the parallel channels. These are two different channels in the downcomer. So you can see that in RELAP you have a small variation but a -- it falls midway between the upper and lower temperatures you get from UPTF.

MEMBER WALLIS: So somehow the 150-degree difference in the cold leg has become a 20- or 30-degree difference in the downcomer. Is that what has happened?

MR. BESSETTE: That's right. Yes. So you're starting off at 270 degrees delta T, and the maximum plume -- here you do see some evidence of a plume, but the maximum plume strength is about --

MEMBER WALLIS: 30 degrees, right?

MR. BESSETTE: It's about 30 degrees. This is at the top of the core elevation. You can see by the time you get to the bottom part of the mid-core elevation, the plume, such as it is, is disappearing.

MEMBER WALLIS: But there is still some plume, right?

MR. BESSETTE: Yes. But as you might

expect, you're getting a decay -- plume decay.

So it's about 20 degrees K in the upper downcomer, and it's down to about 10 to 15 K in mid-plane. RELAP is falling to between -- which is probably what you would expect of RELAP -- is to predict the average.

I'll show you the results from a LOFT test. This was a four-inch break in the cold leg, and LOFT starts with prototypic initial conditions. Core power in this case was about 50 megawatts. Its whole system pressure and temperature, the ECC injection was 89 degrees Fahrenheit. So we're starting off with 460 degrees delta T -- 480 degrees delta T.

And the reactor was tripped just prior to the opening of the break, and the pumps were tripped when the break was open.

MEMBER SIEBER: Pretty stable.

MR. BESSETTE: Now, this is what's going on in the cold leg. So you're seeing temperature stratification of 100 to 200 degrees K. Initially, it's as much as 200 degrees K, then decreasing it with time. So you're getting a lot of thermal stratification in the cold leg, and --

MEMBER WALLIS: What's all the bouncing due to?

MR. BESSETTE: All this here?

MEMBER WALLIS: Well, RELAP is bouncing, but also the thermocouple is bouncing. Green.

MR. BESSETTE: RELAP is -- well, let's see, RELAP is the red and black.

MEMBER WALLIS: RELAP is presumably that black one. It bounces all over the place there.

MR. BESSETTE: This is -- I think this is when the accumulator comes in. This is a sharp drop.

MEMBER WALLIS: Right. It's a squirt of cold water coming in.

MR. BESSETTE: You're seeing the squirt of cold water, and I suspect this is -- these bounces here are probably due to condensation, particularly down here.

MEMBER WALLIS: Later on it looks like some kind of regular oscillation.

MR. BESSETTE: Yes.

MEMBER WALLIS: Well, I guess we can move on. It's --

MR. BESSETTE: Yes.

MEMBER WALLIS: -- a feature of that picture.

MR. BESSETTE: This shows the temperatures in the downcomer, and this is LOFT at two thermocouple rates in the downcomer. One was near the intact cold leg, and one was near the broken cold leg, and --

MEMBER WALLIS: Wait a minute. That's RELAP, that bottom thing there. There's a RELAP --

MR. BESSETTE: Well, two of these are LOFT thermocouples.

MEMBER WALLIS: But they're the top one.

MR. BESSETTE: The green and the blue --

MEMBER WALLIS: Are LOFT.

MR. BESSETTE: -- are LOFT. And the black and the red are RELAP. And the difference between the two is we ran this both ways, with a 2D downcomer and a 1D downcomer. So, basically, RELAP is getting somewhat lower temperatures in here, if you can imagine this down here.

These are part of the statistics I showed in terms of the accuracy of RELAP for predicting downcomer temperature. This experiment was included. But it shows --

MEMBER WALLIS: What about when it sort of wiggles like this, is this what fed that into the thermal hydraulic analysis for pressurized thermal shock? Are you actually looking at all at these oscillatory temperatures like that?

MR. BESSETTE: Well, they would be if --

if a plant calculation had these same particular phenomena occurring, it would be feeding into these wiggles.

So this one is the upper downcomer, and this is the intact loop, and this is the broken loop. So one of the things that shows is that this, at least in LOFT, is no evidence of a plume.

MEMBER WALLIS: Well, I don't quite know what the green -- what's the green thing?

CHAIRMAN SHACK: The green is the data. There's three RELAP calcs there.

MEMBER WALLIS: That saturation is essentially the --

MR. BESSETTE: Oh, yes. Yes.

MEMBER WALLIS: -- saturation temperature corresponding to the pressure.

MR. BESSETTE: So what this is saying is that the data are at saturation.

MEMBER WALLIS: Right.

MR. BESSETTE: And to look at the comparison of the broken loop and the intact loop --

MEMBER WALLIS: It could be saturation, yes. It could be because it's boiling.

MEMBER SIEBER: The blue and the green.

MEMBER RANSOM: The blue triangles are not

actually a calculation I guess. They're just the
saturation temperature?

MR. BESSETTE: Yes.

MEMBER RANSOM: From the RELAP prior
pressure.

MR. BESSETTE: Yes.

MEMBER WALLIS: Is that because it's
flashing or something, or the data is at saturation?

MR. BESSETTE: Well, what this says is
that it looks like the water in the downcomer, the
saturation, was --

MEMBER SIEBER: That's not unreasonable.

MEMBER RANSOM: Well, I guess the RELAP5
calculation is showing some subcooling, right?

MR. BESSETTE: Yes, it's showing some
subcooling.

MEMBER WALLIS: So what's the bottom line
here? You're showing us that temperatures aren't
going to be very different, that 20 or 30 degrees
doesn't matter? Is that the bottom line?

MR. BESSETTE: Well, I think the bottom
line, you know, since we look at such a -- since our
PTS analysis encompasses such a range of conditions,
the best we can show you is to take a range of
representative experiments and show the comparison

between RELAP and the data, which showed that on average RELAP is very accurate. And then, secondly --

MEMBER WALLIS: Well, it depends what you mean by "accurate." And here it's not very accurate. So you're really telling us that 30 degrees inaccuracy doesn't matter?

MR. BESSETTE: Well, I -- over the scheme of things, you can't focus on one particular inaccuracy and say, well, the worst is always going to happen.

MEMBER WALLIS: Well, you see, maybe what matters is D temperature/D time, in which case RELAP is showing a much bigger quenching D temperature/D time at one point than the data.

MR. BESSETTE: Yes.

MEMBER WALLIS: Does that matter or not matter?

MR. BESSETTE: That's not --

MEMBER RANSOM: Well, there's a lot more to that than you would think, I believe, because the -- I assume those measurements are near the wall. For example, if you're in subcooled boiling, the wall is seeing essentially a saturation condition, whereas the bulk fluid, which is the RELAP5 calculation, is actually somewhat subcooled.

MR. BESSETTE: Another thing is you can't -- you can't say if something matters or not until you run it through FAVOR, because FAVOR is the bottom line. I mean, sometimes you'll see 30 degrees doesn't matter at all when you run it through FAVOR, and sometimes you'll see it makes a difference.

But you don't know -- you can't tell just from looking at thermal hydraulic calculations if something matters or not. I mean, you can get some general -- you get some general ideas, but you don't know how much it matters until you run it through FAVOR.

MEMBER WALLIS: That's why I have trouble with the conclusions of this RELAP part of the report, which says RELAP is good. Now, on what basis is it good?

MR. BESSETTE: Well, it's good as far as we can define it.

MEMBER WALLIS: But is it good enough? What's the -- how good does it have to be?

MEMBER ROSEN: That's almost a philosophical question.

MEMBER WALLIS: No, no, that's the key question. That's an engineering question always: is it good enough?

CHAIRMAN SHACK: Well, doesn't that go back to your argument that the change you get from the boundary conditions sort of covers this whole range of histories that you're getting?

MR. BESSETTE: That's right. Since we covered the whole --

MEMBER WALLIS: The whole claim?

MR. BESSETTE: Since we covered the whole map, we -- we had to have found the worst thing that can happen, because we've covered everything you can think of.

CHAIRMAN SHACK: But I guess the other thing from that graph is, you know, the fact that it really doesn't seem to make any difference which side of the loop you're on, I mean, whether you're under the --

MR. BESSETTE: That's the other point. What I'm trying to show in these experiments is that from the experiments we look at we don't see -- the worst -- the worst plume we see is UPTF, which was about 20 degrees K. And we'll show you later on that doesn't matter again with a sensitivity study.

MEMBER RANSOM: Well, from a PTS point of view, what part of that transient is most important? You know, the early part or the later part?

MR. BESSETTE: Well, the whole -- I mean, the whole thing really is important, because the whole thing gives you the temperature profile through the vessel as a function of time. You have to have the whole transient.

MEMBER RANSOM: How long does it take for that profile to develop?

MR. BESSETTE: But when I say that, within that whole scheme, obviously something -- when you see something like this, that's potentially important when you run it through FAVOR, because it's a sharp -- it's a large, sharp drop. So we know from -- from looking at a bunch of RELAP analyses and a bunch of corresponding FAVOR analyses, this is probably important in terms of FAVOR.

MEMBER RANSOM: Would you say it's also conservative?

MR. BESSETTE: Well, in this case, obviously, RELAP is conservative, yes.

CHAIRMAN SHACK: I'm going to let you run until lunchtime at noon, but you've still got a lot of slides to get through. So --

MR. BESSETTE: Yes, I've got to go a little faster.

Now we turn to ROSA. And, again, this

appears on that list I showed you earlier. I'm going to show you a test from a one-inch cold leg break.

MEMBER WALLIS: Are you going to get to APEX sometime today?

MR. BESSETTE: Yes, right after ROSA.

In these tests, you had potential for three cold plumes. You had the PRHR, this passive residual heat removal system, feeding cold water through one of the cold legs, and you had direct vessel injection at two locations in the vessel where cold water from the core makeup tanks came directly into the downcomers. You would have no potential for pre-mixing.

I'm going to show you, again, this is the kind of thermal stratification you get in the cold leg as a result of the passive residual heat removal system.

MEMBER WALLIS: That's huge.

MR. BESSETTE: You can see it's quite large, about 100 to 200 K.

This is the PRHR loop. You can see you end up with stratification in the other loop, too. Even though you don't have any injection into this loop, you get backflow from the downcomer into this loop. So despite that large thermal stratification, it doesn't -- in the cold leg, it doesn't show up in the downcomer.

This is -- again, ROSA has two thermocouple stalks --

MEMBER WALLIS: We're just looking at RELAP versus data, right?

MR. BESSETTE: Yes.

MEMBER WALLIS: There's no measurement here of -- I mean, there's almost stratification.

MR. BESSETTE: Well, you're looking at the two -- you're looking at two thermocouple breaks in the downcomer, and the noisier one is the data, and the black one is --

MEMBER WALLIS: Is RELAP.

MR. BESSETTE: -- is RELAP. And here again, the data -- red is data, and black is RELAP. And RELAP is a little bit high, and we think that's due to -- we can trace that back to the modeling in IRWST, get the wrong temperature or too high a temperature.

When we compared the data for the two thermocouple stalks, we see a difference of about 7 degrees K from one side of the downcomer to the other.

MEMBER WALLIS: RELAP is predicting that

because it's 2D RELAP?

MR. BESSETTE: Yes, it's 2D RELAP. Yes.

MEMBER SIEBER: What's the disturbance at the 5,000-second point?

MR. BESSETTE: This is when the IRWST starts to come in, so at this point you're down to containment pressure roughly.

MEMBER SIEBER: Okay.

MR. BESSETTE: And you're getting a different flow rate from the gravity drain of the refueling water storage tank.

MEMBER SIEBER: So the different -- the shift between the temperatures is --

MR. BESSETTE: It might --

MEMBER SIEBER: -- some volume scaling someplace?

MR. BESSETTE: Well, during this part of the transient, pressure is decreasing very slowly. And if you're just a little bit off in RELAP, you can get a significant difference in the -- you can see -- you can end up with a several hundred second difference in the kind of --

MEMBER SIEBER: Right. Okay, thanks.

MEMBER DENNING: Excuse me. Do we believe the -- the thermocouple data, that's a real effect, rather than -- I mean, that's real. Is it noise, or is it --

MR. BESSETTE: Oh.

MEMBER DENNING: -- really responding that rapidly to some really rapid change in temperature?

MR. BESSETTE: Yes. I think what you're seeing is the flow of eddies is kind of going past the thermocouple. So I think this is real -- these are real temperature variations the thermocouple sees.

And let's see, this is at the lower downcomer. Again, this -- you know, generally, you'll see excellent agreement between RELAP and the data and no evidence of plumes.

APEX -- APEX has the best downcomer measurements of the various integral system tests that we looked at. One of the advantages of APEX is it has a very good aspect ratio, so you're getting -- in terms of multi-dimensional mixing effects, you should be doing better.

MEMBER WALLIS: Now, APEX did some salt mixing tests, which were not consistent with the thermal tests. They seem to have been thrown out of the report all together.

MR. BESSETTE: I think so. You know, the original intent of those was just some visual tests.

MEMBER WALLIS: Well, they look very interesting. They showed plumes and everything else and --

MR. BESSETTE: Yes.

MEMBER WALLIS: Now they've been thrown out?

MR. BESSETTE: That's because you didn't like them.

MEMBER WALLIS: I didn't like them. Okay.

(Laughter.)

MR. BESSETTE: So, again, we --

MEMBER WALLIS: Selectively presenting the evidence here, and they're not presenting the salt, because you didn't like it? Or was there something wrong with the tests or --

MEMBER SIEBER: Didn't get the right answer.

MR. BESSETTE: Yes, yes. We seem to be getting too much mixing for some reason. They couldn't interpret them, really, when it came right down to it, with their minimal measurements. Too much uncertainty in interpretation.

Again, you see that the same -- in all these different facilities, you see the same kind of characteristic thermal stratification occurring in the cold leg due to the injection. We're getting about 50 to 150 K, which, given the fact that it starts at a colder temperature is --

MEMBER WALLIS: This temperature difference disappears in the first one diameter or something when it falls out of the cold leg? Because this stuff is cold when it comes out of the cold leg on top of the --

MR. BESSETTE: Well, that's right. This is -- this stuff you see down here is what's flowing toward the downcomer.

MEMBER WALLIS: Oh. It comes out of the cold leg.

MR. BESSETTE: Yes.

MEMBER WALLIS: How does that temperature difference disappear?

MR. BESSETTE: Well --

MEMBER RANSOM: That's top to bottom, is that right?

MR. BESSETTE: Yes.

MEMBER RANSOM: Across the cold leg?

MR. BESSETTE: Yes. This is top to bottom. This is the three-and-a-half-inch pipe, so you're getting this much temperature --

MEMBER WALLIS: What pours out of the cold leg is that cold stuff on the bottom.

MR. BESSETTE: That's right. So what's going on in the downcomer is you're getting a lot of mixing at that change in the -- at the down turn, and on top of that you're not dealing with a free plume. You're dealing with large eddy circulation.

MEMBER RANSOM: In fact, is that some of that at the top of the cold leg actually backflow?

MR. BESSETTE: It could be. It probably is.

MEMBER KRESS: If you --

MR. BESSETTE: Generally, you do see backflow toward the -- when you look at the experiments, you generally see backflow coming from the upper downcomer into the cold leg, and then from the ECC is flowing underneath in the opposite direction toward the downcomer.

MEMBER KRESS: If you assume that flow coming in, or the cold water in the downcomer, instantaneously mixed 360 degrees around, would you get that kind of temperature in the next curve?

MR. BESSETTE: Yes. Well, that's the thing. I've looked at all of the data --

MEMBER KRESS: That's what it looks like to me. It looks like -- it looks like it's just mixing almost instantaneously all the way around the 360 degrees.

MR. BESSETTE: And this is the most persuasive set of experiments for me, because it has the most complete measurement system in the downcomer.

MEMBER WALLIS: That's what puzzles me, because then you have this purple plume which looks very intact. At some point it isn't cold and it mixes instantly --

MR. BESSETTE: Yes.

MEMBER WALLIS: -- as far as the thermocouples go. But the purple plume doesn't seem to mix at all. It comes down --

MR. BESSETTE: Which one is --

MEMBER WALLIS: Figure 1136 is a beautiful, purple plume.

MEMBER KRESS: Which one are you looking at?

MEMBER WALLIS: I'm looking at the APEX -- the APEX report.

MEMBER KRESS: Oh.

MEMBER WALLIS: I just can't reconcile this business of the -- thermally, it's perfectly mixed. But when it's colored water, it doesn't seem to mix at all.

MEMBER KRESS: Well, the question I would have is: if the mixture is by strictly eddies --

MEMBER WALLIS: It'll mix up the color, too.

MEMBER KRESS: -- you're mixing up -- it's going to be the same.

MEMBER WALLIS: It is going to be the same, yes.

MR. BESSETTE: You're referring to this --

MEMBER KRESS: If I transfer some way --

MR. BESSETTE: You're referring to those Finnish experiments?

MEMBER WALLIS: I'm just referring to the APEX report, which is part of the package we got.

MR. BESSETTE: Well, I think -- so one of the things I conclude, because you do see -- well, I'm not sure that I place any faith in colored plumes.

MEMBER WALLIS: But it's the same thing, it's mixing.

MEMBER KRESS: It's only the same if your mixing is by eddies.

MEMBER WALLIS: Well, it is by eddies. There's no mixing by diffusion. That's infinitely slow.

MEMBER KRESS: Yes. But the temperature

may influence the eddies.

MEMBER WALLIS: No way that you can mix the fluid, the temperature --

MEMBER KRESS: The fact that you actually have temperature differences is going to influence the eddies, and you don't really have that influence in the --

MR. BESSETTE: Well, one of the conclusions is that, you know, back in the '80s we ran a lot of experiments in these separate effects mixing experiments, like Creare, and in Finland, and so on, and Purdue. And those experiments -- of course, they were in these -- they were not in full system geometries. They were -- typically had a sector of the downcomer, like Creare had a 90-degree sector of a downcomer unwrapped, so it was a slab.

So they didn't include a lot of the flow processes, which I think you see in these integral system tests. You didn't have typically break -- you didn't have break flow, constant pressure, basically a -- you had a mixing cup environment, which is not to say that's incorrect, but it had -- it didn't have the full integral system test in terms of break flow, in-vessel bypass flow. You didn't have heated cores.

MEMBER WALLIS: So you're saying there's

some large eddies in the downcomer which are stirring things up, keep from getting it well mixed.

MR. BESSETTE: Yes. And you had the additional boundary conditions, because you only had 90 degrees of the downcomer with the wall. You had additional wall boundary conditions that you don't have in the 360-degree geometry.

MEMBER KRESS: I think that's your answer right there.

MEMBER WALLIS: So in that case, this would --

MEMBER KRESS: Your initial conditions of flow in that are --

MR. BESSETTE: So I conclude that the separate effects tests that were done in the '80s have missed some things that we're picking up in integral system tests.

MEMBER WALLIS: With those big eddies stirring up and mixing the fluids, then this is also going to affect the heat transfer, and it's not going to be governed by Dittus-Boelter or Petukhov, or anything. It's going to be governed by these big eddies.

MR. BESSETTE: Yes. Well --

MEMBER WALLIS: So you count it both ways.

You can mix it up very, very quickly and not have that affect the turbulence level, which affects the heat transfer.

MR. BESSETTE: Well, RELAP -- if RELAP is calculating -- under Dittus-Boelter, of course, RELAP is calculating a Reynolds number, which is -- I mean, basically what you have to do is calculate the right velocity to get the right answer.

MEMBER WALLIS: Well, the Reynolds number characterizes the turbulence.

MR. BESSETTE: Yes.

MEMBER WALLIS: And if you've got these big eddies, then it would seem to me they're bigger than the thick -- than the width of the downcomer.

MR. BESSETTE: Yes.

MEMBER WALLIS: So you've got the wrong dimension in there. You should bring the azimuthal dimension in there, so that the width of the downcomer --

MEMBER KRESS: Yes, I think I'd -- rather than do these as eddies, I think we're thinking the flow coming straight down the downcomer everywhere at 360 and going up, but it's not. It's coming in and spiraling around, and coming up, and that's the eddy we're talking about. And that -- that may or may not be much different than the Dittus-Boelter type equation.

MEMBER RANSOM: Well, the other thing is these --

MEMBER KRESS: We're talking about well-developed flow anyway in this --

MEMBER RANSOM: -- cold leg connections here impinging directly on a wall across from the pipe, which undoubtedly you get eddies created from that.

MEMBER KRESS: Yes. And that tends to make you spread out also.

MEMBER RANSOM: Yes.

MR. BESSETTE: But, yes, I've looked at all the APEX data. It all looks like this. I'm going to show --

MEMBER RANSOM: But could I ask you: where are those temperature measurements? That first bullet down there, it's not quite clear. It says at 0, 1.3, 8 cold leg diameters axially. Do you mean down the downcomer wall?

MR. BESSETTE: Yes. Well, you see, 1.3D or 8D, that's -- that's 1.3 cold leg diameters down --

MEMBER RANSOM: Down the downcomer.

MR. BESSETTE: -- and 8 means 8 cold leg

diameters down.

MEMBER KRESS: These are sort of in the middle of the annulus?

MR. BESSETTE: Well, this includes -- unless I -- we let that -- we include thermocouples immediately below each cold leg, and then away from the cold leg --

MEMBER RANSOM: Where are the "away from the cold leg"?

MR. BESSETTE: Let's see.

MEMBER RANSOM: Because those are the ones that I think would show these plumes that you're talking about.

MEMBER WALLIS: Are you going to show us the circumferential variation?

MR. BESSETTE: Right. That's --

MEMBER WALLIS: In the APEX report, there's some nice pictures of the circumferential variation of temperature.

MR. BESSETTE: Yes. Well, I picked these out -- I mean, basically, when you look at all of these -- the tests, and you look at circumferential variation, you see this. And when you look at axial variation, you see this behavior. You just don't -- the maximum non-uniformity I could find was about five degrees K.

MEMBER WALLIS: Yes, five to eight degrees.

MEMBER RANSOM: Well, are the lowest temperatures under the cold legs?

MR. BESSETTE: Yes. In fact, that includes temperatures just this 1.3 diameters below the cold leg. So when you travel down one cold leg diameter, it's already mixed.

MEMBER WALLIS: It doesn't seem to be anything like the usual plume.

MR. BESSETTE: No, that's what I'm saying. It's --

MEMBER WALLIS: What's happening?

MR. BESSETTE: It's nothing like --

MEMBER WALLIS: What's happening? There's something --

MR. BESSETTE: This is not like the plumes we've come to know and love, you know?

MEMBER WALLIS: Something different is happening.

MEMBER KRESS: You'll recall the flow rate of the plume going down is overwhelmed by these other things.

MEMBER WALLIS: What are these other

things, though?

MEMBER KRESS: I think it's spiral flow in the downcomer.

MR. BESSETTE: This is showing -- I can't -- this is showing -- for example, the green is directly under Cold Leg 4 -- under Cold Leg 4. The black is 1.3 diameters down, and 2 diameters away. And I don't know if you can see -- the red is 1.6 diameters down and 1 diameter away.

So we looked at all possible combinations of thermocouples trying to search for plumes and non-uniform effects. I'm just showing a couple of representative cases here. But basically this shows either top of core elevation, plus or minus one and plus or minus two diameters away from this -- in this case Cold Leg 4.

And this is just showing a direct comparison between RELAP and the data, and so it shows on the average we're getting things about right with RELAP.

Now, this is the COMMIX calculation of H.B. Robinson two-inch break. And you can see that generally what COMMIX shows is you're getting the downflow regions -- still, you have downflowing regions beneath cold legs, but upflowing regions in between.

MEMBER WALLIS: So it shows definite plumes there.

MR. BESSETTE: It shows something, but it's -- but I think -- I tried to use this to illustrate the fact that COMMIX seems to support this idea of a large -- basically, on a large-scale basis, these large eddy flows, and then undoubtedly you get smaller eddies if you had more complete velocity in that.

MEMBER RANSOM: Could you tell us a little bit about the nodalization? How many nodes across the downcomer?

MR. BESSETTE: This was seven nodes across the downcomer, and so it's about a 4,000-node downcomer model. And so it's coarse in terms of today's standards.

MEMBER WALLIS: What sort of velocities have you got here compared with the average velocity? You're using Dittus-Boelter based on some average velocity. It seems to be completely wrong, because you've got local velocities here which are far bigger than the average.

MR. BESSETTE: Yes. What we're showing here are velocities of basically something very small, up to about 1 meter a second.

MEMBER KRESS: This is a steady-state calculation after you run the thing for a long time?

MR. BESSETTE: I think so, yes.

MEMBER WALLIS: It's got a big -- I would think those things would wobble around, especially if it --

MR. BESSETTE: Yes. This is a point in time, but, you know, Oregon State ran CFD calculations in some of their experiments, which showed these meandering plumes, and what not.

MEMBER WALLIS: Right. That's what bothered me, too. I saw those pictures with those colored plumes wandering around.

MEMBER ROSEN: Well, you know, figures lie and -- but pictures never do. Is that -- the picture that Graham is --

MR. BESSETTE: Well, thermocouples we know are accurate within one degree Fahrenheit. Color is not so well defined that --

MEMBER WALLIS: Well, see, if you look at that picture there, you've got some cold water coming in and flowing pretty rapidly right down to the bottom, and yet it's never detected on the thermocouple. It's very strange.

MR. BESSETTE: Well, like some of those thermocouples show that when -- when you look at the data, you see -- you do see fluctuations. If you recall the noise, you're seeing fluctuations of maybe 10 degrees.

MEMBER KRESS: Are the thermocouples on the wall itself, near the wall?

MR. BESSETTE: These are normally in the -- these downcomers in these experiments are typically about two inches wide with a thermocouple in the middle.

MEMBER KRESS: Oh, I see. So they're looking at fluid in the middle.

MEMBER WALLIS: So now we've got some bigger plumes, a plume strength of 100 degrees F?

MR. BESSETTE: Well, now that I've shown you all this evidence that plumes are weak or non-existent, I'm going to show you what would happen if we did have plume.

MEMBER WALLIS: Ah, okay.

MR. BESSETTE: This is a study we did -- where we did a plume calculation using REMIX, and that's this middle line. And then we basically doubled and have this plume strength, and we fed that into an early version of FAVOR. We had -- so we had a plume, and we had a nominal ambient that was calculated by RELAP.

So we used -- we imposed this plume strength on top of the nominal RELAP calculation. And we applied it to an area of 30 -- basically 30 percent of the upper circumferential weld. This is a reasonably conservative approximation of, if you did have a plume, how much --

MEMBER SIEBER: Would it cover.

MR. BESSETTE: -- would it cover.

And so what to focus on is -- or Case 1, which is case RELAP; Case 2, which is nominal REMIX plume imposed on the upper weld. And you'll see it's just about the same as Case 1.

Case 5, which we doubled the plume strength, so that's a pretty severe plume compared to what we've been looking at. And you can see maybe 10, 20 percent increase in CPF.

And we did this back around 1997, and this is one of the things that led us to say, well, we've got to keep checking as much as we can upon -- about this plume stuff, but it doesn't seem to effect the result too much.

MEMBER SIEBER: What's Case 4?

MR. BESSETTE: Oh. On top of that, we did some heat transfer sensitivities, which Case 4, which was -- we lowered the heat transfer coefficient I think by a factor of 2, and then Case -- this would be where we doubled the heat transfer coefficient, Case 3.

MEMBER WALLIS: Do you have a factor -- it's a factor of two, or so?

MR. BESSETTE: Yes. In fact --

MEMBER WALLIS: Well, a factor of two on probability of failure is not insignificant.

MR. BESSETTE: No. But it's kind of a similar effect that what we -- what I showed you, these factors of two to three that we found in our more recent calculations, where we varied heat transfer coefficient. So I would say our recent heat transfer studies look to be consistent with these.

MEMBER WALLIS: Did you put in an infinite heat transfer coefficient?

MR. BESSETTE: There's another study where we did that.

MEMBER WALLIS: And you could do that and forget about all this stuff. Just put it in and show that it's conservative and that --

MR. BESSETTE: Well, you could do that, but you keep getting these incremental increases in heat transfer, in CPF when you do that.

MEMBER WALLIS: Yes. But it's still tolerable.

MR. BESSETTE: In fact, well, I think we could define how much of an increase you get, you know, the worst it could possibly be.

MEMBER WALLIS: That would give you an upper bound, which would give everyone a lot of security, instead of having to talk about we don't quite understand the eddies, and we don't understand whether Dittus-Boelter really applies, give us an upper bound.

MR. BESSETTE: I guess that's something we could do is put in the very --

MEMBER WALLIS: It's the first thing you ever do, isn't it, usually, before you do anything else?

MR. BESSETTE: Well --

CHAIRMAN SHACK: You call it your 95th percentile, and sample from it, right?

MR. BESSETTE: So I think I -- on the average, RELAP predicts --

MEMBER WALLIS: See, that's not -- that's not a true statement. It predicts things which are within 20 or 30 degrees, which for the purpose of this analysis is insignificant. It doesn't predict it accurately.

MR. BESSETTE: I accept your words.

(Laughter.)

MEMBER SIEBER: Good move.

MR. BESSETTE: These large -- we consistently see large thermal stratification in the cold leg, but that doesn't translate to non-uniform conditions in the downcomer.

MEMBER WALLIS: We don't know why.

MR. BESSETTE: Well, the -- like I said, I think even when you go back to these facilities like Creare, the plumes in Creare were typically only about 23 degrees Fahrenheit, thereabout. The fact that they don't seem to exist in these integral tests I believe is due to the additional mixing processes that seem to be present in an integral facility compared to these separate effects tests. So I think the results from the -- all of the separate effects tests are conservative.

MEMBER KRESS: That's just another way of saying that the interim tests are --

MR. BESSETTE: Are more --

MEMBER KRESS: -- are not the same as the separate effects tests.

MR. BESSETTE: That's right, yes.

MEMBER KRESS: But you don't explain what these mixing processes are exactly.

MR. BESSETTE: Well, I gave some examples -- the fact that you have break flow, the fact that you have a heated core.

MEMBER KRESS: Yes. But you don't translate those into action, things that would create this non-mixing, or would create this mixing. You need to translate those some way, I think.

MR. BESSETTE: Yes. There's an existing study on UPTF Test 1, which shows that you had to account for the bypass flow from the upper plenum to the downcomer. That has a significant effect on downcomer temperatures. So we know that in-vessel circulation has an effect.

And I'm going to talk about this further on -- when you look at the sensitivity of CPF due to the heat transfer coefficient, you see these factors of two or three. This is still small compared to the variations we get in the -- from the boundary conditions where they've been given a bin, because of the importance of the bulk fluid temperature.

CHAIRMAN SHACK: Can you speed it up?

MR. BESSETTE: I'm done.

CHAIRMAN SHACK: You're done.

(Laughter.)

That's fast.

MEMBER ROSEN: He did all of that in a microsecond after you said it.

CHAIRMAN SHACK: Time for lunch.

MEMBER KRESS: Lunch.

MEMBER ROSEN: Okay. We can all agree on that.

CHAIRMAN SHACK: Back at 1:00.

MEMBER KRESS: Lunch for the bunch.

(Whereupon, at 11:59 a.m., the proceedings in the foregoing matter recessed for lunch.)

CHAIRMAN SHACK: Can we come back into session? Mark, onward.

MR. ERICKSONKIRK: Yes, did Allen discuss with you making Nathan's presentation?

CHAIRMAN SHACK: He's going to follow you.

MR. ERICKSONKIRK: Right. Right now we're going to go through the item on PFM fundamental assumptions. And then I'm going to move directly to PFM changes and methodology.

We've already done thermal hydraulics methodology. In terms of PRA methodology we have one slide that says there have been no methodological changes in the last two years.

So we can skip that presentation. So on with PFM fundamental assumptions. The fundamental assumptions are first and foremost that a linear elastic fracture mechanics model is appropriate for analyzing this problem, that we can ignore the effects of sub-critical crack growth both due to environmental mechanisms and due to fatigue.

And finally, that we can eliminate a priori as a contribution to through wall cracking frequency of certain flaws and transients. And I've just got a few slides on each of these.

The details on these are in section 3.3.3 of NUREG-1806. And they're also a separate chapter in NUREG 1807. So, in terms of LEFM applicability, the first graph here shows the toughness, the aleatory distribution of initiation fracture toughness that we sampled from.

And that's represented by the red, green, and blue line. So we're drawing randomly from toughness values within that. And then what we've over plotted on that is from FAVOR simulations where each little dot represents a crack initiation.

So the point that we're trying to make

here is that the applied K at crack initiation never gets very high. They're all hugging the bottom of the distribution.

And we can translate that in this graph into a --

MEMBER WALLIS: Is that because you have a lot of cracks?

MR. ERICKSONKIRK: That's simply because the driving force can't get that high.

MEMBER WALLIS: It never gets high enough.

MR. ERICKSONKIRK: Yes, the combination of thermal stresses and pressure stresses is never sufficient to get the applied K -- I'm sorry, the combination of stresses and the crack sizes that we sample from is never enough to get the applied K above, you know, like 45, 50 ksi root inch.

So you can use that information on applied K along with material properties to construct what we have on the right hand size, which is a cumulative probability distribution of plastic zone sizes.

And now, of course, this depends upon the yield strength of the material involved. But, looking at the range of yield strengths, both for lightly irradiated materials and heavily irradiated materials, we can say that the plastic zone size ranges from 30 mils to .13 inches, and also, of course, in general that as the plants become more damaged by irradiation, that increases the yield strength, and so therefore will decrease the plastic zoning size.

The general rule of applicability or the general test of applicability of linear elastic fracture mechanics is that the size of the plastic zone should be very small relative to all relevant structural dimensions.

And certainly .03 to .13 inches satisfies that bill with the additional note that as we get out to the conditions that we care the most about, which are the more highly embrittled conditions, the plastic zone size is tending towards the smaller end of that range, rather than the latter.

So, if we have an error in using LEFM, it's made in regions where the yield strength is low and irradiation is low. So we believe LEFM is applicable as a general methodology.

And I've just got a few slides here that show -- and this of course goes back to the late 1980's -- I'm sorry, late 1970's, early 1980's where the NRC sponsored several series of large scale experiments at the Oak Ridge National Laboratory to apply thermal shocks and pressurized thermal shocks to cylindrical vessels.

And obviously whole presentations can be made on this. In fact, we briefed this committee some time ago bringing in Richard Bass and Claud Pugh from the Oak Ridge National Laboratory, and went through this in detail.

But, suffice it to say, we conducted experiments where we applied prototypic thermal shocks and pressurized thermal shocks to vessel materials.

And we found that, using the LEFM technique such as those that had been programmed into FAVOR, allowed us to predict the run, arrest, re initiation, and re-arrest of cracks through thick-walled vessels well.

We find that the toughness values that we would infer from those initiated and arrested cracks agree well with the scatter bounds predicted from small specimen data, which is where we get our aleatory distributions of crack initiation and crack arrest toughness.

And also, these experiments, both thermal shock and pressurized thermal shock, validated the principle of warm pre-stress. And that's all that slide says, again, for the pressurized thermal shock test.

So, we have what I would call scientific proof that you should expect LEFM methodology to work well in general for these type of loading conditions, flaws, vessels, toughnesses.

And we perform mock-up experiments on vessels subjected to thermal shock loadings and found that we predicted the results well using the FAVOR type techniques.

The next major assumption is that sub-critical crack growth is sufficiently small that we can ignore it. And you see this assumption manifest by the fact that the flaw distribution that we sample from was constructed based on data and based on expert opinions on initial fabrication flaws.

We don't attempt to grow those flaws by either a fatigue mechanism or an environmental mechanism. So, that means that when we conduct an analysis at 32 effective full power years, and when we conduct an analysis at, let's just say, a large effective full power years, the flaw distribution we're sampling from is the same.

It's not time dependent. In terms of fatigue, all of the pressurized water reactors now in service were designed to satisfy ASME Section three, fatigue design rules.

Several studies have been conducted recently by the industry which show that neither fatigue initiation nor propagation of fatigue cracks from pre-existing flaws is anticipated over 60 years of nominal operation.

In terms of the non-occurrence or non-significance of environmental sub-critical crack growth, first, of course, you've got a barrier to environmental crack growth of the ferritic steel.

And that's the austenitic stainless steel cladding. That's why it's there. Presuming that you could get a flaw in the austenitic stainless steel that would allow ingress of the reactor vessel environment to the ferritic steel we can note that SCC requires three things to be present: the aggressive environment, which you'd get if you had a flaw in the cladding, a susceptible material, and significant tensile stress.

The low oxygen content to the coolant water during operation keeps the electrochemical potential sufficiently below that of the ferritic RPV steel to generally preclude SCC.

And, during outages, it certainly proved that the oxygen content increases, which would therefore increase the probability of SCC. But the temperature is down.

So, under all likely conditions, I think we can say that we're --

MEMBER ROSEN: It's a good thing Dr. Ford isn't here. That's all I can say.

MR. ERICKSONKIRK: It's a real good thing. And I'd just defer to him, of course.

MEMBER SIEBER: At load temperatures, general corrosion though is taking place.

MR. ERICKSONKIRK: But that's going to require a long period of --

MEMBER SIEBER: Yes.

MEMBER ROSEN: It's on the wrong slide, but that's okay.

MR. ERICKSONKIRK: Maybe Dr. Ford could give me better words to use.

MEMBER SIEBER: He certainly could, a lot of them.

MR. ERICKSONKIRK: I'm sure he could. Okay. And then, just for purposes of computational efficiency, when we're running the FAVOR code, we still calculate many, many more zeroes than we do numbers that are positive.

But we try to eliminate from the analysis calculation zeros, just so that we can, you know, get answers in this century. One of the things we do is that, in FAVOR, we simulate a flaw is equally likely to occur in any position through the vessel wall thickness.

But, because at least the crack initiation is driven by thermal stresses, it's only the cracks that are very close to the inner diameter of the vessel that play any role in crack initiation.

In FAVOR there is a logical gate that says if the flaw is simulated to occur deeper than three eighths of the way into the vessel and three eighths of the thickness, we just pass that and go on.

What we had Terry do was to make this graph, which shows the percentage of flaws that are predicted to initiate plotted versus their location. And what we find out is that by ignoring everything beyond three eighths T we haven't eliminated any significant contributors.

In fact, we can probably back the limit up and still not change the calculated results. The other thing, and this is something Donnie has alluded to before, is based on experience, previous experience performing calculations of this sort, we had decided that if the minimum temperature developed by the transient didn't get below 400 degrees Fahrenheit, there wouldn't be sufficient combined driving force and load toughness to generate any crack initiation probability.

When all was said and done, we went back and we looked at our calculations. And we found out that we could have actually set the limit about 50 degrees Fahrenheit lower and still not eliminated any contribution to through-wall cracking.

MEMBER KRESS: You don't really mean percent axis, do you?

MR. ERICKSONKIRK: Percent wall -- yes, it could be fraction, yes. Okay. So that's the summary of PFM assumptions. And I should say we call these fundamental assumptions because these are the big ones that you make in starting the analysis.

Obviously there are a lot of modeling judgments, all sorts of things that go on. But if you don't buy off on these three or four, we may as well just stop here.

Let's see, going to PFM procedure, what this section is going to do is not go into the procedure in detail because we've already briefed that.

And, indeed, we wrote a report about it which didn't get out. But, to provide a high level overview of the PFM model, show you how it interfaces with the PRA and TH models, and highlight significant changes that have been made to the model since we last briefed you.

And, in most cases, those changes -- or I should say in some cases those changes have resulted from the more significant of the peer group comments.

So we're also going to highlight those. So this just shows the overall PFM model. We take the input from PRA, gives us the event sequence, RELAP then tells us the pressure temperature and heat transfer coefficient variation.

That's an input into the crack initiation model as well as what the distribution flaws is, what the fluence loading on the inside of the vessel is.

All that goes into crack initiation model. The crack initiation model predicts the probability that a crack will initiate given this loading, these flaws, this fluence loading, and also it should have the material and composition information.

That initiation probability then goes through the through-wall cracking model, which assesses the probability that that now initiated crack can make it all the way through the vessel wall.

MEMBER ROSEN: Is there some significance to why you did this backwards?

MR. ERICKSONKIRK: Backwards? Oh, you mean going left to right rather than right to left?

MEMBER ROSEN: As Hebrew is written, for instance.

MR. ERICKSONKIRK: I must have woken up on the wrong side of the bed.

MEMBER ROSEN: Oh, okay.

MR. ERICKSONKIRK: No, there's no significance.

MEMBER SIEBER: Turn it upside down.

MR. ERICKSONKIRK: Okay. I'll turn it around. Okay, so now what I'm going to do is have one slide each on the four major sub-models in the PFM model.

So, with regards to the flow distribution, there have been no changes since we briefed you last. Just to say a few things, relative to the flow model that was used in the old calculations, both in SECY-82-465 and in the IPTS studies, this flow distribution has many, many more flaws than we had before.

Those flaws are generally smaller, although not entirely so. The big difference is that the huge majority of these flaws are buried, rather than being on the surface.

And we believe that's justified based on both physical and empirical observations. And also, another important factor is that all the weld and the cladding flaws have orientations, I'm sorry, that are tied to the welding direction.

We view the flaw distribution as being either an appropriate or a conservative representation of the flaws in any PWR for a number. Obviously we can't justify that on an empirical basis, that's absurd.

But, based on the support of physical models and their incorporation into the flaw model, and by the fact that as we constructed the flaw model, obviously you go through and you don't know certain things.

Every time we had to make a judgment, that judgment was made systematically in a conservative way. And the one big one I'll point up is that all NDE indications were treated as flaws, whereas many of the NDE indications are, of course, volumetric and therefore not deleterious to the vessel.

With regards to the nucleonics model, again, no changes since 12/02. We estimate the ID fluence per Reg Guide 1.190 procedures. And then that irradiation damage is then attenuated through the wall using Reg Guide 1.99 procedures.

And that will be called out later as an implicit conservatism. We have had changes in the crack initiation model. I'll say what those are. But just the significant features of the crack initiation model is that the conservative bias in RTNDT is removed on average.

The material uncertainty modeled is conservative relative to any plant specific variability, which is to say that when we constructed our distributions that we sample from on unirradiated transition temperature, copper, nickel, phosphorous.

All of those distributions that we sample from were based on large populations of material and different heats of material. So, unquestionably, the uncertainty that would be characteristic of any plant specific analysis would be smaller.

We've modeled the aleatory uncertainty in initiation fracture resistance. We have had a bug fix since 2002 that came out of the FAVOR V&V process that had to do with an improper allocation of weld or plate properties to flaws located on the fusion line.

So that's something that came out of V&V that was fixed. That didn't have any numerical effect on the results of Palisades or Oconee, but it had a big numerical effect on the results at Beaver Valley because, of course, they have the highly embrittled plates.

Since then we've also implemented temperature-dependent thermoelastic properties rather than using valene values. Based on one of the results from -- I'm sorry, one of the comments from our peer reviewers, Dr. Schultz, we realized somewhat embarrassingly that we had not modeled the effective crack-face pressure.

And so we put that in. That, however, turned out to have a small effect. But it was important to have it in just for the sake of completeness.

And this is not new since 2002, but we've accounted for the effects of warm pre-stress. Moving on to the through-wall cracking model, we've modeled the effect of embrittlement on the separation of the arrest and initiation toughness curves.

In the previous calculations, meaning SECY-82-465 error, the initiation and arrest transition fracture curves were assumed to have the same temperature separation independent of the level of material damage.

And that was an assumption that didn't

agree at all well with either physical understanding or published data. We've modeled the aleatory uncertainty in arrest fracture resistance.

We've allowed the arrest fracture toughness to exceed 200 ksi root inch. And that's premised on -- I'll show you the graph on that. That's based on data from wide plate experiments, thermal shock experiments, pressurized thermal shock experiments.

And that's new since 2002. We've modeled through-wall material property gradients and we've also now allowed for the possibility of failure of the vessel in a ductile mode on the upper shelf.

And that's also new since 2002 and comes out of one of our peer reviewer comments from Dr. VanWalle.

MEMBER SIEBER: A quick question on the Beaver Valley vessel. You said that the plate is highly embrittled at Beaver Valley.

MR. ERICKSONKIRK: The plate is more embrittled than the welds, yes.

MEMBER SIEBER: Well, my understanding of the major problem with Beaver Valley is that they used copper clad welding rod, so the copper content is higher than most plants.

MR. ERICKSONKIRK: Yes.

MEMBER SIEBER: But that wouldn't affect the plate, that affects the weld.

MR. ERICKSONKIRK: That affects the weld. And I might defer to Bruce, but my -- certainly the data that's in orbit shows that the plates are also high copper. Isn't that correct Bruce?

(No verbal response.)

MR. ERICKSONKIRK: Yes.

MEMBER SIEBER: Okay, for the record, he answers yes.

MR. ERICKSONKIRK: Yes.

MEMBER SIEBER: Okay.

MR. ERICKSONKIRK: So what I was -- I already said this. Two of these changes were motivated by comments made from the review group, that being the inclusion of crack-face pressure, which, as I said, was important to include for the sake of completeness, but didn't really change the results because the only transients -- didn't change the results significantly -- because the only transients where pressure is an issue is, of course, the stuck-open valves.

And there was already enough pressure in the model. There was already enough stresses generated by pressure in the vessel wall to cause those cracks, once initiated, to go through virtually 100 percent of the time.

So that didn't really make a major difference. But we have also included the possibility of failure on the upper shelf, which can be anything.

I just wanted to show you some of the new aspects. One is that, before in our previous -- in the FAVOR calculations that we reported to you previously and indeed in all previous probabilistic studies done in the United States, the arrest toughness was capped at 200 ksi square root inch due to -- initially -- due to lack of data above that showing that the arrest transition curve went up higher.

In the 1970's and 1980's the NRC did a number of wide plate tests, pressurized thermal shock tests, thermal shock tests to generate data in that regime.

However, that data was never cycled back for use in the PFM model. So we've done that here. And now what this says is that as the vessel, as a crack is propagating through-wall, you actually can generate stable arrest at applied K's above 200 ksi root inch.

But what you also find out happening, and this was -- these two things were actually linked. This was the reason why we needed an upper shelf model, is this graph now shows the transition fracture toughness behavior of a typical RPV steel, both before irradiation and after irradiation, and then the variation of upper shelf toughness.

And what we find out is that -- this is again fairly typical -- is that on the upper shelf -- I'll go to this slide -- on the upper shelf, over the range of temperatures of interest or reactive service, 200 ksi root inch represents, if anything, an upper bound to the toughness distribution, not a lower bound.

So, by allowing crack arrest at higher applied K's, it was also incumbent upon us to calculate the possibility of ductile tearing and subsequent vessel failure on the upper shelf.

Now I'll defer to the Chairman on this one. I have a few more slides describing some of the basics of the upper shelf model because it's new.

We can go through that or we can just skip on through to --

CHAIRMAN SHACK: No, I think we aught to, because that is one of the major changes since the last time --

MR. ERICKSONKIRK: Okay.

CHAIRMAN SHACK: -- we've been here.

MEMBER SIEBER: And before you rush on, let me ask, there's been a curtailment in the heavy section steel research in the NRC budget, as I understand it.

Does that interfere, or provide you with a lack of data with regard to establishing certainty here?

MR. ERICKSONKIRK: Yes, we could always use more money for more data. Now, after that little commercial advertisement --

MEMBER SIEBER: That's a not a good - -

MR. ERICKSONKIRK: No, no. The information that we used to develop this model is all data that was previously available through --

MEMBER SIEBER: Right.

MR. ERICKSONKIRK: -- multiple years of testing both on our parts and internationally. The peer reviewers have seen this. And, as we pointed out, this is indeed a new model and somewhat of a break from the past, not just in terms of what's included in PTS, but in terms of toughness models in general.

I think it's fair to say that the peer reviewers were generally happy to include this type of model. But one of the comments they made is that a continuing effort should be made to collect more data to further validate it.

I am also aware that the IAEA is considering launching a program to develop further data to validate this type of model.

MEMBER SIEBER: Okay, thank you.

MR. ERICKSONKIRK: That was sort of a roundabout answer, which is to say we've got good data but more is always better.

MEMBER SIEBER: Yes, I like the second answer better than the first one.

MR. ERICKSONKIRK: Okay. So, we started out by saying once we lift the cap on crack arrest toughness, and so we can potentially develop stable arrests at very high applied K's as you move through the wall.

But what's going to happen next is that that applied K is above the crack initiation toughness on the upper shelf, the crack will most certainly start to tear and may go all the way through the vessel wall.

So we start -- this just shows how a

ferritic steel will behave on the upper shelf. You'll start to what's called blunt, you'll start by blunting the crack.

And then you'll begin to tear the crack. So the crack actually initiates here at a value called J_{IC}. And then the other characterization parameter is the slope of -- this is called the JR curve -- is the slope of the JR curve that's characterized by this parameter N.

So the two things that we need to characterize is the value of J_{IC} and the value of N, and the variation of those values with temperature and irradiation.

So we started off by collecting together the data that we could find both in our own testing programs and in the literature. And what we show here is just a plot of J_{IC}, that's the applied driving force at which a crack will begin to tear on the upper shelf, and how that varies with temperature.

And we've got a bunch of different materials on here. The blue and the red specs are reactor pressure vessel steels, both irradiation and unirradiated, both welds and plates and forgings.

It's all on there. And just for -- well, both for scientific interest and sort of to test the bounds of the model -- we've also included on here some ferritic steels that are different.

We've got A710 steels and HSLA steels, which are all copper precipitation hardened steels that are used in naval surface ship hull construction.

And we've also got something even more different, an HY-80 steel, which is of course a martensitic steel, a different crystal structure. So obviously there's a lot of scatter here.

What we wanted to see as a first cut is to see if there's a consistent variation of J_{IC} with temperature. And so what we did to try to normalize out the material-to-material variability and just look at the aleatory uncertainty, since we were focusing on reactor steels, and since in all the irradiation programs everyone always did tests at the PWR operating temperature, we said, okay, let's try normalizing all these data by the average value of any given data set at 550 degrees Fahrenheit or 288 C.

And when we done that -- and as I flip here I'll note that the vertical scale on this is the same -- you find out that much of that scatter compresses out, and that we now do see a very consistent trend with temperature.

Those of you that aren't too sleepy after lunch will notice that I've also scrubbed off the HY-80 data because what we found is that the temperature dependency here very much mirrors that of flow as predicted by dislocation motion.

That's the equation that you see, and that you would not expect that same temperature dependency to hold for a non-ferritic steel. But, for all ferritic steels that we've seen, this temperature dependency holds very well -- both for irradiated, unirradiated, welds, plates, and forgings, and indeed for things that would be considered in terms of their basic hardening mechanism very metallurgically different from ferritic steels.

So, this is what we use, this is what we sampled from to establish the aleatory uncertainty of initiation fracture --

CHAIRMAN SHACK: Why would I expect a precipitation hardened steel to have the same temperature dependency?

MR. ERICKSONKIRK: Because the only thing that controls the temperature dependence is the lattice structure. Only the lattice is able to -- it's the lattice atom vibration that can impede -- that controls the flow strength, right?

All the other -- the precipitation

hardening elements, the interstitials, all of that, those are all --

CHAIRMAN SHACK: I would have just thought the precipitation hardening mechanism would have overwhelmed.

MR. ERICKSONKIRK: We see this -- it's a consistent theme with what's happening in transition. You're a master curve man, right?

CHAIRMAN SHACK: Right.

MR. ERICKSONKIRK: Okay. And all ferritic steels, irrespective or irradiation damage, basic hardening mechanism, fit the same temperature dependency.

It's the lattice structure. It's got nothing to do with any of the things that make the steel stronger, weaker, work hardening, none of it. Okay, so then -- okay, so we've got the temperature dependency of the curve on the upper shelf.

Now the question comes, how do we or can we hook that onto the transition curve? Or, another way to look at it is, where do we truncate the cleavage fracture toughness curve and start going into the ductile fracture toughness curve?

So what we did is, now that we know the temperature dependency of cleavage fracture, toughness and transition, and we know where to put that based on T_0, and now that we know the temperature dependency on the upper shelf, we define just the temperature where those two curves cross.

And what we found is a very strong correlation between T_0, which is estimated in a roundabout way in the probabilistic code via an artifice called RT_{ndt}.

So, anyway, in the data we found a very strong correlation between the cleavage fracture transition temperature and the temperature at which the upper shelf and transition curves cross.

And we presented this at a meeting in Europe in September a year ago where Kim Wallin was present from VTT in Finland. Of course, he's the gentleman that developed the master curve.

And he became interested in it, went back to his laboratory, looked at datasets he had on VVER steels, both irradiated and unirradiated, and even a ferritic stainless steel, and found that they all fit the same trend.

And, having looked at materials data and materials correlation for years and years and years, all I can say is that's the best trend I've ever seen based on that variety of materials.

So, what this gives us an ability to do now is, in our FAVOR simulation we know -- we've estimated the transition fracture index temperature. We call it RT_{ndt}. On this slide it's called T_0.

But, in any event, we've estimated a temperature that is placed the cleavage fracture transition curve. What we can do now is use this relationship to tell us how far out we have to go before we hook on the upper shelf master curve.

And we've already established temperature dependency of that based on data several slides back. So that's what we're using. And all of that -- sorry, all of that was developed and presented in a recent EPRI Materials Reliability Program Report, number 101, which you can get Stan Rosinski.

To use that information in the FAVOR model we needed to add a few more things. We needed to quantify the scatter in the upper shelf toughness, which we did by just developing this variation of standard deviation on J_{IC} from the mean curve as a function of temperature.

And we also needed to have some information which EPRI hadn't developed yet on the temperature dependency of the JR curve exponent. And so we used data that was produced in NUREG, I think it's 4880 by McGowan on a variety of RPV plates and welds, irradiated and unirradiated, to establish that temperature dependency and that scatter relationship.

I think we're onto summary. So we've made some changes to the PFM model used and reported to you two years ago. The changes were motivated by both reviewer suggestions and by staff and ORNL initiatives to improve the model.

And we believe that overall those changes have improved the physical realism of the model, reduced our dependency on empirical correlations. Overall, both of these changes have had overall a small affect on the through-wall cracking frequency.

However, they have had larger effects on the prediction of what material regions are responsible for vessel failure. We're tending to see a better order than we have before.

CHAIRMAN SHACK: What exactly does that last bullet mean?

MR. ERICKSONKIRK: What exactly that last bullet means is, when we presented the results to you in the 12/02 report, there was no upper shelf model.

Shortly after that we put in an upper shelf model. But the positioning of the upper shelf is all based on Charpy correlations. We used the

Charpy upper shelf energy values from RVID to attempt to place the upper shelf.

The comment that we got -- and we ran those calculations. That was an intermediate set that never got reported outside of the NRC. One of the difficulties, one of the curious things we were seeing in those prediction was, for example, in Beaver Valley we were seeing materials that had lower values of RT_{ndt} contributing more to the through-wall cracking frequency than materials that had higher values of RT_{ndt}.

And the reason for that was that the materials that had lower values of RT_{ndt} had simulated values of upper shelf fracture toughness that were higher because there was no linkage between the upper shelf energy and the RT_{ndt} value.

Whereas, when we went back and looked at the data motivated by Dr. VanWalle's comment, we saw a very consistent relationship in toughness data that wasn't apparent in the Charpy data.

And so, once we wired that in, now what you see is in the model. Everything is indexed, all the toughness values, initiation fracture toughness, arrest fracture toughness, and upper shelf fracture toughness.

It's all indexed RT_{ndt}. And so now things are coming out consistent. Any other questions?

MEMBER SIEBER: Time for lunch.

MR. ERICKSONKIRK: Time for lunch, snack time.

MR. ERICKSONKIRK: Okay. So now I have a very few -- 72 slides on the, what we've called the baseline results. These are the results -- oh, sorry. Oh, Nathan is here. Sorry.

Now we'll have Nathan. No, no, I could use a break.

MEMBER ROSEN: Nathan will wake us up.

MEMBER SIEBER: That's tough.

MEMBER ROSEN: Remember, Nathan, what they say about sleeping dogs.

MR. SIU: I'll try to say as little as possible. How's that? Good afternoon. My name is Nathan Siu, Office of Research. With me is Mike Junge, who has been very helpful, as I was pulled off this project a while ago to work on other things the Committee has heard about.

Mike has helped pick up some of the pieces and take care of -- has taken care of some of the comments that have come through since the Committee was last briefed.

And, since I don't think Mike's been introduced to the Committee before, I'll ask him to just say a few words about himself.

MR. JUNGE: My name is Mike Junge. I'm a new hire to Probabilistic Risk Assessment Branch, working with Nathan. My previous experience was I came from Calvert Cliffs.

I was an SSRO there, shift engineer. And the latest position I held there was as an engineering supervisor in the auxiliary system branch in the plant engineering group.

MR. SIU: Mark, the slides.

MEMBER WALLIS: Is it inside this somewhere?

MR. HISER: It's in the package. It has the agenda on the front about 40 percent of the way through.

MR. SIU: To save the Committee time, I could just say that nothing has changed since the last time we briefed you. But I don't think that would be good enough. So I'll just give you a few.

MEMBER WALLIS: There are so many page ones in here, it's impossible. Could you find it for me.

MR. SIU: So what I'd like to cover,

basically just go over quickly the history of this particular activity. Then I'll mention the one result that we came out with, basically a recommendation regarding the risk-informed reactor vessel failure frequency that Mark has already shown you, and some of his earlier slides.

I'll mention a few observations that support that result. And then I'll briefly tough on peer review comments. It was mentioned this morning we just got those comments.

Fortunately there were very few in this area. And so I can mention what they were and then I think we can wrap up. Okay, back in May of 2002, of course, we wrote SECY-02-0092, which identified potential issues in establishing criteria for the reactor vessel failure frequency.

And we identified a number of options. We briefed the Committee, both the sub-Committee and the full Committee in July of 2002 and received a letter.

That letter encouraged us to consider an additional option. If you recall that was to consider a reactor vessel failure frequency much less than 10^{-6} per year because of possible concerns with air oxidation.

As a result of that letter, we had

performed a scooping study. It's a very qualitative assessment of the potential aftermath of a PTS event, and tried to determine if there was a strong reason to do a lot of analysis in the area of air oxidation events.

We briefed the ACRS in February of 2003. That was a fairly extensive briefing. And, in fact, if you want more details, I do have some of the material from that I'll put on the computer.

But I hope what I present to you will be sufficient without having to go to that detail. We received a letter from the ACRS. And that basically said that the proposed criteria of 10^{-6} per year was probably good enough to ensure adequate protection of public health and safety.

MEMBER KRESS: I do not recall that letter.

MR. SIU: I'm sorry?

MEMBER KRESS: I do not recall that letter. Does it have a date on it?

MR. SIU: Yes. I do not have the precise date. It was in February of 2003 though. And in that letter you pointed out that the criterion that we employed should be based on LERF, and basically said that the Staff was following that approach.

You made the observation that it was likely that our proposed criterion of 10^{-6} per year should ensure the PTS risk is acceptably low. Yes, here it is, February 1st, 2003, Tom.

MEMBER KRESS: Yes, I found it.

MR. SIU: And it recommended further consideration of late containment failure if rule making is pursued. But that was an optional conditional on the rule making process, which, it sounds like from this morning we're going to go ahead with.

So, that was the recommendation we took for future activity. Okay. So practically the only thing that has happened since February 2003 is that we've received comments from industry and incorporated those into the report.

And also we've received peer review comments. And I'll touch very briefly on those as I mentioned in just a second. Okay. So, just to recap where we are, we believe the analysis supports a reactor vessel failure frequency criterion of 10^{-16} per year where the reactor vessel failure frequency is interpreted as a through-wall crack frequency, not as a crack initiation frequency.

This is something that is -- of course

it's a metric that's closer to risk, even though it isn't all the way out to risk. And it also is consistent, of course, with how we've been doing things in the past.

That 10^{-6} per year is consistent with Reg Guide 1.174. And it's looking at the LERF criterion and trying not to have any particular initiator constitute a large percentage of risk.

So this is 10 percent of the 10^{-5} criterion. Obviously it's also saying that there's no special consideration here for the possibility of air oxidation events.

The reason for that is because we actually believe that there's very little likelihood that you'll get to such events, even should a PTS event occur.

We briefed the Committee in February of 2003 on the potential forces involved with such events and basically said that the Delta P's, the forces were on the order of the design basis accidents.

We didn't expect to see any additional failures of ECCS or containment isolation, certainly not containment sprays. So there's not dependency mechanisms that would increase the likelihood of such failures.

And, therefore, there should be substantial margin between the occurrence of a PTS event and failures to mitigating systems. Therefore, the conditional likelihood of the large early release is low.

And that's conditioned on the PTS event occurring, which Mark has also shown you that frequency is very small. And so that the large early release event does include the air oxidation event as a subset.

And we presented a qualitative accident progression event tree showing the very small number of sequences that had the possibility even of leading to some large early release events.

I think out of roughly 200 sequences we had in that APET maybe four were worth any consideration in terms of likelihood. And even those, once you consider the forces involved, it's really unlikely that we can follow along those paths.

MEMBER KRESS: Four out of 100?

MR. SIU: Four out of the 200.

MEMBER KRESS: Four out of the 200, okay, for an 02 conditional --

MR. SIU: If they were equally weighted. And they are certainly not equally weighted.

MEMBER KRESS: They're not equally weighted.

MR. SIU: Yes, the likelihoods of going down those paths were very small. That's what -- if you recall from the report -- we use qualitative terms, very low.

MEMBER KRESS: So you think the conditional large early release is two orders of magnitude below the through-wall crack?

MR. SIU: It could be. Now, we did not do, obviously, any serious quantitative analysis. We did some scooping calculations looking at the deformation of pressure vessels.

And then we presented to you the RELAP results looking at the delta P's associated with postulated break sizes and break opening times. And they were not, you know, outlandish.

So one would guess that it could be a substantial margin. But, again, we did not do they quantitative analysis.

MEMBER ROSEN: So, Nathan, turning this around to an operator in the control room, I take it that you're suggesting that this would look like a LOCA to him, not the vessel failure in this case.

MR. SIU: It could be.

MEMBER ROSEN: He wouldn't see anything different in terms of -- which would lead him to a different set of responses.

MR. SIU: Yes, in terms of, you know, tearing out ECCS piping, pulling penetration -- no, we didn't think that's going to happen -- don't think that's going to happen.

So, if there are failures, these are going to be independent failures, just like you have a normal garden variety PRA. In fact, in some cases you're better off because you probably have electric power and support systems, which is why you've got the overcooling event, because the pump systems are running.

Okay. The last point I mention on this slide, that most of the discussion, of course, regarding the PTS rule concerning if this is a reactor pressure vessel embrittlement, and anything that we do to that does not affect the conditional probability of failure of the mitigating systems, including containment.

You may affect the frequency of the frequency of the reactor vessel failure. And that's of course what we're talking about here, the criterion on that.

But, in terms of the defense-in-depth, you're not affecting the defense-in-depth through --

MEMBER KRESS: Does that assume that the sump blockage issue would get resolved.

MR. SIU: That's true. That's a good point. We do bring that on in the report, I believe, that this is conditional on sump blockage being taken care of. And we do not try to address that through this.

MEMBER KRESS: Let's not wait until --

MEMBER ROSEN: But to me the important conclusion here, which is sort of a surprise, is that operationally we don't really need to change anything if we think through the PTS problem in detail, as has been done.

The operators will respond as if were some sort of small break LOCA or a medium size break LOCA perhaps, maybe even large break LOCA. But it won't require them to do anything different.

It comes back to the old argument about symptom-based procedures versus event based procedures.

MR. BESSETTE: In fact, for a lot of these, the way you get into a PTS risk scenario is you start with a LOCA anyway. So, probably your ECCS is on, your containment spray is on at the time the vessel fails, for example.

MR. JUNGE: They're already in the middle of a scenario through the scenarios that we modeled.

MEMBER KRESS: I'm intrigued by your comment that the likelihood of having an air oxidation event with pressurized thermal shock failure is low. How did you arrive at that conclusion?

MR. SIU: That's the -- if you follow through the APET, we had labeled the sequences. In fact, on a back up slide here, the last back up slide, this one here, this is a figure in the report.

And you notice in the right hand columns there -- it's kind of hard to read.

MEMBER KRESS: That's all right.

MR. SIU: But this is early core damage possible, large early release possible, and air oxidation possible. And we've identified --

MEMBER KRESS: And what's the criteria for air oxidation? Is it that there be --

MR. SIU: Simply a big hole, possibly. You see, we're talking -- the break size is here. We had 100 to 1,000 square inches.

MEMBER KRESS: You're coming up with actual break sizes.

MR. SIU: Yes. Let me walk you through the tree.

MEMBER KRESS: Okay.

MR. SIU: Okay. So we had crack orientation, axial circumferential. We had whether the crack extends, and how far it extends. And blow down forces, are they roughly designed basis or significantly beyond design basis?

We had whether or not the containment was isolated. So we're accounting for possible dependencies there. We have the containment spray is working, yes or no.

Location of the fuel, whether it was spewed outside the vessel or retained in the vessel. Whether ECCS continues to run, and whether the reactor cavity is flooded.

Now, it was pointed out in one of the industry comments that maybe some of our logic here is a little flawed in terms of asking this question after ECCS has failed.

But I'll leave that alone for the moment. Okay. So the events with air oxidation here, you see we had failure. The large early release, of course, requires that you have failed the isolation containment and that your sprays aren't working, and that ECCS is not working here.

MEMBER KRESS: Okay.

MR. SIU: So we've had a core melt, no isolation. And we just simply said it's possible. And if you look at an event where we might have a large early release but no air oxidation, this one here, you recall that -- the difference is this is a small hole in the reactor pressure vessel.

So we did not track the flows through the system. We did not model, you know, the real way that the air would go through the system. Again, in terms of a scooping analysis, just a very quick and dirty --

MEMBER KRESS: Now, how did you arrive at the break size?

MR. SIU: This is parametric, some sense of the length of the crack here. This is the one, the crack, for example, that runs to the circumferential welds and then opens up a little bit.

But, again, this is just parametric. You have small, medium, large, and then there was a very large here.

MEMBER KRESS: Did you ascribe some probability to that?

MR. SIU: We didn't play that up very much. Most of the likelihood assessments were based on the mitigating systems and the lack of dependent failure mechanisms for those.

So, yes. Mark mentioned this morning, for example, that --

MEMBER KRESS: The reason I said that, it may be that the probability goes down towards the air oxidation side.

MR. SIU: Oh, well, if you look -- again, looking at the forces involved, saying, do you really feel that you would bias it towards the larger openings, it's not clear to me why you would?

Now, I haven't done the analysis, so we can't say. But I wouldn't as my first choice bias it down towards the larger sizes.

MEMBER KRESS: The reason I am going on like this is because I feel like you need at least two orders of magnitude on the LERF acceptance criteria compared to the one times 10^{-6}.

You need two orders of magnitude to make up for the air oxidation. And, you know, you're saying that the probability of air oxidation is .1 and the probability of containment failure also is .1, you get that two orders of magnitude. But I need to see some definitive --

MR. SIU: Yes. The one thing I'll say,

you know, again, we hadn't done a numerical analysis. But, in the end, the acceptance criterion saying 10^{-6} is being applied to the reactor vessel failure.

So that's saying that -- that was the point that Mark raised earlier, that --

MEMBER KRESS: That's right.

MR. SIU: -- it's equating the through-wall crack frequency, LERF.

MEMBER KRESS: Right. So I need these other orders of magnitude to come out of the conditional probability.

MR. SIU: Right. And we think -- let me say, I personally think you'll get there if you actually do the numerical analysis, just by looking at the things that have to fail to get you to that.

MR. BESSETTE: Yes, there are only a few ways to get there. You've got fail containment spray. You've got to fail to isolate containment. You've got to fail ECCS or you've got to break the vessel in two pieces.

Because, if you have, even if you have a fairly large axial crack, if you have ECCS on, even if you don't have adequate core cooling throughout the top of the core, if you have enough steam generation so that you can't get air ingress.

You've got pretty high velocities out the break, I'd say 50 miles-an-hour or so.

MEMBER KRESS: Yes, but, by the time you're getting ready to get the air coming in, you don't have those -- you've gotten rid of most of this steam. You're at low velocity and --

MR. BESSETTE: But, you sort of have to fail ECCS to get air ingress.

MEMBER KRESS: I do not know that.

MEMBER DENNING: Well, he was saying that he has ECCS on, and that water has to go out the break, even though it's not getting to the core, if he has ECCS on.

MEMBER KRESS: Those are statements I need to see some sort of technical reason. But we know that in our oxidation source term we probably increase the prompt fatalities by a factor of 100.

That's where I get my two orders of magnitude. And I can see that it's likely you could get two orders of magnitude out of these conditions. But my problem is, it's --

MR. SIU: Yes, it is qualitative. There's no doubt. And it was that way back in 2003.

MEMBER BONACA: This event tree, again, it's only addressing LOCA, right? No secondary site breaks. And the concern I had that I expressed this morning was mostly the B&W type of this the pass-through steam generator.

Now, the reason why I bring it up, I notice in the follow-up slides here there are some comments, in fact, and answers to peer review questions.

MR. SIU: Yes.

MEMBER BONACA: Okay.

MR. SIU: I'll mention it in a second. But, again, the hole size refers to the PTS induced hole. So we're coming in here whatever way. It could be from the transient.

MEMBER BONACA: Right, post-event.

CHAIRMAN SHACK: And again, Tom, although his acceptance criterion might be one times 10^{-6}, if you look at their actual frequency of through-wall cracking, I think it starts at 10^{-7} and goes down from there at the end of license renewal.

MEMBER KRESS: Yes, but that doesn't affect acceptance criteria. I mean, it just saves you -- the one --

MR. SIU: It sort of says --

MEMBER KRESS: If you had to meet one two orders of magnitude lower, that wouldn't be so good.

CHAIRMAN SHACK: No, but it says the likelihood of this thing happening is pretty remote.

MEMBER KRESS: There's a point there.

MEMBER DENNING: And, Nathan, the acceptance criterion is interpreted as a mean?

MR. SIU: Yes, that is correct. That was the recommendation as well, and just to be consistent with how we do other acceptance criteria. Okay, this slide talks to the initial set of peer review comments we got.

And we got two. One concerned air oxidation and basically said, while it was recognized as a potential issue, the reviewers didn't think that it was a good use of resources to pursue this in any great depth and that the PTS project wasn't the project used to look at establishing different guidelines in LERF.

I'm just saying what the comment was. And the second comment we got was basically, it was a question about documentation. I guess at the time they hadn't been able to review the full chapter.

And so they just wanted to see how we documented the analysis. Since these two interim comments have gotten the -- I guess we would characterize them as draft comments. Mark, is that right?

MR. ERICKSONKIRK: Draft final.

MR. SIU: Draft final, okay. And, again, there were two comments. Basically one reviewer said that the 10^{-6} was reasonable and appropriate. And the other one said that basically he agreed with the framework for addressing these issues.

But there was no similar concern about the air oxidation expressed by the members of the peer review committee. So that's what we have now. And that's all I had to say. Are there any questions?

MEMBER KRESS: When they -- the peer review comments, when they made their comment that they didn't think it was cost beneficial, I guess, to go after the air oxidation part, were they aware, do you think, that the prompt fatalities could be increased by a factor of 100 when they said that?

(No verbal response.)

MR. SIU: Mike, is he nodding yes?

MR. JUNGE: Yes.

MEMBER KRESS: They were aware of that.

MR. JUNGE: I believe it is still written in chapter 10. It does discuss the number increase that we would see with air oxidation.

MR. ERICKSONKIRK: I apologize, we've had another slide copying mix up. So --

MEMBER SIEBER: Turn to page one.

MR. ERICKSONKIRK: Yes, turn to page one. You don't have these slides yet. Shah is trying to get them to you as quickly as our copy center will accommodate him.

In the meantime you have the ignominity of having to look at me.

MEMBER SIEBER: You may want to move that water bottle.

MR. ERICKSONKIRK: Oh, yes I may. Okay. So, in these slides I'll be reviewing the information that's presented in chapter eight of NUREG-1806 where we discuss the plant-specific analyses we have performed at Beaver Valley, Palisades, and Oconee.

The overview of this set of slides is that we're going to start by discussing the through-wall cracking frequency estimates and their distributions.

And then we're going to talk about the material features that contribute or not to TWCF and the transient classes that contribute or not to TWCF.

First I'll just start with a table. This is sort of a -- stop looking, you don't have them. You don't have these. That was a mix-up. If you find them, you really win, like getting the white M&M.

And, if you do have a printed copy or an electronic copy of NUREG-1806, on as many of these slides as I could think to do it, up in the title you'll see the section number that the information is presented in.

In any event, this table shows sort of the high level results coming out of FAVOR. So we've got analyses of Oconee, Beaver Valley, and Palisades at four different embrittlement levels.

I put RT_{pts} on there, not because I like it, just as sort of a reference and then the values of frequency of crack initiation and through-wall cracking frequencies that have been calculated.

I'd like to make two observations. One is that the TWCF is very low for the current lifetime and into the period of license extension ranging from E to minus 11 to E minus eight failures per year.

And that was the reason of having the RT_{pts} column on here. If you look at RT_{pts} numbers at the current screening limit -- and you have to sort of do some mental interpolation to get to 270 -- you'll find out that the current screening limit per these calculations corresponds to a yearly through-wall cracking frequency in the E to the minus nine range, not five times 10^{-5}, which is the result of the previous calculation.

So, that comparison just gives you some sense regarding the level of conservatism, or some would call it margin in the --

CHAIRMAN SHACK: Five times 10^{-6}, right? That was what you were aiming for.

MR. ERICKSONKIRK: I'm sorry, yes. You're right. Five times 10^{6}. It's still a big difference.

CHAIRMAN SHACK: Still a big difference.

MR. ERICKSONKIRK: Okay. Throughout -- and I've mentioned this before, and I think used this slide before. Throughout the bulk of our presented material, we talk about through-wall cracking frequency as if it's a single number.

And we're always recording mean values. I just wanted to point out here that those mean values are drawn from distributions that are both highly skewed and very broad.

And those -- the distributions are that way simply because there are many, many situations where we sample a flaw, we sample embrittlement, we sample a transient, and we come up with a calculated failure probability that's zero.

So, you know that the physical underlying processes are producing these distributions where you've got a big tail on the lower end. And I call your attention to the scale.

The vertical scale on my little graph is the percent contribution to through-wall cracking frequency. And the vertical axis only goes up to one percent.

So the values of merit, the mean values that we're drawing from these distributions are all way up here in the upper tails. And this graph makes that point, I think, a little bit better.

We looked at the mean values that we were recording and figured out what percentile of the distribution they corresponded to. And I said this in my introduction, that these mean values correspond to something like the 90th percentile or greater of the distribution.

So, that is the end of the overview on just looking at through-wall cracking frequency values, and not trying to draw any causal relationships about what materials cause the frequency or what transients cause the frequency.

So now we'll go into the discussion of what materials cause the frequency. So, just to -- sort of a fundamental tenant of flaw analysis or structural integrity analysis.

In order to correlate or predict failure of a component, you need to know what the toughness properties are at the flaw location. And, in this analysis, and this is a common approach, we use a reference temperature to characterize what those toughness values are.

And, as we discussed earlier, the reference temperature indexes the location of the cleavage fracture initiation toughness curve, the arrest fracture toughness curve, and indeed of the upper shelf fracture toughness curves.

And the aleatory scatter of those three different toughness metrics about those curves has been quantified, sampled from in every case, and is shown to be the temperature dependency of those curves.

And the scatter about those curves has shown to -- has been shown to be, I'm sorry, consistent for all the materials that we're interested in.

So, if you know the reference temperature at the flaw location, then you know everything you need to know about the toughness of the material to perform an assessment as to whether that flaw at that location will fail or not given a certain loading challenge.

So then, given that, what we need to know is where are the flaws? And that gets back to a graph which I showed you before, and the basis of our flaw distribution, which is that we have embedded weld flaws that follow the weld fusion lines.

This is not to say that all flaws in welds are on the fusion line. Certainly you can have porosity, entrapped slag, blah, blah, blah. But, the ones that get you are invariably the crack-like defects which are lack of fusion defects, which are, logically enough, preferentially oriented along the fusion lines, which are axial for axial weld and circumferential for circumferential welds.

So, all the flaws associated with axial welds are axial. All the flaws associated with circumferential welds are circumferential. And all the surface breaking flaws that are postulated to be generated, even though we never observed any, are postulated to possibly exist between the passes of the austenitic stainless steel cladding, are oriented circumferentially.

Our destructive analyses of plates showed that plate flaws have no preferred orientation. So in FAVOR we simulate a coin toss. 50 percent of them go in as axial, 50 percent of them go in as circumferential.

Oops, I'm sorry. One thing I forgot to point out is, so now we know where the flaws are. They're either -- they populate the weld fusion lines, or they occur somewhere out here in the bulk.

And so, now we know where the flaws are. We also have our fluence map, which tells us what the level of irradiation is at those locations. And so those are several steps towards calculating the reference temperature at those locations.

MEMBER SIEBER: When these vessels are fabricated, are these welds machine welds?

MR. ERICKSONKIRK: I'm sorry, are the weld preps machined?

MEMBER SIEBER: No, the weld itself.

MR. ERICKSONKIRK: Yes. The fabrication welds are invariably automatic. The repair welds are invariably stick. Repair welds characteristically will have larger flaws because that's more likely in a manual process.

And we've included those flaws in our flaw population. However, it's also important to point out that stick processes don't have the copper problems that the automated processes did.

MEMBER SIEBER: Right.

MR. ERICKSONKIRK: So that's another implicit conservatism in our analysis, is we sample a small number of the larger flaws associated with the weld repair process.

But those large flaws can have high copper because we're using the composition of the fabrication welds, not the repair welds, whereas that just simply can't happen in practice.

MEMBER SIEBER: Did you go back to the fabrication documentation to look at individual characteristics of individual vessels? Or did you just make general assumptions about --

MR. ERICKSONKIRK: No, only in the sense that, for the vessels that we destructively evaluated, we did that. But, no, in terms of placing repair flaws into our three plant specific analyses, those repair flaws were smeared out.

They were part of the general flaw population that was sampled from. So, the repair flaws can be simulated to occur anywhere on the vessel, which means -- let's see, let me think about that.

Unless you happen to be so unlucky as to have the repair located smack dab at the peak fluence location, I'd argue that that procedure is generally conservative.

MEMBER SIEBER: Okay, thank you.

MR. ERICKSONKIRK: Yes, Bruce?

MR. BISHOP: Bruce Bishop at Westinghouse. I know that the expert panel that was involved in addressing some of the flaw distributions and so forth, and some of the questions they were asked were, you know, what's the probability of large flaws, small flaws occurring during different fabrication processes.

And they actually went back and got retirees and people that actually helped fabricate some of the vessels, and tried to take maximum use of that advantage -- you know, take advantage of that information.

And so, while it wasn't specifically, you know, destructively, or taken into account, it was, in fact, factored into the general distributions that were -- they subdivided into small and large.

And there were specific factors that applied to account for some of that variability based on their experience.

MR. ERICKSONKIRK: Thank you. The other thing that we'll get to when we talk about sensitivity studies, which will either be much later today or tomorrow, is I know a concern that people frequently had is that somehow we've give short shift to the larger repair defects.

But, when you look at the defects that are responsible for the Lion's share of the through-wall cracking frequency, it's not the big flaws that get you, it's the smaller flaws.

Obviously there's a limit to that. They can be so small they won't initiate at all. But, if we were to pour in five times more large defects, it wouldn't have a big effect because once you get a large defect, you've got to go down farther in the vessel to get the thermal shock.

And the driving force just isn't there. This slide points out the reason that axial flaws contribute so much more to the through-wall cracking frequency than circumferential flaws.

This is a plot for a particular flaw that this is for. These are all either 360 degree circumferential or infinite length axial. But, what you see is the driving force for crack initiation of both a circumferential flaw and an axial flaw of the same initial depth is the same.

So, given all the same conditions,

circumferentially and axial flaws are equally probably to initiate. But, as you ago through the vessel wall out to the eight inch thickness, the driving force produced by thermal shock loading steadily climbs to reach a peak only very close to the back wall for an axial flaw.

Whereas it reaches a peak very early on and then starts to drop off toward the circumferential flaw. So, circumferential or cylindrical vessels subjected to thermal shock loading have essentially a natural crack arrest mechanism when it comes to circumferential flaws.

So, I said before that, if you're going to do the defect assessment right, if you're going to hope to correlate the through-wall cracking frequencies, if you're going to hope to predict what transients are worse than other transients, you need to have flaw locations, specific reference temperatures to characterize all these things.

So we've come up with a couple. And I promise these are -- I think these are not only the worst equations I'm going to show, they're also the only equations I'm going to show.

MEMBER SIEBER: Good.

MR. ERICKSONKIRK: Good, yes good. But,

I'll jut put them all up. What we've done is we've come up with reference temperatures for flaws and axial welds, reference temperatures for flaws in circumferential welds, and reference temperatures for flaws in plates.

And, even though the specific formula is different, the idea behind calculating all of these is the same. And that's to say let's look at the axial weld.

If you've got a flaw in an axial weld, you want to find the location of highest fluence along that axial weld fusion line. And then, since an axial weld can have either -- it's got a potential of one or two material properties, the properties associated with the weld or the plate.

So you calculate the irradiated RT_{ndt} at that worst fluence for the weld in the plate, and you take the higher of the two. And that's the reference temperature for that axial weld.

Now, the axial welds can have fluences that aren't the peak fluence of the vessel, depending upon how the welds line up with the core flats.

Whereas, the reference temperature for the circ welds and the plate is much easier to calculate because, you know, ignoring vertical variations influenced, within the core region essentially the circ welds will somewhere see the peak fluence, as will the plates.

So, calculating the reference temperature for the circ welds and the reference temperature for the plates is a simple matter of figuring out what the peak fluence is in the vessel, calculating what the irradiated RT_{ndt} is for all the plates and all the circ welds in the belt line and just picking the maximum value.

And that way we get a metric that is associated with the worst conditions that a flaw could see at these various locations. And those are the values that we then use to correlate the through-wall cracking frequencies.

So, what can be said about the failure probabilities of these flaw populations, just by inspection, before we run any analysis. So the axial weld flaws are generally larger than the plate flaws.

They can be up to two inches deep, although very rarely, whereas the plate flaws can only be up to half an inch deep, again although very rarely.

So they are generally larger than the plate flaws, and they are axially oriented so they have the high through-wall driving force. The circ weld flaws, since they are weld flaws, are from the same population as the axial weld flaws.

They are the same size. In all likelihood the circ weld flaws are burdened with a higher fluence because they have to see the maximum fluence in the vessel, whereas the axial welds don't.

However, the big thing, again, to differentiate circumferentially weld flaws from axially oriented weld flaws is the difference in the through-wall driving force.

The plate flaws you've got two differences going on. First, they're half circ half axial, so the circ ones effectively don't matter. The plate flaws are much smaller than the axial flaws.

But, again, if we use Beaver Valley, which is the most interesting case because it's got welds and plates that sort of compete for what's driving the through-wall cracking frequency.

And what you find out is that as you go to higher and higher levels of embrittlement in Beaver Valley the higher -- and I'm using Beaver Valley as an example.

The higher fluences that occur in the middle of the plates overwhelm the smaller flaw size of the plate flaws. And so you start to get contributions of those plate flaws through the through-wall cracking frequency at the higher embrittlement levels.

At the lower embrittlement levels the flaw size dominates the axial welds. So I showed you this graph before, which now, I guess hopefully will make a little more sense.

The statistics that come out of FAVOR tell us not only what the through-wall cracking frequency is, but it's, you know, it's something I dream of being at home.

Something breaks and I look at my seven year old son and my 11 year old son and say, who broke it? And they both say I didn't. But FAVOR gives me statistics saying who broke it.

And it will tell me when the axial weld flaws are responsible and when the circ weld flaws are responsible, and when the plate flaws are responsible.

And what we see here is that when we correlate those failure frequencies, which are calculated by FAVOR and plotted on the vertical axis, with these three reference temperatures, calculated using the equations that are designed to give us the reference temperature of the worst location of the axial weld, circ weld or plate, we find we get a pretty reasonable correlation between the different vessels.

And, again, point out that in general terms, at an equivalent level of embrittlement, axial weld flaws are responsible for 100 times more the through-wall cracking frequency than are plates.

And then circ welds are 50 at reduction even on that.

MEMBER DENNING: Before you go on, could you go back two view graphs to the equation and show us -- I missed the, what looks like an averaging on the axial. What's the --

MR. ERICKSONKIRK: Yes, what -- and that's been pointed out before. And that's something that, you know, probably deserves a little more thought. The axial welds can be -- well, lets go with circ and the plate.

The circ and the plate always have the highest fluence in the vessel. Whereas the axial weld flaws, depending upon how the core is oriented with respect to the welds, can have sometimes different levels of fluence along each axial weld fusion line.

So the averaging is an attempt to take that into account. The fact that you might have one axial weld fusion line at a much higher fluence than another axial weld fusion line.

MEMBER DENNING: And the L-prim is what?

MR. ERICKSONKIRK: I'm sorry, the L is the length.

MEMBER DENNING: The length.

MR. ERICKSONKIRK: The length of it. Because, obviously, if you have a very short weld, it's going to have less flaws than a very long weld, because the number of flaws scale the length or the fusion line.

MEMBER DENNING: And this is preferable to looking at every one of the flaws in its location? That's what I'm missing here. Why do you do an averaging?

I mean, what's the logic of doing an averaging rather than looking at ever flaw?

MR. ERICKSONKIRK: Well we fundamentally can't look at every flaw because there are thousands of them. But, what we're trying to do is construct a metric to represent the level of embrittlement of the vessel based on things that you can know without having performed a probabilistic analysis.

But, the intent of the averaging is to take into account the fact that, again, depending upon the core orientation, you might have one axial weld that's at a much lower fluence than another axial weld.

And, for example, if you had an axial weld that's at a fluence trough and another axial weld that's at a fluence peak, the axial weld at the fluence trough is going to contribute much less to the through-wall cracking frequency.

MEMBER DENNING: And you -- but I'm still -- it sounds to me like you're averaging something that you don't want an average value of, that you really want to look at a -- do you actually use the RT_{aw} in the FAVOR analysis?

MR. ERICKSONKIRK: No, this is all --

MEMBER DENNING: Oh, this is just a --

MR. ERICKSONKIRK: This is post-processing. This is -- we use the RT_{awcw} and plate to effectively characterize the level of embrittlement for post-analysis correlations.

In the same way that you would use, you could calculate these values for any vessel that's out there. And I think perhaps the general comment is, you know, maybe you want to think about this, or maybe you want to try other relationships.

You know, yes that's probably so. And

certainly of all these three, in terms of implementation, this is the most complex to calculate.

So, if we could do something simpler just by taking a maximum and get an equally good correlation, that would be a good thing. And that's probably something to look into.

MR. ERICKSONKIRK: I thought you were actually using this in the analysis.

MR. ERICKSONKIRK: No, this is a post-process, because, remember, before meeting and current regulations, we have one metric that tries to characterize the embrittlement of the entire vessel, RT_{ndt}.

And we get that by taking the worst fluence in the vessel, and the worst chemistry in the vessel, and the worst unirraidated toughness in the vessel and combining all those things together, despite the fact that all those things might not physically be possible to have at the same time, and there might not be a flaw there anywhere.

So what we're trying to do is to develop sort of flaw location specific metrics. But no, this is an input to FAVOR. This is calculated after the fact.

MEMBER ROSEN: But thinking about this in terms of if I were to send you out there to a vessel and say find me the most -- the worst threatening flaw, it seems to me you'd go and look at the axial intersection of the -- the axial weld intersection with the circumferential weld.

And right at that, on the axial weld itself, though, right above the circumferential intersection -- I think you've got a slide there, a cartoon that shows this.

MR. ERICKSONKIRK: Yes.

MEMBER ROSEN: And you'd say, if I find a significant flaw there -- a two inch flaw two inches into the material -- on the axial weld, on the fusion line of the axial weld, but very close to the circumferential, that would probably be a very serious flaw.

MR. ERICKSONKIRK: Yes.

MEMBER ROSEN: That would be -- and I could go looking around all the rest of the vessel, and I probably couldn't find anything more serious than that. Is that one way of looking at it?

MR. ERICKSONKIRK: Possibly. But, I have to say it depends. Because, for example, in Beaver Valley they've intentionally located all of the axial welds at the fluence troughs.

And so, even though I might be able to find a much -- and I know I could find much larger flaws along the weld fusion lines irrespective of if it's at the circ intersection -- sometimes the smaller flaws out at the fluence peaks would be more damaging. So it's not --

MEMBER ROSEN: Okay.

MR. ERICKSONKIRK: This is one of those cases where it's not just size that matters.

MEMBER ROSEN: But if a plant hadn't taken that precaution?

MR. ERICKSONKIRK: Yes. But, I mean, I agree. Just in terms of the reasons flaws are where they are, if you have an intersection of two welds, yes, it's more likely to find a flaw there.

And it's more likely it will be bigger. Although, if I went to my inspection record and I found where the repairs were, I'd actually start looking there.

But, those repair flaws, even though they are large, are associated with low copper materials. And so, they probably have a higher toughness. And I want to hasten to point out that these are all things that the analysis has considered probabilistic.

You've got finite probabilities of having very large flaws. You've got finite probabilities of having very high coppers. And that's essentially all in here.

It's not incumbent upon us to find the worst flaw or the worst location. Even if you did, that's not going to drive the through-wall cracking frequency.

It's not going to make it one. Okay. That ended the presentation -- excuse me, the part of the presentation on materials. So now I'm going to go into what's the most lengthy part of this discussion, which is, what are the classes of transients that control through-wall cracking frequency?

What are their characteristics? What's important, what's not? So, in our analysis we considered both primary system faults, secondary system faults, and indeed something this slide doesn't say, which is combined primary and secondary system faults.

Primary system with the pipe breaks, stuck open valves are later re-closed. Feed and bleed secondary system faults, main seam line breaks, stuck open valves, steam generator tube rupture, and pure overfeed.

These graphs like this are in the report.

This shows -- and I just want to draw one impression from this, and then I'll take it away. On the horizontal axis it shows all of the different transients in this case that were analyzed for Oconee.

And on the vertical axis it shows the percent contribution to through-wall cracking frequency. And there's one line for each embrittlement level we analyze.

And the main thing I wanted you to take away from this is that, again, we calculated an awful lot of zeroes even though we a priori eliminated way more transients than we've ever analyzed.

We still -- our screening criteria for what gets into the analysis isn't so -- we don't assume that we know so much more that we're eliminating things that actually contribute.

We're still calculating an awful lot of zeroes. And what we find out is we perform -- we analyze 30 to 60 transients. And invariably a handful to two handfuls are the ones that are dominating the through-wall cracking frequency. And the rest just don't matter at all.

MEMBER ROSEN: Could you go back for a minute?

MR. ERICKSONKIRK: Oh sure.

MEMBER ROSEN: I've taken away another piece of information from that. And that is that in some cases going to 60 EFPY -- I was going to say shows like it matters.

Let's take a look at the right hand peak, my right hand, at SO 1.65, I guess.

MR. ERICKSONKIRK: Yes.

MEMBER ROSEN: That one says what to you?

MR. ERICKSONKIRK: Well that says that -- okay, that's a stuck-open valve. It stays open for 6,000 seconds, re-closes, operator doesn't throttle until you get full system re-pressurization.

At 32 EFPY it was over two thirds of the through-wall cracking frequency. But, by the time you increase the embrittlement level to what we've called extended levels of embrittlement to avoid using ridiculous numbers of EFPY on slides, the through-wall cracking frequency of that transient at the extent of embrittlement level has continued to climb.

The absolute contribution has gone up. But, the percent contribution is now highest for the LOCAS. And we see that very consistently. And Bill pointed that out before, that, at lower levels of embrittlement, you need the over-pressurization associated with stuck-open valves that later re-closed to punch the crack through the wall.

Once you get to the higher levels of embrittlement, what people often call thermal only transients, or transients with a small pressure component, the vessel is sufficiently brittle that those cracks can go all the way through.

So, just looking at -- and remember, we've analyzed for each of these vessels a spectrum of embrittlement levels, and indeed taken it out to embrittlement levels that are just ridiculous, not to say that those embrittlement levels are likely or even achievable, but just to say that the transients that matter -- you can't just look at one snapshot and say, oh, it's main seam line break, oh, it's a stuck open valve.

You need to look at the whole embrittlement spectrum in order to get a good feel for the types of transients you contribute. So, to summarize that, dominant transients -- and this is looking across the embrittlement spectrum.

The transients that contribute 80 percent or more to the through-wall cracking frequency are either medium or large diameter pipe breaks -- and by that I mean four to five inches and above and stuck-open valves in the primary side but later re-closed.

Secondary system faults only play a minor role, and then only at very much higher levels of embrittlement, again because in a secondary system fault you can get a really fast cooling rate.

Because the primary is still sealed you can have that really fast cooling rate in combination with pressure. But, the temperature's just not low enough to drop the toughness enough to allow the vessel to fail.

But, at higher levels of embrittlement we do get some contribution to main seam line break. And we'll talk about that. And then everything else is essentially negligible or zero.

Small seam line break, small breaks, pure overfeeds, feed and bleeds, those all fell into the transients and contributed next to nothing or absolutely nothing to any of our calculations.

So, in the following sets of slides, we're going to present a more detailed examination of both the dominant and the minor transient classes. And my aim in this is to be very boring.

I'm going to go through this in exactly the same way each time for each transient class. We're going to start with a general description of the transients in that class, how they progress, what the operator actions that could be take are.

What are the operator actions that we've modeled? In the next part we'll discuss how we've modeled this transient class. Then in the third part of the discussion we'll discuss the relationships between the system characteristics and the thermal hydraulic response.

We'll then go on to tie those thermal hydraulic responses to PFM results. And finally, at the end of each presentation on each dominant class of transients, we'll discuss how the model that we've adopted in these calculations is either similar to or different from those previously employed.

And we'll be contrasting both our current results with both those that we presented in February of 2003 and those that were used to establish the basis for the current PTS rule.

Okay, so we're going to start with primary site pipe breaks. In primary site pipe breaks you've got two cooling mechanisms. The major one at the beginning of the transient is of course the rapid depressurization because of the break that causes a rapid temperature drop.

It's the only thing that matters for the very large breaks. And it's what's dominating early on for any break size. But then later on you start to get injection of colder ECC water.

The injection temperatures can range from -- actually that should be 40 degrees if the water is stored in an external tank in the winter up to 120 if you exhaust what' sin the RWST and you start to pull from the sump.

The temperature of the injection water can become an important factor, but only for the smaller break diameters, because only those last long enough to see the warmer injection water.

And also, the break location can be a factor. For example, cold legs for given break size, cold legs tend to be somewhat less sever than hot legs because you can lose injection water flow out of the cold leg break, so it's not going into the downcomer and it's not cooling,

The minimum -- I've got another error, it should be 40. But, the minimum temperature is controlled primarily by the ECC injection temperature.

Which means it can go down to the temperature of the water stored in the external tanks, which of course can vary with seasonal conditions.

But that eventually you exhaust that water supply and you have to start pulling from the sump, at which point you're pulling in something that's like 120 degree Fahrenheit water.

And we've modeled that where it's appropriate. The cool down rate as I'll show you when we get to the thermal hydraulic part is controlled primarily by the break size.

And then it is moderated by the secondary factors, which is the total RWST inventory, safety injection pump set points when you switch over to sump and so on.

In terms of our initiating event frequencies, the graph shows the initiating event frequencies we used for the PRA bins as a function of break diameter.

And, for all practical purposes, there are two populations here. There are the larger breaks, four inches and above, that have an initiation frequency of something like one times 10^{-5}.

And then there are the breaks four inches and below where the initiating frequency is something like one times 10^{-4}. As we mentioned before, we've modeled no operator actions here because safety injection --

MEMBER WALLIS: Why does the trend go like this for palisades, 16, and goes up again?

MR. ERICKSONKIRK: I'll have to defer to Donnie to answer that specific question. But, I'll just point out --

MEMBER WALLIS: Is this a surge line, or what is that 16?

MR. ERICKSONKIRK: Donnie? It should be point out --

MEMBER ROSEN: The palisades surge line is not 16 inches.

MR. ERICKSONKIRK: Palisades is different from the other two because Palisades did its own analysis.

MR. WHITEHEAD: Yes. As I said earlier this morning, there were two cases that we dealt with. The Beaver Valley and Oconee analyses were done in-house.

The Palisades analysis was done by the utility using their model with adaptations necessary to account for the issues that we were interested in for PTS.

The Palisades model actually modeled four break sizes, a small break size, a medium break size, a medium-large break size, and a large break size.

And the difference that you see for the event here, the 16 inch diameter break, has to do with the way in which we collapsed their four break sizes and the frequencies that they assigned to them to our three classes of break sizes.

So, there's just a -- there's a small variation in frequency that was used for the two types of analysis, the one done in-house and the one done by the utility.

But that frequency, if I'm remembering correctly, was, you know, typically on the order of maybe a factor of two, possibly a factor of three difference in the overall frequency.

And we did not believe that it was necessary to force them to use the numbers that we were actually using for our initiating event frequencies.

So it's just an artifact of the differences in the models, basically.

MEMBER WALLIS: Did the 16 cover the main -- RCS piping, it's --

MR. BESSETTE: The main diameter, or the diameter behind Palisades is about 30 inches.

MEMBER WALLIS: But Palisades is even bigger.

MR. BESSETTE: Palisades is about 30 inches.

MEMBER WALLIS: Sixteen is covering that as well, it's an average?

MR. BESSETTE: Yes. You know, we did break spectrum. And we analyzed ourselves breaks up to 22 inches. But we found the answer wasn't changing between eight and 22 inches.

MR. ERICKSONKIRK: Bruce, did you have something?

MR. BISHOP: The Palisades hot leg is much larger than any of the other plants because they only have two hot legs and four cold legs.

MR. BESSETTE: Yes, the hot leg might actually be 36 inches and the cold leg about 30 inches.

MR. BISHOP: The OD is around 40 something.

MR. ERICKSONKIRK: What we'll get to in the PFM results is -- you and David alluded to this -- is once you get into break diameters, half a foot and above, the thing that's controlling the cooling rate of the vessel, and therefore the thermal stress of the vessel is the vessel itself, not the rate at which it can deliver water.

So now we get into the part where we can look at different system characteristics and how they control the thermal hydraulic response.

CHAIRMAN SHACK: Does that mean you're really governed by the four to eight inch break then, because they're just so much more likely?

MR. ERICKSONKIRK: I'm not sure I can answer that. We get big contributions from both medium breaks -- which are four to six -- and large breaks -- which are six and above. I do not remember the relative percentage.

CHAIRMAN SHACK: Because based on the elicitation that looks like an awfully high frequency for the 16 inch break.

MR. ERICKSONKIRK: I haven't gotten the new numbers from the elicitation. So, if they wish to drop their numbers, I'll add them. I do not know.

That's something where -- and it's relevant to the rest of your briefings this week. Rob and I need to get together to make sure that I'm using his -- well, I know that I'm not using their final results right now.

So, if you're saying their numbers are lower, that's a good thing.

CHAIRMAN SHACK: I mean, your numbers look like, what is it, 35 to 50 percent, the INEL numbers.

MR. WHITEHEAD: Donnie Whitehead again.

The local frequency numbers that we used in the PRA analysis actually were the numbers that were provided to us in an interim letter, memo that I think came in somewhere around the middle of 2002.

I do not have the date on the top of my head. But, it was a reflection of what was believed at that time to be the numbers that were coming out of the consensus group that was looking at initiating event frequencies for breaks.

MR. ERICKSONKIRK: Yes. That's correct. We got interim results from the internal expert elicitation. And we've not yet synched with Rob on what those frequencies are.

But that's just a post-processing -- I should say just, calculationally it's easy because it's post-processing step. And we do intend to synchronize that.

So there won't be an inconsistency between the information you're getting on large break LOCA re-evaluation and the information you're getting out of this.

CHAIRMAN SHACK: Yes. With their targeted adjustment, which I think is their best estimate for a 14 inch pipe, they get like three times 10^{-7}.

MR. ERICKSONKIRK: The screening limit

just went up.

CHAIRMAN SHACK: Good.

MR. ERICKSONKIRK: Okay. So we'll be looking at the effects of break diameter, break location, season of the year, and makings and plant-to-plant comparisons to look at how sensitive or non-sensitive the thermal hydraulic response is to these variables.

So, first off, just looking at a complete break size spectrum, this being for Beaver Valley, you see other ones in the -- in NUREG 1806. And, obviously, reducing the break size considerably reduces the cooling rate.

And what you also see out here is that, as you go out in time for the larger breaks, you can completely drain the reactor water storage tank. And so, in order to continue safety injection you have to switch over to the sump.

And that's why you get this pop here between the low temperature stored in the external tank and the water that's in the sump. But you see this very nice gradation of very rapid cooling rates with eight and 16 inch breaks, and then becoming much more gradual as you go up to the smaller break sizes.

Looking at pressure, same transients, the one point I'd like everybody to take away from this graph is -- except for the very largest of breaks -- it takes a very long time to get to pressures that can truly be regarded as negligible.

And I think this is in part a contribution to the reason why large breaks, large medium size breaks which weren't previously considered to be LOCA contributors are.

It's because the old experiments where we severely thermally shocked the vessel at Oak Ridge, and we found that the cracks could go almost all the way through, but not -- but, at unequivocally no pressure.

And that's just clearly not case for a real vessel. I should skip anything on heat transfer.

MEMBER WALLIS: Wait a minute.

MEMBER SIEBER: Just keep flipping.

MR. ERICKSONKIRK: Heat transfer coefficient is at this scale similar irrespective of break size.

MEMBER WALLIS: Would it be used for -- per hour per foot-squared?

MR. BESSETTE: That's the units that are coming of relip.

MEMBER KRESS: It's on the back.

MEMBER WALLIS: It's pretty low. You multiply by 3,600. It's still pretty low. Okay.

MR. ERICKSONKIRK: Okay. Now, looking at break location effects, and I need to orient myself, the surge line break is the red curve, whereas the cold line break are not the red curves.

Thank you. So, to compare the same size, a four inch surge line and a four inch cold leg, compare red to green. And what you find out is that the surge line is cooling more rapidly because all of the injection water is going into the downcomer, whereas, with the cold leg, you're starting to lose injection water.

It's not all getting to the downcomer. The other thing I want to -- so, you do see some differences between surge lines and cold lines in this intermediate break size.

But the other thing I wanted to point out is that, you know, here's a four inch surge line, here's a four inch cold leg. They're still in basically the right -- back up.

Break location effects are still -- should be considered secondary to break size effects because both four inch breaks are still being bounded on one size by 2.8 and on another side by a 5.7.

So, it's an effect. It can be important in the intermediate break size. But, by and large, break size is still the controlling factor. Seasonal effects, let's see, everything here is winter, except for the green is summer.

And, again, summer is somewhat less severe, but not out of the break size order. And now some cross plant comparisons. Here we will just do a spectrum of break sizes going from large to small, and comparing the various plant analyses.

So, very large breaks, 16 inch and eight inch, not much difference plant-to-plant. You get differences out here in terms of when you switch over to sump and how hot the water is in the sump.

But the cooling rates are still very similar.

CHAIRMAN SHACK: Now, is the Palisades also a surge line, or is it a different line?

MR. ERICKSONKIRK: I can't tell you based on what's on -- I can tell you, but I can't tell you based on what's on this graph.

MR. BESSETTE: Well, for 16 inch, I think we switched the break location from the surge line to the hot leg.

MR. ERICKSONKIRK: This is eight inch.

MR. BESSETTE: Oh, eight inch surge.

MR. ERICKSONKIRK: Palisades is call.

CHAIRMAN SHACK: So all eight inch breaks look alike.

MR. ERICKSONKIRK: Eight inch breaks looks alike. Four inch break similar, 2.8 inch breaks. Given a certain break size and a location, we've got very good similarity plant-to-plant.

So, looking at the conditional probability of through-wall cracking, so conditional means, assuming the transient occurs, what's the probability of through-wall cracking.

And this is what David was referring to. The larger diameter breaks pose a very consistent challenge from plant-to-plant because under those situations the steel can't cool as rapidly as the depressurizing water.

So it's in what's been called a conduction controlled situation. And that means the thermal stresses are controlled solely by the thermal conductivity and the vessel thickness, and nothing else matters.

MEMBER WALLIS: Presumably the temperature of the water.

MR. ERICKSONKIRK: But the temperature of the water --

MEMBER WALLIS: It is the driving force. It may take time to penetrate. It didn't have any cooling. You wouldn't have any thermal stress. It's got to be the proposed --

MR. ERICKSONKIRK: Well, yes. If you were injecting water at 212 it would be different. But the injection temperature of the water is also very simple situation.

So, with those provisos the details of the transient become unimportant.

MEMBER WALLIS: As long as it is depressurized and cooled down?

MR. ERICKSONKIRK: Yes. Go to smaller breaks and now the transient properties, more of the secondary effects can become important. Because, in this situation the steel vessel can cool as rapidly as the depressurizing water.

And so, it's the water that's controlling the cooling rate and the thermal stresses in the reactor coolant system.

MEMBER WALLIS: If the vessel cooled as rapidly as the water it would be uniform temperature and there wouldn't be any stress in it. It cools comparably or something.

MR. ERICKSONKIRK: Yes.

MEMBER WALLIS: The resistance to heat transfer is --

MR. ERICKSONKIRK: Yes, you're right. I'm sorry. But, of course, the thing to point out here overall is --

MEMBER WALLIS: The outside of the vessel doesn't cool in any of these transfers.

MR. ERICKSONKIRK: No. As you get to these smaller breaks the through-wall cracking frequent becomes much, much lower than for the larger breaks.

Looking at break location and seasonal effects, at the intermediate break size we see that they can be important, you know, to the order of magnitude or to --

CHAIRMAN SHACK: What degree of embrittlement are we talking about here?

MR. ERICKSONKIRK: This is at 60, Palisades at 60, which would be beyond the current limits. Some other sort of interesting facts, if you will, in terms of break time, if the breaks occur -- break time on the left hand side of the screen.

If the breaks occur, they occur very early in the transient. And so, you know, again, some of these things tend not to matter. For example, we at one point thought we had made a terrible over conservatism by not including the higher temperatures of re-circulation from the sump.

We thought if we did that that the large break frequencies or the large break failure probabilities would go way down. It turned out it didn't change at all.

The thing we weren't paying attention to is that, for the large breaks, the failures occurred long before you ever get to switch over to sump. So it doesn't matter.

And also, as I pointed out before, over here, that while pressure is certainly not a dominant factor in controlling the through-wall cracking frequency of these transients, it's not zero.

There is some finite level of pressure there. So, if the thermal part of the transient is sufficient to propagate the crack to vary near the back wall of the vessel, the lining pressure is sufficient to fail.

MEMBER WALLIS: You're not showing the stuck-open valve here.

MR. ERICKSONKIRK: No, because that's next. So, to summarize, primary site pipe breaks, there are several factors that suggest the applicability of these results to PWRs in general.

First, there's no influence of operator action. So differences in training, protocols and so on plant-to-plant can't be a factor. It's the large diameter breaks -- five inches and above -- that dominate the pipe break through-wall cracking frequency.

Five inches and above contributes 70 percent to the pipe break portion of the TWCF on average. And then it's just the four inch breaks that contribute most of the remainder of that.

And everything else smaller you may as well forget it. So, you know, the take away here is that the transients that dominate the pipe break through-wall cracking frequency of the class are the least dominated by plant specific factors.

And that's a good thing for generalization, which is why I think that when we plot the through-wall cracking frequency that's due to the class of primary site pipe breaks, versus a reference temperature derived from where the falls are, we find a fairly consistent trend plant-to-plant because the level of challenge is fairly consistent plant-to-plant.

Okay, so differences from previous analysis relative to our December '02 results, obviously our specific numerical results are somewhat different.

But the general trends are the same. Relative to the analysis that establish the tech basis for the current rule, there's a big difference because medium to large diameter pipe breaks were included a priori from those analyses due to the erroneous assumptions made regarding the need for significant pressure to fail the vessel.

CHAIRMAN SHACK: Do you track which of the failures actually involve tearing?

MR. ERICKSONKIRK: Yes, we do. Terry, yes we do?

PARTICIPANT: Yes.

MR. ERICKSONKIRK: Yes. And no, I haven't looked at that. But I will. Yes, that's part of the statistics that come out. Okay, so now stuck-open primary valves, because this of course involves re-pressurization components.

So we begin with a demand on an SRV. The open SRV depressurizes the primary with a rate equivalent to something like the two inch diameter pipe rate.

So we've got relative to large break LOCAS a very slow cooling. ECC injection accelerates the cooling by direct injection of cold water. At some time later the valve recluses.

The continued safety injection will now begin to refill the primary. Right after the valve re-closes throttling will probably not be satisfied because of combination of factors.

And, of course, the throttling criteria different plant-to-plant. But generally right when the valve re-closes there will be no sub-cooling. And the pressurized level will be too low.

After about 15 minutes the pressurizer will be full. The throttling criteria will be met. And now, unless the operator acts very promptly, the system will rapidly re-pressurize to full system pressure or to a safety valve set point unless the operator throttles.

So that's the general -- in very generic terms, that's what we're trying to model.

MEMBER ROSEN: How long does he have before he has to throttle typically?

MR. ERICKSONKIRK: He needs to do it within a minute to stop re-pressurization.

MEMBER ROSEN: A minute from the beginning of the transit?

MR. ERICKSONKIRK: No, I'm sorry. A minute from the time that his throttling criteria is met. When the valve re-closes -- and we'll see some thermal hydraulic transients in a minute.

Once the valve re-closes he can't throttle because the pressurizer lever is too low and there's no sump cooling. So you're going to start to slowly refill the vessel.

Temperature and pressure are going to start to rise slightly. But, once you collapse the bubble, pressure is going to go through the roof very quickly unless you throttle.

And what the calculations show is that unless you show catch it very quickly, you're going to go to full system pressure.

MEMBER ROSEN: So, any kind of look at the HRA involved would say it's very unlikely he's going to catch it?

MR. ERICKSONKIRK: We'll go into that.

MEMBER ROSEN: Okay.

MR. ERICKSONKIRK: So our model of stuck-open primary valves, we've looked at initiations of these types of transients from both full power and from hot zero power.

We of course stick -- the number of valves that stick open we've looked at. We've re-closed the valve at either 50 or 100 minutes. And I'll talk about why we believe that's an appropriate discretization of the complete possibility of re-closure times.

We've considered that the operator might throttle, might never throttle, might never get to it, might throttle one minute or ten minutes after their throttling criteria is met.

And then we've looked at other minor variations on theme. More than one valve open, less than the total number of valves open re-closing, summer versus winter, and so on.

Looking at the initiating event frequencies, which is shown by the histogram and just for purposes of comparison, show those relative to the initiating event frequencies for large diameter breaks and small diameter breaks, we find that these transients are just a little bit less likely than the primary site pipe breaks in our model.

So, looking at -- now on to the part where we look at thermal hydraulic response. We're going to look at the effect of the timing of valve re-closure, the power level at transient initiation, and the timing of operator action to throttle charge once throttling is allowed.

Okay. We're going to start off, these are plots. And here I'm using for example plots from Oconee. We've looked at the plots from the other plants.

The same trends exist. So you've got a temperature on the left hand side of your screen, pressure on the right, and valve re-closure at 3,000 seconds.

So the valve is slammed shut here. But what you see is you've got about another 1,000 seconds before re-pressurization is going to occur. But, during that time it's only at the end of that time that the operator would be allowed to throttle. So what you see here is --

MEMBER WALLIS: Does the pressurizer fill or something? Or why does it --

MEMBER SIEBER: Yes, it fills. And then the flow goes to zero and the pressure goes to the shut-off --

MEMBER WALLIS: There's no pressure and the pressurizer is the problem. The water is too cold. Isn't it?

MEMBER SIEBER: Say again.

MEMBER WALLIS: There's no vapor pressure in the pressurizer because the water is too cold.

MEMBER SIEBER: Right.

MEMBER WALLIS: It goes up till it hits the roof.

MEMBER ROSEN: It's called no bubble in the pressurizer.

MR. BESSETTE: Basically the steam bubble collapses and then you go quickly to the PORV set point because the whole system is water solid.

MEMBER WALLIS: That's right, because there's no hot water.

MR. BESSETTE: There's no compressibility anymore.

MR. ERICKSONKIRK: So, what you see is -- if I were to put a 16 inch or an eight inch pipe right here you'd see a very much faster cooling rate.

But you wouldn't have that late stage re-pressurization. And what you see from these three curves is that, unless the operator throttles within a minute of meeting the criteria, you can't prevent re-pressurization to full system pressure.

And also, I might point out from a fracture perspective -- and I know I'm getting a little ahead, but, once you get -- when you get the full system pressure, it doesn't matter that he saved it out here, because you went to full pressure at a time when the temperature was low.

Dropping the pressure out here when the temperature is higher, if the vessels failed, it will already have been gone at that point.

MEMBER SIEBER: But throttling at that point in time, if the system is solid and tight -- in other words, the PORV is closed -- you can't control pressure by throttling because there's no flow.

MEMBER WALLIS: It's controlled by the set point.

MEMBER SIEBER: No, you can't do that. You have to shut the pump off.

MEMBER WALLIS: It's the valve that controls the pressure.

MR. ERICKSONKIRK: Okay, so now I'm going to overlay --

MEMBER SIEBER: Yes, the PORV does.

MR. ERICKSONKIRK: So the title of the slide was looking at valve re-closure time. So I'm now going to wipe and show what happens at 6,000 seconds.

And now we can have fun and go back and forth. So you see that at 6,00 seconds, looking at temperature, just continues to cool until about, again, 1,000 seconds after the valve re-closes.

And then we see the same thing happening again. Unless the operator throttles very rapidly, you'll go back to full system pressure.

CHAIRMAN SHACK: Mark, where are we in your presentation?

MR. ERICKSONKIRK: We're at Viewgraph 42 of 72. You want to get to the end of this, of stuffed valves and take a break or --

MEMBER WALLIS: I think we're about 6,000 seconds.

MEMBER SIEBER: Page 22.

MEMBER ROSEN: What's this NRC New with Chicken at the bottom?

MR. ERICKSONKIRK: That's me.

MEMBER ROSEN: You're the chicken?

MR. ERICKSONKIRK: But I took away -- the chicken is the logo. But I took the logo away because --

MEMBER ROSEN: Oh, that chicken.

MR. ERICKSONKIRK: It's an eagle.

MEMBER WALLIS: It's only a chicken when it's at Sandia.

MR. ERICKSONKIRK: that's it. And no more questions about the logo.

MEMBER KRESS: That was uncalled for.

MR. ERICKSONKIRK: That's right. So later valve re-closure produces lower temperatures at re-pressurization. Here, at 3,000 seconds when we re-pressurize, the temperature was up here.

Whereas now we've re-pressurized and the temperature is considerably colder. And that would tend to make the transient worse. But you've also got lower stresses at re-pressurization because the temperature -- you're starting to get out of the transient, and the cold is soaked into the wall.

So, at least without performing the fracture calculations, you couldn't necessarily say which of these is worse. Now looking at valve re-closure time, first we'll note that the valve can re-close at any time after the transient begins.

And we haven't attempted to model causal factors here. As we just said, the competing effects of thermal stress, which tend to go down as the re-closure time goes out, which reduces the severity of the transient, and minimum temperature, which again goes down, but increases of the transient compete to give us situation where re-closure -- almost immediate re-closure yields very low through-wall cracking frequencies, and long time re-closure yields lower through-wall cracking frequencies.

And there's sort of a, you know, a worst of all possible times where re-closure could happen. However, after about two hours we don't really consider re-closure, because after this long a period, if you're that far into a transient, the operators would have initiated new procedures.

And so you wouldn't be in this type of transient anyway. And we haven't modeled that. And so --

CHAIRMAN SHACK: So your scale is wrong there, that's seconds rather than minutes.

MR. ERICKSONKIRK: Absolutely.

CHAIRMAN SHACK: I thought that was a pretty long transient.

MEMBER ROSEN: After two hours.

CHAIRMAN SHACK: Nine thousand.

MR. ERICKSONKIRK: There we go. Okay, seconds. Sorry. So after two hours something else would have happened. So that's beyond the scope of this model.

And so, what we've done is we've divided this part, which is important to us, into two bins. We've modeled valve re-closures after 3,000 seconds and after 6,000 seconds.

And the thing to point out here is that what we're trying to do is we're trying to represent this entire continuum of a through-wall cracking frequency using only two re-closure times.

So, at least in my view, it's not terribly important that we've missed the peak out here. And it's also not terribly important that we perhaps overestimated things back here, because what we're essentially trying to get is the area under the curve.

And it seems that we've done a fairly reasonable job on that. Now, going on to look at power level effects on transient initiation time and of operator actions.

Here we've got a transient initiated from full power, re-closure at 6,000 seconds. And what you see is the full power. Of course, if the operator does nothing, you go to full system pressures.

If the operator throttles after ten minutes, you go to full system pressure. If the operator throttles within a minute, they are able to delay the time of re-pressurization.

But you still go to full system pressure. And you see that consistently in all the analyses, because there's enough heat in the system that you wind up re-pressurizing.

That's not to say that the operator hasn't helped because, by delaying the time that I go to full system pressure by about 1,000 seconds, I've gone from the temperature that's down here, up to a temperature that's almost at the point where I don't care about it.

So, transients initiated from full power can't stop the -- at least in our model, within the confines of our model -- throttling within a minute after you're allowed to do so.

You can't save yourself from re-pressurization. But you can save yourself -- the operator action does give you some benefit in through-wall temperature.

MEMBER WALLIS: But your temperature is only on the surface. You've still got a temperature wave going through the wall.

MR. ERICKSONKIRK: That is correct.

MEMBER WALLIS: So there may be places in the wall which are still cooling down. So the stress could be actually going to a place where you had a bad flaw.

The stress could be rising in a place where you have a bad flaw conceivably, even though the surface is heating up.

MR. ERICKSONKIRK: Yes, you're right. The metal temperature and the metal stresses are going lag that, which is the fluid. Yes, absolutely.

MEMBER WALLIS: The wave going in.

MR. ERICKSONKIRK: Yes. Whereas, if we go to a transient initiated from hot zero power, the difference that you see between those two plots is if you focus on the red line, which is if you focus on the red line, which is throttling after a minute, in this case there's not enough residual heat in the system.

And the throttling within a minute keeps you from re-pressurizing to full system pressure. The other thing to notice is that hot zero power transients are more severe on the front end because the cooling rate is faster and you go to a lower temperature.

If you now focus on the temperature side, here is full power, and there is hot zero power. So you've got a more rapid transient, and you're going to a lower temperature, which is going to make the hot zero power transient more severe assuming that the operator is in successful -- modeling.

So thermal shock, more sever, but the

operator action is more effective under hot zero power. Throttling within a minute will stop re-pressurization under hot zero power, whereas it only delays it under full power conditions.

And throttling within ten minutes is the same as not ever throttling at all. Okay, so looking at plant specific effects, there are some, but they are minor.

Okay. I'm sorry, like the number of valves that stick open and fractions of them closing, or perhaps a valve only sticking open 30 percent of the way, those are all really minor factors relative to these three dominant variables that we've just gone through.

Let's see now, probability. General observations on vessel failure probability, just the fact that we've re-pressurized doesn't necessarily lead us to conditional probability through-wall cracking that's either non zero or even large.

If you re-pressurize, if the temperature is above 400 degrees Fahrenheit nothing happens. However, again, as I pointed out, the re-pressurization makes it a virtual certainty that, if a crack initiates, it's going all the way through.

The valve re-closure time, obviously, as we showed before, influences the through-wall cracking frequency as does the power level of transient initiation.

The conditional probability of through-wall cracking for odd zero power transients is approximately 1,000 times that for full powered transients, again, if re-pressurization occurs.

And that has to do with the lower fracture toughness and the higher thermal stresses associated with the hot zero power transients. And that in fact generally overwhelms the fact that hot zero power transients occur less often.

The increased severity of hot zero power transient overwhelms the fact that it doesn't happen as often.

So now a few words on effectiveness of operator action. It's shown in the plots. The operator really has to be on top of things. They have to throttle within less than a minute of meeting their throttling criteria to either delay or prevent re-pressurization.

In terms of the credits for operator action in our analysis -- and if you have questions here I'm going to have to direct them to Donnie. But, based on simulator observations, discussion with plant engineers, we gave credit for Oconee operator successfully throttling approximately seven times out of ever ten.

Beaver operators weren't quite as on top, but they basically successfully throttled within one minute 40 percent of the time. Now, Palisades, I need to say this carefully.

In Palisades the PRA analysis said that operators would successfully throttle. But that didn't get into the thermal hydraulics model because, by that point, we knew that if the operators were successful, they generally stopped the re-pressurization.

And it didn't count anyway, so we figured that was a zero we didn't need to calculate. So, in the end model, even though the PRA said the Palisades operator should be given credit for throttling, in the transients that were analyzed, there is no credit for operator action at Palisades. And that should be taken --

MEMBER ROSEN: So even though it's -- well, I'm not sure if I agree with that 68 percent and 40 percent. But, nevertheless, even though you're not likely to stop the damage, the procedure should require operators to throttle.

MR. ERICKSONKIRK: Oh, absolutely.

MEMBER ROSEN: Because they have a chance of doing it. Even if it was a non-zero chance, it may even be a 50 percent chance. So that's absolutely.

Yes, the procedure should -- and training and all the rest -- should include this.

MR. ERICKSONKIRK: Right. Just because the welds can be made even though you don't inspect them, you should still inspect them. Sorry Bruce. Yes, this is not to say that operator action is a bad thing.

MEMBER ROSEN: The flavor as always the operator -- what you've been saying all along, maybe you don't see it, but the flavor has always been well, there isn't much the operators can do.

But, quite the contrary. There is quite a bit they can do. They just wouldn't always succeed.

MR. ERICKSONKIRK: That's right. And the other thing to mention here is that, you know, indeed saying, you know, that the operators get it right basically half the time, that's effectively saving yourself half the time.

But the other thing is, remember that the time when the operator really saved the day was for hot zero power initiation, where they actually could prevent the re-pressurization, rather than just delay it.

So, saying that the big contributors here to the through-wall cracking frequency are stuck-open valves that re-close where the operator hasn't been successful in preventing the -- hasn't throttled within a minute, but the ones that are contributing under hot zero powers, again, is tending to diminish the effect of operator actions in the end result.

MEMBER WALLIS: The only thing which would really change these numbers here is putting in elicitation results, which might reduce the Palisades frequency of breaks in large pipes and might have a big effect on the highest number here.

MR. ERICKSONKIRK: Yes.

MEMBER WALLIS: Let's see what would change these numbers. That's the only thing.

MR. ERICKSONKIRK: Well, actually, as it turns out, now looking at this, where we've plotted now the through-wall cracking frequency due to stuck open valves, and this --

MEMBER WALLIS: This is valves.

MR. ERICKSONKIRK: Yes, all of them agglomerated together. Remember I said Palisades, even though we said -- even though the PRA said they should get a credit for operator action, that actually didn't get into the final analysis because we figured, you know, why calculate more zeros.

So there's no operator action credit here, whereas there is some operator action credit here. And clearly it's not making a huge difference in the numbers whether you include it.

So, again, factors suggesting that these results, while they are for three specific plants, have some applicability to PWRs in general. Is it re-pressurization?

Is it dominant factors influencing the transient severity? And all PWRs have similar system pressure, so similar loading challenge. And that while we have provided reasonable and appropriate credit for operator actions, the physical factors that control the transient severity limit those effects on through-wall cracking frequency.

So, if we were to take them all out, like we did at Palisades, we're not seeing the Palisades with no operator action credit, you know, way up here, relative to these where we've given what we feel is an appropriate level of operator action credit.

MEMBER SIEBER: Is it fair to be able to extrapolate the Beaver Valley and the Oconee data?

MR. ERICKSONKIRK: With the usual provisos on extrapolation, yes. I mean, I think we're seeing, you know, the same curve shapes going out. I wouldn't take it too far.

And certainly, you know, it would be interesting to test these by going out to get a higher level of embrittlement on Beaver Valley. But, I, you know, I'd bet you a beer that it still agrees.

MEMBER SIEBER: Okay.

MEMBER WALLIS: How much is the actual probability of a stuck-open valve? And this is the whole story. This is the conditional probability and the probability of initiating event. This is the whole story.

MR. ERICKSONKIRK: Yes.

MEMBER WALLIS: How much is the probability of an initiating event?

MR. ERICKSONKIRK: The probability of the initiating event -- I need to go back. Yes, that's it. Here we go. On average 10^{-6} to 10^{-5}.

MEMBER WALLIS: So it's a large part of the whole story?

MR. ERICKSONKIRK: Yes, it is.

MEMBER SIEBER: Well, that's one scenario.

MEMBER WALLIS: It's most of the story in fact.

MR. ERICKSONKIRK: If you take the contribution of stuck-open primary valves and a medium and large break LOCAS, you've got 80 percent or more of the through-wall cracking frequency, which means that 20 percent or less is on the secondary side.

But after the break that I know the Chairman wants to take, I'm going to tell you that the only reason that the secondary side is 20 percent in Beaver Valley at high levels of embrittlement is we used a conservative analysis.

But I do have one or two more slides on stuck-open valves if you'll indulge me, because the last part of the story was -- oops -- how does our modeling now compare with before?

And this is the one area on transients where our story has changed from that we wrote about in December of 2002 and briefed you on in February of 2003.

And we have the egg on our face of thinking that we knew too much, because the previous way we conducted the FAVOR analyses was to start out by performing an analysis of a particular plant at a very high level of embrittlement, figuring out what were the transients that dominated there, taking the top ten, and just running those at the lower levels of embrittlement.

What I've already showed you suggests that was a really dumb thing to do because different transients make their important contributions at different embrittlement levels.

So we stopped doing that. And now we analyze all the transients that are given to us from thermal hydraulics and PRA and run them through FAVOR.

And so, before we believed that the primary site stuck-open valves were only important in Oconee. And that was an erroneous conclusion because of that flawed methodology.

Whereas, what we see now is when we take all of the transients that have been specified by PRA and TH and run them through the PFM model, we get a very consistent plant-to-plant, responds very consistent challenge to this type of transient from all the different plants.

So, that's a major change. Previously we thought this was a plant specific effect. And now it's quite clear that it's not. In terms of the differences between this model and that, which establish the technical basis for the current PTS rule, in the previous analyses of Oconee and H.B.

Robinson, stuck-open primary valves weren't really considered at all.

They were considered in the previous analysis of Calvert Cliffs using a much less refined treatment than we have here. And when it was analyzing Calvert Cliffs, it was found to be a significant contributor.

MEMBER BONACA: Could you go back to that. I don't understand. When you came in here in 2003, you already were telling us. This is nothing new that you should present to us.

I don't understand this comparison here. Current technical basis not considered in previous analysis.

MR. ERICKSONKIRK: No, I'm sorry. The basis of the current rule.

MEMBER BONACA: Okay.

CHAIRMAN SHACK: Nineteen eighties vintage tech basis.

MR. ERICKSONKIRK: Yes.

CHAIRMAN SHACK: It's not a current tech basis.

MR. ERICKSONKIRK: yes. Break time?

CHAIRMAN SHACK: Time for a break.

MEMBER ROSEN: When we come back, you'll

tell us what the impact of this is on 50.46 considerations.

MR. ERICKSONKIRK: I'll go haul my colleague back down from upstairs.

CHAIRMAN SHACK: Back at 3:40.

(Whereupon, the above-entitled matter went off the record at 3:26 p.m. and went back on the record at 3:43 p.m.)

CHAIRMAN SHACK: Back into session.

MR. KIRK: Main steam line breaks. As a result of that, you rapidly depressurize the affected generator through a multiple square foot hole and you depressurize the pressured break location. That causes a rapid temperature drop in the affected generator to the boiling point of water at the break location which is 212 degrees, obviously, the boiling point of water outside of containment for about 250, 260 inside of containment because the containment becomes pressurized by the steam escaping from the faulty generator.

The temperature in the primary tracks that in the affected generator because of large heat transfer area of the steam generator tubes. The rapid cooling shrinks the primary inventory and so depressurizes. Safety injection would then be initiated automatically. However, the primary temperature will remain at or above that of the affected generator due to the large heat transfer area provided by the steam generator tubes.

Safety injection can then refill and repressurize the primary and at some point later the operators will be allowed to throttle safety injection.

So operator actions to isolate the break. If a break is downstream of the MSIV, they simply close the MSIV and the event is over. If the break is upstream of the MSIV, but outside of containment, the operator would close both feedwater isolation valve and the main steam isolation valve. At that point, the generator would boil dry and the primary temperature will now be controlled by the intact generator and the event is over.

If the break is upstream of the MSIV, but inside of containment, again, the operator action will be to close the feedwater isolation valve and the main steam isolation valve. However, now we're venting steam into the containment which would cause an adverse containment condition, so the engineered safety features actuation system will automatically isolate containment.

That means that the operators are now obligated to secure the reactor coolant pumps, because they have no coolant water and if they don't stop them, they're going to seize. Without the reactor coolant pumps, safety injection water will not be as well mixed in the primary and so the downcomer will become cooler if the break is inside a containment than if the break is outside of containment.

MR. ROSEN: Why do you say the operators must act to isolate the break. I thought main steam isolation was automatic on most plants.

MR. JUNGE: If you have steam generator isolation signal, 800 pound, yes, they would shut.

MR. ROSEN: So you're going to get automatic MSIV closure.

MR. SIEBER: On low level.

MR. ROSEN: Yes. For a break of main steam, it's going to go to low level faster than the operators can --

DR. BONACA: I'm not sure Oconee has isolation --

MR. ROSEN: Some don't, but many do. But if you have MSIVs, they have automatic isolation and if you have feedwater isolation valves most of those have automatic isolation too. It's just the point, the operators don't really have to do it. The system does it.

MR. KIRK: Well, then it's virtually assured that warping the head of the fracture mechanics results, it doesn't matter anyway because the vessel will have failed before the operators are able to take any action at all.

DR. BONACA: I just want to point out the conversation we had that I think there are significant differences between the B&W design and the particular CE design that has a totally different dynamic in the transience. I don't see how you can lump them all together, draw the same conclusions, etcetera. The Rancho Seco event where they had the cool down for one hour and a half or whatever and could not have been possible in a CE plant, the way I see it.

MR. KIRK: But that influences the initiating event frequency, not what happens after.

DR. BONACA: I understand that. I'm saying that when we met in 2003, I remember the gentleman was sitting there and gave a very specific description of the B&W response which is different. I mean you have four steam generators with no inventory practically, so you blow down one and you are flashing through and the others are not providing back any heat to the primary side. The CE plant has these huge pots of water. One of them is blowing down, but still it takes a long time to empty it and the other one provides back heat to the primary side. Therefore, you have a much slower transient and --

MR. KIRK: I'm sorry, which plant has slower transient?

DR. BONACA: The Combustion Engineering type plant. And all you have to do is go to the FSAR analysis and look at the curves and see that. I'm only saying that I'm not sure you can lump together the secondary side breaks for these two types of analyses. I remember that plants are so fundamentally different and the whole TMI experience shows a different response and other kinds of behavior.

I'm not saying that the conclusions of this should not be similar. I believe that the gentleman who spoke there spoke of the fact of the operators were successful, they implement their procedures, they isolate manually and they're able to control the cooldown and to make the likelihood of leading to the conditions for plant initiation and expansion to very low probability.

MR. BESSETTE: There are a number of plant-specific features that affect the events. For example, after the early 1980 study, B&W implemented automatic isolation of feedwater.

DR. BONACA: Yes.

MR. BESSETTE: Things like that.

DR. BONACA: But again, some plants still have main steam isolation --

MR. BESSETTE: Yes. Oconee doesn't -- for example, Oconee does not have MSIVs.

DR. BONACA: Right.

MR. BESSETTE: They just have it -- the stop valves near the turbine.

DR. BONACA: That's right. So all I'm trying to say is that even when you compound the probabilities of success of certain actions, etcetera, it makes a difference whether or not you have a treatment and whether or not the system responds one way or the other.

I think you have to look at the different behavior of those plants.

I see that the peer review raised the issue of the Rancho Seco event.

MR. KIRK: And we've -- Roy might remember that response better than me, better than myself, I'm sorry, but in looking through our analyses to find the transient that most closely matched Rancho Seco, even at the highest level of embrittlement analyze, it had a failure probability of zero.

DR. BONACA: I believe that. I'm only saying that you ought to have a solid technical basis that is not arguable.

MR. KIRK: I think we need to take an action to better understand, describe the differences between the two plant types.

MR. ROSEN: Right, and don't say that operators have to close valves in plants where the valve action is dramatic.

MR. KIRK: Right.

MR. BESSETTE: I would say the operators will close the valves faster than the signal will get to them.

MR. KIRK: Okay. So our model of main steam line breaks, somewhere on this slide I should have big words that say intentionally conservative. As we got to this stage in the modeling process, our preliminary analyses had showed us that as bad as we tried to make main steam line breaks, they were still a small contributor to the total through-wall cracking frequency relative to primary system faults stuck open in valves and primary system breaks.

So we didn't refine these analyses as much as we would have had they made a large numerical contribution. So our model features delayed operator actions relative to what I think most people would consider creditable. For specific examples, we allow feed to the faulted generator for 30 minutes or indefinitely. Certainly, you'd have to have a fairly dumb operator to allow that to happen. And throttling of HPI 30 to 60 minutes after allowed.

We include exacerbating equipment failures, MSIVs failed to close, if there are MSIVs and I think at least for me the easiest to understand is because I'm not a systems guy and a very significant conservatism is that we have physically unrealistic minimum temperatures even for breaks inside containment, we haven't modeled containment pressurization. So for breaks inside containment, we allow the minimum temperature to go down to 212 which is clearly too low. It should be about 40 degrees Fahrenheit higher and that 40 degrees can have a big effect on the calculated through-wall cracking frequencies.

Again, the initiating event frequencies of all the main steam line breaks, we've analyzed, not trying to separate out plant-specific facts in any way, but as shown by the histogram and again shown relative to the LOCA break frequencies, so we've got -- excuse me, initiating events that are somewhat less likely.

And again, as I said, a conservative treatment, motivated by scoping calculations, shows main steam line breaks have a small effect anyway.

So looking at the effect of system characteristics on thermal hydraulic response, we're going to look at power level of transient initiation, break location inside or outside of containment, feedwater flow isolation and timing of --

MR. ROSEN: High-head safety injection.

MR. KIRK: Yes. Power level effects are minimal. In the cooldown rate, generally, you'd expect the hot zero power transient in red to have a faster cooling rate than the full power transient in black. And indeed, that's true, but remember, this is a big break. This is bigger than any of the primary side breaks we modeled. You're at the point where the temperature is crashing down and so even though you initiate under hot zero power and it cools faster, for the failure frequencies, it just really doesn't matter.

DR. KRESS: I'm not sure what you mean by lack of heat on this slide. You mean like a stored energy or like a heat production due to decay energy. It's stored energy?

MR. KIRK: Yes, stored energy, I'm sorry.

MR. BESSETTE: Decay heat.

DR. KRESS: And decay heat also?

MR. BESSETTE: It's primarily decay heat because -- yeah, primarily decay heat. Basically, your initial system energy is quite close, whether you're at hot standby or full power. It's a little bit higher at full power, but you don't have the decay heat component as well.

DR. WALLIS: The fuel is a lot hotter.

MR. BESSETTE: Yes, but if you look at the total system energy at hot standby versus full power, it's not a lot different.

Decay heat is the more important factor here.

MR. KIRK: Looking at the break location effects, again, break outside of containment is -- I'm sorry, is less severe than break inside of containment because when you get the break inside containment you have to shut down the RCPs and so you get faster cooling in the primary.

Lack of feedwater isolation allows the temperature to continue to drop whereas once you isolate the feedwater, the temperature starts to rise again, so you're isolating there.

And then high head safety injection throttling allow obviously the pressure to drop sooner than it would if you didn't throttle.

However, not much of that matters at all because from the fracture calculations we learned that failures occur, if they occur, between 10 and 15 minutes into the transient. So going back, 10 to 15 minutes is 10 times 60, 600 to 900 seconds. So the second tick mark here, about 1000 seconds, and if you go back through these various effects, the only thing that's happening out to 1000 seconds is the initial cooling. So that means that break inside or outside of containment is going to have an effect as that affects the initial cooling rate, but not isolating feedwater as is it included in our model can have an effect because it's out beyond the time that the break has occurred and similarly with high head safety injection throttling, you're dropping the pressure, but the event is over anyway from a fracture perspective.

So that's a very important finding and tends to mean that all these differences in plant design, operator actions, automatic systems and so on don't really have a big influence on the through-wall cracking frequencies, said the first secondary bullet, but that things have changed the initial cooling rate like the power level and break location can have an effect on the through-wall cracking frequency albeit minor.

So again, we've got several factors that suggest the applicability of these results to PWRs, in general. We've got intentionally conservative model which we did not because we're nasty regulators, but simply because we realize it didn't matter much anyway.

We've got essentially no effective operator action credits because all the operator actions we've credited didn't happen until after the break had occurred. And it's the rapid cooldown that controls the vessel failure probability. It's so rapid, it's in the conduction limited regime and that really tends to mitigate plant specific factors. So you've got big breaks, intentional conservatisms and even with that, the failure probability is still low, relative to all the primary side events.

DR. WALLIS: Why is there just one point for Oconee?

MR. KIRK: Because Oconee never got --

that's the Oconee 1000 BFPY analyses. All the rest of the results are down here and they didn't even get on the scale.

DR. BONACA: Why didn't they get on the scale?

MR. KIRK: Because the embrittlement was so low all the other analyses are down here.

DR. BONACA: Okay.

MR. KIRK: And the failure probability is zero.

Differences from the previous analyses, relative to our previous analyses that we presented in February of 2003, we've got different numerical results with the same general trends, relative to the analyses that establish the basis for the current PTS rule. In Oconee and H.B. Robinson, MSLB was the most important transient, but that's because the medium large break LOCAs and the stuck open valves weren't modeled, so MSLB was pretty much all that was left.

In Calvert Cliffs, stuck open primary side valves were modeled and found to be more important than main steam line breaks consistent with these analyses.

So now we move on to stuck open valves in the secondary side. So steam supply system contains several valves to control the pressure. All those valves have opening areas that are much, much smaller than those in the main steam line which means the depressurization rate is going to be smaller and the cooling rate, consequently will be smaller. Other than that, the progress of stuck open valve transients on the secondary side is generally similar to MSLBs with the notable exception that all the valves are outside of containment, another factor that tends to limit their severity.

As you can see I'm saying less about the things that matter less.

Again, our model of stuck open secondary valves is not a best estimate, motivated by the fact that we thought it didn't matter. We tended to examine bounding cases and also we'll point out that the Palisades -- even though all of these analyses we didn't do a very -- as refined an analyses as we did say for primary side pipe breaks and stuck open valves and Palisades was even less refined than Oconee and Beaver Valley. In Palisades, more sequences were binned together. We needed higher initiated event frequencies as is shown here. And we made a conservative selection of transients to represent the bin.

So that means you'll see some contribution to the through-wall cracking frequency for Palisades, but we believe that's because of the intentionally conservative modeling, not because of anything that's inherently bad to Palisades.

Let's see, effects of valve opening area. Okay, so in the following slides, we're going to look at main steam line break transient for reference. Then we're going to look at all secondary valves stuck open, all together, and then one or two secondary valves stuck open. So main steam line break for reference. Here's the comparison of break inside containment, break outside of containment and we've got through-wall cracking frequencies in 10^{-5} to 10^{-8} regime.

Overlay on that all main steam safety valves stuck open. We get similar cooldown rate, similar bottom temperature, somewhat lower through-wall cracking frequencies.

And then with just one valve stuck open, again, we're stretching out the cooling rate because we're not depressurizing as fast and the minimum temperature is going higher to the point where it just doesn't matter.

So to summarize, stuck open secondary side valves, the through-wall cracking frequency contribution, stuck open secondary valves is negligible, except for that at Palisades where we got a small percentage contribution, but we believe that that contribution is due to the conservativeness of the model, not due to anything in particular at Palisades that makes it different.

Factors that suggest that these results apply to PWRs, in general, is that we've got a -- we've intentionally done a conservative model and we still get little to no contribution and that even something as bizarre as sticking open all the secondary side valves produces conditional probabilities of failure that are truly negligible relative to that produced by the dominant transient classes.

Again, comparison with previous analyses, no real differences from the results we presented you before and relative to those analyses that established the tech bases for the current rule, even though we've done a conservative analysis generally. It's been more refined than what was done before.

Okay, so now I've ignored all the other transient classes, just pure overfeed, feed and bleed, steam generator tube rupture and mixtures of failures in both primary and secondary system. In all cases, a combination of low probability of occurrence and low consequence combined to make the contribution of transients in those classes to through-wall cracking frequency either negligible or zero.

Now here's something we could argue a lot about. So I'll put a big disclaimer on it to say that this is an attempt to qualitatively collect together in one slide in what my wife would call a garish color scheme, all the information we presented in the last two hours. So we've looked at the various transient classes and looked at the factors that control the transient severity, the cooling rate, minimum temperature and the pressure and the transient likelihood and just categorized whether those classes of transients made large, small or essentially zero contributions of the through-wall cracking frequency.

And you can pour over this and again, these are judgments that are made relative to the information we had before us and we haven't really tried to do anything rigorous, but just tried to condense the results in a form that hopefully summarizes it all.

And I think main take away from this is that of the various factors, the minimum temperature and the likelihood are the most important things. Obviously, if things happen a lot, they're going to be more important than things that don't happen a lot and you need to go down to low temperatures to get failure probability. Then the cooling rate is important and then finally, pressure. But of course, it's the combination of all these things that matter. But again, we've said it before, primary side breaks and stuck open valves that later reclose make up almost everything that's going on. We've got a small contribution to main steam line break because we've used a conservative modeling approach and nothing else matters.

So to put all that on one slide and finally now compare the through-wall cracking frequency attributable to the different transient classes. And what I've done here is I've just drawn upper bound curves to the plant-specific results in an attempt to draw a comparison and we find -- we've said all these things before. Primary side events matter, main steam line break matters a little, but we believe only because we've taken a conservative modeling approach. If we were to refine that, I think you'd see the contribution of main steam line break actually go down quite a bit.

And again, those -- the differences between primary side and secondary side is mostly tied up in the fact that you just can't drive the temperature in the primary for a secondary side failure below the boiling point of water and remember, all of these have in them the conservatism that even if the break is in containment, we're boiling at 212.

This is a slide I used in the intro and I think I said it all already, so I'll spare you me going through it again. But I will focus on the last one in that the next section we're about to go to is what we call generalization. But I do want to point out that even going through the plant-specific analyses, we found factors that suggest strongly that these analyses can be applied to develop a PTS screening criteria that applies to PWRs, in general. And that's because the transients that contribute the most to the through-wall cracking frequency have for all intents and purposes, similar occurrence rates and similar severity across the plants, even though we've modeled operator actions for the dominant transients where they either have no influence or small influence. The PWR designs are similar and we've got a fair number of conservatisms left in our model.

DR. BONACA: Yes, I must say that I still have heartburn on this issue of secondary side breaks for the following reason. We debated it a year ago, again, and the issue that was driven home was a long discussion on the emergency operating procedures, why the operator would not allow the feedwater to continue to run indefinitely.

We discussed at length all these issues and those were central to why the main steam line break had become the top dog in 1980, especially for the BLM requirement, had become a no-nevermind issue.

Now today on slide 60 says Oconee MSLB was most important because LOCAs and stuck-opens were not modeled.

They were not modeled because they never assumed isolation of main feedwater. They kept feeding, they kept cooling, so they made a transient which was very artificial. I agree with that. And therefore they thought the LOCA will never be as severe as that one.

So it wasn't they ignored. They simply made the steam line break so severe, so limiting, they couldn't make anything more limiting than that. And that -- and so I listened to this presentation a year ago and I bought it, I bought all these procedures, isolation and so on and so forth. Now I'm told that

that wasn't the issue. The issue is all PWRs behave similarly and all you need is to look at the initial cooldown and that's it. So there is a change in the basis that you're presenting to me and it troubles me a little bit.

I really would appreciate it if you would look back in the record.

MR. KIRK: Okay, we'll do that.

DR. BONACA: To what was presented because it's different from now and I think you have to have a consistent basis for eliminating the most severe transient that has caused 20 years of heartburn in this industry from the board. That's gone.

And that's an important issue because if it hadn't gone away, it would still be here giving us problems.

Any way --

MR. KIRK: The staff can talk afterwards and maybe we'll get a better answer to your question.

DR. BONACA: Sure. But again, all you have to do is go back to the record and the presentations we have. The gentleman, I can't remember --

MR. KIRK: That was Alan Kolasckowski.

DR. BONACA: Exactly.

CHAIRMAN SHACK: And it may be that he included all that in his modeling, thought that made the difference.

If you change something and you get a difference, you assume that was the reason for the difference.

MR. KIRK: I hope I'm correct in saying that neither Alan nor I have said anything that's wrong and I'm hoping that we're looking at two different parts of the elephant and --

DR. BONACA: Maybe.

MR. KIRK: We'll try to get a response to that tomorrow.

DR. BONACA: He clearly spoke of the B&W, the Oconee plant and in fact, he spoke very clearly of the operating procedures, interviews they had with the operators, the training they're having and all these things being affected negating the event that in 1980 became the basis for PTS concern. It was an B&W with assumptions of no isolation of feedwater isolation support.

MR. KIRK: I think in all fairness we did mention at that time the fact that just from a fracture perspective the secondary side events have to be less severe simply because you can't go to a lower temperature.

MR. WHITEHEAD: Donnie Whitehead. Let me see if I can answer that, your question a little bit. I think part of what we're seeing here is we're looking at two different aspects of the problems. I think what Alan Kolasckowski was talking about was that the frequency of the occurrence of secondary side problems, main steam line break, if you account for the changes in operational procedures and actually give credit to the operators for being able to perform some of the actions that they can and will perform, that would tend to drive the frequency of the occurrence of what we call the initiator for the PTS bin, that would drive that down, but not only does that happen. And we get lower frequencies than we had originally from the original analyses. But I think we've also found that from a fracture mechanics point of view, we see that the events that are analyzed now are not as important from a fracture mechanics point of view as they were perceived to be during the original analyses back in the early 1980s. And it's the combination of those two that really make secondary side breaks really particularly all that important from a PTS point of view.

DR. BONACA: I'm only saying that I think you have to go back to the record to look at it because I mean you can look around, all these issues that come together, but the event that was the driver of the analysis has been eliminated from the table. And for good reasons, probably. But the reasons that were presented a year ago are different from what I heard today and so I want to make sure that since it is a major step, I mean the very driver of all this pain and suffering for the last 24 years has been eliminated as the driver.

I think it's interesting that one of PR comments was essentially focused on Rancho Seco. Why is it gone? And you have some answers there which are different from those even here.

But anyway, I think I have belabored that enough, but I think it has to be looked at.

DR. NOURBAKHSH: We don't have a hard copy of this presentation.

MR. KIRK: That's all right. It's a short one.

Okay, this is just the intro to what we've called the generalization chapter or Chapter 9. The question that we're trying to address is to what extent can our detailed analysis of pressurized thermal shock at these three specific plants be required to develop a screening limit that our colleagues in NRR could use to apply, in general, to assess all PWRs operating in the U.S.

So our methodology is to perform sensitivity studies on our thermal hydraulics and PFM models, both to assess robustness of those models and to assess the applicability of those models to the assessment of PWRs, in general.

We've also looked at plant design and operational features of the three study plants that are the key contributors to PTS risk and seeing how those design and operational features either represent or bound those features in the general PWR population.

And finally, we've looked at the question of if there's a significant contribution to PTS risk posed by external initiating events like earthquakes and fires that we've ignored, and I'll spare you the rest of the details because we just said it. But I think it's also important to remember what we just went through and that's that our baseline analyses is already demonstrated that there are many factors that suggest that our results should be expected to apply to PWRs in general. And we've just gone over that.

So with that, by way of introduction, I'd like to invite Dave Bessette up to do -- I think Dave is up first, PRA? Who ever wants to come up.

DR. WALLIS: This is on sensitivity studies of thermal hydraulics? Is that where we are?

MR. KIRK: Yes. What's on the agenda? I don't have the agenda in front of me. Okay, then it's Don.

MR. WHITEHEAD: As Mark indicated what I'm going to talk about is basically the generalization approach that we used.

DR. WALLIS: Is this something we have in the handout?

MR. ROSEN: It's on the disk they sent us.

DR. WALLIS: Which one?

CHAIRMAN SHACK: It's in the one with the agenda on the cover.

DR. WALLIS: The one with all the pages 1s in it?

CHAIRMAN SHACK: Yes.

(Laughter.)

DR. WALLIS: It's the second page one?

CHAIRMAN SHACK: Yes, the second page one.

DR. WALLIS: Generalization. I don't like all these slides entitled judgmental analysis. Maybe you'll explain what that means.

CHAIRMAN SHACK: We could be here for a

long time once we hit those.

MR. ROSEN: Qualitative PRA.

MR. WHITEHEAD: The objective of the generalization approach that we took was basically to determine whether or not the design and operational features that were key contributors to the risks that we identified in the detailed analyses, whether or not those would vary significantly enough amongst the rest of the plants in the industry to whether or not -- whether or not they would vary enough such that what we had identified from the detailed studies would no longer be valid for the plants in general.

And we did this generalization work by first of all identifying a set of PWRs that have, if you will, they're close to the current rule, the current screening baseline for PTS. And we wanted to look to see whether or not those plants or at least a subset of those plants, if we look at what was important from the detailed analysis plants, whether or not conditions, operator actions, temperatures of various water injection sources, things like that, whether or not they would vary enough that we could -- we would have a problem with any generalization plant when it came to trying to extrapolate the results that we had to determine for our plants that we had looked at in detail.

So what we did was we developed a questionnaire that we asked various utility members to provide us information from and for which we were eternally grateful. It was one of the good things about this process was the cooperation that we had from the utilities and I believe it was EPRI who was responsible for helping us to get some of the information.

We used that information that we collected from the questionnaires and analyzed it basically to determine whether or not the results from the detailed analyses would be applicable to the additional PWRs. And we finally determined whether the generalization plants could be bounded by the detailed analysis plants.

This slide just basically gives you a listing of the plants that we looked at, the ones that we looked at, in detail are in blue; the ones that we looked at from a generalization point of view are in the - -I guess the yellow color. And you can see that we have corresponding plants for each of the vendor, NSSS vendor types. We have three Westinghouse and one each for B&W and Combustion Engineering.

So we have plants that are similar from

the NSSS vendor point of view and typically we try to choose plants that were high on the parameter that we used to identify the most important plants.

MR. ROSEN: Which was?

MR. WHITEHEAD: Which was at this point in time, this was -- this list was generated approximately two years ago was RTndt with an irradiated shift of 40 degrees at -- I think this was done at end of life, is that correct?

CHAIRMAN SHACK: End of license.

MR. WHITEHEAD: End of license.

MR. ROSEN: Wait a minute, RT, a positive, ndt? That was the only criterion? It had to be positive?

MR. WHITEHEAD: It's just a ranking.

MR. ROSEN: Okay, when I read that report it didn't have all the plants, all the PWRs on it. It only had like 30 of them.

MR. KIRK: That's because all the rest of them were lower.

MR. ROSEN: Uh-huh.

MR. WHITEHEAD: Right. I'm just showing you the ranking here, a list here --

MR. ROSEN: You're showing us a list that's even abbreviated from the report list and the report list was incomplete.

MR. WHITEHEAD: That's correct. The reason why we didn't show any of the ones below that was because the lowest one that we looked at was Oconee and the values that we were getting for Oconee were -- from a through-wall cracking frequency point of view
-- were extremely small and so it was felt that looking at any plant that would be ranked below Oconee would not give us any new and insightful information. So we tried to pick our plants from the top portion of this ranking because those are the most embrittled plants, if you will.

MR. ROSEN: But all the rest of them will still -- this will apply to generalized --

MR. WHITEHEAD: Yes. If we can generalize to the ones at the top of the list, then the ones at the bottom of the list should be no problem at all.

Basically, in the questionnaire development, we used the insights that we gained from our three plant specific analyses. We focused and collected information on five general event types: secondary breaches, secondary overfeeds, LOCA types, PORV- and SRV-related events and feed and bleed related events.

We requested information on 28 generalization issues and we were able to obtain information from that.

The process that we used was a two-step process and it is truly a judgmental process. Step one, we produced separate PRA/HRA and then TH judgmental analyses for the information that we obtained and then taking the insights that we gained from that judgmental process, we combined them to produce an overall observation and final conclusion as to the generalization to all of the plants.

DR. WALLIS: What is a judgmental analysis?

MR. WHITEHEAD: A judgmental analysis that we used was basically to pull together the engineering insights that we had gained from doing the detailed calculations, doing the detailed probabilistic calculations, the PRA calculations, the determination of the frequency of each individual bin, determining from a TH point of view the expected response given the changes that we had based upon the information or the similarity in response that we had given the information that we obtained from the --

DR. WALLIS: So it's kind of extrapolation?

MR. WHITEHEAD: It's an extrapolation where we didn't really actually go back and physically run the analyses through the models, except for one case. There was one case where we found that by a combination of both frequency of the bin and the thermal hydraulic response that we couldn't eliminate that one and that one we actually did a surrogate type of analysis on and we were able then to make a judgment, a final judgment as to the importance of that one, but I'll talk about that a little bit later.

But this is basically applying engineering knowledge and judgment as to -- given that you have the same types -- for example, for LOCA frequencies, large and medium break LOCAs, the frequencies that we used for the Oconee and the Beaver Valley analyses are generic frequencies. We would expect there to be no reason why those frequencies would be different from one plant to the next. So therefore, we would conclude that from a frequency point of view, all large and medium break LOCAs should be the same regardless of which plant you're looking at.

So it was those types of judgments and analyses that were being done. Except only in the one case did we do anything that was, if you will, a detailed calculation.

Let's go through each of the sets of information that we collected. I'll talk about, first of all, the PRA/HRA judgments that were made and then I will go through the process that was used on the thermal hydraulic side and then we will put together both those and see what happens at the end.

For the secondary breaches, we had two issues or actually we had only one issue where we thought that there might possibly be some difference between the plants. And this was issue 7 which is basically the auto isolation of the turbine-driven aux feedwater pump. This had the potential to be worse for one of the generalization plants, the TMI plant.

However, when we combined that one generalization issue with other issues that were collected, Generic Issue 3 and 4 which are respectively the procedures associated with secondary breaches and the training associated with secondary breaches, we felt that the importance of the potential difference in Generic Issue 7 would be minimal. And so therefore, from a PRA/HRA point of view, we don't really expect there to be any real difference in the secondary breach set of scenarios.

In the secondary overfeed, overfeeds and the LOCA-related issues, these were really not PRA/HRA issues. They more or less dealt with the things that would have affected the thermal hydraulics calculations such thing as main feedwater and aux feedwater capabilities, the nominal steam generator inventory, the different feedwater temperatures that could be introduced into the reactor vessel, things like the injection temperature of the primary water, recirculation temperatures, flows and pressures of the injection sources. Those are not things that we would have looked at from a PRA point of view, but they were looked at on the thermal hydraulic side of the analysis.

For the PRV/SRV-related issue, we had two Generic Issues, 20 and 21; 20 being the number, size and operational features of the valves, and 21, the instrumentation indicating the status of the valves. We found a potential difference there. We performed some subsequent investigation and basically found that the potential differences associated with Generic Issue 20 which really affected the probability of sticking open and subsequent reclosure of valve, we found that we could resolve the issue and thus we basically eliminated it from consideration. And so the final judgment for General Issue 21 which is the human error probability that's associated with the failure probability, the throttle high pressure injection, we found that this possibly could have a factor of at most about a factor of five higher than the one that we calculated for Beaver Valley.

Basically, it came down to final -- this one had to do with the fact that there was less clear indication in the information that we got from Salem that would lead us to believe that we would have the same human error probability assigned to the particular event, failure to throttle, than we were able to assign for Beaver Valley.

For feed and bleed-related issues, the only one that had any potential of being different would be the one that has to do with the unavailability of the aux feedwater or emergency feedwater and this was only for Fort Calhoun and going through the process of looking at the what the differences were, we found that at most we might expect that the unavailability for aux feedwater at Fort Calhoun might increase by a factor of three.

Getting into -- looking at the information from a thermal hydraulics point of view, it was decided because -- well, the thermal hydraulics analysis looked at this in a little bit different light than the way we looked at it from a PRA point of view. And the reason for that was because it lent itself better to collapsing some of the information into a different grouping that we had actually solicited the information from the utilities. And this was based upon an examination of the dominant types of scenarios that are important. We looked at those in more detail than we did for the scenarios that were less important.

The TH characteristics of the scenarios in the group, we had to understand what was the differences amongst the four groups that we collapsed this into and we also had to understand the systems and how those systems determine the downcomer fluid temperature behavior.

Basically, we simply collapsed the five general scenarios that we had into four. These were the large break or large diameter pipe breaks, the small and medium diameter pipe breaks, stuck open valves in the primary system that reclosed and then the fourth group were the main steam line breaks and other secondary side failures.

Group 1, the large diameter pipe breaks, we really found no differences in the plant system designs that could cause significant differences in the downcomer fluid temperature from a TH perspective.

While it's possible that there will be some temperature variations due to power level, these were not necessarily felt to be particularly all that important for these large diameter pipe breaks because basically what happens with the breaks in this range, they're sufficiently large such that the water that's being injected into the system due to both the high pressure and low pressure injection and the injection from the safety injection tanks will basically largely govern the downcomer fluid temperature.

So the injection of water from the higher pressure and low pressure systems and the temperatures associated with those injections that are important in the large break LOCAs, as well as the fact that I believe as was mentioned, we're in a regime where if a blowdown is happening so fast that we're conduction limited in our cooldown.

The small and medium diameter group, Group 2, the conclusions that we reached for this one is that all generalization plants should basically have depressurization in cooldown rates that are comparable to their corresponding detailed analysis plants.

Here, the points that are important there, the break flow and the energy released through the break will govern the rate of cooldown and depressurization. We do expect for the hot full power cases, the rate of cooldown and depressurization would be slower for reactor systems that operate at a higher thermal power than those that operate at a lower thermal power.

However, it's important to note that the flow capacities of the injection systems, the high pressure injection systems, particularly at Fort Calhoun, which has a lower thermal rating than its detailed analysis plant, Palisades, is only half the flow capacity. So we have less energy, but we also have less flow from the systems that are important to determining the cooldown rates.

And differences in cooldown and depressurization rates should have less of an impact on the downcomer temperature if the transients begin from hot zero power conditions than they would if they began at hot power.

Okay, now feed and bleed LOCAs and LOCA is in quotes here, should have thermal hydraulic behaviors that were similar to the smaller end of the pipe break LOCA category, if you will. So we were able to collapse the feed and bleed LOCAs into this group here and the same things that we've said about the pipe breaks above would be characteristic of the feed and bleed LOCAs here also.

Group 3, stuck open valves and a primary that reclose. Basically, we found that all generalization plants, except for Fort Calhoun, will be warmer than their corresponding detailed analysis plants. And we'll see that Fort Calhoun showed up both here and at TH and it also showed up in the fracture -- the PRA part of it. This is the one that we had to look at in more detail.

Group 4, main steam line breaks and other secondary side failures, basically, here for the steam line breaks, the generalization plants should be warmer or about the same as their corresponding detailed analysis plants. For simple overfeeds, the plant-specific analyses show that PTS challenges, that the PTS challenge associated with completely filled steam generators is not significant and that's something that Mark has already alluded to.

These types of events, where we just had simple overfeeds, are just simply not important to the analysis.

Okay, if we combine both the PRA and the thermal hydraulics observations that we had for each of the groups, for Group 1, we found that there were no real differences expected from a PRA/HRA point of view. And effectively, that there would be no differences from a TH point of view. We conclude that the generalization plants could either be bounded or represented by our detailed analysis plants. We found nothing to indicate that there would be any real differences for the larger diameter pipe breaks.

For Group 2, from the PRA/HRA perspective, no real differences were found. We did find that for the feed and bleed LOCAs, the only difference that might affect the frequency for the Combustion Engineering generalization plant. However, this difference was estimated to be only about a factor of three higher for this particular type of scenario. And it was judged that this factor of three increase wouldn't really affect the overall generalization of the plants based upon the detailed analysis results because feed and bleed LOCAs in our detailed analysis just simply were not particularly all that important. And so even if you increased them by a factor of three, it's not important to begin with, raised by a factor of three is still not going to be particularly all that important.

From the TH perspective, all generalization plants should have depressurization and cool-down rates that are comparable to their corresponding detailed analysis plants. Thus, we would conclude that again, the generalization plants can be bounded by what we -- the information that we have on our detailed analysis plants.

Group 3, this one was the interesting one. This one posed the most challenge for us. From the PRA perspective, we didn't find any real difference in the way the accident scenarios could progress. However, we did find that we could have a frequency difference associated with the Westinghouse plant that we looked at, the generalization plant Salem. There could be a factor of five increase associated with the frequency.

The importance of this factor of five increase was approximated by taking the detailed analysis plant, Beaver Valley, modifying the failure probability for that particular basic event in the model, requantifying the results. Once you do that, the total point estimate for the Beaver Valley increases by a factor -- 2 percent change. So we didn't -- there was really nothing important there.

However, for Fort Calhoun, it was initially a different story. We had both -- for Fort Calhoun, we had an expected downcomer temperature that could be colder than its corresponding detailed analysis plant at Palisades.

We performed a surrogate analysis using the Palisades model and overlaid on the Palisades model the differences in the size of the valves and the differences in the flow rates of the injection systems.

Because what we had here was -- we had a case where Fort Calhoun, which is a plant that has a lower thermal rating than its corresponding detailed analysis plant, happened to have larger SRVs so if -- than the detailed analysis plant. So if a valve at Fort Calhoun were to open, one would rightfully expect that the cooldown rate would actually be worse for Fort Calhoun than it would be for Palisades.

Since the stuck-open valves that reclose was one of the important groups that we had identified from the detailed analysis, we felt it prudent to do a surrogate analysis where we took the Palisades model, modified it to reflect conditions that, you know, we might expect from Fort Calhoun, and then propagate that TH information through FAVOR, again using the Palisades -- the Palisades model in FAVOR and see what would happen with the conditional probability of through-wall cracking.

What we found out was that, yes, indeed, if you look at this particular analysis here for this -- for this set of conditions, that it's having a larger stuck-open valve that subsequently recloses -- we found that you could result in much higher through-wall cracking frequencies for Fort Calhoun than you could for Palisades for the same sequences. In some cases, many orders of magnitude greater.

However, if you put it all together, the -- in an absolute sense, the through-wall cracking frequency was still low in the approximately 10^{-8} so, you know, in the end even though you could have some, you know, quite large difference between, you know, one plant and the other, the absolute value, the 10^{-8} value is still low and so basically we assumed that Fort Calhoun can be bounded by Palisades.

Group Three -- well, basically, this -- that's what I just said. You know we could combine both the PRA for Salem and the thermal hydraulics part for Fort Calhoun -- we basically think that, you know, the plants can be bounded.

For Group Four, no real differences from a PRA/HRA perspective. From a TH perspective, we expect that we can bound these. The worst is that the temperature, the downcomer temperatures would be about the same, however, in some cases they could actually be warmer than the temperatures that we calculated for our detailed analysis plan.

Okay, this all put together is looking at both the PRA and the HRA part of it, considering what we did with the Group 3 for the stuck-open valve that could reclose case. Overall conclusion is that the generalization results indicate that our detailed analysis plants can be used to bound the generalization plants that we looked and thus, by inference, all of the remaining PWRs because the ones that we looked were typically the highest ones on the list and so if we can bound those, then we would expect to be able to bound the ones that would be lower on the list.

DR. BONACA: I have a question on the HRA.

MR. WHITEHEAD: Okay.

DR. BONACA: I mean I have already spoken enough about system differences and I must be coming from a different perspective, but the HRA also is an issue, it seems to me -- we talked about the fact that some B&W plants do not have automatic isolation of main feedwater, of steam -- steam isolation valves.

And they have to rely on operator action to isolate a steam flow. And I think there are differences of that kind on the feedwater side.

We also know from presentation we had last year that it was significant reliance on operator action consistent with EOPs. I don't think that those are true of other PWRs which are more automatic.

So I don't understand how we can conclude that from an HRA perspective no differences were found. I mean -- the significant differences between operator action required in some plants and not required in others, wouldn't it make a difference on the HRA?

MR. WHITEHEAD: There obviously are differences in the HRA values that would be estimated, depending upon the different, let's say NSSS vendors. What the generalization process did was look at what was important and what the expected, if you will, HRA human reliability estimates would be within a particular class of plant, that is, if we wanted to look at BNW, we looked at what did we know about the plant that we looked at in our detailed analysis and how did that compare with the information that we collected from our generalization plants.

If in looking at that information we saw no reason to see any difference in what we would calculate for an HEP for the generalization plant than we did for the detailed analysis plant, we concluded effectively there would be no real difference within that plant.

Now that's not to say that, you know, there might not be some actual real difference in the human error probabilities that are calculated for B&W versus Westinghouse versus CE plants. But within B&W plants, we think that our detailed analysis plant bounds the one that we looked at in the generalization process.

Within the Westinghouse set of plants, we believe that the detailed analysis plant that we looked at bounds the -- I think it's three that we looked at on the Westinghouse side, and subsequently the same thing for the Combustion Engineering. So I mean the generalization process tried to account for the differences in the plants and looked at them within NSSS vendor type.

DR. BONACA: But you say it does it by inference that remind them of PWRs. You are making a further step. You're saying that all PWRs pretty much from the perspective of this concern behaves similarly.

MR. WHITEHEAD: Again --

DR. BONACA: Or the conclusions that you can draw is the same.

MR. WHITEHEAD: The conclusion would be the same within a particular NSSS vendor class and since we believe that all three NSSS vendor classes are bounded by what we did in the detailed analysis, and we looked at the most important plants in the generalization process, we would suspect that the same would hold for any of the other remaining plants in the various NSSS categories that what we looked at would bound them.

DR. BONACA: The previous slide, what do you mean the outcome of temperature -- if you could go -- or warmer. At what time? The outcome of temperature changes, as opposed to the transient, so --

MR. WHITEHEAD: Yes.

DR. BONACA: Are the same or warmer? When? How? Where?

MR. WHITEHEAD: We would expect that the trace, the time history trace that we would have for the downcomer temperature for Westinghouse and Combustion Engineering to be about the same as we had for the trace we had for the detailed analysis plants which, let's see --

DR. BONACA: Okay, I see what you mean.

MR. WHITEHEAD: And subsequently for the

B&W plant, we actually expect that the trace would be slightly warmer than what we calculated and looked at in the detailed analysis plant.

DR. BONACA: So less severe?

MR. WHITEHEAD: Less severe, yes.

DR. BONACA: By the cooldown rate --

MR. WHITEHEAD: The cooldown rate would be less severe, therefore, everything else being equal, you would expect that fracture mechanics-wise, there would be less of a problem for this particular case here, would be less of a problem at the generalization BWR plant than there would be for the detailed analysis plant.

DR. BONACA: Thank you.

CHAIRMAN SHACK: Other questions? Allen, I was going to suggest that everybody can be here tomorrow, that we actually break at this point and just finish up tomorrow morning. I think everybody would be fresher in the morning.

MR. HISER: How much time do we have in the morning?

DR. NOURBAKHSH: You have until 11:45.

MR. HISER: Because I'm looking at about two hours yet today on the agenda and we had about an hour and a half of the PRA or the peer review, so it's three and a half hours there. That would take us right up to noon.

CHAIRMAN SHACK: Do you want to take another half hour tonight then?

MR. HISER: I think we should probably get done what we can tonight.

DR. WALLIS: What is next?

MR. HISER: Dave's -- sensitivity.

MR. BESSETTE: I can do it now since you're all worn out and thermal hydraulic sensitivity and then PFM sensitivity.

CHAIRMAN SHACK: We'll take on Dave, you can take everybody tuckered out.

(Laughter.)

You don't want to do this the first thing in the morning.

DR. WALLIS: He doesn't want to do it at all.

MR. BESSETTE: You might have to help me find my presentation on here.

DR. WALLIS: The last thing we hear before dinner is what we remember.

(Laughter.)

MR. SIEBER: It helps us digest. More acid.

MR. BESSETTE: Okay, we did a fair number of sensitivity studies, generally in part, motivated by peer review comments, so this presentation also relates to the last agenda item which is peer review comments.

So these studies included heat transfer, which I talked about earlier today. I'm not going to go back to it again.

The cooldown rate sensitivity study also combined heat transfer which I will talk about. We looked at comparing 2D downcomer nodalization versus 1D downcomer nodalization.

MR. SIEBER: Dave, could you speak into the mic?

MR. BESSETTE: I'll look at this print instead of that.

MR. SIEBER: All right.

MR. BESSETTE: The 2D downcomer nodalization versus 1D and the use of damping in the cold legs to counteract the numerical effects.

DR. WALLIS: Is this where you're going to talk about momentum?

MR. BESSETTE: I'm going to touch on momentum here, yes.

I just wanted to show this. This is a

point similar to what Mark showed in his presentation. This is a conditional probability of failure versus break size and I just wanted to illustrate again the fact that once you get beyond a break of about 6 inches, the CPF remains about constant after that. And the breaks smaller than about six inches, you can see there's quite a large sensitivity, about within that break range. And this kind of -- we felt how we subdivided our three basic categories of small breaks, medium breaks and large breaks, into smaller categories.

For small breaks, breaks less than four inches, we represented that range by five individual RELAP runs; four to eight inch by three or so RELAP runs; and beyond eight inch by one RELAP run.

One of the points to make here is that it certainly, from this, seems that you're reaching asymptotic maximum of probability to vessel failure, so in a sense you can bound your overall LOCA risk by taking the LOCA probability which is about 10^{-3} times the probability of vessel failure which 10^{-4} and you get a bounding number of about 10^{-7} for risk.

DR. WALLIS: You have pretty high LOCA probabilities there.

MR. SIEBER: Yes.

MR. BESSETTE: This is for the entire --

MR. SIEBER: All kinds of LOCAs.

MR. BESSETTE: All kinds of LOCAs and I didn't check to make sure I have the latest. These numbers, I think were accurate as of May. Okay, those are the latest.

DR. WALLIS: The latest, large break LOCA 5 times 10^{-4}?

MR. BESSETTE: I think so.

MR. KIRK: Those are the same data that we showed earlier. Check the slide.

MR. SIEBER: They can only use what's on the record now as opposed to the proposed --

CHAIRMAN SHACK: It's a six-inch break.

DR. DENNING: What is "uncertainties are bounded"? How are we supposed to really interpret that?

MR. BESSETTE: Let's say for small breaks, for example, the results can be sensitive to many things include break size and so on. But these -- you have uncertainties in very small numbers. You might have a large uncertainty in a number that's very small and so rather than worrying about each individual contribution to uncertainty and say how do I know, you know, how do I know that I know this, you can do something like what Graham proposed.

You know, why don't you just use an infinite heat transfer coefficient and bound the result? Well, I'm trying to say here is rather than going into all the details of uncertainty, you can say well, I'll just take this S on top of maximum for CPF multiply it by our probability number and get a bounding number for failure.

MR. SIEBER: But you don't know this uncertainty in CPF.

MR. BESSETTE: What I'll show, we did a lot of sensitivity studies in this range and nothing seemed to affect the answer because again, the overall event is so dominated by a large flows out the break in the large ECCS flow.

DR. WALLIS: It depends what's in there. I mean there's uncertainty in the flaw distribution, things like that.

MR. BESSETTE: Well, this is looking at -- well, that's true. I think that's what's in here.

DR. DENNING: Are you limiting this to a thermal hydraulic perspective in saying --

MR. BESSETTE: That's what I'm trying to guess -- it's from a thermal hydraulic perspective. The TH parameters that affect temperature and pressure and so on, the uncertainties in those parameters don't seem to impact the probability of vessel failure.

DR. WALLIS: It's a very simple problem. You just cooldown, you match the pressure pretty well. And the conduction in the steel limits the thermal shock.

MR. BESSETTE: Yes. That's the implication is that we get down to a very simple problem.

DR. DENNING: But then the part that isn't in there is how well do we really know probabilistic fracture mechanics?

MR. SIEBER: Yes, that's the next topic.

DR. WALLIS: We're going to get to that. That's the bit that's going to keep us awake.

DR. DENNING: But then you're bounded by that 10^{-4}.

MR. BESSETTE: The peer review group liked it a look so I thought I ought to show it to you guys.

We did sensitivity studies to look at the cooldown rate and we took a stuck-open pressurized SRV transient which is Palisades Case 65 and we represented the cooldown rate by this -- you see the simple exponential decay equation and this, by the way, the Creare people did the same sort of thing in the early 1980s when they ran their experiments. And they were able to fit their cooldown -- the cooldown data to this thing --

DR. WALLIS: By varying beta?

MR. BESSETTE: By varying beta. So the bottom line, I'll show you --

DR. NOURBAKHSH: That beta was inconstant based on the flow and volume of the mixed volume.

MR. BESSETTE: Yes. Now I'll show you what we did. But in fact, to show you again the simplicity of the problem, you can represent the system cooldown, whoops. If you don't want to use RELAP, you can get the approximation of the system cooldown by this equation.

This was a study we did. The curve that has some --

DR. WALLIS: You can probably get a solution to the temperature of transient in the steel, too.

MR. BESSETTE: Yes. The curve that has some squiggles to it is the actual RELAP 5 calculation, is beta value of -- here of 0.00029 is the best fit to the RELAP calculation and using that as a basis, we varied the value of beta in both directions. To get a spread and cooldowns that encompass the uncertainty in the RELAP predictions of temperature that I showed earlier, the RELAP had an accuracy of seven degrees Fahrenheit and a standard deviation of 18 degrees Fahrenheit. So I had a 2 Sigma level, this range encompassed that uncertainty.

DR. RANSOM: Dave, I have a question. What information is fed to FAVOR to determine the possibility of vessel failure from say the thermal hydraulic calculations? I know you've said the heat transfer coefficient and downcomer temperature, but what about the distribution of temperatures through the wall? Does FAVOR do its own conduction?

MR. BESSETTE: Yes, FAVOR does its own conduction solution.

DR. RANSOM: Okay, so you trust the gradients that are predicted, I guess.

I'm a little concerned about the kind of nodalization they use for the vessel wall?

MR. KIRK: FAVOR has been benchmarked against ABAQUS.

DR. RANSOM: Pardon?

MR. KIRK: FAVOR has been benchmarked against ABAQUS and reported as a NUREG CR.

DR. RANSOM: Okay, good.

CHAIRMAN SHACK: There's one calculation

one probably believes is the heat conduction in the metal, right?

MR. BESSETTE: So using a family of curves, we got -- on top of that we vary heat transfer coefficient by factors of 0.7 and 1.56.

DR. KRESS: Those seem like strange numbers to me. Is there a basis for that?

MR. BESSETTE: The 0.7 comes with the same basic uncertainty of plus or minus 30 percent that you often see for heat transfer. The 1.56 is 1.2 times 1.3. So what it is is the -- if you remember, I said that the Petukhov-Catton gives about a 20 percent higher heat transfer than RELAP, so if I introduce this 1.2 assay as a bias, and then put an uncertainty on top of that, that's where the 1.56 comes from.

DR. WALLIS: These numbers aren't very impressive. In the previous slide you said an order of magnitude change?

MR. BESSETTE: Yes, I wanted to point that out.

DR. WALLIS: The previous slide you've got an order of magnitude change. What was the one that said there was an order of magnitude change?

MR. BESSETTE: Yes, okay, for the range of cooldowns we looked at which is on the following slide

--

DR. WALLIS: This is such a big effect.

MR. BESSETTE: We see a variation in CPF between --

DR. WALLIS: Factor of 10 from these transients?

MR. BESSETTE: Between this bottom curve and the top curve.

DR. WALLIS: Factor of 10?

MR. BESSETTE: It's a factor of 10.

DR. WALLIS: But some transients are much steeper than that.

MR. BESSETTE: I wanted to show --

DR. WALLIS: Maybe that's what it is.

MR. BESSETTE: I wanted to show this to show -- again, to illustrate which I've been saying here and there is that the cooldown transient is more significant than the uncertainty in the heat transfer coefficient.

That's why I keep saying in terms of ranking these three parameters as temperature, pressure and then heat transfer coefficient.

DR. WALLIS: Assuming that one of those equations is really relevant to predicting heat transfer.

MR. BESSETTE: We also looked at the use of the 2d downcomer nodalization.

DR. WALLIS: See, that's the thing that's missing from all. I'd like to see a comparison between these heat transfer correlations and some data for downcomers.

MR. BESSETTE: Certainly, there's been comparisons done by Dittus-Boelter with heat -- this type of data which shows good agreement. So RELAP in Dittus-Boelter, they say well, there's no reason to disbeleive RELAP as long as RELAP calculates are anything else correctly. What does it need to calculate correctly? You need to calculate temperature and velocity. Fluid temperature and velocity.

DR. WALLIS: Velocity is an average over the whole downcomer.

MR. BESSETTE: That's for sort of higher flow rates. Once we get into stagnation, velocity is not even there any more.

DR. WALLIS: It predicts no heat transfer.

MR. BESSETTE: It's basically temperature, it calculates wall temperature and fluid temperature and it calculates the thermal physical properties from the temperatures.

So in a downcomer issue is RELAP being a one-day code doesn't have cross flow momentum.

DR. WALLIS: I think if you put it in, you get into trouble.

MR. BESSETTE: So again, we use the same set of 12 Palisades transients I've been talking about and we compare the 1D model with the standard 2D model that we use for all the calculations.

DR. WALLIS: Is this the one where you put momentum in. You've got a fluctuation of a factor of 10,000 or something? Is there some enormous -- where did I read that? In the report, summary report?

MR. BESSETTE: I'm not sure.

DR. WALLIS: The APEX report.

MR. BESSETTE: When we compared to 1D results with the 2D results, what is that for a hot side break, for a hot leg breaks, main steam line breaks, we got similar values for a CPF between the two sets of calculations.

For the cold leg breaks, we found the lower values of CPF using 1D downcomer compared to the 2D and I attribute that difference to the difference in the calculated EEC bypass, the 1D downcomer has a tendency to bypass more of the flow from the impact cold leg, out of the broken cold leg.

DR. RANSOM: When you say 1D, you mean 5, 6 stack sources as one?

MR. BESSETTE: That's right, one single channel for the whole downcomer versus parallel channels.

So we no disadvantages in using a 2D and we see -- we did comparisons with terminal data. This is the same LOFT experiment I showed earlier. The 4-inch cold leg break. It shows the results for 1D and 2D downcomer.

The black is the 1D and you see on average it's somewhat warmer than the 2D. In fact, it's on the average of about 10 degrees K warmer than the 2D. If I've got this correctly -- the 2D is colder by 10 degrees than the 1D.

So from that we think that the 2D downcomer is appropriate.

DR. WALLIS: Is appropriate?

MR. BESSETTE: Is appropriate. Is appropriate to use a 2D downcomer.

DR. DENNING: Because it's more conservative? Is that why you said it's appropriate or you think that you've demonstrated that it shows reality?

MR. BESSETTE: Well, I think, I'm

convinced that the 2D downcomer is a closer representation of reality than the 1D, particularly, in particular for cold leg break.

DR. WALLIS: The test data are further from it. The data must be wrong.

I thought all of this PVS analysis was based on a 1D downcomer?

MR. BESSETTE: No. We use a 2D.

DR. WALLIS: This was used in the stuff that Mark was talking about? I thought that was a 1D downcomer.

MR. BESSETTE: In all the comparisons I showed earlier were all using the same -- a consistent nodalization between experiment of facilities with what we used for the plant models.

And all the statistics on the temperature comparisons and pressure comparisons --

DR. DENNING: For the 2D model to have lower values. Does that imply that there has to be bypass, ECC bypass from an energy balance?

MR. BESSETTE: Well, you're always going to get some bypass from the -- if you model each cold leg individual which we do, this one cold leg is going to have to break. So the ECC injection into that cold leg tends to be bypassed, but you also tend to get some bypass from the three intact cold legs. You see similar results between hot leg breaks because while the ECC flow has to go through the downcomer to get to the break, so it results in similar whether they use the 1D or 2D nodalization.

DR. RANSOM: Well, is a possible explanation of buoyancy with the 2D downcomer, the cold water tends to, by natural convection, reach the lower parts of the downcomer?

MR. BESSETTE: I think that's part of it. Yes, because you don't have that degree of freedom when you just have a 1D downcomer.

Another issue that arose early on, which we noticed in the inial part of the study --

DR. WALLIS: Did the 2D downcomer predict the thermal plumes that APEX measured the variation of temperature around the downcomer?

MR. BESSETTE: Well, in general, we looked at axial and circumferential variations in the RELAP calculations and in the order of 5 degrees K or so.

DR. WALLIS: That was also measured in APEX.

MR. BESSETTE: But that's what RELAP says and then you say how close is RELAP to reality and reality is as reflected in the experiments and we see basically --

DR. WALLIS: I saw the APEX report. They've got these flumes. They've got temperature distribution and they've got places which are called, they're underneath the call letters, these plumes. That's something that I didn't see compared with the RELAP projection.

Then you'd say ah, RELAP is -- predicting reality as you call it.

MR. BESSETTE: Well, this morning, I did show comparisons of RELAP with APEX.

DR. WALLIS: The circumferential variation?

MR. BESSETTE: Well, circumferential and axial.

DR. WALLIS: Are you sure it's circumferential?

MR. BESSETTE: Yes.

CHAIRMAN SHACK: But he showed the stacking four together, right? You didn't actually have a 360?

MR. BESSETTE: Let's see, in RELAP, I think our APEX model was six channels, if I remember correctly and you tend to get more distribution of thermocouples. But we compared, tried to compare pick thermocouples that fell within particular nodes of RELAP for comparisons.

DR. WALLIS: Six channels are supposed to correspond to four coldlegs and two hotlegs?

MR. BESSETTE: I'm trying to remember. Did we use a six channel? I'm trying to remember everything.

I believe it was six channels to represent APEX because it's four coldlegs and -- so while we're waiting for that. The other thing we were concerned about was we noticed the presence of recirculating flows in the coldlegs when we were looking at Oconee. And when you make a code model and you have two parallel coldlegs those two coldlegs are identical.

We only see this in a situation where you have like a two by four arrangement that you typically have in B&W and CE and what you have in a situation is you're connecting an outlet plenum of a steam generator to a downcomer through two parallel paths and as far as the code is concerned is identical friction, identical elevations and so on. But then -- I guess I should have Vic explain this, but when you go to the matrix solution you start at one spot and you work your way around.

So because of round off errors, you start to accumulate this what you might say flows or forces that exist that are induced by these small numerical round offerors which tend to accumulate with each time step.

DR. WALLIS: So you can flow it around circular. It has no definite.

MR. BESSETTE: That's correct, yes.

MR. ROSEN: Perpetual motion.

MR. BESSETTE: Now the only way we found how to deal with this is to put in damping to counteract the numerics and so what we did was we added damping at reactor coolant pump --

DR. WALLIS: This is the only place RELAP does this, too, isn't it?

MR. BESSETTE: Certainly you have -- well, I should also say that TRAC does the same thing. And if you swap -- whatever your nodal scheme is, if you just swap the nomenclature, the flow reverses.

DR. WALLIS: Solution scheme, it drags the fluid around.

MR. BESSETTE: That's right. Yeah. So the only way to deal with it is when you get these flows when there's no physical mechanism to -- where there should be a recirculating flow. I mean what starts to flow is solving things in one node. You build up a small physical difference like temperature or buoyancy.

So you have a physical component to this, but it's actually induced by the numerics.

We went back and looked at the 1984 reports. We found the same kind of behavior there and they sort of noted it in passing, but didn't worry about it.

So we added high loss coefficient and reverse flow direction to provide damping.

And we did a comparison with experimental data. This is data from APEX. This is the same experiment I showed earlier today for a downcomer temperature comparison and you can see the effect, we put in the entire loss coefficient.

DR. WALLIS: It doesn't look important.

MR. BESSETTE: The green is a higher loss coefficient and the red is without it.

You get maybe here it's -- it's 8 degrees difference. And so it's not a big effect, but we thought this could be a nonconservatism, so we decided to get rid of it.

That's it for --

DR. RANSOM: This only occurs, I guess, when you have the 2D representation.

MR. BESSETTE: Of the downcomer?

DR. RANSOM: Recirculation.

MR. ARCIERI: This is Bill Arcieri from ISL. When we looked at the IPTS study, it was a 1D downcomer.

You saw the recirculating flow for the 2-inch break for Oconee.

MR. BESSETTE: So whether it was a 1D downcomer or 2D downcomer, it doesn't --

DR. WALLIS: I thought this was actually seen in an experiment. Was it SPES or something where they actually had a recirculation?

MR. ARCIERI: MIST had it.

MR. BESSETTE: That's a funny thing. There was actually a MIST experiment that showed a recirculating flow. But it's because there's so much heat loss in the cold leg and MISt that the flow didn't have to go to the steam generator. There was the cold leg acted as a heat exchanger.

That's the problem with very small facilities. That's why in SPES they had more temperature compensation for heat loss that their actual decay heat was.

DR. KRESS: One way to deal with round offerors is to increase the number of significant figures. Did you try that?

MR. ARCIERI: RELAP was already in double precision.

DR. KRESS: It's already in double precision.

MR. ARCIERI: That's as far as you can go.

MR. BESSETTE: But I guess -- you have a numerical solution scheme. You have to keep an eye out for --

DR. WALLIS: It's not a roundoff because of the outgoing difference, something like that. It's not a numerical thing.

DR. RANSOM: Well, you have to be careful. When you ignore the momentum flux term you can actually -- that can act as a loss actually. That doesn't show up in the calculation, so it's a nonphysical sort of thing. You're not satisfying the energy equation.

MR. BESSETTE: That's it.

DR. WALLIS: So what's your conclusion? What's the bottom line of all this stuff?

MR. BESSETTE: Well, the bottom, bottom line for us is that in the end, we're not dealing with a highly complex system. We're dealing with basically a consummation of mass and energy.

DR. WALLIS: What's the effect of PTS. Is the message you're doing a few degrees here and there? And the effect on the curves, certainly on the log scale, it's almost invisible -- is that --

MR. BESSETTE: I think I showed some examples. I showed for example that the cooldown rate is more important than heat transfer co-efficient. It's not to say that heat transfer coefficient has no effect, but --

DR. WALLIS: But do we adjust the Kirk curves that are 10^{-6}? Do we put a fuzziness around that of a factor of 10 or a factor of 1 or 2?

MR. BESSETTE: If you look at the dominant character rates, you have basically medium and large LOCAs which experience a rapid cooldown or rapid ECC injection so it's basically being controlled by the inflow and outflow of the system, dominating the energy and inventory.

So those are temperature dominating, temperature rate of change dominated. Then the other class of events where these stuck open SRVS are reclosed. There you have a fairly mild moderate cooldown when can get pretty cold if it goes far enough. But at the end, those tend to be pressure dominated. What tends to dominate the transient is the repressurization to the valve setting and the valve setting is a pretty definite thing. At your reset valve setting, if you don't throttle HPI.

So I think you can divide the total risk base into these two groups of transients which I think basically the behavior is pretty well -- can be pretty well understood with thermal hydraulic behavior.

DR. WALLIS: What does it mean? I thought this curve, it's a red curve and a green curve and a green curve, all relative resofracture versus RT. How much does this change that bottom line? Does that make it very fuzzy or does it --

CHAIRMAN SHACK: He's off the Kirk curves. Off in failure space.

DR. WALLIS: Yes, in failure space. Well, maybe Mark can tell us. Does it make much -- how fuzzy do these lines get when you do this?

MR. BESSETTE: Well, I think the best indication of that is this --

DR. WALLIS: Not your curves, his curves. The failures --

MR. SIEBER: Solid as a rock.

MR. BESSETTE: Within this kind of variation we see a one order of magnitude.

DR. WALLIS: So that sounds significant to me. I mean we're talking about 10^{-5} instead of 10^{-6}? Which way does it go?

MR. BESSETTE: Well, I would say it's all -- because we looked at so many transients, you see all these effects are in there.

DR. WALLIS: I don't know what that means.

MR. BESSETTE: This, you recall is a fairly slow transient. This is a stuck-open SRV. It's a cooldown transient for a stuck open SRV that recloses.

DR. WALLIS: He covers his uncertainties by statistical approach and that's the whole idea of his analysis for all the statistics and uncertainties. And you just get one curve at the end of it. But now you're introducing some new uncertainties are you?

MR. BESSETTE: Not exactly. I think this is supporting --

DR. WALLIS: Where do you figure it into his analysis?

MR. BESSETTE: He showed, for example, the effect -- the temperature at closing the valve at 3000 seconds versus 6000 seconds.

That variation in the valve reclosure time is more important than the uncertainty in the RELAP calculations of downcomer temperatures. So I think again, this all kind of illustrates the fact that it's really these boundary conditions about when the valve recloses. We chose to categorize it and closes at 3,000 seconds, 6,000 seconds or never.

DR. WALLIS: But in all the statistical treatments that he does, is this figured into it or is this a separate thing?

MR. BESSETTE: Well, you know, the only way thermal hydraulics is captured directly in the bottom line which is the probability of vessel failure is by individual RELAP calculations.

DR. WALLIS: Yes, with different plant conditions.

MR. BESSETTE: Yes.

DR. WALLIS: Is that where we left areas or the uncertainties in RELAP are not figured into the --

MR. SIEBER: Each curve has a set of uncertainties associated with it.

DR. WALLIS: RELAP is assumed to be deterministic.

MR. BESSETTE: That's correct. Each RELAP --

DR. WALLIS: Are you telling us here --

CHAIRMAN SHACK: But the RELAP boundary

conditions are distributed things, so you get an aleatory uncertainty, so it's the aleatory uncertainty overwhelms the model uncertainty.

MR. BESSETTE: That's correct.

CHAIRMAN SHACK: He does capture the aleatory uncertainty.

DR. WALLIS: Whatever applies.

CHAIRMAN SHACK: This is aleatory uncertainty here. His next page has an epistemic uncertainty in his heat transfer coefficient and he's saying 1.38 is less than a factor of 10.

MR. BESSETTE: I couldn't have said it better.

MR. SIEBER: Just capture that and say I agree.

DR. DENNING: But the whole issue is have you really bounded -- I shouldn't say bounded, but have you really covered the true uncertainty range in those epistemic uncertainties and I don't thin you've developed a convincing argument that you have -- I think you're right, but honestly, we don't trust 2D RELAP through the comparisons between RELAP and at least for the examples you're using here with the loft one where you've done your sensitivity study but they don't even look like the environmental results.

I think there are serious concerns that we're not really modeling accurately what's happening in the downcomer and whether they have a big enough effect to be greater than this kind of 30 percent type of uncertainty that you're dealing with. That's the whole issue.

MR. BESSETTE: Mark has some graphs that showed the variations in temperature that you get for different sizes of LOCAs and different times of valve reclosure. And I think if you could put those side by side you could see that the range of variation that you get by changing the time at which the valve recloses is much greater than these --

DR. DENNING: If you believe that heat transferred the uncertainty and the heat transfer coefficient is 30 percent, rather than a factor of 10.

MR. BESSETTE: All I can say is what heat transfer models were in the code, but extensive work to benchmark to assess those. It's correlated against data.

DR. WALLIS: That's the flow in pipes and things like that. It's now a downcomer with these weird flow patterns and flumes and all that.

MR. BESSETTE: So really don't think there's any question about the correlations that are in RELAP. You might say the uncertainty comes from these, like you said, the secondary considerations like well, in order a correlation to work properly, what does RELAP have to calculate correctly? It's things like Reynolds number which is velocity. So it really has to calculate things like a fluid velocity and a fluid temperature for the correlation to get the right answer out of the correlation.

DR. DENNING: It's a question of flow regime.

DR. RANSOM: What's more or less saving is the fact that you're adding cold water at some rate and it cannot instantly become cold. In other words, it's not a step function type of thing. It's more of a dilution curve like you're showing in these parametric results and the rate of cooldown of the vessel wall is related to that rate of drop in temperature and the cooling medium.

MR. BESSETTE: If I can get the same cooldown with this equation as I get with RELAP and if I can also know that this equation is going to apply to beta like Creare, how bad can RELAP be? If the cooldown is basically a mixing cup analysis or a backmix volume.

DR. WALLIS: See, I have a problem with

this because the Dittus-Boelter flow in a pipe. It's a straight pipe, flows down the pipe. It's a slope flow. Now you're telling me it's a well mixed downcomer and this is sort of an equation for a stirred up downcomer. So I say how can you use heat transfer coefficient based on a one dimensional flow in a pipe to a mixed situation, where the mixing itself is what's creating the heat transfer?

MR. BESSETTE: What RELAP has to get corrected is the fluid temperature and the velocity.

DR. WALLIS: I don't understand. It's a different flow pattern. A mixed downcomer isn't a flow in the pipe, so Dittus-Boelter shouldn't apply to it.

This idea, I forget the Russian's name --

MR. BESSETTE: Petukhov.

DR. WALLIS: That is a Reynolds analogy. There's a friction factor there and again, it's based on a one-dimensional sort of flow in the pipe. I get the impression that things are going on with these big eddies in the downcomer which are giving this kind of mixing cup behavior. That's not what's in the heat transfer models.

I think you have to somehow justify the heat transfer models when the flow pattern of the downcomer isn't one dimensional flow in the pipe.

MR. BESSETTE: Well, I tried to indicate this. This is a second order effect.

DR. WALLIS: We don't know that.

DR. RANSOM: A lot of this, I think though, is resolved. You took the plus or minus 30 percent which is characteristic of what's been observed when you use simple Reynolds analogy type models like Dittus-Boelter and apply them to rather complex situations. Typically, if you know more about this system they can be cut down to less than that, but plus or minus 30 percent, I think, pretty well covers the spectrum other than boiling and phenomenon of that type.

DR. WALLIS: It covers it for flow in pipes, but this is --

DR. RANSOM: Well, it's used for flow -- it was originally for flow in radiators which are pipes.

DR. WALLIS: What's the velocity when it's doing something -- the fluid is going down here and up there and around somewhere else. What's the velocity?

Dittus-Boelter is simply taking an average velocity over the whole thing which is much less than these local velocities.

DR. RANSOM: I wouldn't argue that it's correct.

DR. WALLIS: So you need some data for the heat transfer in the downcomer.

I think you have from APEX.

MR. BESSETTE: Well, I can say Dittus-Boelter has been compared with the Creare data.

DR. WALLIS: How about the APEX data? Does Dittus-Boelter compare with the APEX data?

MR. BESSETTE: We didn't have good enough wall temperatures in APEX to make a comparison.

DR. WALLIS: The whole idea of APEX was to do enough heat transfer measurements to be useful for PDS work. The whole idea of the experiment.

MR. BESSETTE: Yes, but --

MR. SIEBER: It failed.

MR. BESSETTE: Yes, they put in a lot of money to instrument the vessel in an adequate fashion.

DR. RANSOM: I think one thing that I'd be concerned about is they feed the heat transfer coefficient in FAVOR. And I assume FAVOR wants the heat transfer coefficient because it wants to know how much of a gradient is initially produced in the vessel wall and if you just let in the surface temperature equal to the downcomer temperature which implies an infinite heat transfer coefficient, you break the vessel because of thermal stress or at least track it, you know, initially. And so the results do seem to be quite dependent on how big this heat transfer heat coefficient is that you feed the FAVOR.

I don't have much grief with the downcomer temperature. I think it's, just from a mixing cup point of view, you can estimate that quite well, but the heat transfer coefficient is more difficult.

DR. WALLIS: I thought it was so big that heat conduction in the wall got --

DR. RANSOM: What?

DR. WALLIS: Weren't we told that it was so big the heat transfer coefficient and heat conduction in the wall governed?

MR. BESSETTE: So like PO analysis.

DR. WALLIS: So it was like an infinite heat transfer coefficient?

MR. BESSETTE: Yes.

DR. WALLIS: Suppose we wrote you a letter saying all this is so uncertain that you ought to assume an infinite heat transfer coefficient. Does that really throw a wrench into the works?

MR. BESSETTE: We could do that. There's a study like that done by Terry, I think it was. You did a study, didn't you, about 1997? Do you want to --

MR. DICKSON: Yes.

CHAIRMAN SHACK: You showed the 1997 study with a factor of two above and below your best estimate.

MR. BESSETTE: Yes. There's another study I didn't talk about, but Terry did.

MR. DICKSON: I think there are a couple of studies being talked about here. One study was just to try to find the value of H, conduction convected heat transfer coefficient at which it no longer matters, at which point the stress becomes esentotic and I wrote a letter report, I don't recall off the top of my head, but I'm pretty sure it was considerably higher than the values that we're inputting into these analyses.

MR. BESSETTE: I think you were up to 100,000.

MR. DICKSON: If you made me quote, I would say somewhere around 3,000, 4,000 English units.

DR. WALLIS: EDUs per hour per square foot?

MR. DICKSON: Yes. Which typically, I think, if you look at the input that RELAP puts out, that's typically a value at the beginning of the transient, but it decays away pretty quick.

DR. WALLIS: That has sort of lost several feet a second. It would seem that this has to be somewhat crisper in terms of rationale and conclusions.

MR. BESSETTE: You know, when you look at this kind of result, for example, when you vary increased heat transfer coefficient by a factor of 1.56, we get only a 1.38 change in CPF for this particular family of curves.

DR. WALLIS: What we're saying is we don't really believe 1.38. Maybe it should be five or something. Maybe the heat transfer coefficient should vary by 5, not by 1.56.

MR. BESSETTE: Well, you know, the impact -- when we look at -- I can tell you that -- what can I tell you? Under flow stagnation conditions, Churchill-Chu gives a high value of heat transfer coefficient than Dittus-Boelter, so you're not even applying Dittus-Boelter.

DR. WALLIS: The same name, I suppose, it gives you nothing.

MR. BESSETTE: You're not even using velocity. We then compare that with Catton-Swanson.

Catton-Swanson gives about 20 percent higher in the end --

DR. WALLIS: Catton is based on data from downcomers?

MR. BESSETTE: Based on his data from the downcomer.

DR. WALLIS: So that's the most reliable correlation, it would seem.

MR. BESSETTE: I think so. So if Catton is 20 percent higher than Churchill-Chu, we stick that in to RELAP, we show you the result. I don't know what else we can do.

DR. KRESS: I think we need to see the Catton --

CHAIRMAN SHACK: That's a fairly convincing sort of thing. It's relevant.

DR. KRESS: Show us the test data and how it was run to show we know it's relevant.

MR. BESSETTE: I'll give you the references. Yet there's an EPRI report and there's a couple of journal papers he did.

DR. WALLIS: Now what does he do, he modifies someone else's correlation?

MR. BESSETTE: He puts a multiplier on Petukhov.

DR. WALLIS: Petukhov is very simple-minded. It assumes you know the friction factor and he uses Reynolds analogy, it looks like.

MR. BESSETTE: Yes, and he puts this multiplier based on the ratio of Grashoff number over Reynolds number squared.

DR. WALLIS: That's reasonable. So ratio of convection to natural to force convection.

MR. BESSETTE: Yes.

CHAIRMAN SHACK: It's a transverse gradient that he's worried about, right? Across the channel. Is that --

MR. BESSETTE: Well, you get more of a velocity rating across the channel under this opposed flow conditions and since you increase the velocity gradient, you're increasing the turbulent exchangers. It gives you a heat transfer enhancement.

MR. KIRK: Is this the correlation where we weren't getting stable results out of RELAP because when velocity went to zero, the heat transfer coefficient just bounced all over the place?

MR. BESSETTE: We made one attempt in May which we then had or May or June time period which we then had to go back because were getting too much instability in the calculation. So we repeated that in the July-August time frame. That's what I showed here was the --

DR. WALLIS: See, the problem I'm having is you're telling us it's a well mixed downcomer. If I had a pipe and I put in some dye or something, it takes a while to get mixed in. I think it takes much longer to get mixed in than you are mixing in your plumes here.

So it appears there's some mixing going on in the downcomer that's more effective than in the pipe.

MR. BESSETTE: That's true. I think in the pipe geometry you have more of a tendency to be stably stratified. There's less mixing between the hot layer and the cold layer.

DR. WALLIS: This mixing must be due to turbulence which must somehow affect the --

MR. BESSETTE: You've got enhanced turbulence in the downcomer.

DR. WALLIS: You can't have turbulence for the mixing and not have it again for -- not have it for the heat transfer, the two are really based on the same physical phenomenon.

MR. SIEBER: Different orientation, so the buoyancy is different.

MR. BESSETTE: That's what -- Catton's whole thing is you get enhanced turbulence which increases the heat transfer.

CHAIRMAN SHACK: But there's clearly an enormous amount of mixing that occurs just at that entrance. As the flow comes in, it hits the flat wall and does all sorts of strange things up there.

DR. WALLIS: Does it jump across and hit the inside of a wall, the internal wall --

MR. BESSETTE: As best I can tell, the size of the flow stream as it enters the downcomer is about the same size as a downcomer gap.

DR. WALLIS: The question of the velocity, does it --

MR. BESSETTE: Does it go? That's --

DR. WALLIS: Or does it just dribble down the outside wall?

MR. BESSETTE: Does it come down in a sheet? I think it kind of

DR. DENNING: COMMIX kind of indicated it dribbled down.

MR. BESSETTE: I didn't put in that much detail in the COMMIX calculation.

It showed us the mid-plane velocities.

DR. WALLIS: Well, I think you have

condense all this detail into some really convincing arguments for what's being used to make the prediction.

CHAIRMAN SHACK: The Catton experiments sound like a good place to start.

DR. WALLIS: This is a report we haven't seen yet.

MR. BESSETTE: Well, I'll get copies to be distributed, the EPRI report and the Journal.

DR. WALLIS: And I was concerned that APEX, the whole idea of APEX was to do sort of definitive experiments for PTS and they come up with a report which has all kinds of interesting Star-CD, beautiful pictures and stuff. There's nothing that comes out of that which says CDS should use this heat transfer coefficient, this correlation, this so and so. It doesn't do that.

MR. BESSETTE: That's because it's very difficult to merger -- I mean to get a good --

DR. WALLIS: If Star-CD can predict that flow pattern and things, they can predict heat transfer coefficient, can't they? They can be compared with whatever you want to use. I don't see the connection between the APEX report, which I read, and what you need for your analysis here.

There's all kinds of stuff about mixing the HPI line and mixing in the cold leg, but you haven't used that at all. You just used some qualitative arguments.

MR. BESSETTE: I think the objective of the experiment was to look at downcomer mixing.

DR. WALLIS: I thought the objective was very clear. It was to give you what you need to do a PTS analysis.

MR. BESSETTE: But we weren't intending to look at total heat transfer problem.

I'm done. I thought I was done about 20 minutes ago, but it turned out I wasn't.

MR. SIEBER: You're not sure now either.

(Laughter.)

CHAIRMAN SHACK: I think we'll close it up for tonight.

MR. SIEBER: Good idea.

(Whereupon, at 5:48 p.m., the meeting was concluded.)