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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARD

SUBCOMMITTEE ON MATERIALS,

METALLURGY AND REACTOR FUELS

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WEDNESDAY, OCTOBER 1, 2008

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS PRESENT:

WILLIAM SHACK
DENNIS BLEY
JOHN STETKAR
J. SAM ARMIJO
DANA POWERS
MARIO BONACA
SAID ABDEL-KHALIK
OTTO MAYNARD
CHARLES BROWN

COMMITTEE MEMMBERS PRESENT: (CONT.)

MICHAEL CORRADINI
GEORGE APOSTOLAKIS

NRC STAFF PRESENT:

VERONICA RODRIGUEZ
BARRY ELLIOT
MARK KIRK
ROBERT HARDIES
MATTHEW MITCHELL
STEPHEN DINSMORE
MIKE CASE
ED HACKETT
GEARY MIZUNO

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Adjourn

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

8:30 a.m.

DR. SHACK: The meeting will now come to order. This is a meeting of the Materials, Metallurgy and Reactor Fuels Subcommittee. I am Bill Shack. I'm not chairman of the subcommittee, but I've sort of historically been involved with PTS. I'll keep on doing it.

ACRS members in attendance are Sam Armijo, Mario Bonaca, Dennis Bley, Otto Maynard, Dana Powers, George Apostolakis will be joining us, Charlie Brown, John Stetkar, Michael Corradini, and Said Abdel-Khalik. Michael Benson of the ACRS staff is the designated federal official for this meeting.

The purpose of this meeting is to obtain an update from NRC staff on the proposed rule amendment to 10 CFR 50.61, Fracture Toughness Requirements For Protection Against Pressurized Thermal Shock Events.

We will hear presentations from the NRC's Offices of Nuclear Reactor Regulation and Nuclear Regulatory Research. The subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register. We have received no written comments or requests for time to make oral statements from members of the public regarding today's meeting.

A transcript of the meeting is being recorded. Therefore, we request that participants in this meeting use the microphones located throughout the meeting the room when addressing the subcommittee. The participant should, first, identify themselves and speak with sufficient clarity and volume so that they may be readily heard.

We will now proceed with the meeting.

I just wanted to note that as you probably have noticed when you picked up the packet, we have an enormous amount of material to get through today and I encourage everybody to ask questions. But if we get off into extended discussions, I'm probably going to try to rein it in a little bit more just so we can at least get an overview of everything that's going on here. So, again, I just warn you that I might try to be more organized than we sometimes are in the ACRS Subcommittee meeting.

MEMBER POWERS: Are they going to explain why the NRC is doing this work and not the industry?

DR. SHACK: Probably not.

MR. ELLIOTT: Yes.

DR. SHACK: Okay, they will, but don't start yet, Barry.

MEMBER BLEY: Mr. Chairman, should this discussion move into the human reliability analysis part of this work, I'll have to withdraw from that discussion because of prior work in that area.

DR. SHACK: Okay. I believe Mike Case from wherever his office is now.

MR. CASE: Good morning, gentlemen and Veronica, our sole female participant today. I'm Mike Case. Right now I'm the Director of the Division of Policy and Rulemaking in NRR, and, actually, I'm moving over to the Office of Research in the Division of Engineering here shortly.

Just a couple of thoughts here before we start down this road. The first thought is perfection versus good enough. You know, people have worked on this PTS rule for a very, very long period of time and I sort of entered in late in the regime, and, guess what, they wanted to continue to work on it. So what we wanted to do with this rule is to consolidate what we have learned to date, so we sort of tried to get people to think about what's good enough for now.

So what we're working on is the PTS rule that's good enough, not perfect. When I listen to those experts in the room, it sounds like they have more ideas that will weigh in on this area in the future. That's great. But what we want to do with this particular activity is to get this rule good enough so that we can get it out the door and consolidate all this wisdom that we've been working on for the past decade or so.

The second thought is I need your help. I don't know whether the word got back to you or not, but you all were very helpful to the Agency on the power reactor security rule and the aircraft-impact rule in that the EDO wanted to accelerate that rule and get it done and he wanted to get it done so that it supported future licensing.

You all stepped up and got that rule done. We could not have got that rule done on time without your help. This is not quite as in the same area, but we do need your help. When we initially thought about this briefing, it was an informational briefing because we knew we were going out to re-propose the rule, but they are pretty much set for the final rule. They have all their thoughts in this current presentation. We got three comments in and they were all identical.

They had quite a bit of time scheduled in order to work on the comments and get the final rule out. So this was just an informational briefing. But, in the back of your mind, we really want to know if you have any hard spots with this rule because this rule's probably going to go final, so we really want your insights from the subcommittee meeting. I know you don't write letters out of the subcommittee. But if you all have hard sports with where these folks are headed, we want to know because we want to fix it now.

And so, once again, you know, I think we're ready for the real meat of the meeting, but we appreciate your help. This is a great ACRS rulemaking because I think that your independent look will actually help the rule. So let the games begin.

MS. RODRIGUEZ: Thanks, Michael.

MEMBER ARMIJO: Can I ask just one quick question? We're going to go over the technical basis for the rule in quite a lot of detail, and the question is, will we also hear today the actual rule as it's proposed and the issues, any remaining issues related to the rule itself?

MS. RODRIGUEZ: The layout of the

presentation is, at first, we're going to start with a description of the current rule as it is right now in 50.61. Then we're going to start about the motivation and the objective of the research, the technical basis, and at the conclusion of the meeting, we're going to be talking about the alternate PTS rule, which is 50.61(a).

MEMBER ARMIJO: Will you raise what current issues you may have with the industry people or other stakeholders?

MR. ELLIOT: We are going to discuss where we today, and we have gone through in the prior -- when we originally put out the rule, we had extensive amount of comments. Based on those comments, we think we have a rule that's very good. We don't expect it to change. We have a couple of new comments, not many, just a couple more. That doesn't mean it won't change, but we think that the last set of comments were pretty thorough and we considered all those comments and what we have today we think is a very good rule and it should be able to last a long time.

MEMBER CORRADINI: When they get to that part of the presentation, I would ask then --

MEMBER ARMIJO: We aren't going to cover there.

MEMBER CORRADINI: -- what are those three comments and how do you all think you're going to approach them? They won't be able to give you the full answer, but they can give you your thoughts. But that's where we want to go. We want to sort of ask those probing questions.

MR. KIRK: And just so you know, we won't be going through a line-by-line recitation of the rule, of course. But coming out of the technical basis, when we get to the end will be -- and, you know, here's the table that went in the rule or here's the equation that went in the rule or here's the essence of the equation that went in the rule. But I thought it was important to lead you up to that point so it just didn't appear out of nowhere and then have a lot of questions coming from all sorts of directions.

MEMBER ARMIJO: Thank you.

MS. RODRIGUEZ: Good morning, everyone. My name is Veronica Rodriguez. I'm the project manager for this rulemaking action. And as you all know, we're here to discuss the alternate fracture toughness requirement for protection against PTS events.

As you all know, this has been an amazing team effort. We're talking about years and years of hard work and dedication and the integration of different offices within the Agency. And we definitely need to give credit to the following working group members: We have Barry Elliot, Matt Mitchell, Steve Dinsmore, Lambros Lois, and myself from NRR. We also have Mark Kirk, Bob Hardies from research, and Nihar Ray from NRO, and Geary Mizuno, which is our attorney.

As discussed earlier, the layout of the presentation today, we're going to discuss the current PTS rule. Then we're going to pass to the motivation and the objective for the research. Mark is going to be talking about the technical basis for the rulemaking, and then Barry is going to conclude the presentation with a discussion of the alternate PTS rule.

With that, I'll leave it over and pass it to Barry.

MR. ELLIOT: My name is Barry Elliot. I've work in NRR for 27 years. Much of that time was spent on embrittlement issues and PTS. I just want to tell you that this is a big step forward in embrittlement and PTS technology that we're taking today.

I think we should be able to answer all your questions about how this technology was developed. We'll start with the old rule, which is currently in effect.

Before I start that, just to bring everybody up to date, what is PTS? Pressurized Thermal Shock are events that produce rapid cooldown from operating temperature. They result in cold vessel temperatures which could result in fracture of the vessel. If it had this event, the event could have repressurization or may not have repressurization.

The point of our presentation today is to show where we differentiate from the old analyses. This is one of the areas about repressurization and Mark will be talking in great detail about how the present analyses differ from the old analyses in that area.

The combined thermal and pressure stresses could induce fracture of the vessel if the vessel is embrittled. The significance of this, of course, is that this is a design base beyond the design-basis event and could result in loss of core cooling.

This issue is not an issue for BWRs. BWRs have a much larger water inventory, so they don't have as much embrittlement, and they also operate at saturation so the thermal stress will be much lower than a PWR.

Next.

The current PTS rule, 10 CFR 50.61 sets limiting levels of embrittlement beyond which a plant may not operate without demonstrating that the risk of vessel failure is acceptably low. The PTS screening criteria is given in terms of pressure vessel material indexing parameter RT_{PTS}.

PTS screening criteria in the current rule was developed from the likelihood of PTS events, the pressure and thermal stresses resulting from thermal hydraulic conditions in the vessel during the event, the likelihood of the pre-existing flaws in the vessel, and the vessel's fracture resistance. All these things were developed using 1980s technologies to develop the current rule.

Next.

I just want to point out before I go any further, we talk about events in the last slide. The limiting events in the early analyses were the steam line breaks, the steam generator tube rupture, small break LOCAs, and extended high-pressure coolant injection. We're not going to go into any more detail about how the previous analyses handled these, but we will talk about some of these in the present analysis.

MEMBER CORRADINI: Could you just repeat those again, please?

MR. ELLIOT: Steam line break, steam generator tube rupture, small break LOCAs with extended high-pressure coolant injection.

MEMBER CORRADINI: So, essentially, all high-pressure events?

MR. ELLIOT: They're all events that have repressurization as part of the analysis.

MEMBER CORRADINI: I guess it's a small thing. But repressurization in all of these cases I'd be at pressure, that means that the pressure would increase somewhere in the transient? When you say repressurization, I'm trying to understand why you say that.

MR. ELLIOT: Some of these events, there's a cooldown. They all have a cooldown, and, as a result, there's some loss of pressure.

MEMBER CORRADINI: All right.

MR. ELLIOT: And then the high-pressure coolant injection would occur and put the pressure back up to operating pressure again.

MEMBER CORRADINI: But the key thing is

I'm still staying at an ambient pressure that's high throughout the transient? That's not true?

MR. ELLIOT: You're not in an ambient pressure.

MEMBER CORRADINI: High ambient pressure.

MR. ELLIOT: No. You could rise to operating pressure.

MEMBER CORRADINI: Okay, fine. I get you. Thank you.

MEMBER BONACA: In the early times, I mean the assumption was made that the operator would not intervene. Right?

MR. ELLIOT: I can't I'm sorry hear.

MEMBER BONACA: I'm saying in the early times, the assumption was made that the operators would not intervene. You would just simply have a cooldown and then you would have the -- so you had certain assumptions which neglected any operator action.

MR. ELLIOT: Right, that's true.

MEMBER BONACA: That's important because I mean much of the new rule also credits operator action. That's right.

MR. ELLIOT: Mark is here to get into more detail about how we analyze these events and other events and which events are significant today in pressurized thermal shock. I just wanted to give you a picture of where we were in the old rule so we can compare it to the new rule.

The original rule was issued on May 27th, 1983 and was amended in 1985, '91, and '96. The important point here is that the amendments to the rule only change the method of calculating the embrittlement. It did not change the basis for the rule. So we're basically using the 1983 rule for the last 25 years.

MEMBER CORRADINI: So you change how you compute the master curve, but the basic basis as to why your worry hasn't -- the screening hasn't changed once you change that.

MR. ELLIOT: Right. It's not the master curve. It's the RT_{PTS} value --

MEMBER CORRADINI: Okay. I'm sorry. Excuse me.

MR. ELLIOT: -- has changed, not the basis for the rule.

MEMBER CORRADINI: Okay. Thank you.

MR. ELLIOT: The PTS rule requires licensees to demonstrate that the projected values of RT_{PTS} meet the screening criteria in the rule. It also requires to evaluate surveillance data as part of the process of determining the RT_{PTS} values.

MEMBER BROWN: When you said you changed the method of calculating embrittlement?

MR. ELLIOT: Yes.

MEMBER BROWN: But not the screening criteria?

MR. ELLIOT: Yes.

MEMBER BROWN: In other words, those numbers, that methodology stayed the same and all you did was update the ability as to how you figured out the actual embrittlement of the vessel due to its operations, radiation damage, blah, blah, blah, all that kind of stuff? Is that --

MR. ELLIOT: Yes.

MEMBER BROWN: Okay. All right. I'm sorry. I didn't quite catch that.

MR. ELLIOT: Okay.

MEMBER BROWN: Yes, fine.

MR. ELLIOT: Then the rule requires that if you go above the screening criteria, you have to provide the NRC an analysis and the analysis could be what modifications to prevent failure of the reactor pressure vessel. And the second thing, the alternative, is thermal annealing of the reactor pressure vessel.

I want to point out that nobody in the United States has ever accomplished these things yet.

PARTICIPANT: Is that a commercial rule?

PARTICIPANT: Or desired to.

MR. ELLIOT: Well, no. Two plants tried to do these, Yankee Row tried to do the analysis and we just never finished it. And then Palisades proposed thermal annealing and that didn't finish either.

DR. SHACK: Now, people do take other actions like limiting flux to the wall.

MR. ELLIOT: Flux, you could keep your flux down and keep your RT_{PTS} value down. All licensees have done that. But these two other options are in the rule and very onerous options. That's one of the motivations for new rule so if people are projected to go above the PTS screening criteria, there is something available other than these two other analyses.

MEMBER CORRADINI: So just to repeat the two analyses, one is thermal annealing. I understand that. What was the first one? Excuse me, I didn't catch it.

There are two onerous requirements. One

was thermal annealing, and I didn't catch the second one.

MR. ELLIOT: Well, it would be a plant-specific risk analysis to see if there is any modifications you could do to the plant that would keep the risk below the screening criteria, the risk criteria.

MEMBER APOSTOLAKIS: The onerous part is not doing the risk analysis I hope.

MR. ELLIOT: The onerous part is --

MEMBER APOSTOLAKIS: Is the modifications.

MR. ELLIOT: No. The difficult part is, I would say, is getting a risk analysis that the NRC can accept. That's probably the onerous part.

MEMBER APOSTOLAKIS: Because it would have to go to materials behavior?

MR. ELLIOT: It has to deal with materials behavior and the overall plant operating characteristics.

DR. SHACK: Basically everything we're going to go through today.

MR. ELLIOT: Right. And since we have provided that, a plant doesn't have to do it any more, wouldn't have to do it.

MEMBER APOSTOLAKIS: All this, though, I mean why don't they do it?

MR. ELLIOT: Excuse me? This is Dana's question.

MEMBER APOSTOLAKIS: Is this the motivation for this?

MR. ELLIOT: Is it what?

MEMBER APOSTOLAKIS: Is this the motivation for the original rule?

MR. ELLIOT: That's part of the motivation. And I think the other part of the motivation is it's been almost 30 years. A lot of technology has changed. We've learned a lot about pressurized thermal shock that we didn't know in the '80s and we know today and you need to uplink your technology, your rules to the technology.

MEMBER CORRADINI: But if I might just understand this a bit more. From a big picture if this was not us that supposedly understanding this technically, what you're really saying is there are more margin than the rule gives credit for, and so in reinvestigation, you're going to allow licensees to take credit for a margin that doesn't exist in the current rule. That's the way I --

MR. KIRK: No. The margin exists in the

current rule. The margin is very real. What we're doing is seeing what portion of that is appropriate to credit on a fleet-wide basis.

MEMBER CORRADINI: that's fine.

MR. KIRK: If we can let Barry get two more slides in, I think these questions are all there.

MR. ELLIOT: We're going to go through that. That's what today whole discuss is about is where are all the margins and how do they fit into the new rule.

MR. MITCHELL: If I could interrupt for just one second? This Matthew Mitchell, Chief of the Vessels and Internals Integrity Branch at NRR.

I'd just like to follow up on a comment I think I hear from Dr. Amijo, Dr. Apostolakis questioning the industry's involvement or why isn't the industry doing this instead of the staff. I think I'd like to point out that the industry has been a major participant in this effort since day one.

Much of the information that our office of research has been able to take advantage of and use as part of their efforts to develop a technical basis has come through industry participation. So this has been well supported by the industry and many of the industry groups since about 1999, and Mark's going to come to this slide later to show you the wide array of people who have participated her. So I'd like just to make it clear that we have had an extensive amount of industry participation and work by the industry in support of this.

MEMBER POWERS: I'd love to see all the experimental data that the industry contributed.

MR. MITCHELL: Much of the information I think that they provided, and Mark can correct me if I'm wrong, was data specifically related to the plants that were analyzed as part of the PTS technical basis. So it was really plant-specific information.

MEMBER BROWN: So there's no experimental information from which this is derived?

MR. KIRK: No, there is considerable experimental information.

MEMBER BROWN: I mean samples were tested and stuff like that or is it just extrapolation?

MR. ELLIOT: Excuse me. Excuse me. We're going to get to all this. We're here until 5:00. If we don't discuss the issue now, we'll be here --

MEMBER ARMIJO: All of that's water over the dam, right? It's all been done? Who paid for or who didn't pay for it doesn't really matter to us. It's what's technically --

MR. KIRK: But it's been done and we'll review it.

DR. SHACK: But I think the important point is the NRC sets the rules. What they need to do is set the acceptance criteria and that's really their responsibility. It's their rule.

MR. KIRK: That's right. And, quite frankly, it's a point that's on a slide about 5M, but it's maybe relevant to make it now was the realization, and I'll speak for NRR and if I say something wrong Matt can slap me, is that the realization that with, if we could go up just a couple of slides to the one with the histogram, that the histogram shows the current status of plants relative to the current PTS rule at 40 years. And all of those that you see in basically the first two blocks in the histogram, maybe the first three blocks, are definitely going to go over in the 40 to 60 year time frame.

All of those plants, because there are large capital assets that have been fully amoritized, are going to want to extend the life for good economic reasons, which means if we leave the current rule on the books, they're going to go over the limit and then they're going to have to do something else. They're going to anneal, they're going to have to go a reg guide 1.154 analysis, and, oh, by the way, that works so well and was so efficient of staff resources the last time, we just want to do it about 18 more times.

And so I think one of the big motivations here was the recognition that a thorough examination of the PTS challenge fleet wide, using both a lot of staff resources as well as a lot of industry resources, subject to thorough reviews by boards such as yourself, by the international technical community, by a group of independent experts, would lead to a better and more technically sound and a more efficient resolution of the issue than Matt and his group reviewing 18 plant-specific applications each of which could, of course, be done a little bit different and would lead to lots and lots of use of government resources.

So, part of this, I mean, obviously, the industry benefits. That's quite clear. But I think the Agency and the taxpayers benefit because we get a better rule with a sounder technical basis with overall, even though, you know, we all make jokes and certainly I do about, you know, it's ten years, I'm a lot grayer now, even though with all that, with overall a much smaller expenditure of taxpayer resources.

MS. RODRIGUEZ: Okay. Let's continue.

MR. ELLIOT: Just again, to go through the rule itself, the current rule, the RT_{PTS} value is a sum of the initial underrated RT_{NDT}, the amount of embrittlement and then a margin term, the margin term and the RT_{PTS} calculation is consistent with the margin in a technical basis that was used to develop the screening criteria.

The prior analysis indicated that the cumulative event frequency of $5x10^{-6}$ occurred at a mean service RT_{PTS} value of 210. And then 60°F was added to provide for uncertainties in the analysis. So the RT_{PTS} value in the old rule, in the current rule, was a term of using margins. This is a change when we talk about it later on.

MEMBER APOSTOLAKIS: What did you say about the margin?

MR. ELLIOT: Excuse me?

MEMBER APOSTOLAKIS: The current rule, you said what?

MR. ELLIOT: The current rule, the RT_{PTS} value includes a margin value.

MEMBER APOSTOLAKIS: Of 60°?

MR. ELLIOT: It depends on the plant. But the original, to get the screening criteria, the mean value from the risk analysis for the vessel was 210. And then the uncertainty was not put into the analysis as the risk analysis was performed, it was added on later to cover those uncertainties. That was the 60°.

MEMBER APOSTOLAKIS: Okay, okay.

MEMBER CORRADINI: So I'm sorry for making you do this again even though this is the current rule. So the ΔT_{30} is what?

MR. ELLIOT: Is the amount of embrittlement that the material has based on its projected fluence and chemistry and operating characteristics.

MEMBER CORRADINI: So if I could just say it back to you again so I get it right. So the RT_{NDT} is the nil-ductility transition point for a particular material with radiation -- The $RD_{NDT(U)}$ is --

MR. ELLIOT: the initial --

MEMBER CORRADINI: That's what I meant. I'm sorry. MR. ELLIOT: --the original reference temperature.

MEMBER CORRADINI: Unirradiated and then the ΔT_{30} is a calculation that says this will rise with time and damage.

MR. ELLIOTT: Yes.

MEMBER CORRADINI: And then the margin is added on just to make sure we're deterministically safe?

MR. ELLIOT: Yes, and to be consistent with the rule, with the basis for the rule because the original rule did not include risk in determining the RT_{PTS} value and it was added on subsequent.

MEMBER CORRADINI: Thank you.

MEMBER BROWN: Excuse me. What's the 30 in ΔT_{30}?

MR. ELLIOT: That's the amount of embrittlement. The ΔT_{30} is the embrittlement.

MEMBER BROWN: Thirty foot, it's the shift in the Charpy transition temperature of taken at 30-foot bounds.

MEMBER BROWN: Okay, 30-foot bounds, all right. Thanks.

MR. ELLIOT: Each plant, the rule contains a prescriptive methodology for calculating the embrittlement to ΔT_{30}. The results are compared to the screening criteria of 270 for axial wels, plates and forgings, and 300° for circumferential welds. The screening limit was based on a through-wall crack frequency of $5x10^{-6}$ per reactor year. And in the current rules there are no additional inspections beyond the ASME code requirements. That's another change that we're putting into the new rule.

MEMBER ARMIJO: Just that margin thing, I want to make sure I understand it. Is it the same value for each plant?

MR. ELLIOT: No.

MEMBER ARMIJO: Or will it vary from plant to plant?

MR. ELLIOT: It varies from plant to plant. It's based on the materials.

MEMBER ARMIJO: I understand.

MR. ELLIOT: So each plant has different materials. Many plants have similar materials, so they have similar margins, but there are some that don't have similar materials, so they have different margins.

MEMBER ARMIJO: Okay.

MEMBER APOSTOLAKIS: So what varies is what, the 270?

MR. ELLIOT: Excuse me?

MEMBER APOSTOLAKIS: The question was whether it's the same for all plants?

MR. ELLIOT: The screening criteria is the same for each plant.

MEMBER APOSTOLAKIS: Okay.

MR. ELLIOT: But he's talking about the margin to calculate the RT_{PTS} value.

MEMBER APOSTOLAKIS: Okay.

MEMBER BLEY: And that works because the screening criteria was set for what you thought was one of the most restricted vessels at the time. Is that true?

MR. ELLIOT: Well, at that time they looked at three vessels, also, and they compared it. They looked at the characteristics and they thought the screening criteria applied to all the vessels from that risk analysis.

MR. KIRK: The vessels that were analyzed before, like the ones we did now, were run at a number of embrittlement levels including some postulated very high levels that nobody ever expected to breach. And so based on that, you get a relationship between the risk of vessel failure, and the metric we use for that is the through-wall cracking frequency and the irradiated RT_{NDT}, so the unirradiated plus the shift, and what you see, of course, is as irradiation damage increases, the through-wall cracking frequency increases and the 270 and 300 is just the cutoffs that corresponded to what we then viewed at the time as a risk limit.

MEMBER BLEY: And I guess my question was I think you set that based on what you thought was the most vulnerable vessel. So this would be a good screening criteria for all plants.

MR. KIRK: Yes. But what comes out is I mean I guess you've got to define most vulnerable vessel because certainly a vessel's vulnerable if it's very embrittled, but that's a parameter of the analysis. So we're changing that. So that's the material resistance side.

The other thing that leads to vulnerability, of course, is the level of challenge. I don't want to speak too much about the old analysis, but what we found out in our new analysis is that you need to get the most challenging transients to even get the applied driving forces up to the point where they can break the vessel, albeit, at a lot probability, and those most challenging transients are very similar from plant to plant.

So even though, you know, indeed, the way set up the analysis, and you'll see here, is we focused on the vessels that were sort of, you know, the PTS poster children, the ones that were always right up on the limits. What we found out in studying those, looking at the dominant transients and then expanding that out to other plants, is that that was really an unnecessary step because the transients that cause a PTS challenge are very similar from plant to plant.

MEMBER CORRADINI: But I think I understood how you answered Dennis's question, but can I just, I'm sorry to ask it again. I read these words and take this as a limit-line approach, that is, in the population of all the vessels something told you that 270 and 300 is a good screen criteria for the population of plants. But there may be a plant, a vessel with different materials that a different screen criteria that would be higher would be because -- am I misunderstanding?

DR. SHACK: The challenge is you take the worst challenge and you figure out how much embrittlement you can tolerate and withstand that challenge. What varies from vessel to vessel is how fast you get to that embrittlement limit. Some plants will be to that embrittlement limit in 40 years, some plants will get to that embrittlement --

MR. KIRK: That part I get. But the 270 and 300 is a limit line. It's the lower bound.

MR. ELLIOT: It is a limit line.

MEMBER CORRADINI: Okay. That's all I wanted to hear because that's what he was asking me.

MR. ELLIOT: And the whole purpose of it was to set a limit line where to look at if a plant exceeded the limit, they could look at their plan specifically and do another, more refined risk analysis.

MEMBER CORRADINI: That's fine. I got it now. That's all I wanted to make sure. I got it.

MR. ELLIOT: Okay. We showed this slide before. This is just a summary of the operating plants in the United States.

MEMBER BROWN: What's the histogram mean? I tried to figure out the histogram. I don't have a clue.

MR. ELLIOT: Okay. All it is is shows the relative where after 40 years of operation where each PWR would be and all of it well below the screening criteria. The further you are to the right --

DR. SHACK: The first five are within 10 degrees of the screening limit.

MEMBER BROWN: Where does it say that?

MR. KIRK: On the horizontal axisis, °F, °F from the RT_{PTS} limits, °F from the 270 and 300 values. So this is how close they are to getting

Matt's undivided attention. Some are very, very far. That's why they're not in the back of the room.

MR. ELLIOT: After 40 years everybody is below the screening criteria. It's only when plants go for license renewal, they increase the amount of neutron fluids, the vessel, however, they project more. A few will go over. We estimate about approximately ten plants could use this rule as a result of just license renewal and power uprates.

We have looked at, as part of this implementation, license renewal. There are six plants that we project will need this just for license renewal.

MEMBER APOSTOLAKIS: And these will be the ones on the right?

MR. ELLIOT: These are the ones on the --

COLLECTIVELY: Left, left.

MR. ELLIOT: No, it's the ones on the left. Look this way. The ones that's closest to zero. The ones that are within 10, 20° of the screening criteria, the fluence, neutro fluence from either power uprates or longer extensions of the license --

MEMBER APOSTOLAKIS: What is the key reason why you have such wide variability?

MR. ELLIOT: The key reason is is the materials. In the earlier vessels copper was not known as the embrittlement factor that it is today. Copper is the main embrittlement factor, so there was no controls. So some plants have lots of copper in their weld wires and so they got lots of embrittlement.

DR. SHACK: But it was a feature?

MR. ELLIOT: What's that?

DR. SHACK: It was feature.

MR. ELLIOT: Yes, it was a feature. It made it easier to weld.

DR. SHACK: Right. And then in the '70s copper, it became known that copper in the weld or in the plate causes a major factor in embrittlement. So the later plants knew this and they reduced their risk of embrittlement by not allowing copper. So they'll never use this rule.

MR. ELLIOT: I think the impact may be even bigger though because some plants will now change their operating philosophies, that is, you won't be so concerned about limiting your neutron fluence. If you guys give them margin, they'll take it.

DR. SHACK: They'll use it.

MR. ELLIOT: But we have looked at only

the first license renewal period. The next license renewal period I would think that they're going to operate another 20 years after that, you know, a curve. But they are going to have to face that issue.

MR. KIRK: One thing to point out that actually, you know, you say, well, maybe that's a bad position for a regulatory agency to be in, to allow people to challenge our limits more, and that's certainly true. That's an economic reality that will happen. But the other thing to realize is the histogram went away.

A lot of the reason why the histogram is so flat is people right now plan around with their margin. They buy the margin adjust not just based on the material, but, also, based on the material knowledge. If you get a couple of surveillance specimens that are deemed to be credible, which means close to the correlation, you're allowed to chop your margin in half, where, really, there's not, I would argue, scientifically enough of an increased state of knowledge in going from one surveillance shift measurement to three surveillance shift measurements to allow that. Maybe if you went to 30 or 100, well, okay.

MEMBER CORRADINI: Can you repeat that,

though? I don't think I appreciate what you just said.

MR. KIRK: Okay. If you can go back to the margin equation. Veronica, go back to the equation. Okay.

Right now this is how you calculate on a plant-specific basis how close to these limits we allow you to go. Some people, like the BMW owners' group have gone to a lot of effort to change this number based on making a lot more measurements, but that's, quite frankly, a lot of work. That's a huge investment that BMW went in over the years and NRR saw fit to readjust their RT_{NDT} on irradients. That was one strategy to move away from the limit.

ΔT_{30}, once you make your welds and put your copper and your nickel in, basically you're set. You're not going to effect this value very much. But, margin, you can change quite a bit because, right now, the way the rule is structured, if you have no surveillance data or only say one surveillance data point, basically, you have to take the full margin burden, if I may speak in rough terms, of about 60°F.

But if you get, say, three or four surveillance measurements and they're close to the overall industry trend, that margin can be taken down by about a factor of 2 and that's written right into the current regulation. And so some of these plants that are close to the limit on the histogram are there, not because they have a vastly increased state of knowledge regarding their material properties, not because necessarily they have low copper, but simply because they played by the rules. As Dr. Shack said, you change the rules, people will change how they play the game. They've played by the rules, they've made a few measurements, and, by our current rules, they're allowed to reduce this margin by say from 60 to 30 and you say, oh, 30, you know, what's 30° between friends. I put on a coat and I've defeated 30°.

Well, once you get out to the plateau of embrittlement, your embrittlement at a rate of about 1°F per operational year. So 30° is an entire license renewal achieved only for the price of a couple of surveillance capsules. It's a real good deal. Whereas, now, in the way we've constructed the new rule, we don't include the explidit margin term because all of the uncertainties that that margin term reflected have been included in our calculations, so there's no need to do it otherwise.

And in so doing now, the way the new rule is constructed, we're not allowing any credit, if you will, any way to change the margin because it doesn't exist, and so what you'll see I think is a better, more scientifically accurate ranking of plants relative to the limit. So there's a more one-to-one correspondence between, say, I'm 15° from the limit, therefore, my risk of vessel failure is blah. Whereas, in the current rule, you can be 15° from the limit and you're risk of vessel failure can vary considerably because of how we do these calculations.

MEMBER CORRADINI: Thank you.

MR. ELLIOT: I just want to point out one thing. The rule is very explicit on how to handle surveillance data. Licensees can't play with anything. The data has to meet specific criteria before they can use it for determining the RT_{PTS} value. It is controlled by the NRC, not by the licensees.

MEMBER ABDEL-KHALIK: I have a question about this histogram. If you look at the first bin, are there any plants in the 0-to-10 bin.

MR. ELLIOT: I have a hard time hearing.

MEMBER ABDEL-KHALIK: Well, I'm not sure what I can do about it. Are there any plants in the 0-to-10°F bin that have gone through a power upright?

MR. ELLIOT: Have gone through power

upright, yes, Indian Point. I did Indian Point. I did a power for Indian point and I'm also doing a license renewal.

MEMBER ABDEL-KHALIK: The second question is, amongst the plants that went through a license renewal, which is the closest bin to the left in which those plants would fall into?

MR. ELLIOT: Well, obviously, the closer you are -- it's 0-to-10.

MEMBER ABDEL-KHALIK: No, no, no. Are there plants that went through license renewal that fall in the 0-to-10 bin as well?

MR. ELLIOT: There are plants that are within one degree of the screening criteria at the end of 40 years. There is a plant that's almost one-and-a-half, something like that, of the screening criteria after 40 years. They haven't reached 40 years yet. This is all projections.

MR. MITCHELL: This is Matthew Mitchell, NRR, again.

Barry is exactly correct. There are plants that have gone through license renewal that will be very close to the screening limit at the end of their 40-year license. They would be projected to go over the screening limits in the current rule very soon after entering their period of extended operation.

MEMBER ABDEL-KHALIK: All right. Thank you.

MR. ELLIOT: I mean this histogram shows that there are five plants between Zero- and 10° of the screening limit when they reach 40 years. They haven't reached 40 years yet, but it's a projection if when they get to year 40, that's where they will be. Hopefully, before they get to year 40, they will be implementing the new rule and there will be a lot more margin.

DR. SHACK: But I mean the condition of their license renewal is that they will deal with this problem?

MR. ELLIOT: Right. We have put conditions -- we have reviewed plans. We have reviewed plans that are projected to go above the PTS screening criteria in the old rule.

DR. SHACK: And they hope they won't do it by annealing.

MR. ELLIOT: And we have put limitations in their license so that they can't go above it. And three years before they go above it, they have to come to us and tell us what they're going to do. The earliest plant would be Palisades. And I don't know if anybody is here from Palisades, but they would be the first and they're very interested in what we do today here.

MS. RODRIGUEZ: Okay. Next slide.

MR. ELLIOT: Okay. We sort of went through this slide already in the discussion, why we're motivated to do this. We wanted to show the conservatism in the PTS, the current rule. We think that we could reduce burdens to the NRC and licensees. We don't need to have impediments for license renewal.

There's a very good chance that Indian Point is going to have a hearing on this because they are pretty close to the screening criteria in the first 40 years and they are projected to go over the screening criteria during the license renewal period.

The objective of the research effort, which Mark is going to talk about, is to provide a basis for the rulemaking. And then provide an alternative for licensees that cannot demonstrate compliance with the current rule through the end of their licensing operating period.

And, now, Mark is going to explain all the technology that has been developed so that we could make this new rule.

MR. KIRK: It's been a while. It was tragic. I had to read my own writing. So thank you for having me back. It's been four years. I see some familiar faces and some unfamiliar faces. I'm sure I'll get the most questions from the unfamiliar faces. I'll get the questions I could never answer before from the familiar faces.

So we've largely covered background and motivation. I think I have a short slide on that. I'm going to spend the bulk of the time talking about how we got to the reference temperature limits, the new values of 270 and 300, which, of course, aren't 270 and 300.

That was the getting new reference temperature limits and performing the risk-based analysis and the risk-informed analysis that supported that was the major research effort that went on. Then the issues of the surveillance check and inspection requirement are matters that arose as we assisted our colleagues in NRR with the rulemaking process. And so I'll, also, touch on them as we go through.

We showed this slide before. Just to point out that this project started, Ed and I were trying to pin down the date, something like 1997, 1998 by Mr. Mayfield, who apparently has gone on to bigger and better things because he's not here any more in this room. But we owe our good start to Mike. He convinced some of his friends, and maybe not so much friends, in the industry that it was in their best interest to collaborate -- can't say that word very well -- you know what I mean to work with us in a legal fashion to get access to the plant data that would otherwise not have been very easy for us to get in a formal way, to get access to simulation information, access to thermal hydraulic results.

And, also, of importance in the materials area that I'll just mention is in the initial years of this project we had very frequent public meetings with our colleagues in the industry to go over the many, many different materials and flow models that then ultimately became the favor code and we obtained a lot of good review and comment on those models and I think it made it stronger and more stable product overall.

So moving on from that, this just touches on the provisions of the current rule, which we've discussed a lot. We monitor embrittlement using a surveillance program.

MEMBER CORRADINI: Can you do that again?

(Laughter.)

MR. KIRK: Ultimately, it is the fracture toughness of the material in the plant, of course, going from low values at low temperatures to high values at higher temperatures before operation begins, we have generally reactor pressure vessel materials have RT_{NDT} values that are significantly below 0°F.

Forget about stress for a moment. Just think about temperature. If you think about the minimum temperature that you'll get to in say a primary break or a secondary break, a primary break is going to get you down to temperatures that are maybe close to the freezing point of water because you've got water held in external tanks.

Secondary breaks are only going to generate temperatures as low as the boiling point of water on the primary circuit, which is what we're concerned about. What this shows is the beginning of operations, the index temperature, RT_{NDT}, is so low that the toughness is very high, if not on upper shelf, meaning fully ductile failure, not cleavage failure, at the challenge temperatures.

Whereas, as embrittlement continues over the life of the plant, that curve marches steadily to the right and, you know, of course, this is just a cartoon, but all it points out is what we're doing here by setting limits on RT_{NDT} is we're setting limits on effectively how brittle the vessel can be and don't put a lot of stock in where the curve lined up. This is just for illustration, but it does point out that at these higher RT_{NDT} limits, at the temperatures of both the primary and secondary break, you're in the cleavage regime for the ferritic material from which the pressure vessel steel is made.

So in the event that you have a very bad day, you have a very severe transient, the operators don't catch it, there's a big flaw in a high fluence region of your vessel, if a crack was going to start, it would start in cleavage and it would become very, very large very, very fast, which is, of course, a very large concern and that's why we pay so much attention to this.

So the limits that we impose just keep this transition curve to low enough temperatures that we ensure that the vessel failure -- I'm sorry -- failure probability of the vessel is acceptably low.

MEMBER ARMIJO: So those are bands, right? Those are bands of data. There's variability within --

MR. KIRK: Yes, yes, there is.

MEMBER ARMIJO: And the lower limit is

absolutely a lower limit or 95 percentile?

MR. KIRK: The lower limit is absolutely a lower limit. We know from our physical understanding of the cleavage fracture process that there's just an applied stress intensity, if you will, below which you will just not get failure. But we'll talk more in a less cartoon-ish fashion about the bands, but there is a statistical model, backed up by physics, that's wired into favor that reflects this whole relationship.

So our rules right now limit how embrittled we allow the vessel to be. As we've already talked about, if you go above that limit, that doesn't mean you have to shut down. It just means you need to do something more to satisfy the regulatory requirement.

Well, what can you do more? You can do something physical to keep RT_{NDT} below the limits. The thing that most people do is, of course, they put in flux suppression to reduce the embrittlement rate and so keep from moving so far to the right on the diagram. The other thing that we would allow people to do, but so far nobody has seen fit to do it, is to anneal so as to effectively reset to the unirradiated curve and start the whole process over again.

MEMBER CORRADINI: Has there been any annealing of any vessel anywhere in the world?

MR. KIRK: In Russia, yes, yes.

MEMBER ARMIJO: I've heard numbers as big as 12, 13 vessels that they've annealed.

MR. KIRK: Yes. It's a countable number. I mean relative to the entire population of vessels on the plant, it's small, but it's not inconsequential.

MEMBER CORRADINI: Okay. And the second question is, in terms of location, this shows it as if the vessel is the thing. Is there a spatial location where this is preferable?

MR. KIRK: I'm sorry.

MEMBER CORRADINI: Where would this occur or do you even bother to try to estimate where? If I had, if I hit that lower red line and something were to pop open, where would it pop open?

MR. KIRK: Where would crack occur in the vessel?

MEMBER CORRADINI: Where in the vessel, yes.

MR. KIRK: Yes. Our analysis is restricted to the belt line region because that's the region where this marching from -- if you're out of the belt line region, you don't see any significant kind of a reaction to that.

MEMBER CORRADINI: So in the active core length?

MR. KIRK: In the active core length, say, plus or minus a foot --

MEMBER CORRADINI: Okay.

MR. KIRK: -- up and down.

The belt line, that’s right.

MR. ELLIOT: And the actual belt line has different materials in it.

MR. KIRK: Right.

MR. ELLIOT: It has plates, forgings.

MEMBER CORRADINI: That's fine.

MR. ELLIOT: It would be the one that has the most embrittlement that is most likely to fail.

MEMBER CORRADINI: Okay, but you've answered the question.

MR. ELLIOT: It doesn't mean the whole belt line is going to fail.

MEMBER CORRADINI: I understand. But you've answered my question. I wanted to make sure I understood. Thank you.

MR. KIRK: Okay. And the other way out in addition to actually doing something to either physically reduce the embrittlement rate or physically remove the embrittlement from the material, in addition to physical changes a licensee is allowed to analyze their way out of the problem, essentially showing that a higher RT_{NDT}, while it's not in compliance with our general limits, is, indeed, safe and they do that by performing a plant-specific analysis using regulatory guide 1.154.

MEMBER CORRADINI: And nobody's done that?

MR. KIRK: Yankee Row tried to do that and, for a number of reasons that would require far more time than we have here, was unsuccessful related to many things.

MR. ELLIOT: I would just like to show you what I reviewed at Yankee Row. They had a lot more uncertainties than most plants. That's why it wasn't successful.

MEMBER CORRADINI: Okay. That's fine. I just wanted to make sure I understand. Thank you.

MR. KIRK: Okay.

MR. ELLIOT: But because have this new alternative rule, we don't have to go down that path.

MR. KIRK: Okay. So that's how we regulate today and I think a hopefully little clearer view as to what these RT_{NDT} limits mean in terms of materials performance.

We talked about some regulatory and some industrial motivations for revising the rule. But Barry, also, reflected on the idea that it's been 20, 25 years since we opened up the black box and looked at the technology inside. And, in fact, that was a major motivation for doing this was a recognition that what was inside the former analytical procedure, that there were many, many conservatisms in there that were taken just because the stated knowledge at the time, in the early 1980s, didn't allow us to do anything better.

And on balance, those conservatisms suggested to the staff and to the management that we could aim to have a general relaxation of the rule if we did a much better job at doing the analysis. Here on this slide I just indicated some of the more major changes that were made in the three major technical modules, the PRA/HRA, the thermal hydraulics, and the PFM.

The arrows indicate just qualitatively. They're not scaled in any way. That certain changes that we've made, we expect making the change to reduce the risk.

For example, RT_{NDT}, the embrittlement metric, when you measure it as ASME requires you to do, it has a very significant conservative bias. It far overestimates the degree of embrittlement relative to the actual fracture toughness data.

So by removing that, we've removed a significant conservatism and, thereby, reduced the risk. However, the other point of this is to point out that we've taken a balance here. We haven't cherry-picked, and, in looking at updating the models from 25 years of inactivity, we've included new features in the model that, in fact, increase the risk.

When we considered operator action, of course, we had to consider the idea that not only would the operator do things that stopped an overcooling sequence from occurring, but the operator might do things that caused an overcooling sequence. So we considered acts of commission and other areas.

One that I know will come up, which I should point out there, is that in the past the medium- to large-diameter pipe breaks, where medium to large is defined as, say, anything six inches and above, in the primary cooling circuit were never analyzed before. They were eliminated a priori because the notion was that, unless there was significant pressure in the primary, the vessel couldn't break.

Well, we didn't make that a priori assumption. We analyzed those transients and it turns that high embrittlement levels, not only are they a significant risk contributor, they're a dominant risk contributor.

Now, you might say that begs the question, well, is the current rule safe enough. Well, it turns out it is because of the balance of all the other conservatisms that were piled into the rule, it's okay. But there were significant things that were missed in the previous analysis just because honest people making reasonable judgments that were reviewed by large committees such as yourselves said that this is reasonable to ignore and everybody agree, but turns out it wasn't so reasonable.

MEMBER CORRADINI: The arrows confuse me. So can I ask a different question at this point?

MR. KIRK: Yes.

MEMBER CORRADINI: So if this is your limit and this is with time -- I was waiting for a plot like this but I never saw it -- which is limit, independent of time, and the actual thing approaching limit as time goes on, but just stay with me just for a minute.

MR. KIRK: I'm going to get your plot.

MEMBER ARMIJO: Yes, it's in there.

MEMBER CORRADINI: It is? Sorry.

MEMBER ARMIJO: Two more slides, three more.

MR. KIRK: I want to go to a real plot. There it is. Go ahead. Keep asking and I'll get the graphic.

MEMBER CORRADINI: So if you go to this new approach, forget about the green and the red arrows, the limit line or the screen criteria actually is, in some sense, itself potentially moving, and the computation of where you are relative to it is change, what, in the final rule that is being promulgated, you'll get to and explain to us what's an acceptable factor of safety? That is, if the limit line is $5x10^{-6}$, am I allowed to only get to within an order of magnitude of that from the standpoint of probability?

MR. ELLIOT: We are going to explain how where we set the criteria as we move along today.

MR. KIRK: Yes.

MEMBER CORRADINI: The reason I asked the question like I did is in some sense your green and your red arrows are somewhat misleading because when you say significant conservative bias in toughness model removed, what you're saying is the estimated risk goes down?

MR. KIRK: The estimated risk goes down.

MEMBER CORRADINI: But it was always what it was. So once you determine what it supposedly really is, if I was an operator of a plant, I will operate it so I will reduce that margin. So I want to ask my question: eventually, what is the minimum approach distance to the criteria?

MR. KIRK: And we'll get to that eventually today.

MEMBER CORRADINI: Okay. Thank you.

MR. ELLIOT: That will be the new screening criteria.

MEMBER CORRADINI: I got it. I just want make sure, eventually, we're going to address how close is close enough.

MEMBER BROWN: Right now, I take from your comment about one plant would be one degree from the screening criteria at the end of its 40-year life.

MR. ELLIOT: 40-year life. It's one, one-and-a-half, two. It's close.

MEMBER BROWN: It's a small number.

MR. ELLIOT: Right.

MEMBER BROWN: And that's the way I looked at it.

MR. ELLIOT: It's close enough.

MEMBER BROWN: That's pretty darn close.

MR. ELLIOT: That's a projection, 40 years.

MEMBER BROWN: I understand. I understand all that. I understand all that.

DR. SHACK: That builds your margin into the limit.

MEMBER APOSTOLAKIS: The limit has margin in it.

MEMBER BROWN: Yes, yes, yes, I understand. I understand perception is worth a lot of stuff when you see people approaching some number you got in there, it makes people nervous. That's all.

MR. ELLIOT: I would just like to point out that we know those are close. Those are the ones that we look at the most.

MEMBER CORRADINI: Just to repeat Charlie's question, again, another way is, if this is a line and I'm approaching a line and it's one degree, what they're saying, if I understand it, is this one degree actually is the upper limit and the real risk is much lower. And so at the end, when we get to the new rule, I guess just for my edification, I want to know what the best estimate and uncertainty in it, what is that approach distance so I understand a better estimate of risk and how close is close enough.

MR. ELLIOT: And we're going to get there.

MEMBER CORRADINI: Okay. Fine. Thank you.

MEMBER APOSTOLAKIS: What are the acts of commission? Oh, commission, commission, okay.

(Laughter.)

MEMBER STETKAR: Mark, I'm not a materials guy and I'm a risk assessment guy, and I've always thought of the PTS stuff as cooldown, pressurized-type things.

I've noticed that under that PRA, the last one is medium-, large-break LOCAs. Those are things that I've never really concerned myself in risk assessments before. Is that telling me that the amount of primary cooldown that you get, regardless of pressurization, is sufficient to challenge the vessels?

MR. KIRK: That's correct. Yes, that's correct, but the proviso should be put in at a very high embrittlement level that few plants will ever get to, and, also, with the proviso saying that when we talk about -- I mean I'm going to slip into being imprecise with my words and I'm going to say things like vessel failure, and everybody should understand when I say that, I mean a vessel failure probability of 1×10^{-6} is the limit, which is, still, pretty low.

MEMBER STETKAR: What I really want, if that's the case, and I wanted to understand that, you mentioned earlier that this whole issue, I hate to say this, is only related to PWRs. Why, then, if it's simply a rapid cooldown does it not apply to boilers, which can be susceptible to rapid cooldown?

MR. KIRK: If it was a question only of the challenge, then you'd be absolutely right. A medium-to-large break in a BWR would lead to a rapid cooldown and the same amount of stress. However, the resistance of the BWR, as a clad, the material resistance to that stress or stress intensity is much, much higher for two primary factors: one is that the diameter of BWRs is so much bigger that there's a lot more space between the core and the vessel, and the result is that the fluences in the BWRs are much lower so the degree of brittlement is much lower.

So, you're right, the BWRs will be challenged to the same stress level, if you will, but their resistance is so much higher that it's still not a problem.

MEMBER STETKAR: Thank you.

MEMBER BROWN: What's PFM before you leave?

MR. KIRK: Probabilistic fracture mechanics.

MEMBER APOSTOLAKIS: That's $5x10^{-1}$? Forget it, Mark.

MR. KIRK: Okay. Rhetorical question. So now we're going to talk a little be more about how we developed our risk-informed modeling approach, which includes, and this is where I expect Dr. Apostolakis to be correcting me frequently --

MEMBER APOSTOLAKIS: No, no.

MR. KIRK: -- how we got to our numeric metric and our definition of what that meant with his, and then how we developed the integration of the probabilistic risk assessment, human reliability analysis, thermal hydraulics, and probabilistic fracture mechanics models to estimate how close different plant scenarios are to that risk limit.

So this my one-slide impersonation of Nathan Siu.

MEMBER APOSTOLAKIS: It does look like him.

MR. KIRK: Surrogates are never as good as the main act, so I hope you all will forgive me. I know there's going to be questions, so I'm going to go to the bottom-line point I wish to make and then kind of let the questions flow is that, in the end, I should back up for those of you that --

MEMBER BROWN: Excuse me. Before you proceed. On the previous slide, so we're going to develop a risk-informed modeling for this whole brittle fracture embrittlement-type issue?

MR. KIRK: Right.

MEMBER BROWN: And I'm asking the question just because I want to see if there's a shift in paradigm here in that from what I came from the naval nuclear program where we took all the data and then you drew a line below all the data. That's how we set from a concept logic, so we didn't allow data to be fall. If you got new data that fell below that, you had to lower your number, which we didn't like.

So is this going to the point now if I took my Charpy test data at whatever the thing's or I now have a limit line where some of that data falls below the line or above the line, whichever the right direction is?

MR. KIRK: Yes. You're right --

MEMBER BROWN: Did you answer that? Did you say yes? I'm sorry. I apologize.

MR. KIRK: I'm not sure I can give you just a yes or no answer. You're right that there is a paradigm shift if you will between an approach that takes all the scatter and uncertainty and fracture toughness data, or you name any other data set, and draws an absolute lower bound and then inserts that in a deterministic calculation.

We are, as you will see, in the fracture toughness model and the fall model and, you know, the you-name-it model where we can incorporating the uncertainties in statistical models thereof into the analysis. So the answer is, yes, we're not doing it the way you are used to in the nuclear Navy. We're doing it in a way where the calculation accounts for all of these uncertainties.

But I think something important to point out is that in the way that you describe doing it, which is, indeed, the way engineering's been doing for years and years and years, you are also treating uncertainties. A group of people in a room who are, in this case since we're talking about fracture toughness, materials experts said where should I draw the line. Where? Hey, Bob got a new data point. I should draw it there.

You're making a judgment, your making a model about how to treat uncertainty, and then the people in the next room, who are fluence experts, are going to do the same thing and they're going to all over bound line. And the people in the next room that are PRA experts, well, PRA experts don't deal in absolutes. Bad example.

(Laughter.)

The people in the next room, who are thermal hydraulics experts, are going to do a similar thing. And each and every technical speciality group is going to make their decisions about how to treat uncertainty and they're going to draw a lower bounding line. And then you're going to somehow add all those up and make a decision.

The approach that we're taking here is to move the decision about how to treat the integrated uncertainties up to a policymaking level, up to the point where boards like yourselves, our EDO, our chairman can review it. They're saying we don't want, you know, Kirk, you're just a GS-15. We don't want you to make policy. We hired you because you're a technical expert. We want you to collect all the data together, we want you to model in a physically appropriate way, we want you to represent it to your probabilistic model using a statistical model or whatever, we want you to vet it with the international community, and we want you, Mr. Fracture-Toughness-Geek to line that up with my colleague over here, the embrittlement guy, who's going to do his uncertainty modeling and the fluence person and the flaw person, and then we're going to amalgamate all those uncertainties in a systematic way that the PRA people believe in, and we're going to tell you that when you put all these things together, it generates a through-wall cracking frequency of blah, and then it becomes a policy decision as to how often can I fail the vessel per year based on an integration of all these uncertainties.

So I guess the point I'm trying to make is, yes, the way we do the sums is different, but our aims remain completely the same and consistent with the traditional engineering way of doing analysis. It's just that we changed the decision point as to who gets to apply the margin or the risk limit and who gets to say when enough is enough.

In the previous way of doing things, it devolved to, say, senior-level engineers in smoke-filled rooms and bars. They can't drink on site any more. Senior-level engineers, whereas, now, we're pushing it up to a policy level to say how much risk is too much.

MEMBER CORRADINI: So, can I ask a different question then, so Charlie to follow?

So I guess I was expecting you to say that now the uncertainty is more scrutable.

MR. KIRK: Yes, I would say that.

MEMBER CORRADINI: And auditable?

MR. KIRK: Yes, yes, absolutely.

MEMBER CORRADINI: Okay, fine. All right.

MEMBER BROWN: Why isn't it auditable? Why draw a line before all the data? That's pretty auditable, isn't it? If the line moves up into the data --

MEMBER APOSTOLAKIS: I think trying to compare one versus another, which one is better, is really not useful.

MEMBER ARMIJO: It's two different ways of doing it.

MEMBER APOSTOLAKIS: Yes. It's a different way.

DR. SHACK: Well, in this one I think it would also be very difficult to do what you want to do because you not only have to bound the Charpy data, you have to bound the flaws that you're going to find.

MEMBER BROWN: I don't disagree. You've got a number of issues to deal with.

MEMBER APOSTOLAKIS: It's a somewhat integrated approach.

DR. SHACK: It's very hard to bound things here.

MR. KIRK: I do think the point of it being more auditable is important because, in fact, that's something that I personally cared a lot about and the staff spent a lot of time on. I mean this is the summary report.

The background report, the one with it, that form the audit trail if you will, stack up to a meter thick and so what this enables people to do, if they're so interested and concerned, is to go in, you know, because everybody's got their own area of specialty and expertise and things that they know a lot about and that's, of course, the areas that people like to make have been done correctly.

The information is there as to how it was treated and we've taken great pains to say, okay, did we try to deal with the uncertainties explicitly by propagating them using a statistical model through the calculation, or did we deal -- there were many cases here where we still had insufficient knowledge and so we had to roll in conservative models, conservative judgments. But, again, they've also been documented for the purpose of audit so that 10 or 20 more years down the road, when operators have changed the way they operate and they're up against the new limits, God forbid, the next set of staff can go in and say, you know, these guys, you know, hopefully, they'll say these guys did a great job, you know, here are the remaining conservatisms and either we we can now we have an improved state of knowledge and can adjust them, or we don't know any better and you're just going to have to live with it.

So I think audit is --

MEMBER ARMIJO: Mark, are you referring to that new reg 18 --

MR. KIRK: 1806, yes.

MEMBER ARMIJO: 1806, well, I reviewed it and I think, first of all, it's very well written and it's very easy to understand for non-specialists and be -- it's really a tour de force I think because I'm a materials' guy, but I'm not a PRA guy, I'm not a thermal hydraulic's guy, and I think you put it together in a very nice way.

And I think if we just let this

presentation go forward, it would clear up a lot of questions that I think are a little premature. So I'd like to see you move along frankly.

DR. SHACK: Just pick a place where you'd like to take a break.

MR. KIRK: Let's do this slide. Now that Dr. Apostolakis has left, I can get through more quickly.

(Laughter.)

MR. KIRK: I actually want his help.

One of the first things that was done in this project was a gentleman who I'm sure many of you know in the research office, still is in the research office, Dr. Nathan Siu and his group did a review both of our modeling process, which we'll talk a lot more about, but, also, of what our risk limit should be. And by risk limit I mean both a numeric value, you know, 10^{-5} or 10^{-6} or 10^{-7}, but, also, some calculable metric that could be compared to that risk limit.

Now, in the existing policy guidance at the time, risk was expressed in terms of things like if you go back to, say, 1986, the commission talks about quantitative health objectives and says that the nuclear power plant operation can't increase the public risk of either prompt or latent fatalities by a significant amount.

Later on, that policy guidance was evolved to get it in terms of things that were more I'll say meaningful, more calculable in terms of nuclear power plant operation like core damage frequency and large early release frequency.

In the work that Nathan and his colleagues did at the beginning of this project, which I've highlighted in the large orange box at the bottom of your screen, first off, Nathan and his group met with the structural analysts and the materials people and said, well, we've got commission guidance on what are acceptable limits for LERF and core damage. Can you calculate LERF and core damage?

And after all the screening got done, the decision was made that no taking the accident sequence from vessel failure and then modeling all the things that one would need to model to get from vessel failure to LERF or core damage was seen to introduce so many uncertainties that the questions would just never end.

So we say, okay, we need an alternative approach. So then the question was asked, well, you materials' guys, what can you calculate that you're willing to hang your hat on? We said, well, we feel very certain, because we've done these calculations a lot and, most importantly, we've compared with experiments where we've gotten the answer right, we believe we can calculate the through-wall cracking frequency, how likely it is for one of these small fabrication flaws to initiate during a PTS transient and propogate all the way through the wall so the pressure boundary is reached, we said we think that we can calculate that in a technically scrutable way.

And so then Nathan and his group said, okay, well, we need to somehow to tie this back to decisions that the policymakers have made, we need to somehow relate this through-wall cracking frequency to either core damage or LERF. And so they did a semi-quantitative/qualitative accident sequence progression analysis --

MEMBER POWERS: I'm dying to know what a semi-quantitative/qualitative accident sequence analysis, but we'll do it in those smoke-filled bars.

MR. KIRK: Okay. This is where the smoke comes in.

MEMBER POWERS: And mirrors.

MR. KIRK: Any sufficiently advanced technology is indistinguishable from magic.

MEMBER APOSTOLAKIS: (Laughing.) Where did that come from?

(Laughter.)

MR. KIRK: Another of my random quotes of the day Google just cracked up. I apologize.

MEMBER APOSTOLAKIS: You were interrupted when you were getting to the heart of your argument.

MR. KIRK: I was interrupted when I said that Nathan and his group determined that if we got to through-wall cracking frequency that, in all likelihood, what would happen next was most probably core damage and most probably not large early release.

And so to be conservative, they said, okay, well, know that the guidance on the large early-release contribution from any individual accident or precursor has been established at 10^{-6} per reactor year, so for the purposes of this project, we'll apply that that 10^{-6} limit on the through-wall cracking frequency fairly secure in the knowledge that in our semi-quantitative/qualitative study that through-wall cracking frequency is much more likely to lead to core damage than LERF and the limit on core damage for any individual cases 10^{-5}.

MEMBER APOSTOLAKIS: Is this another way of looking at it may be -- I'm trying to understand, actually, what Nathan, in his wisdom, came up with.

If I look at your little table there, it says mean, delta mean?

MR. KIRK: And that little table comes out of the -- the lines aren't quite right, but that comes out of reg guide 1.174, yes.

MEMBER APOSTOLAKIS: So if I look at this new approach as a change, then the delta CDF and delta LERF have to be smaller than these numbers?

MR. KIRK: That's right.

MEMBER APOSTOLAKIS: And I know that the through-wall crack will not lead to a LERF?

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: Or there is a probability that it might?

MR. KIRK: Very small, yes.

MEMBER APOSTOLAKIS: So by taking the limit as 10^{-6}, which is the delta LERF, I am guaranteed that I'm meeting the regulatory guide with the change?

MR. KIRK: That's the logic.

MEMBER APOSTOLAKIS: That's logical?

MR. KIRK: Yes, that's the logic, yes.

MEMBER APOSTOLAKIS: Okay. Thank you.

MR. KIRK: You said it much better than I.

MEMBER APOSTOLAKIS: Very smart thing to do. Is Nathan a smart guy?

MEMBER POWERS: It's the same criteria before the Agency even existed, 10^{-6}.

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: Yes. That's very popular.

MEMBER BLEY: One in a million is a popular number.

DR. SHACK: Or you take it as one-tenth of the allowable LERF.

MEMBER APOSTOLAKIS: I know that, but I'm not sure I like that argument.

DR. SHACK: No, you like the delta better.

MEMBER APOSTOLAKIS: I like the delta better.

MEMBER POWERS: After all that work you came back to something that was established back in 1968.

MR. KIRK: A limit that was established in 1968, and the point of this slide is that, okay, even though it wasn't done in a smoke-filled room, but even though it was a panel decision and volitative shall we say, the basis of this through-wall cracking frequency limit derides back to policy statements by the commissioners about how much risk nuclear power plant operation can put to the public and that's the main point.

I mean, yes, 10^{-6} is a popular number. It's used a lot. But there's a logic process that gets us back to a policy situation.

MEMBER APOSTOLAKIS: Is this also meeting the delta CDF?

MR. KIRK: Yes, by definition.

MEMBER APOSTOLAKIS: Why?

MEMBER ABDEL-KHALIK: Because it's less than 10^{-5}.

MEMBER BLEY: Yes, but we're not doing a change here. That delta is for a change, right? We're not doing that.

DR. SHACK: We are looking at the change, and then in CDF due to embrittlement it meets it. The risk of CDF at an unembrittled vessel is 10^{-10}. So we've increased the likelihood of through-wall cracking frequency by four orders of magnitude, but we've only increased Δ CDF.

MEMBER POWERS: The more interesting question is: how did the fundamental law probabilistic fracture mechanics that all events are 10^{-45} change so much?

MR. KIRK: We can take a break now with the chairman's blessing.

DR. SHACK: We will take a break until 10:10.

(Whereupon, the foregoing matter went off the record at 9:54 a.m. and resumed at 10:10 a.m.)

DR. SHACK: Gentlemen, if we can come back into session.

MEMBER APOSTOLAKIS: Are we on the record now?

DR. SHACK: Now, we're back into session. Mark, back to you.

MR. KIRK: So where last we left off, we were discussing how the popular value of a one-in-a-million per year change of vessel failure was arrived at and that's represented on the graphic you see in front of you as the red line. As we pointed out, that as established consistent with commission policy guidance.

So now we have a notion of how often we'll permit vessel failure to occur per year. But, then, the next thing we need to do is construct a model --

MEMBER POWERS: A most unfortunate use of terms.

MEMBER APOSTOLAKIS: Yes, you shouldn't say that, but that's okay. Or should we make the frequency?

MR. KIRK: Then the next question becomes: how do we perform a calculation to get numbers to compare with that low acceptable frequency?

MEMBER BLEY: Can I ask one question?

MR. KIRK: Yes.

MEMBER BLEY: Since all of this is aimed at probabilistic analysis and probabilistic fracture mechanics, we have a limit like, but you're doing a probabilistic study, so is there a probability of frequency that goes with that line that's in your probabilistic study? There's probably always some chance you're above it.

MR. KIRK: Yes. What you'll see as we go on is the cartoonish green line that you see here, which would represent the analyses that we're doing, our comparison metric out of each analysis is the 95th percentile of the through-wall cracking frequency, which we plot on there for all the plant analysis we've done, and then, in fact, we take an upper bound to that.

MEMBER BLEY: Okay. Thank you.

MEMBER APOSTOLAKIS: But you are really, now, compounding margins like in the deterministic case because you said that the limit, unless I misunderstood you, Mark, so be patient.

MR. KIRK: No.

MEMBER APOSTOLAKIS: The limit is the 10^{-6} per year, which is already conservative from what you said earlier?

MR. KIRK: Right, right.

MEMBER APOSTOLAKIS: Then you're going to estimate the frequency of through-wall cracking, or something like that?

MR. KIRK: Right.

MEMBER APOSTOLAKIS: And compare the 95th percentile of that --

MR. KIRK: Right.

MEMBER APOSTOLAKIS: -- with the already conservative limit?

MR. KIRK: Right. Yes. And that's an excellent point because the flavor that I'd like to leave the committee with is that, you know, we're not, as we said before, we haven't established a limit and then crept right up to it.

First off, as you said, the limit, since through-wall cracking frequency is much more likely to lead to core damage than LERF, already, essentially, has a factor of 10 safety margin in it, if you will. We compare the 95th percentile to that limit instead of the mean, so there's a bit of a margin error depending upon the shape of the distribution. And then on top of that, in the details of this model, you'll see that while we've tried to incorporate the best, most comprehensive state of knowledge and technology as we can, when there were still areas where we didn't have a very advanced state of knowledge and so in those areas we made conservative assumptions.

MEMBER APOSTOLAKIS: I understand.

MR. KIRK: And so that 95th percentile limit is in the context of a model that, where we didn't know any better, we erred to the conservative side.

MEMBER APOSTOLAKIS: I think this is very good what you're doing giving the big picture. If we look at the figure on the left, this dashed green line means what?

MR. KIRK: That represents the results of the analysis of the three plants that we ran through the PRA, thermal hydraulics probabilistic fracture mechanics model that we would say do Palisades at 40 years, Palisades at 60 years, Palisades at 100.

MEMBER APOSTOLAKIS: So it's a real line, it represents something?

MR. KIRK: It's real, yes.

MEMBER APOSTOLAKIS: It's not just show.

MR. KIRK: It's data, yes, and you'll as a real line, if you will later. It's calculated values that come out of this model when the model is executed against different plants and different embrittlement.

MEMBER APOSTOLAKIS: Now, for the rule there would be an extra step to convert this to something related to the --

MR. KIRK: Right, and that's what's shown notionally here.

MEMBER APOSTOLAKIS: Where?

MR. KIRK: Here. If you just think about, okay, so say this green curve is the upper bounding line from all of our plant analysis, the red line is your 10^{-6} limit --

MEMBER APOSTOLAKIS: Right.

MR. KIRK: -- and then you just say, okay, this is an upper bounding curve, this is the limit, and so this is now the new value of 270 if you will where, if I can allow operation only up to that temperature, but not any more.

MEMBER APOSTOLAKIS: But it would be a different graph?

MR. KIRK: A different graph, yes.

MEMBER APOSTOLAKIS: Okay, good.

MEMBER ABDEL-KHALIK: Let me just ask a

clarifying question. The logic you presented on the previous slide established a 10^{-6} per year limit as being sort of conservative with regard to the Δ LERF for a specific transient? Or is it for all cooldown transients?

MR. KIRK: For all cooldown -- yes.

MEMBER ABDEL-KHALIK: So that's even more conservative than what I thought it is.

MR. KIRK: Yes.

DR. SHACK: But you don't get to pick your transient.

MEMBER ABDEL-KHALIK: But you look at the worst transient rather than --

MR. ELLIOT: Yes, but what you're going to show is that there are only certain transients that really matter.

MEMBER ABDEL-KHALIK: Maybe I didn't pose my question correctly. Does this represent the sum --

MEMBER CORRADINI: Yes, that's what I thought you were --

MEMBER ABDEL-KHALIK: -- of the through-wall crack frequency for all cooldown transients?

MR. KIRK: Yes, it does, yes.

MEMBER ABDEL-KHALIK: So this represents a sum rather than the maximum value amongst all cooldown transients?

MR. KIRK: This represents, yes, a sum.

MEMBER APOSTOLAKIS: It's actually a distribution.

PARTICIPANT: But not the line. This is the line for the plant. This is the allowable through-wall crack frequency per year for the plant.

MEMBER ABDEL-KHALIK: 10^{-6}?

MR. KIRK: That's right. Oh, you're talking the green line.

MEMBER APOSTOLAKIS: The green, yes. So you have uncertainty of the green line?

MR. KIRK: That's right, yes.

MEMBER CORRADINI: But, can you just, because I am with Said on this, I want to make sure I understand.

The red line is the sum of the small breaks, main steam line breaks, medium LOCAs, large LOCAs.

MEMBER APOSTOLAKIS: The green line? The red is the limit.

MEMBER BLEY: The red is the limit per year for the plant.

DR. SHACK: Just think of the horizontal axis as irradiation, loss of toughness. So every year you're frequency of something happening keeps going up, which is the green line.

The green line is really a cloud of data. His green line all the stuff for -- all the transients will have different frequencies, but his green line kind of bounds all those.

MEMBER ABDEL-KHALIK: Again, that doesn't answer my question. You're saying that the green line, you know, there are a lot of data points below that for different transients, et cetera, but is the limit being established for the sum of all cooldown transients?

MR. KIRK: Yes. That is a correct interpretation, yes.

MEMBER APOSTOLAKIS: And that's the power of this, it integrates everything.

MEMBER ABDEL-KHALIK: Right. Okay.

MR. KIRK: So now we're going to spend a lot of time trying to describe how we got the green line and that's the analysis that's show in the box with the blue squares.

MEMBER APOSTOLAKIS: I think in the future it would help if you show maybe three green lines to indicate that you have done them separately.

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: That would really send a message much better.

MR. KIRK: that's an important point.

So how we did the analysis, we start -- and, of course, the analyses were done in an iterative fashion. It wasn't just a single pass through and then we were done. But, in any event, just notionally we start with the PRA event sequence analysis.

The outputs of that are in general two things: one is the definition of sequences that can lead to overcooling events, either with our without repressurization; and the other estimate is a notion of the frequency with which that series of unfortunate events would occur. We'll hold the frequency estimate in abeyance for right now.

The sequence definitions then got passed to the thermal hydraulic code, relap, which used those sequence definitions along with models of the different plants that we analyzed to estimate the temporal variation of pressure temperature and heat transfer coefficient and the downcomers of the various vessels.

That information from the thermal hydraulics analysis was then one of many inputs that went into the probabilistic fracture mechanics analyses. Other inputs that aren't shown on here, that go into the PFM analysis are, of course, the material conditions in the plant, the fluence, the flaw distribution, and many other things that we'll touch on later.

The PFM analysis then calculates based on all that plant characterization, material information, and thermal hydraulic challenge, the conditional probability of through-wall cracking and we call it a conditional probability because, in this case, it's conditioned on transient 1 happening or transient 2 happening or transient 3 happening.

The final step is then, essentially, a matrix multiplication where we multiply the conditional probability of through-wall cracking for sequence 1, which is not a value, but is a distribution, with the sequence frequency from PRA, which is, again, not a value, it's a distribution, and then we come out with an estimate with uncertainties of the yearly frequency of through-wall cracking, which is what's shown by the green box, and we do that for transient 1, transient 2, transient 3, dot, dot, dot, through transient and sum it all up and then that generates a single point on the graph, and then we do that for different plants at different embrittlement levels so we get a bunch of points, and then the green curve just, notionally, could be taken as a upper bound for all those points.

MEMBER APOSTOLAKIS: So the different embrittlement levels don't meet the left-most box?

MR. KIRK: Yes. The different embrittlement levels actually are a box that's not shown on here. But, yes, they would just be a change in one of the inputs to the PFM, the PRA, and thermal hydraulics for that variation remain exactly the same.

MEMBER ARMIJO: Where does operator action get input here, or do you exclude that to mitigate --

MR. KIRK: In the sequence.

MEMBER ARMIJO: -- in the event, or make the potential where either sequence is --

MEMBER APOSTOLAKIS: PRA, the all-powerful.

MEMBER BROWN: The blue box that says conditional probability for each sequence, is that correct? Then you've got the sequence frequency, you multiply those --

MR. KIRK: Right.

MEMBER BROWN: -- for that particular event. How does this get to the sum of all the events that Said brought up a minute ago for, I mean how does that get translated into this line? Do you get a number, or is something a probability for that thing, then you add that onto all the other event?

MR. KIRK: Yes, because each event, say, maybe a simple example, say we had three events: a large break, a stuck-open valve, and a main steam line break. Each of those occurs with some frequency and each of those, so, say, one of them main steam line break has a frequency, I'm just going to make up numbers, mean frequency of 10^{-4} with a range of 10^{-3} to 10^{-6}. So that's how often it can happen with the uncertainties calculated.

Then it's got a conditional probability of through-wall cracking that might, say, have a mean value of 10^{-6}, range from 10^{-7} to 10^{-8}. You multiply those two distributions together, you get an output distribution of through-wall cracking frequency --

MEMBER BROWN: For that event?

MR. KIRK: For that event, and then you do that, again, for event 2, for event 3, and so on, and you just keep adding the events together because all of them can possibly occur with some low probability.

MEMBER APOSTOLAKIS: I guess there ought to be maybe another circular box there after--

MR. KIRK: To add them, yes.

MEMBER BROWN: Your point was, I did not derive that from the rest of what I saw until you asked the question.

MR. KIRK: Yes, I'm glad that was brought out because that's a very important point is this is the cumulative through-wall cracking frequency for --

MEMBER APOSTOLAKIS: It's a convolution.

MR. KIRK: -- for all of the possible events, yes.

MEMBER APOSTOLAKIS: Before you leave this, it's not SAPPHIRE, only one "P".

MR. KIRK: Well, there's an error in this report.

MEMBER STETKAR: It's probably cheaper for SAPHIRE to put a new "P" in there.

MEMBER APOSTOLAKIS: SAPHIRE is one "P". I'm never right. That's a name.

MR. KIRK: And then the final point that I'd like to make, because we'll talk about it, is --

MEMBER APOSTOLAKIS: -- but the name is misspelled. Excuse me. I mean it was just a quick comment and that's fine.

MR. KIRK: No, I should have understood that.

DR. SHACK: Can we move on?

MR. KIRK: The last point here, which is important, we exercised this model for three different plants we'll talk about in just a minute. But out of those three detailed plant-specific analyses, we gained a lot of in sight as to, you know, we modeled many, many classes of transients, in the hundreds. But in the end only a few of them mattered very much at all, and most of them were not challenge events at all.

So we got a pretty good view of what was important versus what wasn't important for PTS out of those three plants. But then we went on, and that's what the gray box illustrates to look at the characteristics of other plants throughout the fleet to make sure that, say, stuck-open valves on a primary side that may later re-close are important transients. We then took the additional step to make sure that in a representative democracy sense the three plants that we'd done the detailed analysis of represented the fleet that we had to regulate.

MEMBER APOSTOLAKIS: Now, which of these boxes, for the analysis they represent, were not done when the present rule was formulated?

MR. KIRK: I mean in some way all of these were done.

MEMBER APOSTOLAKIS: But you said the PRA analysis that you ended up with very few number of sequences that mattered. Those guys who developed the existing rule had in mind some sequence. Do they have those in mind?

MR. KIRK: Not in all cases. I mean, like we've already said, if you want to just look at the initial PRA, which is the vision of what things can go wrong, medium to large break LOCAs were ignored. However, main steam line breaks were, of course, treated and most everybody knows that those were among the dominant transients. Well, those were among the dominant transients because they modeled very conservatively.

They had the fast initial cooling rate, which is what actually happens, but the difference in the initial model is that that fast cooling rate was taken down to 75°F and the primary can never get that low.

Each of these boxes was done in the previous analysis is just based on the state of knowledge at the time, there were different views about what should be done, and, largely, it was pretty good, but, you know, some things were missed.

MEMBER ABDEL-KHALIK: Has that insight

been sort of integrated into the functional restoration guidelines of the plants?

MR. KIRK: I can't answer that because I don't even know what you just said.

MEMBER ABDEL-KHALIK: Well, there are two functional restoration guidelines for PTS.

MEMBER BLEY: They don't need to be. If you go to the functional restoration guidelines and read them, the things that get you there aren't keyed on the initiating events. They're keyed on the plant conditions. So when you get the temperatures and pressures that are appropriate, you keep into those.

MEMBER ABDEL-KHALIK: But I don't know if when these functional restoration guidelines were developed the assumption was made that you would need repressurization in addition to cooldown to cause PTS.

MEMBER BLEY: It's going back many years, but that was in the models at the time. But the way they're written, we could look at it, but the way they're written I think gets you out of that problem completely.

MEMBER CORRADINI: I had a simpler question. You said these were done before, but I want to go back to your different approach that is in the current rule in some sense that is different.

In each of these boxes, if I could just paraphrase it briefly, is that the engineers involved essentially developed a limit line on each of these and then these were combined non-probabilistically. The difference here is you were in some sense combining these in a probabilistic way so you see a spread in what is occurring and that spread is consistently calculated and carried through the calculation.

In each of the blue boxes before, there was a set of sequences that was worried about, there was a thermal hydraulic analysis with a lower limit to a concern, and there was a fracture mechanics analysis and they were combined more deterministic. Is that fair characterization?

MR. KIRK: Yes, that's a fair characterization.

MEMBER CORRADINI: Okay, fine. That's all I wanted to know.

MEMBER APOSTOLAKIS: But this analysis is more thorough?

MR. KIRK: That's right, yes.

MEMBER CORRADINI: I guess I'd say it that you're carrying through all the spread in a consistent fashion?

MR. KIRK: Yes. That's certainly --

MEMBER CORRADINI: Thank you.

MR. KIRK: All right. With any luck, okay. I should let Dr. Apostolakis talk about this one.

So as you were just saying, our approach here features a systematic treatment of uncertainties and echoes some of the words that were said before, we've tried to take a very comprehensive look at all of our models, both the high level, say, PRA, TH, and PFM models, as well as the sub-models that make up each of those bigger models, to see how the uncertainties interact as they propagate through the models.

Like I said, we took a very comprehensive look at this and I think in addition to just being the right thing to do, it really improves the comprehensiveness, makes it more reviewable, more trackable, and things of that nature. The uncertainties that we found in the models while classified as being aleatory versus epistemic, all uncertainties were treated, but they were treated in different ways.

The ones where we had an advanced state of knowledge and that were identified as being significant to the result were, in all cases where possible, numerically quantified using data, physical models as a primary source, in some cases expert opinions, supported the quantification and those numerical quantifications were then propagated through and appear in the end result.

However, in some cases, harkening back to the traditional deterministic approach, in some sub-models, say, buried within the PFM model, for example, uncertainties were treated, but I would call them as being accounted for by the structure of the model, not numerically quantified.

I'll just give an example that's at least familiar to me. The attenuation model, of course, you've got neutrons impinging on the interdiameter of the vessel and the steel closest to the interdiameter takes the largest dose. And then, of course, as you go through the thickness, there's progressively less neutron damage to the steel because there's steel in the way protecting it.

To account for that, you need to account for that because flaws may occur at different depths through the vessel, and the way you account for that is using something that's called an attenuation model.

The attenuation model that we're using in this calculation is exactly the same attenuation model that was adopted in the 1980s, and the reason that we're doing that is that the staff, in working together with the industry, didn't feel that there was sufficiently advanced information that was compelling enough to motivate the change in the model. However, the information that is available all says that that model is conservative.

So, in that case, we accounted for what are obviously uncertainties in that particular model by adopting a conservatism into the calculation.

MEMBER BROWN: You mean by sticking with the previous method with the attenuation model?

MR. KIRK: In this case by sticking with the previous attenuation model.

MEMBER BROWN: Which was developed 30 years ago or something like that?

MR. KIRK: Yes. So I wanted to point this out because I think we made kind of a big deal, and it was a big deal at the beginning of this, of being very systematic and thorough in our uncertainty treatment. I think some people were left with the mistaken impression that that means that each and every uncertainty is propagated through this model and that's definitely not true numerically. But what we have done is treated them all, in come cases numerically, in some cases by conservatisms, in some cases by judgment that it just wasn't important.

I mean to take another example, the diameter of a vessel is not the same all the way around. You go around, it's made of welded plates. It's not perfectly circular and that changes how far some metal is relative to the core than others.

But that was, in the case of this group, judged to be unimportant to carry through in the model, but it was identified as such so if somebody wanted to do that, I'm not saying they necessarily would, you could go back and take account of that.

MEMBER BROWN: Mark, you almost said it in that last statement. But can you say something about what you gained from the process of just searching for and identifying all the uncertainties?

MR. KIRK: Well, that's a long questions and Matt has told me to be short. So I'll try to just pick two.

One is I think it helps in terms of where we are right now and where we've been, which is that not only do you have to build these models, but then you have to defend them and defend their results to groups such as yourself, our external review board, industry groups, the public, and so on.

And by going through a very systematic process, and by documenting it all, not only in the summary report, but also in the detail reports, you know, when questions arise, you don't have to sit there scratching your head and saying, well, I don't remember. I mean I might not remember online, but I do know that it's documented somewhere so that interested parties can go back and see how each and everything was done and the basis for the decisions are documented, even if it's just, well, it was the best we could do at the time. We actually wrote that down.

So I think it improves the reviewability of the models, and it, also, is good in the long term in that if anybody wanted to look at this again, they know where to start in terms of what things to look at.

MEMBER APOSTOLAKIS: I have two questions.

First of all, I'm wondering why the quotation from Einstein is not repeated later when you talk about thermal hydraulics and PFM?

(Laughter.)

MR. KIRK: We can change that if we get a chance to have a break.

MEMBER APOSTOLAKIS: And the physical models.

PARTICIPANT: George, this applies to all, the thermal hydraulics --

MEMBER APOSTOLAKIS: No, no. It should be then in an earlier slide.

The physical models you have under numerically quantified, what exactly did you -- I know we discussed this a few years ago. Did you actually put uncertainty on the prediction of the model?

MR. KIRK: Yes. Well, I should maybe be a little more precise. When I say physical models, and this is mostly in the materials area, where instead of just, you know, we were talking about fracture toughness data earlier, instead of collecting together the tens-of-thousands of fracture toughness data points that have been tested, you know, plotting them and just doing a parametric statistical fit, we used the physical insights of how we expect fracture toughness to behave to guide the fitting form. That's what I mean there. And, in many cases, those physical insights keep you from being jerked around by what's otherwise noisy data and putting fits in your overall model that might be appropriate for one material or one small data set, but don't reflect the overall--

MEMBER APOSTOLAKIS: But at the end you had one model?

MR. KIRK: At the end you have one model, yes.

MEMBER APOSTOLAKIS: And that model's prediction is the prediction?

MR. KIRK: That prediction with uncertainties, yes.

MEMBER APOSTOLAKIS: And that uncertainty comes from where?

MR. KIRK: The numerical quantification of the uncertainties come from the data. The physical models provide, say, the overall trend with temperature or the notion of how the scatter at any one temperature should be modeled. In all cases right now, the numerical quantification of the temperature dependence of the scatter of the copper dependence of whatever comes from calibration of the physical models to data.

DR. SHACK: George, as another example, I know where you're driving, when they did the thermal hydraulic analysis, they accounted for uncertainties, they looked at both the uncertainties in the inputs and they considered uncertainties in the predictions of the model.

What they found was that the uncertainties and the inputs overwhelmed the uncertainties in the model because the boundary conditions --

MEMBER APOSTOLAKIS: Was RELAP.

DR. SHACK: Is RELAP.

MR. KIRK: Yes. The integrated model was RELAP.

MEMBER APOSTOLAKIS: Well, that's very interesting because yesterday we had the subcommittee where we talked about model uncertainty and the question of the issue of putting uncertainty of the model prediction was kind of dismissed.

I mean here they did it and they found that it was not important. But that's very different from being surprised --

DR. SHACK: Well, I would say they dismissed it yesterday. They treated it in a way different than you would have wanted it treated.

MEMBER APOSTOLAKIS: No, no. When the letter-writing times comes, we'll resolve it.

Aleatory and epistemic, I remember we had a presentation, again, a few years ago. What is an example of aleatory uncertainty besides the usual occurrence of the steam line break?

MR. KIRK: I'll use my favorite and it's obvious I'm a materials guy because I keep going back to that.

MEMBER APOSTOLAKIS: No, no. Go.

MR. KIRK: In that an aleatory uncertainty is the scatter in fracture data because if you, say this whole table was a plate of steel and I cut it up into a thousand identically-sized specimens and I put petite pre-cracks in them all in the identical place in the microstructure and I sent them to a testing lab and I got them all tested. I wouldn't come back with a thousand numbers that were exactly the same.

I would come back with some range of scatter, and that, to me, is an aleatory uncertainty, which, as an example, is propagated through the model because even your perfect state of knowledge tells you that there is an underlying physical uncertainty that, again, in this case, we've used that to propagate it through the model.

Conversely, an example of, say, an epistemic uncertainty and the materials --

MEMBER APOSTOLAKIS: No.

MR. KIRK: Okay.

MEMBER APOSTOLAKIS: So the numerical

calculation, then, the way I would imagine it, would be you fix the numerical values of the epistemic parameters and you have the aleatory. You do your Monte Carlo. You find the mean response, or whatever, the mean value of the quantity, and then you vary epistemic parameters in another Monte Carlo to find the epistemic?

MR. KIRK: Exactly.

MEMBER APOSTOLAKIS: Very good.

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: Thank you. That's good.

DR. SHACK: All you got to do is get right what you put in which loop.

MR. KIRK: That's right.

MEMBER BLEY: Mr. Chairman, I need to sneak in a correction to something I said earlier. The things that get you properly into the FRHs are fine. The actual FRHs, themselves, have not been looked at and there are potential problems.

MEMBER ABDEL-KHALIK: Right. That would be my guess. They're FRPs.

MEMBER BLEY: FRP1.

MEMBER APOSTOLAKIS: What are these by the way?

MEMBER BLEY: These are the functional response guidelines for pressurized thermal shock, the one I just looked at kicks you out on low pressure.

MR. KIRK: Okay. So the slide you have now, slide 18, just shows the three detailed study plants. You'll also hear me refer to these as the baseline analyses that we spent a lot of time working on. So we did detailed analyses of three PWRs, one from each of the domestic PWR manufacturers, one of these plants, namely Oconee, was used in the 1980s PTS study, whereas, the other two, Palisades and Beaver Valley, are two plants that are in that first ten-degree bin. They're very close to the current PTS limit.

So these were the three plants which we applied our detailed models to to get the through-wall cracking frequency out, and they also gave us a lot of insight, like I said, into what are the characteristics of the materials and transients dominate the failure frequency.

We, then, expanded our scope of investigation to look at five more high- embrittlement PWR's to see if those plant characteristics in these three plants that gave rise to the bulk of the PTS challenge, these three plants well represented the other plants that were likely to give us problems.

So with that, then the next step in the presentation is to go through at least a few more details in each of the major model components. We'll start with PRA. We'll go on to thermal hydraulics, and then we'll go onto, and I'm glad to see that Professor Wallis is not here. While I enjoyed his questions, I could never answer them.

DR. SHACK: He was always consistent about spelling SAPHIRE.

MR. KIRK: Yes, I'm very consistent in my spelling errors.

MR. KIRK: So in PRA, the goals of the events sequence analysis were, of course, to define the universe of potential PTS overcooling sequences using an event tree construction approach. The sequences were represented by an initiating event, followed by certain equipment or operator responses

The PRA analysis also defined the bin sequences and selected representative sequences from each bin for the TH model to actually run for the TH analyst to actually run through RELAP. And then, as I indicated in the graphic, the third main PRA goal was to estimate the frequencies, including the uncertainties with which each bin occurred.

This slide just summarizes the major information sources that were used in the PRA analysis. A review of LERs from 1980 to 2000 was performed. During that time 128 of the so-called more significant events were identified. These were primarily secondary overfeeds that led to minor overcooling, and in these sequences, the severity of them is obviously controlled by the operators.

MEMBER APOSTOLAKIS: I'm wondering why, I mean do these plants have plant-specific PRAs, Palisades, Oconee? Oconee must have.

MR. KIRK: The plants we studied, yes, yes, they dod.

MEMBER APOSTOLAKIS: So why did you have to go back to LER. I mean presumably they had done this.

MR. KIRK: I think this was just a background step to sort of review the history to see what things had happened in actual service. And, interestingly, the events that happened we can say at the end now, ten years later, none of these events that actually happened would calculate a non-zero failure probability under even the most severe embrittlement conditions.

DR. SHACK: They might have been looking

for that operator influence, as George and the LERs.

MEMBER APOSTOLAKIS: Some more insight is in the PRA because the PRA didn't have the PTS only.

MR. KIRK: Right, right. The PRAs had to be updated, expanded to include PTS. And like you just indicated, our starting point was the previous PTS PRAs from the late 1980s for all, well, I've got Robinson in here, which we didn't do. We did do Oconee, Beaver Valley, and Palisades, which doesn't appear on my slide, but they all had their plant-specific PRAs as a starting point.

We used generic initiator frequency and probability data representing industry-wide experience, and that summarized in several, both old and recently published NUREGs.

And then we had quite a bit of plant specific information for the three detailed study plants. A lot of interactions with plant personnel. We reviewed their operating procedures, we looked at their existing PRA, and, also, as I recall, at least two different simulator exercises at each plant.

Yes?

MEMBER BROWN: You said you reviewed the LERs. He asked why. But you said you look at those for background and when you applied those events to your --

MR. KIRK: Put them through the models.

MEMBER BROWN: The whole thing, this event, then coming up with the probability, none of them generated a non-zero result. Where do you define non-zero? Is that 10-to-the-minus-57-million? Because everything you've got in here --

MR. KIRK: Well, no because the fracture --

MEMBER BROWN: I'm not trying --

MR. KIRK: Well, no. The fracture toughness distributions all have an absolute lower bound.

MEMBER BROWN: Okay. All right. So that's in the fracture mechanics part of this whole thing?

MR. KIRK: Right. That's how you get a zero failure probability.

MEMBER BROWN: Okay.

MR. KIRK: Yes, we spent ten years calculating an awful lot of zeros.

MEMBER BROWN: I keep thinking $1x10^{-6}$. That's five decimals, five zeros and a one. So that is a above zero probability.

MR. KIRK: Yes.

MEMBER BROWN: And so far out that you can't --

MR. KIRK: And what we found out is that at low embrittlement levels, going back to the animation, the fracture toughness curve is at such low temperatures that the applied driving force to fracture from these events just never gets up to your lower bound line.

MEMBER BROWN: I got it.

MEMBER APOSTOLAKIS: This slide says, Mark, that the frequencies and failure probabilities in the PRA, say for Palisades, were generic. But then you looked at plant-specific information operator actions.

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: Why are the frequencies and probabilities also plant specific?

MR. KIRK: To take I think one easy example, medium- to large-break LOCAs have never occurred. We haven't even had precursor ones.

MEMBER APOSTOLAKIS: These are generic, I agree.

MR. KIRK: Yes.

MEMBER APOSTOLAKIS: But other things like pump failures.

MR. KIRK: And where you see, there's one slide later on, and what I've done is I bend the bins. I've added up all the initiating frequencies for, say, all the medium- to large-break LOCAs, all the stuck open valves that later re-closed to get five things I could put on a slide, and what you see in that is it's on the valve re-closure events where we get, in the three study plants, some plant-specific differences.

MEMBER APOSTOLAKIS: So the PRAs were plant specific to the extent possible?

MR. KIRK: To the extent possible, yes.

MEMBER APOSTOLAKIS: Because this slide gives a slightly different impression.

MR. KIRK: Yes.

Next.

This just discusses, at a high level, some of the different things that were considered in the PRA model. We have initiators, both at full and hot zero power. I've been cautioned by my PRA colleagues that LOCAs aren't really PRA events, but we'll leave that.

Anyway, we considered LOCAs. We considered various forms of transients, and, also, steam generator tube ruptures and large and small steam line breaks. Those initiators were followed, of course, by various equipment functions. They could happen in a primary pressure circuit, the secondary pressure, secondary feed, and primary flow-in pressure.

Basically, this is a comprehensive model of what's going to happen in the plant in response to a challenge, both automatically and by human intervention, yes.

MEMBER BROWN: The steam line breaks, some of the other ones you talked about, obviously, you can have a repressurization-type circumstance.

MR. KIRK: Yes.

MEMBER BROWN: But the steam line breaks are fundamentally a cooldown issue, aren't they?

MR. KIRK: It's a very rapid cooldown.

MEMBER BROWN: How do you get repressurization of a reactor vessel if you have a steam line break? I mean I guess -- Pardon? Okay. Just due to the high pressure injection for --

MR. KIRK: Right, right. And, in fact, you never really lose much pressure.

MEMBER BROWN: Hold it. You've got to put water in somewhere. I mean if you do that, that goes into the core, right? Your high pressure injection is

--

MEMBER ARMIJO: Goes cold leg.

MEMBER BROWN: And it goes in the cold leg. But there's no water coming out of the primary system under this circumstance in a steam line break?

MEMBER ARMIJO: It's solid. We fill it up with water.

MEMBER BROWN: Well, you've got a pressurizer. That hasn't gone solid in a steam line break necessarily. It seemed like more of a cooldown issue to me than it was a repressurization. It's just an academic --

MEMBER ABDEL-KHALIK: Academic to the shut-off head of the SI pump.

MEMBER BROWN: Well, that's true. That's true. Okay.

MR. KIRK: That's where you will go.

MEMBER BROWN: If there's no flow, that's where you will go. Thank you. Okay. Thank you very much.

MR. KIRK: The next slide just, again, at a high level lists some of the operator actions that were considered, again, in the primary integrity control, secondary pressure, secondary feed, and primary pressure and flow control. And not to go into the details here, but just to emphasize that we accounted for both things that the operators could do to end the event successfully and restore the integrity to the system, and, also, things that the operators could do that was wrong.

MEMBER STETKAR: Mark, I had to belabor this, but I'm intrigued a bit about the low pressure stuff.

In the secondary pressure control, bottom thing says operator creates an excess stem demand. Just stop me if you're not the guy to ask about the PRA stuff.

MR. KIRK: Depends on how deep you go.

MEMBER STETKAR: Is that an error of commission type thing, or is that also -- in a lot of emergency response procedures these days, the operators are told to rapidly cool down -- if you have no high pressure injection, rapidly cool -- blow down the secondary side, make sure you get primary pressure as low as you can get it through a combination of rapid cooldown and even open up the PORVs to get to low pressure.

Are those types of scenarios considered in this analysis --

MR. KIRK: Yes.

MEMBER STETKAR: -- where the operators are actually doing what they're supposed to do --

MR. KIRK: Yes, yes, yes, but --

MEMBER STETKAR: -- but because of that getting -- Okay.

MR. KIRK: The sequences we modeled followed the procedures. So, yes, yes, that's correct.

MEMBER STETKAR: Okay. I didn't know what the connotation was per excess steam line --

MR. KIRK: In some cases the operators may be doing things like you said that are increasing the thermal stresses. I mean you're playing a balance between thermal stresses and pressure.

MEMBER STETKAR: That's right. That's right. That's right, yes. Okay.

MEMBER APOSTOLAKIS: Now, what model was used here to quantify this?

MR. KIRK: To quantify?

MEMBER APOSTOLAKIS: The probability.

MR. KIRK: That was, I believe, in most cases based on the simulator observations and expert elicitation.

MEMBER APOSTOLAKIS: Yes, but there is a model, an HRA model.

MR. KIRK: That I don't know. I would have to find that for you.

PARTICIPANT: You can tell him that much.

MEMBER BLEY: Yes.

(Laughter.)

DR. SHACK: Matters of fact you can --

MEMBER APOSTOLAKIS: If I asked you how good it was, you could not say.

(Laughter.)

DR. SHACK: Moving on.

MR. KIRK: Okay. So we developed, as we've said, plant-specific models for our three detailed study plants. We started off, our first model was Oconee, and since we didn't have a lot of insights at the time the PRA model for Oconee was being expanded, if you will, to account for PTS, there weren't a lot of insights from the thermal hyddraulic modeling because that effort was just -- I should back up and say the PRA, thermal hydraulics and PFM-working groups all got working at about the same time.

So, initially, when our PRA team was building the Oconee model, they weren't getting a lot of feedback from thermal hydraulics and PFM because we hadn't finished building our model. So the PRA group couldn't send us a transient they were concerned about and said tell us how back this is because we didn't have functioning models at that time.

So that means that in the Oconee model was very much more detailed than any of the other plant models because we said these guys can't provide us any guidance, we'd better model everything we can. Whereas, later on with Beaver Valley, which was also built by our contractors, and Palisades, which was built by the licensees and reviewed by our contractors and ourselves later on, we had the insights regarding what sequences contributed most to the risk and what sequences didn't contribute hardly anything at all, and so the Beaver Valley and the Palisades models, I've said here, were less detailed, but I think I'd like to change that to say they were more detailed where it mattered because we knew where to focus our attention.

MEMBER MAYNARD: And I think was done taking into account differences in design? Oconee and Beaver Valley, some considerable difference, what may not be significant for one may be very significant for another.

MR. KIRK: That's right. That's right. Yes, each plant had it's own thermal hydraulic model, it's own PRA model and that was all accounted for, yes.

Again, talking about uncertainty and PRA, the aleatory uncertainties were implicit to the model that was used in terms of how particular event sequences were modeled, how the event sequences were binned, and how representative sequences from each bin were selected, and, also, say in discretizing the time for operator actions.

Obviously, an operator can act at any time. It's continuum. But we didn't model every time. We might have modeled operator acting never, or operator acting one minute after the procedure has allowed them to, or ten minutes after procedures allow them to. So these are uncertainties that we thought about and treated and they're implicit to how the model was constructed, but they're propagated numerically.

Whereas, the epistemic uncertainties that quantified the frequency of each modeled scenario were explicit and quantified and propagated through in the combination.

MEMBER APOSTOLAKIS: This is not true for later analysis as you explained earlier --

MR. KIRK: Right.

MEMBER APOSTOLAKIS: -- for the materials problems?

MR. KIRK: Yes.

This is, just again -- now I realize the type's too small. I apologize -- summarizes the more significant differences between the current PRA analysis and the PRA analysis that supported the current rule, 10 CFR 50.61.

On the left-hand side we've sort of binned these up into categories. We've included a lot more detail. We've treated operator actions and we've used new data. There are various individual things in here.

To take just one example, refinement of detail, there's a lot less gross bending of the thermal hydraulic sequences. If you go back to the circa 1980s analyses, the entire challenge to the plant may have been represented by only a handful of thermal hydraulic sequences. So when you have to put all of reality into only five bins and you're a regulator and you know you need to be conservative, inherently, you're saying that the challenge for an awful lot of your sequences is much, much more than it really is.

Whereas, in the case of our analyses, we have on the order of hundreds of thermal hydraulic sequences. And while that's certainly not everything that could happen, we were able to get a lot more refined, and, therefore, a lot closer to reality. And then that effect on the risk, since you don't have to be so conservative, is drives the risk down.

However, as is indicated by the arrows, some of these things that we've considered were considered in a different way, now versus in the 1980s has, in fact, driven the risk up.

MEMBER BONACA: One thing that they pointed out before, the huge difference between now and 1980 was the fact for the B&W plants, like Oconee, the steam line breaks were dominate because the operator action was denied. So, therefore, you had these cooldowns, blowing down, steam line break and feeding with main feed, no operator intervention. So you had this incredible cooldown that took us out and varies now.

For the new analysis, that scenario has been eliminated practically because, as was presented to us, credit for operator action has been given, and justifiably so. So I think it's important that that change be recognized in the report because it's a dominant issue, the fact we allowed for the operator action to be credited and that's very important.

I thought that when you look at now the steam line breaks, they're not contributing any more.

MR. KIRK: That's right.

MEMBER BONACA: And they shouldn't. But that's a big difference from what I was assuming in the 1980s.

MR. KIRK: Yes, that's correct.

DR. SHACK: Do you have a response yet to the Duke comment on the thermal hydraulic analysis of Oconee?

MR. DINSMORE: Hi. This is Steve Dinsmore from the staff.

Yes, we had the Duke comment and we went back and re-evaluated that sequence. And the Duke comment was pretty much what Dr. Bonaca just said, that you could turn a steam generator into a heat exchanger by just running it solid, running water continually through it.

The short answer to why the frequency is low is also that there's two independent control systems. There's a main feed water runback system and a high steam generator trip system, and they're independent. Both of those have to fail and then the operator has to fail.

So we got around about 10^{-7} sequence frequency for that event, and then we gave it back to Mr. Kirk there, who did some thermal hydraulic analysis and maybe he can explain that.

(Laughter.)

MR. DINSMORE: It was along the lines that you had twisted the curve a little bit. It didn't make a large effect on the final green curve for that plant.

MR. KIRK: Yes.

MR. DINSMORE: Because the initiating event frequency was pretty low.

MR. KIRK: So low.

MR. DINSMORE: And the embrittlement, it only made a difference if there was high embrittlement.

MR. KIRK: Yes. We did the PFM analysis and the conditional probability of through-wall cracking from the PFM analysis for the sequence that Duke asked about was

10^{-5}, conditioned on it happening, which --

MR. DINSMORE: It was about 10^{-7}.

MR. KIRK: 10^{-7}, so a 10^{-13} add to a 10^{-6} limit is nothing, but not absolutely zero.

MR. DINSMORE: Right. That would be addressed in the comment responses, somewhere in the final documentation.

MR. KIRK: Okay. So, now moving on to a few details on the thermal hydraulic analysis using RELAP, which I did spell correctly, and now I enter this with some degree of trepidation.

So the fundamental assumptions in our thermal hydraulic analysis is, first and foremost, that the RELAP probe provides an appropriate and accurate representation of conditions in the downcomer. Obviously, that needs to be right or we have no business being here.

That's true both overall for the transient conditions modeled and it's also true that no plues or thermal streaming of significance to the through-wall cracking frequency needs to be modeled. And I'll talk about each of these in detail in just a minute.

MEMBER BLEY: That means there was always good mixing, is that what that means?

MR. KIRK: That's right. There was always good mixing. I mean from our interval systems test, we'll get to it. The interval systems test said maybe it wasn't completely mixed. Maybe there was like a 10 or 20°C plume. But when we feed that to the probabilistic fracture mechanics analysis, and given that the plumes increase the axial stresses much more than they do the circumferential stresses, they don't really have an effect on the through-wall cracking frequency because the through-wall cracking frequency is driven by the axial flaws.

The axial stresses open the circumferential flaws, and the circumferential flaws can't go through the wall because of the orientation of the vessel. They'll initiate, but they'll stop about halfway through because they just run out of steam. So the thermal plumes, albeit small, whatever we ignore would increase only the axial stress, which increases the driving force on a circumferential flaws, but in vessels, in pressurized vessels, circumferential flaws have a natural crack arrest mechanism so they just don't contribute to through-wall cracking frequency.

MEMBER ABDEL-KHALIK: So stratification in the cold leg, which results in essentially radial gradient in the downcominer, you say that's negligible?

MR. KIRK: Yes. And since we're talking about it, we should go to that slide.

First off, just in terms of the physics of what's going on, our thermal hydraulic group looked at the -- in fact, performed much of the available experimental data. Obviously, there's significant stratification in the cold leg where you get injection. But by the time the plumes reach the downcomer and reach the belt line, there is significant mixing.

The biggest plume we saw in any of our interval systems tests, which are the best models of an actual vessel, were less than 10°C at the belt line location.

MEMBER CORRADINI: So just remind me, this is experiments back in the '80s. Where was this?

MR. KIRK: A number of different places. Rosa was 600. We've got an entire list and I can get that for you.

MEMBER CORRADINI: That's fine. That's fine. I'm just trying to remember the time frame and the key point.

And so, was the physical phenomena observed was that you mixing as it proceeding from the injection point to the downcomer enough that you got a minimal amount of what I'll call a cold spot that the vessel saw?

MR. KIRK: That's right. And then what we did, so we used the integral systems test to define the biggest magnitude of the cold spot. From that we get about 10°C. In our favor sensitivity studies, favor being the probabilistic fracture mechanics code, excuse me, we input, in fact, much stronger plumes than were ever observed in any of the integral systems test.

We used plumes from 40 to 80°C, and even at that plume strength, which was never observed, there was virtually no effect on the through-wall cracking frequency because what we were talking about was the fact the thing that saves you here is that the plume, since it's so much longer and the axial, whatever it is, it's much longer in the axial direction than in the circumferential direction, so it's producing a much larger axial stress than the circumferential stress is virtually negligible.

MEMBER CORRADINI: Can I say it differently?

MR. KIRK: Yes.

MEMBER CORRADINI: Your cold spot is axial, which creates an axial stress which stresses in the circumferential direction. There's no way to generate a cold spot that's circumferential which would create an axial stress that would give you a problem.

MR. KIRK: That's right, yes.

MEMBER CORRADINI: Okay. And people tried to do that? In other words, they looked for the region that I wouldn't get some sort of circumferential cold spot? Do you see what I'm getting at?

In other words, in the experiments I understand what you said. I'm just asking a slightly different question. People look at ways to see that it's out of the envelope of reality that I would get a circumferential cold spot that the flow would come in and meander this way and create a -- okay.

MR. KIRK: And that wasn't observed.

MEMBER CORRADINI: Okay.

MEMBER ABDEL-KHALIK: Let me try to understand again.

This 10°C is variation in which direction?

MR. KIRK: Variation-- I mean, of course, the water pours over the side of the vessel and goes down the side. So 10°C is the difference at any -- and it's working its way down the vessel. So is 10°C is the difference at the belt line elevation from the coldest spot notionally in the center of the plume to the ambient temperature outside the plume.

MEMBER ABDEL-KHALIK: And so if you look at a quadrant between two cold legs, two neighboring cold legs, you're saying that this 10° is between the center of where that cold leg is, presumably because that's the center of the plume?

MR. KIRK: Right, yes.

MEMBER ABDEL-KHALIK: And the midpoint between two neighbor --

MR. KIRK: Yes, yes.

MEMBER ABDEL-KHALIK: So it's in the azimuthal direction?

MR. KIRK: That's right.

MEMBER ABDEL-KHALIK: And that creates a stress that tries to open axial cracks?

MR. KIRK: Yes.

MEMBER CORRADINI: No, just the opposite. It's so local --

MEMBER BLEY: It's a long, vertical plume.

MEMBER CORRADINI: I think what he's saying, just to say it slightly differently, is I get a cold plume that's longer actually than it is circumferentially, which causes an axial stress and tries to open a circumferential crack. I'd need a cold plume that was this way to create a stress which would open an axial crack.

MEMBER ABDEL-KHALIK: No. I'm worried about temperature gradient in the azimuthal direction, okay?

MEMBER CORRADINI: Yes, 10°.

MEMBER ABDEL-KHALIK: And we're looking at the coldest spot, which is presumably some line that is co-incident with the midpoint of a cold leg, and then the warmest point in the downcomer, which is some line which is co-incident with the midpoint between two neighboring cold legs.

And if the plume is very narrow, that means there is a very severe trangential temperature gradient and that must presumably create a stress in the azimuthal direction that would tend to open axial cracks.

MR. KIRK: I didn't mean to imply that it created no stress. But it creates a very small increase in stress because, as your colleague was saying, it's so localized. The amount of thermal stress is roughly proportionally to the length over which the temperature gradient exists.

MEMBER CORRADINI: What I thought they said to us is everything you said is right, you get this cold thing, but it is more of a hurt on the circumferential pull than it is on the axial pull. You'd have to take the cold spot and make it like this to have more of a pull axially.

When you started to explain to Dennis, that's how I understood it.

MR. KIRK: That's right.

MEMBER BLEY: Because this long thing tries to get shorter?

MR. KIRK: Right, that's right. But if you take the cut --

MEMBER BLEY: But it's stress by the axle.

MR. KIRK: Exactly. If you take a cut through the plume axially, there's a very long distance over which there's a very cold, at the injection point, to the operating temperature. There's a very long distance over which there's a thermal gradient.

So there's a lot of distance over which the metal was trying to shrink, but the continuity of the vessel is resisting it. So you're building up stress or strain, which is generating stress, over a very long distance.

Whereas, if you take your cut azimuthally or circumferentially around, yes, there's a temperature gradient, but it's only over a very small distance. I mean if you think about it in taking it to the limit, if I had only something the width of a sheet of paper that's 10°C colder, the metal under that it can't move because it's constrained --

MEMBER ABDEL-KHALIK: By the outside?

MR. KIRK: Yes.

MEMBER CORRADINI: The thing I guess I'm most curious about is is that even what you observed experimentally, and these were heated experiments or similar experiments with salt concentrations, both?

MR. KIRK: Both.

MEMBER CORRADINI: Okay. And then back over here on the fracture mechanics side, even thought you saw ΔTs of 10 and 20, you then fed the fracture mechanics double or triple that to see the effect?

MR. KIRK: Yes.

MEMBER CORRADINI: Okay.

MR. KIRK: And there wasn't any.

MEMBER CORRADINI: Okay.

MEMBER BLEY: One last question just to tie this back to the old work. These crack-arrest mechanisms that take care of the circumferential data weren't in the earlier models, were they?

MR. KIRK: Actually, they were. They were because the crack arrest, it's not a material. I mean, obviously, the materials have a crack-arrest resistance, but that's not what we're hanging our hat on. We'll get a graph in a little bit.

I mean you're starting off, of course, with little bitty flaws and a big, thick vessel. So for cracking it, the driving force for crack initiation, whether that little bitty crack is oriented a little bit up and down or a little bit circumferential doesn't matter. The applied k for initiation is the same axial on circumferential.

But as the crack then initiates and propagates through the wall, if it propagates circumferentially, what our vessel experiments that were performed in '70s and '80s at Oak Ridge showed is that little surface crack, once it initiates, will first zip all the way around the vessel. It'll make a complete circle and then it'll start to move out.

And so what's happening there is that's a symmetric propagation if you will, and the vessel is much stiffer in resistance to the propagation. And so what happens, and you can see from our finite element calculations, is the k applied goes up quite rapidly until the crack's about a third of the way through the vessel and then it falls off.

So the driving force stops. And so, even if you initiated a crack, it wouldn't get all the way through the vessel.

Whereas, in the axial case, you start an axial crack, it runs long to the length of the belt line, but then that's not an axi-symmetric problem any more. And what happens is the vessel just keeps dumping stress into the crack tip and the k applieds just keep going up and up and up until the crack is out of the vessel.

MEMBER CORRADINI: So it actually speeds up instead of slows down?

MR. KIRK: They're all running very fast. It starts fast and it just keeps going versus starting fast and stopping. But that's fine.

MEMBER CORRADINI: What you're saying is one is damped and one is undamped?

MR. KIRK: Yes.

MEMBER CORRADINI: I'm sorry. Okay.

MR. KIRK: Okay. So then getting onto the second major assumption of the thermal hydraulic analysis is that a binned representation of the thermal hydraulic challenge to the vessel is appropriate and, more specifically, that it was appropriate for us to, as Dr. Shack was referring to earlier, while we studied and thought about the parameter and modeling uncertainties in the thermal hydraulic analysis, we didn't actually propagate that through to the PFM analysis.

And the reason why I've tried to summarize here is that, basically, the uncertainties, which we've ignored, are very small relative to the uncertainties which are implicit to have been representation of the PTS challenge.

In other words, we've got out of the PRA sequence analysis, we've tens-of-thousands of things that can possibly go wrong to create an overcooling sequence. Those tens-of-thousands, or even more, of things that could possibly go wrong are eventually represented down into numbers of in the order of the hundreds of thermal hydraulic analyses that are actually done.

So you've got, say, one thermal hydraulic analysis, say, for a medium-break LOCA that's now representing other medium-break LOCAs perhaps of different diameters, perhaps occurring at different seasons of the year, perhaps with different particular injection profiles, and the variability within that bin that this one sequence is representing is very much larger than the uncertainties that we've elected to ignore in terms of the differences in the thermal hydraulic diameters.

MEMBER ABDEL-KHALIK: Let me go back to the idea that you combining the risk or the probability of crack propagation from all possible scenarios. Some scenarios are more severe at the beginning of life, at hot zero power, rather than at the end of life. Or vice versa depending on the role of decay heat and model rate or temperature coefficient in terms of feedback.

How do you account for the possibility that the transient can occur at different times during the cycle given the fact that the consequences may not be the same?

MR. KIRK: I'm not sure if I'm answering your question, so let me try and we'll see if it works.

We've got the thermal hydraulic model, which includes many different sequences for each plant. Say, for Oconee, we modeled 200 different sequences. We took that thermal hydraulic representation of the challenge to Oconee, those 200 sequences, and we put it through our probabilistic fracture mechanics model at different points in the plant lifetime.

We ran it at 40 years, at 60 years, at 100 years, and so on. So we got the different through-wall cracking frequencies, the different response to the plant to the thermal hydraulic challenge at different levels of embrittlement.

Am I answering your questions?

MEMBER ABDEL-KHALIK: No, no. My question essentially focuses on --

MR. KIRK: Fuel cycle?

MEMBER ABDEL-KHALIK: A steam line break is not the same for all times during the cycle. The severity of a steam line break depends on --

MR. KIRK: On when it occurs.

MEMBER ABDEL-KHALIK: When it occurs during the cycle.

MR. KIRK: Okay. And that, yes, I'm sorry. I thought I was misunderstanding. I just didn't know what.

Yes, and that was accounted for because perhaps something I glossed over too quickly in the PRA discussion is the PRA analysis considered both initiation at hot full power and hot zero power and that was modeled in the thermal hydraulic analysis that we would look at the possibility of a main steam line break happening in your example under both hot full power and hot zero power conditions.

Those were different sequences, different bins.

MEMBER ABDEL-KHALIK: And you just account for the fraction of time that the plant will presumably be under one or the other condition?

MR. KIRK: That's right.

As you correctly pointed out, they generate very different PTS challenges. Initiators at hot zero power are much more severe because there's less, if you will, thermal inertia in the vessel. The compensating fact, of course, is that hot zero power happens a lot less than full power conditions.

But both of those, the increased severity of the hot zero power transient and the lower probability are both accounted for in the analysis. And that's true not only of main steam line break, but of all the other transient classes.

MEMBER POWERS: Suppose I had a set of conditions that were absolutely guaranteed to cause vessel failure, 100 percent probability, given those conditions, but those conditions only arose once every roughly 10^{-4} --

MR. KIRK: But does such a sequence exist?

MEMBER POWERS: I don't know. But supposed you had a sequence once every tenth of a time, it was absolutely guaranteed that it was going to fail, but it only arose once every 40 years, you'd say it was 10^{-5}, so I don't worry about it, right?

Maybe I have to get numbers down a little farther and it's actually guaranteed to occur or a very high probability of it occurring, you don't throw that out?

MR. KIRK: No, and I don't believe it wouldn't have been thrown out.

MEMBER POWERS: What weighting of these events --

MR. KIRK: Yes.

MEMBER POWERS: -- rather than evaluating them by themselves, especially hot shutdown events. I mean seems to me they should be examined all by themselves, not weighted by the amount of time you're there. Because you know you're going to be in shutdown, your cold shutdown every once in a while.

MR. KIRK: I mean certainly you can do that at the risk of appearing to dodge the questions. I mean that's a policy decision as to whether you want to look at an integrative risk assessment or take the worse transients that might occur and assume they do occur.

And, in fact, that's the approach that is taken in many other countries, Germany to just throw out one that I'm aware of. They identify the worst transient that could credibly occur and that becomes the design-basis transient that then has to be protected against, and that would lead to a different set of screening criteria.

That's not our model. That's not say it's not a reasonable model, it's just not the one that's been adopted here.

But in what you've described, as far as I know and I'd have to go into the details, an event wasn't excluded from consideration due to the incredibility of its occurrence unless the probability of its occurrence was something I think less than 10^{-8}, 10^{-9} per year.

So it doesn't strike me that we have been completely blind to any high probability events. High should be taken in context.

MEMBER BLEY: Conditionally high, right.

MR. KIRK: I can tell you right now the high consequence events just don't happen that frequently. The high consequence events are medium- to large-break LOCAs. They've never happened.

And stuck-open valves that can later repressurize, which have happened but are a lower consequence event. Main steam line breaks haven't happened. So you've got to judge the probability of these things I guess, and I'm not a PRA expert, based on precursors.

MEMBER POWERS: I mean when you speak in that fashion we're still averaging the year and what I worry about is going through periods of high risk that are guaranteed to happen. I mean I'm guaranteed to be in any core shutdown sometime in the plant's lifetime and that's a very high risk thing. It seems to me that there has to be an alert and say, hey, this PTS is very important and core shutdown and please pay a lot more attention here than you do --

MR. KIRK: But wouldn't that be, and I'm now stepping clearly out of my expertise area, but wouldn't that be covered by the operating procedures? I mean the operating procedures, from what we observed, and I'm not saying this is all reality, but just based on what we observed in the simulators, we couldn't make a PTS event happen.

In all three plants our PRA team was unsuccessful in feeding in an event to the simulator that would have generated any kind of a failure probability at all once the operators got a hold of it. Now, I realize we're talking about crediting the operator action and things like that, but my novice's impression of observing the simulators is that, I mean people were shall we say sensitized to PTS, had been sensitized to PTS for many, many years following Rancho Seco, following Three Mile Island. The procedures were all extensively rewritten and they're executed.

MEMBER BLEY: Mark, can I follow up?

MR. KIRK: You know better than I.

MEMBER BLEY: -- statement a little bit because what he's getting at is suppose you were given all these different conditions that were analyzed using the PFM and you calculated conditional probability. If any of those showed up high, those conditional probabilities, regardless of what the likelihood of getting to that condition was, but if they showed up very high, the question is, would you have looked at those harder, would they have been flagged in some way?

There's a parallel in shutdown PRA and that comes -- when we started doing those, you found that in one configuration with the level of drain-down, the conditional likelihood of failure was very high, and people then reacted and tried to, one, they put up warnings, you know, whenever you're in this state be especially alert to the following kinds of things; and, two, they tried to minimize the time error and they've done that.

But if you didn't flag those as being conditionally troublesome, even though on the average they're not big contributors, it slipped through the screen of this analysis. So at least that's the way I'm interpreting what Dr. Powers raised. When you saw something that had a high conditional probability, did you just drop it if it didn't get surfaced in the PRA, or did you flag those as being something to look at a little bit?

And if it were absolutely guaranteed, then, by golly, if you know the conditions under which it's guaranteed, you'd better do something about it.

MEMBER POWERS: It becomes a question of what's absolutely guaranteed. Is it a one-in-ten? If it's one-and-one, yes, we agree. If it's one-in-ten, it's guaranteed. If it's one-in-a-hundred, guaranteed.

MEMBER BROWN: But even when it elevates substantially there's no reason to live with that.

MEMBER BROWN: I mean is an example of what he's talking about inadvertent actuation of high pressure injection when you're in a cold? Is that the kind of thing you're referring to?

That's a plant condition in which you are going to be.

MEMBER BLEY: Yes, but it's not a real high threat.

MR. KIRK: But there is something. But I think the point --

MEMBER BROWN: No. I said cold. I said you're cold now, got a cold plant.

MEMBER MAYNARD: They have cold overpressure for protection. You'd have to have a number of different --

MEMBER BLEY: But you're right. Because of that, there are overinflations --

MEMBER BROWN: Well, I mean just relating back to what we used to do, I mean at least in our program was when you had that type of circumstance, you had breakers open with tags on them, or you isolated the high-pressure injection system, and so forth. You do something such that somebody can't inadvertently during a maintenance event accidentally turn one of those one.

Now, those are things you do to prevent them, but they get highlighted because of the severity of the conditional probability if it has in a high impact. But, yet, that cold plant condition exists only once every three years or some God-awful time.

MEMBER BLEY: Maybe I can ask it another

way. If I were to go not to the summary report, but to the detailed report on PFM, would I find a catalog of the highest conditional probability events anywhere?

MR. KIRK: No, no.

MEMBER BLEY: Somewhere I might look at them?

MR. KIRK: I mean you don't even have to go to the detailed report. I was just looking through the summary report, and, for example, this is at -- let's see, now, the other thing you need to take into account is, of course, the level of embrittlement. I'm looking at a very high level of embrittlement in Palisades and once I get above a break diameter of about four inches, the conditional probability of through-wall cracking is up in the 10^{-5}, 10^{-4} range.

But, I mean for this graph, that's at an embrittlement level that we wouldn't expect to see in Palisades until 200 years. I mean the straight answer is, no, we did not explicitly take that step or think about things that way. Certainly the information is available for one to do so, but it isn't, I mean just in trying to run through this in my brain, it isn't apparent to me that we have any of the conditions that have been postulated.

We don't have any bet all your money here it's going to fail every time unless -- well, I would say even if I crank up the embrittlement level to something that we'll never see in not only your lifetime and my lifetime, but my nine-year-old's lifetime.

MEMBER BLEY: I think you've just hit on it. You've looked at one. You said, gee, that's kind of a high value, but it's conditional. Here are the following reasons why this isn't a problem.

But when we have this wealth of information from this analysis, it seems it would have been wise, would still be wise for somebody to go back and look at those and say for any of these where it's high, could there be conditions such that we might get here that we could do something about.

And I think an answer like you gave to the one you identified is a perfect.

MR. HACKETT: Let me see and I like Dennis. This is Ed Hackett, ACRS staff. I want to see if I could add a helpful comment here, especially going to Charlie's point because one other answer in this regard because I see where the committee is going with this, is to look at the LER database, and I know staff has done that.

When you look at worldwide events, in fact, and Barry may remember this, a long time ago we looked at an event that happened I believe at the Kuosheng plant in Taiwan and it was going to Charlie's point, they had a coldover pressure event. So Mark's earlier answer, outside the population of PTS, are there events that have happened that could have challenged vessels? In that case, a BWR in cold shutdown and they managed to inadvertently plug certain lines and overpressurize the vessel in cold shutdown.

I think that's kind of where you were going, and Dana's not here, but I think there is another population that wasn't necessarily addressed as part of this study since this study was focused on pressurized thermal shock.

But, have some of those events happened? The answer is, yes, they have. And the controls hopefully that would be in place would be what are in the operational guidelines and in recovery procedures. But at least as I recall with the Kuosheng event, that still happened despite the procedures.

I don't know if that's helpful, but that's an example.

MR. KIRK: And that's certainly an

interesting way to look at it and I would agree, you know, useful and could easily be done because all of those answer, or all of those numbers are in Appendix A of Volume II of NUREG-1806 parsed up by embrittlement level.

So, perhaps, one way to do it would be to just go down and look at -- for each plant we did a 60-effective full power year, which would be beyond the end of the first license extension, just go through and see what the numbers are. My sense is there's not anything alarming, but it would be a good exercise to go through.

All right. Well, I'm going to try to change the slide and see if I'm successful, and I really have lost track how we got to here.

The point I was trying to make regarding the fundamental assumption and the thermal hydraulic analysis is that while we thought about the uncertainties in the thermal hydraulic analysis itself, and certainly recognized that there are many model uncertainties and parameter uncertainties in a RELAP analysis, those uncertainties are small relative to the uncertainties implicit to a bin representation of the PTS challenge, and they're also small relative to the frequency of occurrence of each of the PRA bins, and those much larger uncertainties were modeled and propagated through the analysis.

So the take-away point here is that even thought the thermal hydraulic uncertainties have not been explicitly modeled, they have been addressed. They're much smaller than the bin uncertainties, and, moreover, our model-building process, the PRA people didn't work in isolation, through a set of bin definitions over the wall to the thermal hydraulic's people who ran it, throw a set of thermal hydraulic sequences over the wall to the PFM people who ran it, and called it a day. If we'd done the project that way, the year would be 2002.

But there was a lot of iteration here and the main point is that the bin definitions changed over time. Because when we did the initial analysis, we were basing that initial analysis on insight from the 1980s analysis, which we've already identified while it was a pretty good analysis for the time didn't include all the important things.

And so when we got those initial results back, we say, hey, we did a hundred thermal hydraulic runs; look, only ten of these accounted for any risk of all and most of that risk is in these bins 36 and 98; gosh, maybe we better do a better job about subdividing those bins better to get a more refined view.

So I think that's another point is that then the bins that were driving the analysis got a lot more attention from the analysts and, in many cases, were subdivided and subdivided yet again, each with it's own thermal hydraulic representation, and so a further discretation of reality.

MEMBER ABDEL-KHALIK: So how is this subdivision done? What do you mean by being subdivided?

MR. KIRK: Okay. Just to take an example and I don't know if this is actually what happened, but let's just say it was.

Let's say we included all break diameters of four-inch and above in a bin. And, you know, we know now because we've done the analysis, that that would be a very significant bin and we'd look at it and we'd say, oh, gosh, that accounted for 90 percent of the risk. Well, maybe there's a difference between four-inch break and a six-inch break, and so it got, then, the total frequency of that uber-bin, if you will, remained the same, but it got subdivided, you know, part of the frequency goes here, the second, and part in the third bin, and then we ran a thermal hydraulic sequence in each of those bins.

So the bins that were the most important got the most attention from both a PRA standpoint and a thermal hydraulic standpoint.

MEMBER POWERS: I'm trying to understand better when you say the uncertainties by sequence frequencies are compared to where I presume to be phenomenological uncertainties in your thermal hydraulics?

MR. KIRK: Right.

MEMBER POWERS: That's the point. How do you compare one and the other? My uncertainty and my frequency is--

MR. KIRK: Okay. The only way, I mean because you're right. I put three histograms up there as if they're the same, but they're different. They should at least be different colors.

The comparison metric is the end result of the PFM analysis. You run all of this through the PFM analysis and you get a conditional probability of through-wall cracking or through-wall cracking frequency, and what we did in one circumstance was we took bins and we just kept subdividing it down like maybe at the beginning one thermal hydraulic sequence represented a hundred possible PRA outcomes, and we propagated that through the model and that generated a through-wall cracking frequency.

Then we divided that hundred into, say, four bins of 25, so now we've got four thermal hydraulic sequences each representing 25 PRA outcomes. Propagate that through the probabilistic fracture mechanics analysis, get another integrated result and just keep subdividing down, and, eventually, what you'll find out is you're continuing to subdivide down and get more and more thermal hydraulic-specific models, less and less representation, but, eventually, the through-wall cracking frequency that you calculate isn't changing any more because you're just distinguishing different shades of gray.

PARTICIPANT: You know this line that has the result where you have 95 percent and everything is at the far end, isn't that what you're talking about? It was so far --

MEMBER CORRADINI: They are communicating personally, so you've got to communicate louder.

MR. KIRK: Sorry. Dr. Powers is still puzzling.

MEMBER POWERS: Your explanation didn't help me at all. I'm pondering, as well as puzzling. Thanks for trying.

I presume that the analyses arises from things like heat transfer coefficients and entrainment coefficients?

MR. KIRK: Right.

MEMBER POWERS: And I presume that those quantities, an entrainment coefficient, you know it within a factor of two you're probably doing really good. So something like 100 percent uncertainty there. And heat transfer coefficient, about the best you can possibly do is about 25 percent.

And you're telling me that your sequence probabilities are uncertain by something larger than that?

MR. KIRK: Yes, several orders of magnitude.

(Momentary audio disruption.)

MEMBER POWERS: And yesterday I listened to all kinds of arguments on why we shouldn't worry about the uncertainties and the sequence probabilities.

MR. KIRK: You gentlemen are going to have to tell us what happened yesterday --

(Laughter.)

MEMBER POWERS: I listened to pages and pages of codification of why we should never have to characterize the uncertainty in a CDF. I can discount it totally.

MEMBER CORRADINI: Can I just repeat though? Most of you guys go back and forth. What you're really saying is some physical uncertainties are swamped by the sequence uncertainties, that's what you answered Dana.

MR. KIRK: Yes.

MEMBER BLEY: When you say the sequence uncertainties, that means?

MR. KIRK: Our estimate. I mean if you take any definition of a sequence --

MEMBER BLEY: From the PRA?

MR. KIRK: From the PRA.

MEMBER STETKAR: So that's an uncertainty in the frequency of those sequences?

MR. KIRK: Of that occurrence can be from the histogram that represents that might be from 10^{-5} events per year to 10^{-8} events per year, multiple orders of magnitude.

MEMBER BLEY: And that's because you've lumped a bunch of those sequences into one bin?

MR. KIRK: In some cases it's because you've lumped a bunch of sequences into one bin. In some case it's because the sequences have never happened, and so you're basing it on precursor data and judgment.

MR. DINSMORE: Yes. This is Steve Dinsmore from NRR. I'm going to kind of agree with Dennis. My understanding of this that it's because you lump so many different specific thermal hydraulic sequences into one PRA bin that you're saying the uncertainty, and then you took one of those specific TH sequences and used and assigned the whole frequency of the bin to that sequence.

MR. KIRK: That sequence, that's correct.

MR. DINSMORE: And the sequence that you chose, the thermal hydraulic sequence you chose was the worst one in the sequence. So you covered everything, all the individual sequences, the frequency was assigned to the worst sequence in the bin. But I wasn't involved in this project when it started. So that's my interpretation of what--

MR. KIRK: That's important if that's what's actually done.

MEMBER CORRADINI: I'm feeling better now I guess, to make sure, because what you're saying is there's a range of frequencies and you looked at the thermal hydraulic challenges and took the worst side of that population and then used that as representative of all that going forward.

MR. DINSMORE: Right.

MEMBER BLEY: And then when you say you break them down, you then, instead of looking at the worst for that whole set, you break it into pieces for the worst of the subsets, and now you get a range of things not as bad.

MR. KIRK: Yes.

MEMBER BLEY: So the uncertainty we're talking about is really a lot due to the binning?

MR. KIRK: Yes.

MEMBER CORRADINI: So I want to say out loud what you just asked, which is: In some sense, you've made judgments all the way along. Just pick a couple so I'm clear. For example, the cold spot judgment was it's so small as to not to carry forward, mixing is good. So now you take a RELAP analysis and you chunk along. Then you take RELAP analysis with various initial and boundary conditions and you look for the range of sequences and take the worst set of thermal hydraulic conditions and take that as representative of the range and carry forward?

MEMBER BROWN: Yes. So, in fact, I mean what I got out of it, I'm not a thermal hydraulic's guy, electrical puke-- so you have to I don't think very well.

I walked away from your first presentation. You don't include T&H uncertainties in this part of the analysis. Let me finish here. Based on the subsequent discussion and the comments, I gather, my opinion now is you really do because you've taken that bin, taken that worst case circumstance and plugged it in to cover that whole bin, in which case, it may be the wrong word, but, implicitly, you've taken all the uncertainties tied up in that T&H, the worst one, that you've applied across the board.

DR. SHACK: But what he hasn't accounted for is that worst one is still an uncertainty in that answer.

MEMBER BROWN: That's okay. I understand. I don't have a problem with that being neglected.

MR. KIRK: And you're right. Instead of the different-- the word that you used that I liked is a difference between an explicit treatment of uncertainties where the uncertainty in the worst one would be numerically propagated through versus the implicit treatment of selected the worst one and saying, okay, that's--

MEMBER POWERS: Yes. Okay. Now, I've a much better feel for what you were talking about than what -- I mean I was --

MEMBER BROWN: You're doing better than I am. I'm still perplexed.

MEMBER POWERS: Well, no. Well, that's because I'm not as smart as you are.

MEMBER BROWN: Basic reality states -- and you say, okay, here's the thermal hydraulic for this and this is the worst case.

Yes, but it could be ten times worse than what you just calculated.

MEMBER CORRADINI: How, Dana? I don't understand.

MEMBER BROWN: That worse case has already got its own uncertainties buried in it to develop the worst case in the first place. You don't have to explicitly pull those out, at least I didn't think you would have to explicitly pull those out.

MEMBER POWERS: I'm trying to understand why you thought that. He uses the RELAP code. He's selected some thermal hydraulic situation. He calculated the results from it.

MEMBER BLEY: But knowing is just a straight calculating.

MEMBER POWERS: He just ran the calculation and used whatever default parameters they told him to use.

Okay. But if I went in and looked at those parameters and said, well, you know, this number I don't know very well, and this number I don't know very well, and so at some confidence level the thermal hydraulic conditions could be ten times worse than what he already calculated, but he didn't look at that.

And, for the life of me, I don't understand how we can say, oh, well, the uncertainties in my frequencies swamp that. I mean I just don't know how you can compare--

Yes, without having looked how you compare the apples and the oranges here. I don't know how to do the arithmetic. That's the problem.

DR. SHACK: But as I recall this, when Moderas was doing this, and he was varying those parameters, he was taking each of those sequences and varying the thermal hydraulic parameters and taking, essentially, bounding that in answer. What he wasn't doing was then including the -- he did look at what he thought was the uncertainty in the RELAP prediction, but he found that his uncertainty, his variation due to the parameter changes within the bin was larger than his -- so, he did look at it and he came to the conclusion that those variations were bigger than the --

MEMBER POWERS: And all I'm asking Bill is how you do the arithmetic to come to that conclusion.

DR. SHACK: You do all the calculations.

MEMBER POWERS: Show me the damn numbers.

MEMBER BROWN: Dana, if the worst case is a best estimate analysis, I mean am I familiar with with worse case was worse case. You did an analysis and the worst case was generated by incorporating fundamental within the TH analysis basis, the uncertainties, or you came up with a worst case.

Now, if it's a best estimate where you throw out uncertainties, maybe I get recalibrated here and fall back into Dr. Powers bin.

MR. KIRK: Wish I had something to draw on at this point. I think, if I could step back, the characterization that Steve brought out is correct, that in each of these bins the aim of the PRA and the thermal hydraulic team was to select the worst transient from the bin to represent the bin entirely.

MEMBER BLEY: Worst in terms of challenges for PTF?

MR. KIRK: Worst in terms of challenges. However, that worst one of a hundred was then modeled best estimate. I know I'm using these words vaguely.

But we didn't pick the worst transient out of a hundred and then assume that the heat transfer coefficient was the worst you could possibly be and the flow conditions were the worst they could possibly be. We modeled that worst transient realistically.

MEMBER BROWN: Okay. Best estimate, roughly a best estimate.

MR. KIRK: Roughly a best estimate. But I think, and I can't -- you know, I'd have to go back to the documentation and get people here who know this to answer Dr. Powers' question.

But I think the qualitative answer is that say you do this for one bin and you find out your worst one is important. So you now decide to subdivide the bin and I'm now going to subdivide it into four parts. Each of those four parts, I now have a continuum of a hundred things and I picked the 25th thing, the 50th thing, the 75th thing, and the 100 thing where high numbers are worse to represent those four different quantiles, and, certainly the item number 25 might be worse than 25, it might be as bad as 30, but to some extent that's covered by the fact that I've also got transient number 50 representing the next part of the event challenge.

To me it's a discretization error, like when you, you know, for structural folks, as you refine a finite element mesh for modeling whatever, a plate with a hole in it, you know, if you try to model a meter-wide plate with an inch-diameter hole, if you use finite element blocks that are an inch big, you don't get a very good answer.

But once you get them down to a tenth-of-an-inch big, your answer is fine, and as you make the block smaller and smaller, the answer doesn't change, and I see that --

PARTICIPANT: Mark, is Mark here? Yes. As you're trying to explain this, I'm getting a little more confused than I thought I would. So instead of talking about taking bins and subdividing bins, let's stick with the notion of a bin. You have a bin.

MR. KIRK: Okay, a bin.

MEMBER STETKAR: Take your hundred sequences. They're in a bin and that's all. That's the world exists of 100 sequences in one bin. It's never going to be subdivided. That is the universe.

Now can you explain what you did? It's never going to get subdivided and how you accounted for uncertainties in the thermal hydraulic -- how you know that the uncertainties in the thermal hydraulic analysis performed for that bin that you'll never get subdivided are small compared to the uncertainties inherent in that bin. The bin's never going to get subdivided.

MEMBER CORRADINI: Yes, we got that.

MR. KIRK: I got that. Quite frankly, for that example, our approach would be inadequate because there's no reality on either. You define the universe as if that's all there is. But there's stuff on either side of your bin. There's more than one thing going on there.

MEMBER CORRADINI: But can I just try something?

MR. KIRK: Go.

MEMBER CORRADINI: Because I think is how --

DR. SHACK: I think we don't have someone here who can answer the question. So I think at this point we just call a halt to it. I think the conclusion is clear. They have neglected the so-called model uncertainties in the thermal hydraulics. Just exactly the justification of that --

MEMBER POWERS: Those are parametric uncertainties. I would disagree with that, but, okay, we don't have anybody here that can answer the question.

MEMBER BLEY: We'd like an answer.

MEMBER POWERS: We'd like an answer.

MR. KIRK: And we can get you an answer.

MEMBER POWERS: We can get an answer, but there's no point in pursuing it any further I think here.

DR. SHACK: Go a little further. You can ask the materials guy and the chair these questions, and then--

MEMBER POWERS: Yes, the question's going to come back.

MR. KIRK: Yes, that's fine.

MEMBER POWERS: I just ran it around on the first step.

MEMBER BLEY: We have yet to talk about uncertainties and probabilistic failures. Okay.

DR. SHACK: Is this a good time for a break for lunch?

MEMBER CORRADINI: He's almost done with his thermal hydraulics.

DR. SHACK: Yes, let's finish the thermal hydraulics.

MR. KIRK: There's one more slide on thermal hydraulics, which is just a very high-level description of what the RELAP5 model includes that should be frequent to the 2.2 gama.

Models the coupled behavior of the reactor coolant system, core, and secondary systems. It's just a simultaneous--

DR. SHACK: We know RELAP.

MR. KIRK: You know RELAP. And this just describes what RELAP is and how we used it.

DR. SHACK: Break then for lunch. Return at 1:00.

(Whereupon, the foregoing matter went off the record at 11:55 a.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

12:59 p.m.

DR. SHACK: Okay, gentlemen, if we can come back into session. Mark, the floor is yours.

MR. KIRK: Okay. Now we're going through at least a few of the details of the probabilistic fracture mechanics analysis and the computer code we use for that analysis is called FAVOR, which stands for Fraction Analysis Of Vessels Oak Ridge.

First off, one slide on the three major assumptions that were made in this analysis.

First is that a linear elastic fracture mechanics model was appropriate. From a theoretical viewpoint, that's an appropriate assumption because the plastic zone is a result of even the most severe loadings is very small relative to structural dimensions. And not only do we have that as demonstration, but we also have shown through various large scale tests performed at Oak Ridge and worldwide over the years that an LEFM approach generates accurate predictions of crack initiation failure in pressurized vessels subject to thermal shock.

The second major assumption is that sub-critical crack grown is negligible either due to environmental mechanisms or due to cyclic loading due to fatigue. This is important because our flaw distributions don't have a time-dependent component. They are taken as being fabrication flaw distributions and that's true whether we're doing an analysis of one year, 32 years, 50 years, or whatever.

Environment mechanisms can be neglected, first off, because the conditions aren't right for them, and sometimes because there's the stainless steel cladding in the way. And the cyclic loading just isn't enough to cause sub-critical crack growth.

Thirdly, we a priori eliminated based on deterministic analyses. The contribution of certain contributors because they were always zero, those being flaws. We simulate flaws uniformly through the vessel wall thickness. But when they're bearing more than three-eighths of the thickness into the vessel wall from the ID, they can't initiate, much less propagate because the driving force isn't there. Basically, they're in a compression zone.

And, secondly, transients that have a minimum temperature above 400°F were eliminated from consideration even if they were passed from thermal hydraulics. The last line notes that these were assumptions going in, but we demonstrated that they were appropriate and non-restrictive assumptions at the back end when we showed, based on the results of our calculations, that we could have actually not done any calculations on any flaws that were more than one-eighth of the way into the thickness from the ID, and, in fact, we got no contribution from transients unless the minimum temperature of the transient fell below 325°F.

So they were assumptions, but I'd say we validated them from our calculations.

MEMBER ARMIJO: Were there any experiments that demonstrated that cyclic loading didn't grow any of these sub-critical crack growths?

MR. KIRK: None of which I'm aware. I'm not a fatigue fellow.

DR. SHACK: Certainly, like all materials, these things will grow under fatigue. The cyclic loading on a pressure vessel is very low. Lots of people have looked at that and --

MR. ELLIOT: Maybe I can answer this. Not for part of this, but for other things that we've gotten from industry have looked at fatigue, and we're talking about --

MEMBER ARMIJO: A few cycles compared to --

MR. ELLIOT: Over 40 years you can go over crack a shield, 100th of an inch or one-tenth of an inch, or something like that, very small increment amount, we're talking about much bigger flaws that that so that this is -- the flaw distribution here far dwarfs anything that we could have from fatigue.

MEMBER POWERS: Isn't that where we got in trouble on the uncertainties when we said things dwarfed?

MEMBER BLEY: What are the flaw sizes? What's the initial flaw sizes?

MR. KIRK: the initial flaw sizes were --

PARTICIPANT: Wait two slides.

MR. KIRK: I'll be happy to.

PARTICIPANT: The distribution, of course.

PARTICIPANT: Yes, of course the distribution, but he wants to know how big.

MEMBER BROWN: Why is the elastic, once you get towards the brittle boundary from elastic materials when you're cold and in an embrittled states, why does that model apply to that particular sort of --

MR. KIRK: I'm sorry.

MEMBER BROWN: Well, we're talking about a brittle fracture-type situation here.

MR. KIRK: Right.

MEMBER BROWN: And, yet, you say you use a linear elastic fracture model all the way through, or at least that's the impression. Maybe all the way through is the wrong word. But why does that model apply as you approach -- I mean a brittle fracture is not a elastic?

DR. SHACK: No, it is. You're thinking plastic, Charlie.

MR. KIRK: It's the first part that they can't handle very well when this thing is tough and ductile that the elastic model doesn't work. The more brittle it gets, the better the elastic model is.

MEMBER BROWN: I mean, if you've got something that's brittle, it shatters. That's not a very elastic model, is it?

DR. SHACK: It's elastic fracture mechanics.

MEMBER BROWN: All right. I just seem to be a transition value. I'll take the expert's word for it. I pass.

MR. KIRK: And, in fact, we'll get into this. While we don't consider the possibility for ductile initiation from the first loading, the models that we had do consider the possibility for ductile tearing after the rest. So that's also part of the model.

The screen used to be bigger. I need to increase my font size, or maybe my eyes used to be better.

DR. SHACK: No, it was bigger when he had the old pull down.

MR. KIRK: Okay.

DR. SHACK: Members complained about that.

MR. KIRK: What the diagram would show, if you could read it, is that inside the blue potato-shaped blob are some of the innards, although not the very detailed innards of the probabilistic fracture mechanics model, and there are just four major sub-models that I'd like to highlight some of the details.

There's a flaw distribution model, a neutronic model, a crack initiation, and a through-wall cracking model, and I'd like to go into a few of the details on each of those, highlight what some of the difference are relative to what we did before, and what some of our improvements area.

MEMBER ABDEL-KHALIK: How do you handle the stainless steel liner?

MR. KIRK: The stainless steel liner is modeled in the FAVOR code, so it contributes on the stress side. It contributes residual stresses in the steel cladding.

It also contributes thermal stresses because there is the coefficient of thermal expansion mismatch between the stainless steel and the ferritic steel. So both of those are explicitly calculated by the FAVOR code.

And then the third thing, and perhaps the most important that the stainless steel contributes is a flaw population because you can get lack of inner-run fusion defects between the adjacent layers of stainless steel cladding. Our models include in them the possibility for a surface breaking clause oriented in the circumferential direction.

MEMBER ABDEL-KHALIK: So the flaw distribution includes flaws that are thinner than the thickness of the stainless steel?

MR. KIRK: No, no, no. Again, another one of those basic assumptions, we a priori eliminated the need to perform full tolerance calculations for flaws that were either surface breaking but didn't full penetrate the clad or that were imbedded fully in the clad on the basis that the toughness of the cladding is just so high that given the amount of stresses caused by PTS, that those would never initiate and grow.

But where the clad come in in terms of flaws is there's a finite, albeit small probability, that you could get lack of fusion between two adjacent fees, and that that lack of fusion could possibly penetrate all the way through the cladding so that the crack tip of the inner-run fusion flaw would be in the ferritic material.

MEMBER ABDEL-KHALIK: So when we talk about an initial crack depth of let's say a quarter-of-an-inch, so that would be a crack that just sort of penetrates all the way through the cladding and just barely goes into the --

MR. KIRK: That's right.

MEMBER ABDEL-KHALIK: Okay.

MR. KIRK: And, in fact, since we're talking about flaws, we should talk about flaws.

So, okay, Where do we get our flaw data? Primarily from experiments, destructive and non-destructive evaluation of several ex vessel materials that are listed on the bottom right-hand side of the chart. We had PVRUF. It's short for Pressure Vessel Research Users Facility. It was an ex CE-vessel fabricated in Chattanooga, never used to make a plant, but it was shipped on a barge up to Oak Ridge National Laboratory where it was subsequently cut apart for use in this project and, indeed, in other projects.

The Shoreham vessel was another one that didn't see service. And then there's Hope Creek and River Bend and we got ex service materials out of them. So we've done extensive and very detailed non-destructive and destructive examination of materials removed from these vessels.

We also have information from our expert elicitation that helped guide how these flaw models were constructed in my one graphic to compare. That answers your question on flaw size.

The new flaw distributions with the old, the old flaw distribution is shown in green, labeled 1980: Marshall. One of the main points here is that in the 1980s calculations, all the flaws were simulated as if they broke the inner surface of the pressure vessel, were all surface breaking.

The vertical axis is a measure of the flaw density. So that shows you how many flaws you have. And the horizontal axis is flaw size.

So comparing the all-surface breaking Marshall distribution with the other distributions you see that, in general, the Marshall distribution is predicting larger flaws, but not nearly as many as in our flaw distribution. The main thing to note about the new flaw distribution is that it's, aside for the surface-breaking flaws and the cladding that's shown in red, all of the other flaws are fully embedded. They are not surface-breaking.

The weld flaws go up to a little bit less than an inch, at which point we truncate. And I should note that the truncation limits were based on twice the flaw size that we saw in any of the destructive examinations. We also did sensitivity studies to demonstrate that even if we picked four times the flaw size, it wouldn't make any difference in the calculated through-wall cracking frequencies.

MEMBER ABDEL-KHALIK: Now, this distribution is given on a per unit volume basis.

MR. KIRK: It's shown here per volume. Actually, that's just something I didn't get it all on one plot. The base metal flaws are actually expressed per volume. The weld metal flaws are expressed per unit area because they're occurring predominantly as lack of fusion.

And so, how many lack of fusion defects scales in proportion to the amount of area on your weld prep that you joined. The volumetric flaws in the welds we really don't care about.

MEMBER ABDEL-KHALIK: I'm just wondering, in the base metal as well, wouldn't it be important to know the surface density of the flaws, as well as the volumetric density of the flaws?

MR. KIRK: But there wasn't really a mechanism to cause surface flaws.

MEMBER ARMIJO: Or underclad dense surface?

MR. KIRK: Underclad isn't shown on here and we're going to deal with that separately.

MEMBER ABDEL-KHALIK: Separately.

MEMBER ARMIJO: Is a truncation related to the clad?

MR. KIRK: The truncation on the surface, the surface flaws, which are a lack of inner-run fusion, yes, --

MEMBER ARMIJO: That's why it's truncated?

MR. KIRK: -- that's the thickness of the cladding, yes.

MEMBER ARMIJO: Okay.

MR. KIRK: And, in fact, just to show you one of the embedded conservatisms, in all of our destructive evaluation of cladding, we only found to lack-of-fusion defects of any significant depth and they were only I think 40 percent and 60 percent of the cladding thickness. So we never actually found a surface-breaking flaw.

MEMBER ARMIJO: Those aren't too important because it's most of the circumference?

MR. KIRK: You're right. Those aren't too important because they're circumferential. But we've modeled the potential for surface-breaking cracks to occur.

MEMBER MAYNARD: I just want to make sure I understand this graph.

This is what you use for input, then, in to your --

MR. KIRK: Yes. This is a representation of what we used for input. In fact, there are scatter bands on this. It's a statistical input. But, yes, this is just a pictorial representation of that.

MEMBER MAYNARD: And these come from if you put the actual data points and stuff, you'd have points around here and these lines?

MR. KIRK: That's right, yes.

MEMBER BLEY: Is this the data or is the result of everything including the expert elicitation?

MR. KIRK: Well, since what you've got on there let's say includes truncation limits, that's a result of everything.

MEMBER BLEY: Okay.

MR. KIRK: Because I mean the truncation limits don't come from the data, of course. That's an expert elicitation or a judgment.

But, again, I just want to emphasize, it's not possible to have a graph that represents the flaw distribution. There's actually a program that our contractors at PNNL wrote that express this statistical distribution and then they generate input files for the FAVOR code.

But one thing I think is important to point out before we move on is in terms of how important is the flaw distribution. We did some sensitivity studies where we used the old flaw distribution and the new flaw distribution in analysis of the Oconee plant and found if you fixed all other factors, the new flaw distribution reduced the through-wall cracking frequency by between a factor of 20 and a factor of 70 depending upon the embrittlement level relative to the flaw distribution that was used before and that people knew that at the time.

That was one of the main points in the letter to the commission is we don't know the flaw distribution very well and, hey, by the way, it's important.

MEMBER ABDEL-KHALIK: So what was the basis for the original flaw distribution?

MR. KIRK: The basis for the original flaw distribution was, I can't remember the exact number, but was a population of ex service flaws that were found in non-nuclear vessels. Code-fabricated vessels, but predominantly oil-, gas-, and petrochemical-grade construction.

MEMBER ARMIJO: Including flaws as deep as 25 percent of the wall? That's hard to believe.

MR. KIRK: I don't think that's an experimental result.

MEMBER ARMIJO: That's just somebody--

MR. KIRK: That's just we cut it off, yes.

MEMBER ARMIJO: Okay.

MR. KIRK: So that's it for now on the flaw distribution.

In the area of our fluence model, the ID fluence was estimated using reg guide 1.190 procedures, and there'll be a graphic in a few slides down. I don't think it's the next slide.

A major point that's different from our previous analyses and in this analysis, we fully accounted for the axial and azimuthal variation of fluence over the inner diameter surface of the vessel. Whereas, in the previous analyses, the inner diameter of the vessel was all assumed to exist at the highest fluence.

I don't think it's the next graph. No, so I'll just go on.

When we see the graph in a little bit, it'll become quite apparent that the peak fluence variations are, in fact, very, very small because of differences in the water gap between where the core is and the ID is. So by accounting accurately and in a credible way for this inner diameter variation of fluence, effectively, you take huge regions of the belt line of the vessel and just say there's never enough fluence to get you to an embrittlement situation where it even matters.

So it's like, essentially, taking those parts of the vessel out of contention for causing any kind of failure.

One thing that's not pointed out on this slide is the uncertainty in the fluence estimate is accounted for in FAVOR. And then the final point on here, which I alluded to before, is the other part of the neutronics model is the through-wall attenuation of radiation damage. It's still modeled conservatively using the equation that's in regulatory guide 1.99.

The reference there is an EPRI report that did a very nice job, an up-to-date review I think as of about four years ago, of all the experimental evidence that could be compared with the attenuation model and showed without any exception that the reg guide 1.99 fluence attenuation model always underestimates the amount of attenuation, which means it overestimates the amount of radiation damage. So that's a varied conservatism that's acknowledged.

The next area, and there's a lot of stuff in this box, but we're going to try to hit it at a fairly high level, is the crack initiation model. So this is the model that figures out what the fracture toughness of the material is, how much it shifts with the radiation damage, and what the loading is that challenges that.

I'm just going to try to hit on three high points that I've highlighted. One is that we removed the conservative bias in RT_{NDT}, that we've accounted for the aleatory uncertainty in the fracture toughness model, and that we've accounted for warm pre-stress effects, and I'll talk about each of those in a little bit more detail.

This cartoon just shows you the two parameters of the crack initiation model. The vertical axis is fracture toughness here, K_{1c}. The horizontal axis is temperature. The plot with the actual points shows the database that we used to calibrate the model.

The two parameters of the model is K_{1c} is the vertical scatter and RT_{NDT} is the index temperature that positions the toughness curve on the temperature axis.

And I think I'll be able to explain this a little better with the next slide where you see the same data graphic and the words point out that at the time the RT_{NDT} parameter was invented shall we say, it was made to be intentionally conservative.

When you estimate, first off, RT_{NDT} isn't measure of toughness. It comes from testing Charpy specimens and NDT specimens, neither of which are really fracture toughness.

And because at the time RT_{NDT} was arrived at in the early 1970s, there was not as much knowledge as we have now. Considerable and significant conservatisms were put into the RT_{NDT} model, and then that result is that the RT_{NDT} model doesn't position the transition curve very well on the temperature axis.

If anything, it's going to position it farther to the right at higher temperatures than it should be. And so what happens, and that's why there's this very ghastly degree of scatter here is that the curves aren't all indexed to where they should be by RT_{NDT}.

However, one of the directions from our management was that they wanted to keep expressing the PTS rule in terms of an RT_{NDT} metric because that's the information that all the plants had. So we had to figure out some way of trying to correct for this conservative bias on average while retaining RT_{NDT}.

And to that effect we used the best

estimate of fracture toughness known as the master curve. This is a concept where we indexed the K_{Jc} or the K_{Ic} data not based on Charpy and NDT, but based on fracture toughness itself.

It was originally proposed by researchers in Finland in 1984. In 1997 it was codified by the American Society of Testing and Materials. And in the following year it was adopted by the American Society of Mechanical Engineers as an alternative for arriving at RT_{NDT}.

But to dispense with all the fracture geek stuff that I love to go in so long, but Matt doesn't want me to and that's fine because I want to take my son to driver's ed, the reason why the master curve works so well is it actually uses a fracture toughness parameter to index where the transition curve is. Its transition temperature T_0 is based at the temperature which the K_{Jc} has a medium value of 100 MPam.

So if we take this rather scatter spread of data, indexed RT_{NDT}, means the same data, but now index each and every individual data set and there are probably 100 to 150 individual heats of steel on there. If we now change the index temperature on the horizontal axis from RT_{NDT} to T_0 --

MEMBER ARMIJO: How do you determine T_0

again?

MR. KIRK: You determine T_0 by testing six specimens, six or more specimens, and determining the median toughness from that, and then placing it on this master temperature-dependent curve.

MR. ELLIOT: But that's all in a standard?

MR. KIRK: Yes.

MR. ELLIOT: There's an industry standard now that tells you how to calculate T_0.

MR. KIRK: Yes. It's a measure of where the median curve through the transition is that's based on testing six or more fracture toughness specimens. Again, like Barry said, all of the details of that are outlined in an ASTM standard.

MEMBER ABDEL-KHALIK: So how is T_0 defined?

MR. KIRK: T_0 is defined as the temperature at which the median fracture toughness value is 100 MPam.

MEMBER ABDEL-KHALIK: Okay.

MR. KIRK: And that's why on this plot, you know, if you go up from zero in the middle, you'll find 100. You could have picked 150. You could have picked 75. The only limits are you can't pick it down here where it's athermal because you don't have any information, and you can't pick it up here where it goes on upper shelf. But other than that, it's just an operational definition.

But the key point is is that we're now indexing where this transition curve goes based on the data itself, not based on a correlation, and so it's no big surprise that we take this unordered mess where we've got a mix of epistemic uncertainties and RT_{NDT}, and aleatory uncertainties in K_{1c}. And if we, essentially, for all intents and purposes, eliminate the epistemic uncertainties and where the index temperature is, we recover what the true variability is in cleavage fracture toughness, and then this is what gets put into the model.

MEMBER ABDEL-KHALIK: By doing this, are we collapsing the data for different fluence levels?

MR. KIRK: Yes. And it's not coded this way, but what you see on there are high-copper materials, low-copper materials, high fluence, low fluence, no fluence. You, in fact, see ship steels. As long as it's magnetic --

MEMBER ABDEL-KHALIK: Body centered cubic?

MR. KIRK: Body centered cubic, yes. As long as it's body-centered cubic it works.

So, yes, you're collapsing. But what

that's saying is the effect of irradiation damage is not in either the temperature dependence or in the scatter, but it's all in the index temperature T_0. If you remember my original cartoon, it just marches to the right.

So one of the other models, which we'll talk about later, is the embrittlement trend curve model which says, okay, for my steel that has this copper and this nickel and this fluence, what's my T_0, what's my index temperature.

MEMBER ABDEL-KHALIK: But you're also assuming that that relationship between T_0 and fluence is unique, is universal?

MR. KIRK: Yes.

MEMBER ABDEL-KHALIK: And that is?

MR. KIRK: Yes, and I think that's a good judgment because -- hang on. Ask your question again. I'm about to go off.

MEMBER ABDEL-KHALIK: Implied in this process is the assumption that the relationship between T_0 and fluence is universal for all materials.

MR. KIRK: Index, let's just speak generally in terms of index.

MEMBER ABDEL-KHALIK: The change in T_0.

MR. KIRK: The change in T_0, the change in

ΔT_{30} is universal to fluence combined with copper combined with nickel. I mean there are a lot of things other than fluence that influence the functionality of that relationship.

For instance, if I plot ΔT_0 or ΔT_{30} versus fluence, I'll get a much different curve if I have a 0.1 copper steel than if I have a 0.3 copper steel. So I'm not sure if I'd call that --

MEMBER ABDEL-KHALIK: Yes.

MR. KIRK: I mean you need to incorporate that functionality. And then once you do, you can demonstrate, and what we've done in our work is to show, okay, once I determine that function between ΔT_0, ΔT_{30}, and copper-nickel influence, and so on, I can plot my residuals, my prediction error versus fluence versus copper versus nickel and I don't see any residual trend.

Now, I'd be the first to tell you there's a considerable amount of scatter in that relationship that is, in fact, modeled. But to the extent that we can resolve the trends and marry the physical understanding to the empirical data, yes, we've got a one-size-fits-all function.

MEMBER ARMIJO: What is the difference between the red and the blue?

MR. KIRK: The blue points are K_{1c} values. The E399 valids are there, the old linear elastic valid fracture mechanics values. Whereas, the red values are K_{Jc} values. They've got sufficient plasticity in them before failure that linear elastic --

MEMBER ARMIJO: Different types of test specimens?

MR. KIRK: Different types of test specimens, yes.

So what you see on here is a diagramatic representation of the temperature dependence and the scatter function that appears in the FAVOR model to represent FAVOR fractured toughness so that accounts for the aleatory uncertainties.

The epistemic uncertainties, since we wanted to retain -- if we wanted to go straight to T_0, we could have eliminated the epistemic uncertainties totally. However, the direction from the management is we wanted the RT_{NDT} basis.

So we then used the data sets where we had both T_0 and RT_{NDT} to essentially quantify how conservative RT_{NDT} was, and that's shown in the lower right-hand graph where we've got accumulative distribution function where the vertical axis just shows the percentage of the total data set, the horizontal axis is essentially a quantification of the conservatism in RT_{NDT}.

I'll just look at you and quit trying to point. What this shows is the bigger positive numbers, like a Δ RT_{NDT} minus T_0 of 150 means that the RT_{NDT} model positioned the transition curve 150° further to the right at higher temperatures, more conservative than it needed to be.

At the other end, there are actually a few cases where the RT_{NDT} model was a little bit non-conservative. But the diagram that you see here, actually, again, of course, it's mathematical representation. On the lower right of your screen is what was input to FAVOR.

So, essentially, what FAVOR does is it simulates an RT_{NDT} and then it goes to this model and it simulates, essentially, an error function. It says, okay, for this simulation of RT_{NDT}, how conservative is it, and it could draw a number anywhere from -20° to 150°, and that's, then, used to adjust RT_{NDT}, but if in bulk what this results in is approximately a 65° credit, if you will, to the RT_{NDT} assessment.

MEMBER ABDEL-KHALIK: If we go back to the previous slide, if the definition of T_0 is essentially arbitrary, you say, right, you assign a value?

MR. KIRK: It's arbitrary, but I guess what I would say is I'd be showing you the same picture if I picked some other arbitrary definition.

MEMBER ABDEL-KHALIK: But that was my question. Would you get a better fit had T_0 been selected to a level corresponding to 75 or 125 or 150?

MR. KIRK: No, not really, because the temperature dependence is the same through there. If the temperature dependence was affected by irradiation, if the curve laid over, if you will, got flatter as irradiation occurred, then the arbitrary decision would matter.

If I were to show this on a non-normalized axis and show you before irradiation where T_0 is maybe -150 and after irradiation where T_0 is +100, what you would see, of course, is the upper shelf marches down, so you can't see the very high fracture toughness values. But in the transition regime, which is where we're focusing, the shape of the curve is the same and it just shifts out.

So as long as you haven't selected your arbitrary index, K_{1c} or K_{Jc} --

MEMBER ABDEL-KHALIK: Too high or too low?

MR. KIRK: -- too high, too high being

upper shelf and too low being the athermal part on the lower shelf, it all works out the same.

MEMBER ABDEL-KHALIK: Okay. Thank you.

MR. KIRK: So that's the lower right-hand side of the screen accounts for the epistemic uncertainty in RT_{NDT}.

So that was a major difference in the old analysis where in the old analysis we treated RT_{NDT} as if it were true. And so, we thought materials were on average 65°F and more brittle, higher transition temperature than they actually are.

Another major change in the fracture mechanics model is the crediting of warm pre-stress, and this gets to the question of what's your failure criteria. On the material resistance side, you have a K_{1c} value, as is illustrated in red. And, of course, as we talked about theirs, there is a temperature dependence to that and there's some uncertainty.

But certainly the K_{1c} distribution for any given irradiation condition divides this space up, if you will, into three areas. One situation where I've got $_{k}$ applied values that are so low the fracture just can't occur. One where they're so high the fracture absolutely must occur. Unfortunately, we don't have any of those kind of transients. And then the region in between where fracture occurs with some probability.

Then the question becomes, okay, what's the failure criteria where you start to count failure probabilities, one in the classic linear elastic fracture mechanics sense, the only failure criteria is the $_{k}$ applied must exceed K_{1c}, and then you've got some probability of fracture.

What I wanted to do by way of illustration is to show you a warm pre-stress model that we've adopted that's validated relative to experiments in theory that changes that a bit.

MEMBER ARMIJO: Could you define warm pre-stress?

MR. KIRK: Okay. Warm pre-stress is simply to say that the failure criteria, the $_{k}$ applied, exceeds K_{1c} is necessary, but it's not sufficient to cause fracture.

And the analogy I'd like to use is a tensile test in that if I were to take a tensile bar and I loaded up to a post-yield -- the physics aren't exactly right, but the idea is the same. If I were to take a tensile bar and load it up to a post-yield condition, so the material's flowing, but it hasn't failed, if I now unloaded and I just wait, I can wait until now, I can wait until I die, nothing more is going -- if I'm not in the creep regime, nothing more is going to happen.

We're applying that idea to fracture toughness in that if your $_{k}$ applied exceeds K_{1c} and you're on the loading part of the curve, now you've got a probability for fracture. However, if $_{k}$ applied exceeds K_{1c}, but $_{k}$ applied is falling at the time, you've, essentially, already performed a proof test and you can't fail any more.

That's the intro and I could just show you some examples. This is a case, say, no radiation damage. I've now got an implicit time axis here, so my transient's always started by 50, and this is pretty classic of a PTS transient, driving force goes up, peaks, and then falls off, for purpose of illustration, forget the late-stage repressurization, but, in any event, in this case with no prior radiation damage, the driving force never exceeds the resistance and you just can't get into a failure condition.

If I had a condition where I had a very high amount of irradiation embrittlement, so now our current state is the red curve, and I apply that same transient, certainly now $_{k}$ applied is exceeded the

99.999th percentile of K_{1c} and you have to fail.

The third illustration for an intermediate condition, again, you're in a condition where you may break because you've exceeded your lower bound K_{1c}, but you haven't exceeded your upper bound. But the key point here is that where you went into the K_{1c} distribution, load was increasing. So just like in the tensile tests, you're continuing to pour energy into the cracked tip. You're continuing to move dislocations, and you're continuing to make the situation worse and worse. The fracture's more and more likely to occur.

Where we've excluded from causing failure probability in our calculations is this situation where now we get $_k$ applied values that exceed K_{1c}; however, the load is falling at the time. And in this case, and I'll talk a little bit about why in the next slide, the short summary is, in this situation, even though $_k$ applied exceeds K_{1c}, this can't break because $_k$ is falling.

MEMBER ARMIJO: And that's warm pre-stress?

MR. KIRK: And that's warm pre-stress. That's warm pre-stress.

It's not a new idea. It was first noted

in the technical literature in 1963. The physical mechanisms of why, this isn't a mystery as to why it happens. The physical mechanisms are well established. Some I've eluded to.

One is that once you load the material, you cause a plastic zone. And then once you unload, the dislocation's become immobile, so you're not feeding deformation any more into the cracked tip until you start to load again. If you're feeding more deformation in, if it hasn't fractured yet, it won't fracture now.

Another factor is that it's more favorable geometric situation. Once I load the cracked tip, it blunts and now I don't have a very sharp crack. I've got a blunt crack, so it's harder to initiate fracture.

And then, the third thing is that once you unload, you introduce compressor residual stresses in front of the cracked tip, and so now not only does the driving force to fracture -- from the applied need to exceed the material resistance, but it needs to overcome the residual stresses.

The third bullet points out that warm pre-stress isn't always active during all LOCA transients. It depends on the specifics of the transient and the location in the vessel wall. in very general terms, warm pre-stress matters a whole hell of a lot for medium- and large-break LOCAs because that's the type of transient I showed.

When you've got that initial thermal shock, you get a very quick rise in $_k$ applied and then it just falls off. So having warm pre-stress in your model makes the failure probability of those type of transients much, much lower.

Conversely, for the late-stage repressurization transients, there's really no effect of warm pre-stress at all because of the late-stage repressurization. The late-stage repressurization far overcomes the previous $_k$ peaked and the details of warm pre-stress just don't matter.

Okay. And all the information I just had on the slide could have been shown to you if you were the ACRS committee that was sitting here in 1984, except it wouldn't have been shown on PowerPoint.

So, why didn't we account for this in 1984? Well, it wasn't accounted for for two main reasons and they would both fall under the same category of we weren't confident enough of the fidelity of the rest of our model in both the PRA and the TH area to take this credit, if you will.

In PRA, for example, we didn't have a full accounting of operator actions, so we weren't sure that we had really smoked out all the situations where repressurization could occur. So we didn't want to give undue credit for that.

And the other issue was in the thermal hydraulics, whereas, now, if you'd look at our reports and our thermal hydraulic transients, there are lots of kinks and noise in them just like you'd see if you put a thermocouple in an actual plant. Whereas, in the 1980s, we used very idealized transients with exponential decays, and so there was concern that the idealized transient might show a warm pre-stress effect, whereas, the actual transient, because of little local reloadings, might invalidate it.

So it was for those reasons, not because we didn't understand warm pre-stress or believe it was real, but it wasn't taken account of before. Now both of those issues have gone away, so we've decided to take it into account and just, I eluded to this before, to roll up effect on the rules if you're looking at individual transients, the effect can be very large.

It's a huge effect for pipe-break transients. It's almost no effect at all for stuck-open valves. The integrated effect, considering that the PTS challenge is represented by a variety of transients is about a factor of 3:5 on through-wall cracking frequency, and I'm kind of pulling numbers out of distant memory here, but a factor of 3:5 on through-wall cracking frequency is about 10 to 15° on the screening limit.

So big, not quite as big as flaws, not as big as accounting properly for our fracture toughness models.

MEMBER BROWN: What allows you to take credit? Is this just the fact that before the transients initiated the wall or the material is hot or warm? I mean back this in what is warm pre-stress.

MR. KIRK: Okay.

MEMBER BROWN: I never got a picture of how I actually got a situation where the material was in the "warm pre-stress condition." What creates that condition? Unless I missed something.

MR. KIRK: No, it's not that hard of a test, no.

Let me try it another way. Take an example where I'm loading up a crack.

MEMBER BROWN: As with the driving force thing here?

MR. KIRK: Yes, yes, yes. As I load up a crack, maybe I can just go back to the end of one of those. Okay. So as I load up a crack, I have to start at 550. But as time increases --

MEMBER BROWN: That's the temperature of the crack?

MR. KIRK: This is the temperature of the core.

MEMBER BROWN: The core? Okay.

MR. KIRK: Yes.

MEMBER BLEY: And the inner wall.

MEMBER BROWN: The inner wall? Fine.

MR. KIRK: The inner wall. This is the inner wall temperature. So as time increases, temperature is screaming down. If driving force increased to the point, it's still increasing and it goes into the K_{1c} distribution, now I've got $_k$ applied exceeding K_{1c} and there's some probability of fracture. In the lower bounding models, you'd see it broke when that happened.

But what warm pre-stress says is in this situation where I've loaded the vessel up and it's achieved it's peak $_k$, but now $_k$ is falling, $_k$ applied has meandered into the K_{1c} space, but that's not enough to cause failure because I'm in an unloading phase.

Because I've got a plastic zone now in front of my -- I've essentially blunted my crack, I've introduced favorable compressive stresses, and so now just exceeding K_{1c} isn't enough. I need to not only K_{1c}, but I need to exceed all the previous K_{1c}s. I need to exceed, I'm sorry, all the previous $_{k}$ applied values.

MEMBER BLEY: Can I try one other thing and tell me if I'm saying this right? In the warm pre-stress condition, you're reaching your peak driving force and departing from it before you enter the K_{1c}?

MR. KIRK: Before I get any probabilities.

MEMBER BLEY: And the only way you can get there is if you were hot enough to start with that you're able for that to happen?

MR. KIRK: I mean I'm always at-- always at 550. The only time I can get there is if I move this curve far enough -- not too far this way so that this just comes up and nails it.

MEMBER BLEY: And that's not--

MR. KIRK: -- get it on the south side.

MEMBER BLEY: So you've pre-stressed by the driving force?

MR. KIRK: I pre-stress it. You could think of it as a pre-load, as a proof test. It's not a proof test. Of course, it's a transient.

MEMBER BROWN: By your earlier graph, on page 13, has it driving into the red band --

MR. KIRK: Yes.

MEMBER BROWN: -- before it starts decreasing. Is that the key is where that bend over is? If you turn and you start unloading as you enter this boundary, then that creates this warm pre-stress positive condition?

MR. KIRK: Yes. Yes, that's it.

MEMBER ABDEL-KHALIK: So the lower curve in this region corresponds to a zero probability of failure? Can't break.

MR. KIRK: You've got a zero probability of failure of 99.99, yes.

MEMBER BROWN: But you're saying if you go into that, flip that back to your other, more it where it's just the knuckle is outside. That's it. So this, even though you enter the may break, it can't break based on this scenario?

MR. KIRK: That's right, yes.

MEMBER BROWN: Okay. Sorry, I just didn't understand how that loading/unloading situation applied. It's not a temperature issue, it's a gradient of loading and the transition of the loading has gone negative.

MR. KIRK: That's right.

MEMBER BROWN: But when it enters that may break area?

MR. KIRK: That's right.

MEMBER BROWN: Okay. That's an interesting.

MR. KIRK: Okay. So done warm pre-stress.

Then the final area is in the through-wall cracking model. So once we get to this stage, we've got a crack that we've predicted has some finite probability of initiating and we want to figure out if it goes all the way through the walls.

So now to know this, and it's important to point out that because of the complexity of the load in the crack might initiate, stop, re-initiate, and so on as the loading progresses, so at this point now we need to, essentially, have a linkage between all our different fracture toughness relationships. We need to know where the cleavage-crack initiation toughness is, curve is, the cleavage-crack arrest toughness curve is, and, indeed, where the upper shelf is because as you get to the highly embrittled condition, while the flaws are so small they don't generate adequate driving force to initiate on the upper shelf, once that small flaw, if it were to pop, might grow big, say, a third of the way through the vessel wall and arrest, it could then re-initiate ductilely and we've accounted for that. And, again, this is a major change.

So we've got three models that collectively provide our fracture toughness module. The cleavage-crack initiation toughness, K_{Jc}, the cleavage crack arrest toughness, K_{1a}, and the upper shelf toughness, J_{1c}, this just illustrates that the models that we're using are informed by very large databases for RPV materials and other ferritic steels, including both irradiated, unirradiated welds, plates and forgings.

There are master curves for each case for the temperature dependents and the scatter. But then the other important feature that's new is that there are explicit linkages between all of these transition temperatures such that I'll go back.

Everything, basically, gets indexed back to the T_0 or the RT_{NDT} values such that once you know that, which is what you know from surveillance, you know not only the shape and scatter of the crack initiation, crack arrest, and upper shelf toughness curves, but you also know what the relationship in the temperature toughness space is between these curves.

There are systematic relationships between the T_0 index tempueerature and the K_{1a} index temperature and the upper shelf master curve index temperature.

Just to illustrate, this just shows the wide variety of data that we've used to calibrate the models to show how the models behave and are linked together in maybe an easier-to-understand way. I've just constructed a graphic, sort of like the cartoon.

MEMBER BROWN: What is upper shelf?

MR. KIRK: Upper shelf --

MEMBER BROWN: Is that a part of a curve? One curve back here had a little thing that went off. Is that upper shelf? Is it graphically?

MR. KIRK: Let me go to the next one. So this is upper shelf. I mean if you're at very low temperatures, you're on lower shelf. Sorry, that's redundant.

But you're failing by cleavage. You go up here, you start to get a little bit more plasticity, but it's still cleavage. But, eventually, you reach the point where there's no longer enough constrain in the material to generate that brittle fast-moving cleavage crack and now you're starting to fail by ductile rupture. So it's a change in failure mechanism. That wasn't illustrated on the previous points.

But what this shows is an illustration of how we've used the data and the physical models to link these three toughness distributions together in FAVOR. So what you see here is an illustration of what the limiting actual weld in Palisades would look like at the beginning of life. It has a T_0 of -85. The green curve is cleavage crack initiation, red curve is arrest, and the blue curve is the upper shelf.

And, again, I put the service temperatures on there and so what you see is it's pretty obvious at the beginning of life, even at the minimum temperature for a primary site pipe break, you're fully on the upper shelf. You have nothing to worry about.

However, once you go out to 40 years, the initiation curve shifts to the right, so does the arrest curve, but not so much, and the upper shelf comes down, and now you're clearly in a situation where, again, if you've got a series of very unfortunate events where you have an overcooling transient and a high fluence location and a flaw acting together, you could get cleavage crack initiation.

The other point to make here is we've got 40 years, 60 years, and, indeed, if we take it out to 10^{-6}, most of the embrittlement is occurring by precipitation of copper out of the matrix. Once all the copper is precipitated out, there's not that much more embrittlement that can occur.

So, basically, once we get to 40 years, there's not that much more embrittlement that's going occur, and that makes, at least from a materials' perspective, life extension arguments a little bit easier to make.

But, again, that was just an illustration of the various way, various large industry empirical data sets have been combined to inform the FAVOR model and how all the toughness models when you put them all together.

MEMBER BLEY: I don't know when the right time to ask you this, so I'll ask it now. You can get to it eventually.

FAVOR, as a computer code, has integrated all these different pieces together.

MR. KIRK: Right.

MEMBER BLEY: I assume a lot of the current version of FAVOR was developed during this work, especially the part including uncertainties. Is that right?

MR. KIRK: Yes, yes.

MEMBER BLEY: Sometime are you going to tell us something about how that complicated computer code and all has been validated and why we ought to believe that it's coming out of the end of it.

MR. KIRK: Now would be an excellent time.

Several ways. First, what do you mean by validate? What aspect of that -- the whole thing.

MEMBER BLEY: Why should I believe this stuff?

MR. KIRK: Okay, the whole thing.

MEMBER BLEY: Why should I believe the results that are coming out of this, including the uncertainty?

MR. KIRK: The basic fracture mechanics model is LEFN combined with warm pre-stress, combined with this type of fracture toughness information, the way that's all done in FAVOR, if we use FAVOR to predict, I mean it's a little bit hard because FAVOR is, in the mode we're using it, is generating a failure probability.

When we do large-scale experiments, which we've done a lot of at Oak Ridge and many of them done worldwide, there's not a probability at the end, there's strains you can measure, there's did it fail or didn't it fail, how much cracking was there.

But if we use the same methodologies that are in FAVOR to assess these large-scale experiments that were done at Oak Ridge and other places around the world, invariably, we get results that are in good agreement with those demonstration experiments.

So that's I would say a validation that all of these things linked together predict something that we care about. That's one part.

The other part is just a strict verification and validation of the computer code in that is it doing what we asked it to do. And we went through several years of that where we had external groups, including our colleagues at the industry and other of our contractors, take our program spec and run the cases and write companion codes to do the same thing to make sure that FAVOR was actually calculating what we wanted it to.

MEMBER BLEY: Now, this includes both epistemic and aleatory uncertainty and the different funny-looking distributions you might get for each of those in propagating them?

MR. KIRK: Yes.

MEMBER BLEY: And the people who have

written their own codes against your spec have included that kind of work?

MR. KIRK: Yes. When they pick up the spec and write a parallel code to do what we said we wanted to do it, compares one-for-one with the results of our code. I'm getting a look that's indicating I'm not answering the question.

MEMBER BLEY: I didn't read the peer review section. Does that really talk about this in any detail? Where would we see details so we can say, boy, that really looks like they've done the right thing and they're modeling all of this.

MR. KIRK: We have a separate report. I mean we've got reports on the FAVOR code. We've got a separate report on V&V, which I think is NUREG-1795. I don't know that the peer review got into that matter specifically. But you might also want to look at in NUREG-1807 one of the appendices, and I'll find it in a minute, NUREG-1087 was the PFM report.

If you look at Appendix A in that, that provides a summary of the overall fracture mechanics methodology validated against large-scale testing.

MEMBER BLEY: Part of my question is because I saw some results coming through this because of the part I was involved in and there was a time when things didn't appear stable. People were making little changes and results were moving around. And I assume this will talk about the final product?

MR. KIRK: Yes, yes.

MEMBER BLEY: And how it was benchmarked.

MR. KIRK: You're right because over here. You were involved in the initial stages of the project, and, yes, there was a lot of work, a lot of refinement, a lot of disagreement about what the model should be.

MEMBER ABDEL-KHALIK: Just to follow up on this. I mean, conceptually, I can see how you can verify the boundary that says your failure probability from experimental data. And you can also verify the line that says 99.99 percent failure probability also from experimental data.

But how do you assign probabilities in the intermediate region between the two graphs?

MR. KIRK: Well, that's where we get a lot of help from the physical models because, in the example of the crack initiation model, the physical of cleavage fracture is that it's a weakest-link process. In other words, generally, if I get one little carbide to pop and grow to a size larger than one or two grains, the whole structure fails. It's an unstable fracture at that point.

And so the physical understanding tells us that that should follow a Weibull distribution, and when we look at the experimental data, we, indeed, find that it does follow Weibull distribution. So the physical insights lead us to the proper statistical models to use to represent the data.

MEMBER POWERS: Weibull distribution comes out of a physical analysis? It's not just an empirical.

MEMBER BLEY: Or is it just that you can fit a Weibull to the --

MEMBER POWERS: Yes.

(Simultaneous speakers.)

MEMBER POWERS: -- a priori calculation of Weibull distributions?

MEMBER BLEY: Anywhere, yes.

MEMBER POWERS: It was normal between the combination comminution, but Weibull I think would just empirical. It just fits.

MEMBER BLEY: I mean if possible you can fit a lot of differential --

MR. KIRK: Indeed, indeed, indeed. I'd have to get back to you on that.

MEMBER BLEY: That would be of interest.

MEMBER ABDEL-KHALIK: Aside from that distribution, for a specific transient where you just follow the history of how the loading changes with time or with temperature, and you enter this intermediate zone, how do you assign a probability of through-crack propagation for something that doesn't completely go to the left of the upper curve?

MR. KIRK: We're back to old-fashioned things and get a white board. I mean it essentially depends on how far your $_{k}$ applied by you penetrates into the resistant space. But you could think of any, if you took a vertical cut through at any given temperature, you've got, in this case, a Weibull distribution there. And depending upon how high a $_{k}$ you get up to, that tells you what percentile of the distribution you've reached before you start to unload, and that, effectively, gives you your crack initiation probability for that event given that transient.

MEMBER ABDEL-KHALIK: So you have a different distribution for each temperature?

MR. KIRK: It's a temperature-dependent -- yes. The short answer is yes.

MEMBER ABDEL-KHALIK: So you follow the transient --

MR. KIRK: It's function of temperature. The distribution is a function of temperature.

MEMBER ABDEL-KHALIK: So you follow the transient, and at the point that gives you the maximum probability, that's the probability that you assign to that transient?

MR. KIRK: Yes. Okay.

Then we can just summarize and I think it was short enough that maybe we don't need to summarize. But we made relative to where we were in 1980, made significant improvements in many aspects of the PFM model, based both on physical understanding of the failure phenomena and extensive calibration to data sets.

In many cases we were able to obtain much better, more thorough, more generic models than we had before. However, there were some cases where the state of knowledge didn't permit improvement, and in those cases we retained and documented conservatisms in the model.

So with that, we are now at the stage where we finished reviewing the models we used to perform the calculations and now we get to the really interesting part where we can actually talk about the calculation results themselves.

So the sources of information here are NUREG-1806, particularly Chapter 8, and, also, NUREG-1874, which just is sort of a more up-to-date version of the details in NUREG-1806 where we made a few changes to the 1806 information as a result of comments that we got from the industry and from our external expert panel.

MEMBER BLEY: Can you kind of summarize the kind of comments you got that led to that change?

MR. KIRK: Oh, my gosh.

MEMBER BLEY: What were the important ones? Did anything substantively change?

MR. KIRK: No. Okay, I'll give you an embarrassing one because it's the only one that's coming to mind right now.

One of the members of our external review panel asked if, so our model is we've got all these embedded flaws. They initiate and then they instantly break back to the inner surface, and then they might or they might not propagate through.

And the gentleman said, well, surely you've included crack face pressure in your calculations. And it turned out, surely we hadn't. That was very embarrassing.

MEMBER CORRADINI: To include what? I'm

sorry.

MR. KIRK: Crack face pressure. Once you have an inner surface breaking flaw, then the pressure in the vessel can, in addition to the axial and circumferential stresses loading the crack due the stresses in the vessel wall, the pressure bears on the surface of the crack and tends to pry it open. It gives a little extra driving force.

So I'm not putting this out as the most significant. It's just one example of something that came up.

Well, it turns out, we hadn't modeled it. There was one of those things that a long time ago somebody said that can't be very big relative to the axial and hoop stresses in the vessel. We thought, well, to be fair, we should model it.

So we went back and modeled it. It turned out it didn't have any effect on the calculated results because for the local transients the pressure was low anyway, so it didn't matter. And for the repressurization transients, the pressure was so high it had already broken.

So when you looked at the model in the previous report, you say, well, that's just wrong. You've ignored a key component. And, indeed, in different situations, say you had a pressure-loading-only transient, we would have ignored a lot. It turned out it was okay here.

There were other things like a change the embrittlement correlation, a change in some of the details that I just showed you on the various embrittlement models.

MEMBER BLEY: I don't know if there were any of that sort. But given that one, did you look back and see if through screening mechanisms along the way you had thrown away scenarios for which that might have been important when they disappear because it wasn't important to start with?

MR. KIRK: I'm sorry.

MEMBER BLEY: The pressure, the one you were talking about with ignoring the pressure.

MR. KIRK: Yes.

MEMBER BLEY: And you said under a strong pressure transient it might have been an important things. Were there scenarios that might have developed that sort of situation that --

MR. KIRK: No. I mean --

MEMBER BLEY: -- what little there is, there is for one reason or another.

MR. KIRK: I mean it came up earlier.

There's cold overpressure, and that is a concern and there are plant safety systems set up to address that concern, but that's not a PTS concerns. I believe the answer to that is, no, there were things that we had missed because of that.

I don't have a printout of that. I'd have to get back with you on that.

So what we're going to try to go through here is two major areas of discussion. One is to discuss, what we want to get at is what are the things that are most important in terms of generating through-wall cracking frequency. Divide that into two major areas of discussion. One is material features and one is the class of transients that contributes.

So to start off with, material factors, this is the diagram I wanted to show before that just showed. So the big, blocky thing here is perhaps a poor attempt at an illustration of a vessel that's been sliced along an axial line and rolled out.

Just to illustrate that in the belt line of the vessel we're looking at a combination of axial welds, circumferential welds, and then a cladding laid over top of all of that. But to make the point about fluence, I know it doesn't show axes, but this is actual data I think from Oconee that just shows the characteristic, very large variations more azimuthally than axially of the fluence as you round the vessel.

So taking account of that can become very important in predicting the challenge to the vessel because if you just look at this illustration, this axial weld, which is going to have large flaws, and if the axial weld was made with a lot of copper, it would tend to have a high embrittlement. This axial weld exists at a fluence trough.

So if I take that into account, the contribution of this axial weld is going to be very, very small. Whereas, this axial weld is closer to the fluence peak.

And, again, accounting for those factors, which are completely knowable and calculable based on today's technology, wasn't possible in the '80s. In that case the entire inside of the vessel would be burdened with the peak fluence and then everything counts.

MEMBER ARMIJO: How big are those differences between the peak and the trough? Is it a factor of 10 or 2, or what?

MR. KIRK: I'm going to refuse to give you a number because I don't remember. But it's going from something that matters, like a 2 or $3x10^{19}$

neutrons per centimeters square. Down here, this is like a 10^{18}, 10^{17}.

MEMBER ABDEL-KHALIK: Okay, it's an order magnitude.

MR. KIRK: Something, yes, something that just doesn't matter, yes.

MEMBER ABDEL-KHALIK: And the spatial length scale on these variations is what?

MR. KIRK: Well, it's not necessarily the nozzles because it has to do with how the core is sitting. So four times around the circumference of the vessel, so it's on the order of multiple feet.

MEMBER CORRADINI: So when I see the peak, the fuel assemblies are close to the shield, and when I see a trough, there's water in the non-symmetric or the non-circular parts?

MR. KIRK: That's right.

MR. ELLIOT: I just want to point one things out of all of that. Two of the plants that you modeled, Mark, were Palisades and Beaver Valley, and Palisades is the worst weld plant and Beaver Valley is the worst plate plant. So they would have very bad locations for their welds that was put into this analysis.

MEMBER CORRADINI: And you assumed

presence of flaws in the welds, as well as in the bulk?

MR. KIRK: That's right.

MEMBER CORRADINI: As well as in the clad?

MR. KIRK: That right. And that's a good introduction because that's --

MEMBER ARMIJO: But those aren't assumed. Those are measured, right?

MR. KIRK: They're measured. They're measured from the destructive evaluation. I suppose it depends on how you like to use the word assume.

MEMBER ARMIJO: I think that there is fabrication data.

MR. KIRK: They're measured from the fabrication data. Well, no, not from each plant-specific fabrication data. We're using the model from PNNL, which is based on the measured flaws.

MEMBER CORRADINI: They don't know where they are. They're assuming where they are based on some representative sampling I assume?

MR. KIRK: That's right. And in each FAVOR run is a different FAVOR run through the probabilistic fracture mechanics code is, in fact, simulating tens-of-thousands of vessels and each one of those has a different flaw population seeded into it.

MEMBER BLEY: So it is done as a random?

MR. KIRK: Yes. But to get to your point about the flaw populations, yes, we've got a population of weld flaws that we're drawing from that are oriented along the fusion lines of the axial welds and the fusion lines of the circumferential welds. And we know from our destructive evaluation that those are really the only flaws that we found in those welds.

So then the weld orientation also gives us the flaw orientation. The axial loads have only axial flaws. The circ loads have only circ flaws and they occur along the fusion lines. So we know where they are and we seed them in with the densities and the uncertainty on the densities, and the uncertainty on the flaw side because we measure in our destructive valuation at PNNL.

There's also flaws scattered around the bulk in the plates. Those tend to be more frequent. They occur more often, but they tend to be smaller than the flaws in the welds.

And then over top of this is the clad layer. The clad's laid down circumferentially, then you can add the lack of inner run fusion flaws so, occasionally, on the average of about two to three per vessel, you'll seed in surface-breaking flaws in the cladding.

MEMBER ABDEL-KHALIK: If these peaks are strictly geometry-dependent, right, as to where the bundles are relative to the boundary of the surface.

MR. KIRK: How the core is turned relative to the fabrication, yes.

MEMBER ABDEL-KHALIK: Right. Wouldn't you have a double peak at each individual one of these peaks?

MR. KIRK: In some of the other ones there were double peaks, yes. I'd have to go back to the details of the fluence analysis, but there were some that looked much regular than this and I would expect that has to do with the fuel loading. But I'm not a fuels expert.

MR. ELLIOT: The point that Mark is making is he took a plant-specific fluence map and overlaid it on the vessel.

MR. KIRK: On the plant-specific vessel.

MR. ELLIOT: On the plant-specific vessel, and then he used that to calculate the through-wall crack frequencies. So this is more realistic than we did in the past, but we didn't do that. We just assumed all material was at whatever fluence is worse for that location. This is a big change.

MEMBER ARMIJO: Mark, I guess I misunderstood. I thought you had fabrication data on as-fabricated flaws either from ultrasonic testing or other things that was plant/vessel specific. You do not have that?

MR. KIRK: No.

MEMBER ARMIJO: All vessels will have the same flaws, flaw distributions?

MR. KIRK: Roughly. There are some scaling factors in the flaw distributions to account for their percentage of different weld types. There are, if you will, small knobs or twists on the flaw distribution model. But, yes, if you ask me if it's more generic or more plant specific, it's far more generic than it is plant specific.

MR. ELLIOT: We're going to talk about the flaws in the actual vessel later on today. We'll get to them.

MEMBER ARMIJO: Real flaws in --

MEMBER BLEY: Real flaws, we will get there today, hopefully.

MEMBER POWERS: The part you have been discussing this would be some sort of Monte Carlo sampling. How did you characterize your random number generator?

MR. KIRK: I'm sorry. How do I characterize it?

MEMBER POWERS: When you know it's a random number generator.

MR. KIRK: I'll have to get back to you on that. I don't know.

MEMBER POWERS: If you used a typical random number generator, they have low-cycle frequencies, the numbers tend to be correlated, they're not very good. If you used specialized, I mean there are some excellent specialized ones out there, but the ones that come with systems, computer systems, usually are lousy.

MR. KIRK: Well, let's hope we didn't use one of them. I'm sorry. I don't know the answer to that question, but we can get you the answer I'm sure.

MEMBER BROWN: Take-away on Sam's comment. Data relative to flaws, you said there was a model developed for flaw distributions, or what have you, sizes, based on one vessel that was analyzed by, who was it, PNNL?

MR. KIRK: Based on samples from four vessels, yes.

MEMBER BROWN: So that model would then produce the flaws, distributions and size, whatever, based for those four plants?

MR. KIRK: That's right.

MEMBER BROWN: And then that model is applied to every other vessel in the fleet everywhere --

MR. KIRK: That's right.

MEMBER BROWN: -- regardless of material, I mean they all don't have exactly the same weld materials, exactly the same plate or material characteristics --

MR. KIRK: That's right.

MEMBER BROWN: -- and so that's an extrapolation?

MR. KIRK: Yes, absolutely. As several points to make, though, we recognize that is that (a) what we found is that the distribution of flaw sizes in the various different samples we had pretty much all look the same. There wasn't any clear, weld process or plant-to-plant variability in the distribution of the flaws.

MEMBER BROWN: In the four?

MR. KIRK: In the four, yes. I'm speaking only in the four. However, there were significant differences in the density or number of flaws that were detected. And so the model that we used in FAVOR adopted the highest density that we saw experimentally. So that's one.

Another point is that the flaws that were found were characterized as being planar, in many cases even though they weren't. And so, of course, a volumetric flaw doesn't really count in a fracture mechanics analysis, but volumetric defects were counted as being planar and contribute to the flaw distribution, so, if anything over counting.

Thirdly, none of these things is ever truly planar, but in our characterization they're projected onto their greatest planar dimension, so another varied conservatism.

And then fourthly, which is the point that Barry will talk about, is based on comments we got from previous manifestations of this group, based on comments we got from the expert panel, and so on, it's recognized that four samples, while considerably better by any quantitative or qualitative measure than what we had in the '80s, is still only four samples.

And for that reason, 10 CFR 50.61(a) includes examination of ISI data as an entry condition. So essentially what we're asking the licensees to do if they want to use 10 CFR 50.61(a) is to either perform or review their ISI data and compare it in terms of density and size to the flaw distribution we used in these calculations to ensure that our flaw distribution is either representative of or bounding of the flaws that they find in their plants.

MEMBER BROWN: So that ISI is in-service inspection?

MR. KIRK: Yes, in-service inspection, yes.

MEMBER BROWN: Is that something they do periodically?

MR. KIRK: Yes.

MEMBER ARMIJO: Yes, that was my question you know. That data exists. I was wondering why, as a sanity check on your flaw model, you bear the actual vessels, like two or three vessels that had all this ISI data and say, yes, boundary--

MR. KIRK: Yes. As an example, unless Matt nots affirmatively, I'll remove the name of the plant. NRR has been approached recently by one of the licensees wishing to use there ISI data and they found four flaws.

MEMBER ARMIJO: Compared to your estimate

--

MR. KIRK: Four thousand.

MEMBER ABDEL-KHALIK: Yes, right, right. I expected you should bound, but it was nice to know that that's happening.

MR. KIRK: I can't tell you that we performed this sanity check of a comprehensive review of all ISI examinations. I can just say my impression is that if one were to do that, they'd find, okay, this vessel has four, this has three.

DR. SHACK: My first suspicion would be that it's not sound.

(Laughter.)

MR. KIRK: That's a regulatory question. You can ask Barry about --

MEMBER ARMIJO: It only found four?

DR. SHACK: Yes. I was expecting 4,000 and I found four.

MEMBER ARMIJO: Well, it depends on their resolution. Four sites --

DR. SHACK: Well, of course.

MEMBER MAYNARD: I think it's all in your size characterizations.

MR. ELLIOT: I just want to say that we're going to get to this this afternoon I hope. But just to answer your question, the reporting requirements in the ASME code are, you know, are a lot of flexibility. So they could have flaws and just not be reporting them. We're coming up with a new table of what's allowable, which is very small flaws, so they're going to have to report everything. And so that's --

PARTICIPANT: How long have they been doing --

MR. ELLIOT: I know. But at the end of the day here is people, when they do the future inspections, they are going to have to look at all the signals that they get to make sure that they aren't missing a flaw.

MEMBER ARMIJO: So if they're going to use this new rule, they're going to have to go and verify through their ISI program whether the flaws in their vessel are consistent with or bounded by the flaws in the FAVOR?

MR. ELLIOT: Yes, that's our concept.

MR. KIRK: Okay. So just one final point I'd like to make on this slide is that we forget PTS, we forget all the complexities here. Just pretend you're doing a normal flaw assessment or failure analysis. Somebody's brought you a broken part and you need to try to figure out why it failed or at what load it failed.

One of the key inputs is, of course, going to be what's the toughness at the location of the cracked tip. So that's what we wanted to try to -- we wanted to capture in as simple a way as possible, but not too simple because things can get confusing when you do that, try to capture the variation and fracture toughness on the inner diameter of the vessel.

Now, one thing to just point out is, of course, fluence is varying all over the map, all the way from values that really matter a lot to values that don't matter at all. Each plate and each weld, fundamentally, has a different chemical composition, so different irradiation sensitivity. And, also, the flaw sizes are different in the welds and the plates.

So you've got different levels of challenge all across the inner diameter of this vessel, and, of necessity, it's something of a compromise as to how we're going to try to capture that complexity in as few a number of parameters as possible and this is what we struck on was developing four different what we call reference temperature metrics: one for the axial weld, one for the circ welds, one for the plate, and one for forgings, and we'll just work through the axial weld in detail and I think then the rest will follow.

So our metric is called RT_{MAX-AW}. It represents the maximum irradiated of RT_{NDT} index temperature located anywhere along the axial weld fusion line. Now, obviously, if you go back to the previous diagram, some axial weld fusion lines might have very low RT_{NDT}s. Some might have higher RT_{NDT}s.

But we said, okay, well, what's sensible? Is it something more likely to fail where it's low or high? Well, obviously, where it's high. So we picked the highest value and we knew where we wanted to look, which was along the fusion lines of the welds.

But along the fusion lines of the welds, you've, of course, got two adjacent sets of properties. You've got the axial weld properties and you've got the plate properties. So, again, we picked for the RT_{MAX-AW} whichever one was dominating or higher. We'd compare the sum of the unirradiated RT_{NDT} and the RT_{NDT} shift and the axial weld and the plate, and whichever one was higher was assigned to RT_{MAX-AW}.

Similar idea for circ welds. Now, circ welds, you know since they go all the way around the vessel, are going to get knocked by the highest fluence in the vessel. But, again, they might be limited by either the circumferential weld or the plate properties.

Say the RT_{MAX-AW} and RT_{MAX} circ weld characterized, in a general way, the toughness values associated with the axial welds and the circ welds, and then we've got metric score, the bulk toughness in the plate and the bulk toughness in forgings in vessels that have forgings.

So those are the metrics we used and then we used those as regressor variables to -- we used those as regressors and compared those regressor variables to the through-wall cracking frequency generated by the different flaw populations.

So here you've got, again, let's just put this on axial welds, you've got, this is an embrittlement measure, RT_{MAX-AW}. So low embrittlement down here, high embrittlement here at the upper end. We've got our points on here for Beaver Valley, Oconee, and Palisades. And the reason why there's more than one point for each plant is we've done each plant at four different embrittlement levels. In all cases we started down at the lowest embrittlement level as characteristic of 40 years of service, and then we kept cranking the embrittlement up until we got through-wall cracking frequencies above the 10^{-6}

value.

And then, like I said, this is the through-wall cracking, the 95th percentile of the through-wall cracking frequency due only to the axial weld flaws versus the toughness of the axial weld. Through-wall cracking frequency of the plate flaws compared with the toughness of the plate and circ flaws compared with toughness of the circs.

And what you see here is once you're essentially blaming the through-wall cracking frequency, through-wall cracking frequency is what's gone bad. What's responsible for it going bad? Well, low toughness materials. Once you get the blame right, once you say that the axial weld through-wall cracking frequency is due to the toughness of the axial welds, you see all three plants lining up, for all intents and purposes, on a very similar curve.

MEMBER CORRADINI: Can you say that again? I guess it seemed obvious to me, so you said it. Can you just repeat what you just said?
The reason they all line up is because of what?

MR. KIRK: Well, the fullness of the reason why they all line up, we'll only get to once we discuss transients. So I'll have to break to the bottom line of transients.

What we find out when go through the discussion of what transients are important is that only the most severe transients are important and they're very similar from plant to plant.

MEMBER CORRADINI: Okay. Right.

MR. KIRK: So the level of challenge is essentially very similar from plant to plant. But the distinction I'm trying to make here, which I see the materials people nodding their heads in the risk of everybody looking at me like I've lost my head, so that's maybe an indicator that I skipped something important.

In the current PTS rule, there isn't a different RT for axial welds and circ welds and plates. There's one RT for the whole vessel. It's called RT_{PTS}.

And just as an example, in many vessels, RT_{PTS} is controlled by the circ weld. It's the most limiting. It's the highest value because it has the highest copper.

Veronica, can you go to the --

MS. RODRIGUEZ: This one?

MR. KIRK: Yes, just go to the next slide.

What we've done here is I've taken off the plant points. I've just put all three curves on the same graph. And what you find out is that, say, we go up to a through-wall cracking frequency where I get about a 10^{-6} contribution from my axial welds. At that same level of embrittlement, I'm getting less to 10^{-9} from my circ welds. And the reason for that is, like we discussed before, is that a circumferential oriented flaw has a natural crack-arrest mechanism. It can't initiate, but it won't go all the way through because it runs out of driving force.

So even though this circumferential flaws and the axial flaws are the same size, even though the circumferential flaws are unquestionably burdened with the higher fluence than the axial flaws because they're going to get the max fluence in the vessel, they generate over two orders of magnitude less through-wall cracking frequency simply because a circumferential crack in a vessel is going to arrest once it's initiated.

MR. ELLIOT: I just want to point something out. The current rule has two scrutiny criteria, one for the axial load, one for the circumferential weld. And what Mark is showing is that the circumferential weld evaluation we did in the past was way too conservative. It wasn't even in the ball park.

In essence, circumferentials are going to fall out and so we're going to be only worried in the long run about axial welds, while in the past people spent a lot of money worrying about the circumferential welds.

MR. KIRK: And the idea in the current rule is correct that the screening limit for circumferential welds could be higher than for axial welds.

MR. ELLIOT: Right. But you're showing that --

MR. KIRK: What our current analysis shows is it could be way higher.

MR. ELLIOT: Right.

MR. KIRK: So what we get out of this is that axial welds -- axial flaws and the material properties that can be associated with axial flaws, that being axial weld properties and plate properties, those are the material features that are going to dominate failure.

Plate flaws, and, of course, plate properties, are an intermediate case because they always get burdened with the max fluence of the vessel and they tend to be smaller than weld flaws, but they're a lot more of them. But there are a couple hundred weld flaws in the vessel. There are probably a couple thousand plate flaws because there's just so much more real estate. So it's much more likely to get a plate flaw at a bad location than a axial weld flaw, but they're also not always as embrittled.

Anyway, the roll-up effect of this is that the plate flaws, in and of themselves, produce sort of a minor contribution, maybe an order of magnitude to an order of magnitude and a half less than the axial welds. And the circ welds use a degrees ranking. Sorry about that.

You've got to go out to, say, 900° ranking or 410°F to get a 10^{-6} through-wall cracking frequency from a circumferential weld.

MEMBER CORRADINI: All right. That helped a lot. Thank you.

But the green line still controls. And given the fact that you don't know what's in your vessel, it still dominates and that's in there.

MEMBER POWERS: Yes.

MEMBER CORRADINI: No. But I thought we went through that, that we have specimens that way what they might be, but if you impress upon that, the axial weld dominates the limit.

MR. ELLIOT: I just want to point one

thing out. I don't think we all got it here.

When we look at the axial weld, we are looking at the fusion line between the plate, its adjacent plate and the weld. So that we have to take into account the plate problems. And if the plate could be more limiting so that we assign where the weld is, we assign the plate properties to the weld.

MEMBER CORRADINI: Can I go back to the curve that you showed me?

MEMBER ABDEL-KHALIK: Yes, but wouldn't these results tell you that in none of these cases that you looked at the plate properties were really selected, that you always selected the weld properties?

MR. ELLIOT: In the case of Beaver Valley, the plate is limiting and so --

MEMBER CORRADINI: Wait a minute. Let's just look at the line.

The way I interpret this, unless I misunderstood the explanation we just heard, is that the axial weld is the limiting value for getting to the unacceptable probability.

MR. KIRK: The axial weld flaws.

MEMBER CORRADINI: The actual weld flaws.

MR. KIRK: And the material properties

that can be associated with that flaw on the axial weld, which means the axial weld material and the plate--

MEMBER CORRADINI: Sure, yes, I understand.

MR. KIRK: Okay.

MEMBER CORRADINI: So the green line limits?

MR. KIRK: Yes.

MEMBER CORRADINI: So maybe I took a wild and crazy step. But are you saying that the green and the red and the blue may interchange themselves where different vessels and different locations?

MR. KIRK: No.

MEMBER CORRADINI: So then I'm back to my original thing. Green dominates regardless?

MR. KIRK: Yes.

MEMBER CORRADINI: Okay.

MR. KIRK: The others have a small --

MEMBER CORRADINI: We know all of these are smaller, and, as you had pointed out, exceptionally much smaller than you had previously thought of, the green or the axial weld location dominates in terms of the limit?

MR. KIRK: Right. When we get to I think my last slide before I go swallow three gallons of iced tea and give the microphone to Barry, is in the last slide, my slide, you'll see that all of the reference temperature limits that we're now proposing are higher than our current limits.

But the amount by which they've increased is much, much greater for the plates than the circ welds than for the axial welds. Same.

MEMBER CORRADINI: Right. Okay. All right. Thank you.

MR. KIRK: Okay.

MEMBER ARMIJO: Just one last thing. I want to make sure I understand. The green line is controlled by the flaws in the axial weld?

MR. KIRK: Yes.

MEMBER ARMIJO: And the mechanical properties of the irradiated plate material or the mechanical properties of the irradiated weld material, which is a cast structure?

MR. KIRK: Right.

MEMBER ARMIJO: Which is --

MR. KIRK: Either one.

MEMBER ARMIJO: Whichever is worse?

MR. KIRK: Whichever is worse, yes.

MEMBER ABDEL-KHALIK: And you said on some cases it was the weld and in other cases it was the--

MR. KIRK: Yes. Like Barry said, okay, for two we analyzed, in Palisades, it was always the weld. The weld had far higher copper for more embrittlement. In Beaver Valley, it was the plate that was dominating.

MEMBER BLEY: But in both cases it's the flaws in the weld that were initiated?

MR. KIRK: In both cases it's the flaws on the weld.

MEMBER BLEY: That's what I was getting to.

MR. KIRK: That's the important point, yes. I'm sorry. I've alluded to this, so I should show it.

This shows the variation of $_k$ applied, fracture driving force. As you go through the reactor pressure vessel wall for just for a crack that's initiated and then is propagated through the wall, the lower red curve is for the circumferential-oriented crack. And so you see that's what I've been saying is that the $_k$ applied peaks and then falls off.

And so circumferential cracks, while they can initiate it, tend to arrest somewhere between a quarter and halfway through the vessel. Whereas, the axial weld flaws, the driving force just keeps going up and, eventually, either blows out the back wall or fails by ductile rupture or overload.

In the end this is an important point. This is why the axial weld flaws are the important thing.

Okay. This is the opening summary slide and we'll go through the dominant and minor transients in detail.

As we pointed out earlier in discussing PRA and thermal hydraulics, we modeled a wide variety of both primary system faults and secondary system faults. We found out that the dominant transients are medium- and large-diameter pipe breaks in the primary side. Where stuck-open valves that can re-close later, we get a minor contribution from main steam line break.

Of course, the importance of any given transient depends on both its frequency of occurrence and the severity. If it does occur, the next slide will be on frequency of occurrence. And then the remaining slides, which are several, talk to transient severity.

MEMBER STETKAR: And you might get to

this, but let me just add. You mentioned earlier, way earlier, that the PRA models, the analyses that you used to develop the scenarios and the frequencies of those scenarios, included both internal events if I remember and external events, meaning you looked at fires and floods --

MR. KIRK: Yes. The external events was actually done as an after-step to make sure that we hadn't ignored something.

MEMBER STETKAR: Well, what I wanted to ask about in particular was the external events. How detailed were those external event analyses? In particular, a lot of the fire models that people have developed show a very large contribution to core damage frequency from things like hot shorts, which would, indeed, lead to stuck-open valves, that may be able to be re-closed.

And I was curious about how detailed those analyses were because it can be, numerically, a pretty interesting -- I was curious to see the stuck-open valves that later re-close can be an important contributor, and those are the kind of things that we see coming out of a lot of the fire analysis work.

MR. KIRK: Yes. I'd have to go back and review to give you a detailed answer. But it's true to say that the level of complexity of the external events analysis was not as great as that on the internal events.

I do know, and I'll get to later, the bottom-line conclusion of the external events analysis is that, yes, it did contribute something to the through-wall cracking frequency, but it was a factor of two or less and it was decided that that wasn't relevant.

MEMBER STETKAR: Just out of curiosity. Relative calendar times when the internal and external events analyses were available to you, I mean are we talking about 2006, are we talking about 2001?

MR. KIRK: The internal events analyses were done between 1998 and 2003. The external events analyses were probably
2003-ish. They were done after.

MEMBER ABDEL-KHALIK: Back to the question of how the insights that you have gained are fed to the people writing the emergency operating procedures, and if you're telling me that the stuck-open valve that later re-closes on the primary side is an important thing, I mean if I have a failed PORV and you look at the procedure, the operator is instructed to close the blocked valves or make sure that the blocked valves are closed.

Is there something that you have to tell the procedure writers that they might want to re-think some of these procedures?

MEMBER STETKAR: We'll need to hear a little bit more about whether is the re-close part of that important or is it --

MR. KIRK: Yes, the re-close part of that is important.

MEMBER STETKAR: That's a valid question.

MEMBER BLEY: I have one other. I think you answered it earlier.

I know for some time people got concerned about feed-and-bleed. It's down on the very low end and I assume that's because of the good mixing that you eventually ended up with in the thermal hydraulic calcs. And I guess you hit on it before, but I don't remember it well enough.

What's the level of confidence in that we have good mixing and where does that come from?

MR. KIRK: During feed-and-bleed?

MEMBER BLEY: Yes, because it's the thing people worried about and why it even is on the list is because they worried while you were injecting the cold water in, that it could blanket the inner wall, and it's the mixing assures that that's not happening.

MR. KIRK: Yes.

MEMBER BLEY: And at one point I know thermal hydraulics people couldn't quite convince themselves that they'd get good mixing and I wonder why we're convinced that we have good mixing.

MR. KIRK: I'm not sure --

MEMBER BLEY: It feels comfortable that we could, but I know people worry about it.

MR. KIRK: I'm not sure I can address that. The one thing I do remember about the feed-and-bleed is that the temperatures never really got low at all.

MEMBER BLEY: But I think that's because you're getting mixing. If you had the cold water from the RWST blanketing that wall, which people weren't able to discount a few years ago, you'd get much lower -- really lower temperatures.

MR. KIRK: Maybe I'll ask you a question. When you're feeding, how much inventory are you putting in how fast? Is it more like a two-inch break or a six-inch?

MEMBER BLEY: Well, you're opening all the PORVs you got and you're dumping your high-pressure injection in, which is probably --

MEMBER STETKAR: It's a two- to four-inch. It's probably on the order of a four-inch.

MEMBER BLEY: Four- to five-inch hole with all of those open?

MEMBER STETKAR: Yes.

MEMBER BLEY: Depends on the plant, how many they are going to have.

MEMBER STETKAR: How big they are.

MEMBER BLEY: And how big their injection pumps are. The European, well, in fact one we're looking at, a new plant is coming in with lower head pumps.

MEMBER STETKAR: But they also put in big valves.

(Simultaneous speakers.)

MEMBER BONACA: 50 gpm pumps.

MEMBER BLEY: Yes. Some plants don't have pumps big enough.

MEMBER ABDEL-KHALIK: But in some cases this could be 1,000, 1200 gpm?

MEMBER BLEY: Yes.

MEMBER ABDEL-KHALIK: If the shut-off head is below the normal pressure.

MEMBER BLEY: Normal pressure was--

MR. KIRK: Rather than me speculate about something I don't know, we'll get you that.

MEMBER BLEY: It's the basis for the confidence in the mixing.

MR. KIRK: Yes, yes. Okay.

So one slide on transient class frequencies and here I've combined, obviously, a lot of individual transients together. But the main point is is that in most of these cases, with the possible exception of stuck-open valves on the primary side, which is what SO-1 means in this terminology.

The yearly frequency of occurrence is very similar across all the plants. There's not a lot of plant specificity.

MEMBER BLEY: There's a reason for that.

MR. KIRK: Yes, and that is --

MEMBER BLEY: What it is is the same number.

MR. KIRK: Yes. So if you're looking for plant-specific difference, I guess the take-away message is this isn't the place to look for it.

Getting onto the level of challenge as it occurs, I'm going to look at the two dominant and one minor transient classes. The first dominant transient class is primary side pipe breaks where you can cooldown by two mechanisms.

One is that, of course, the rapid depressurization causes an associated rapid drop of temperature, and then you're also injecting colder water into the primary.

Above about a 2-inch break, there's really no operator actions that are credible. Safety injection can't compensate for pipe diameters of 2-inches and above. And I'm telling you what the PRA people told me. So, apologies. I saw the eyebrows. Moreover, in a large break, the operators aim is to keep the core covered.

So our analyses examined the effect of many different factors, the primary ones being break diameter, break location, season of the year on the plant response.

Just to look at some temperature time traces, this shows the entire break diameter --

DR. SHACK: Mark, how much more time are you going to need?

MEMBER CORRADINI: A practical question.

DR. SHACK: Just for you alone another hour?

MR. KIRK: Yes.

MR. ELLIOT: My portion is only going to

be about ten minutes I hope.

DR. SHACK: Good luck. Are you serious, Barry?

MR. ELLIOT: Well, when you say explain this, he explains how you calculate the screening limits. So our presentation is the screening limits and the inspection limits. That's about where we're going to be.

MR. MITCHELL: Dr. Shack? This is Matthew Mitchell from NRR. I do think that we should allow adequate time for Barry to give his presentation. As we are going to be going into the details of the actual 50.61(a) rule, I think there should be adequate time for the committee to ask questions about the actual substance of the rule. So perhaps an hour for Barry's presentation, including questions, would be reasonable.

MR. ELLIOT: Mark is going to go through all the transients. What he is really is, he shows you how we got the screening criteria. That's I think the most important thing here. So if you guys who want to go through all the transients, that's fine.

MEMBER BROWN: So you're talking about slides 9, 10, 11. There's just a bunch of graphs. I don't know what --

DR. SHACK: I think we'll go to 4:15 with Mark and a ten minute break in here at 3:00.

MEMBER CORRADINI: Thank you. Thank you, Mr. Chairman.

(Laughter.)

DR. SHACK: And then we will go to Barry and we'll run a little bit over schedule.

MR. KIRK: Okay. Well, in the interest of consolidating, I'll skip the thermal hydraulics parts where there are likely to be lots of questions I can't answer and I'll go to these.

MEMBER CORRADINI: Good plan. We'll stipulate that RELAP and calculate occur.

MR. KIRK: Yes.

So the main result to the primary side pipe break, once we put the various RELAP curves into the fracture mechanics code is that now we've got a variation of conditional probability of through-wall cracking, the probability of through-wall cracking, assuming the event occurred, versus pipe break diameter for various pipe diameters' plants and we've got both hot and cold legs on the same graph.

And the main point here is, of course, the larger breaks have the highest level of challenge, but, also, the larger breaks pose a consistent level of challenge above a break diameter of about five inches or so, and they're also very consistent from plant to plant.

And the question arises, well, why is that? The simple answer is that at that stage the inventory in the primary is cooling so fast that the vessel can't cool that fast. It's limited by its finite thermal conductivity and how fast the vessel cools, of course, controls the thermal stresses.

So once you get above a pipe break of about five inches, the details that are plant- and transient-specific all fade away into obscurity and you've got a consistent level of challenge at every plant dependent now only on the thermal conductivity of the steel, and steel is steel at least in that regard, and on the thickness of the vessel.

MEMBER CORRADINI: Can I say it differently?

MR. KIRK: Yes.

MEMBER CORRADINI: So if the break is too big, the thermal inertia of the vessel controls. If the break is too small, nobody cares because I don't squirt in enough cold water.

MR. KIRK: Yes, that's right.

MEMBER CORRADINI: Okay. So there's this

intermediate?

MR. KIRK: Yes. And with that nice summary, I can just go to the end.

So the smaller transients, the characteristic of the transients are more important, but their conditional probability of through-wall cracking is sufficiently low that they don't really count for very much any way.

So in combination, these factors suggest the applicability of these results to PWRs in general. There's no real influence of operator action because the operators can't act to save this kind of event, and because it's the large diameter breaks that dominate and they are very similar from plant to plant.

Going onto the stuck-open valves on the primary side and, again, I'll go to the end which summarizes here on the graph we have, the through-wall cracking frequency, due to stuck-open valves, plotted versus embrittlement.

Again, you see three plants with three different manufacturers, three different operator training procedures, three different credits for operator action, all showing a very consistent trend with embrittlement. And you say, well, why is that?

Well, there are several reasons.

First off is that it's the cooling rate in these transients is very slow relative to the big diameter breaks. A stuck-open valve is like a two- or a three-inch break. So the cooling rate alone isn't enough to fail the vessel. You need that late-stage repressurization.

So the repressurization is obviously a dominant factor influencing the transient severity. And when it repressurizes, it invariably repressurizes the safety valve set point, which is, again, very similar from plant to plant.

Even though we included credits for operator action, we found out that the operator could only really save the day, if you will, prevent the repressurization only if they acted very rapidly within one minute of me being their throttling criteria, and only if the transient initiated from hot zero power.

So when you combine a limited action, the limited credit for operator action, the fact that the operator has to act rapidly, and the fact that the only time when operator action has any influence on the outcome of this transient is when it's at hot zero power, you find out that even though we've credited operator action, it hasn't really made its way into the results that count.

MEMBER ABDEL-KHALIK: Now, sometimes plants are classified either as a low pressure plant or high pressure plant depending on the relationship between the shut-off head of the high pressure safety injection pump and the normal operating pressure of the plant.

So if that is the case, I would have expected a difference between these two types of plants. Are all three plants that you looked at in one category or the other?

MR. KIRK: Steve, do you have any?

MR. ELLIOT: We did a generalization study that looked into --

MR. KIRK: Well, yes, but I can't answer that direct question.

MR. HARDIES: This is Bob Hardies. I can tell you that Palisades has low, they're a low pressure plant and Beaver Valley is a high pressure plant.

MEMBER ABDEL-KHALIK: Okay. So the explanation that these plants essentially repressurize all the way to the safety valve setting and that's why you're not seeing any difference between them could not be the reason.

MR. KIRK: I don't know about the plant question you're asking. But all three of these plants, when they repressurized, went to the safety valve set point.

MEMBER BLEY: It would have to be because they are heating up again.

PARTICIPANT: It would have to be because they're heating up.

(Simultaneous speakers.)

PARTICIPANT: I don't think it's a pump pressure.

(Simultaneous speakers.)

MR. KIRK: I mean the repressurization does occur. It occurs about 1,000 seconds after the valve closes and they are heating at that time. You're right about that.

MEMBER BLEY: I would have expected the high-head pump to repressurize almost immediately and the other one to take some time from the reheating.

MEMBER ABDEL-KHALIK: Right.

MEMBER BLEY: Not a whole lot of time, I mean to fill it back up.

MR. KIRK: And then the final area had to do with main steam line breaks versus we've talked about the core main steam line breaks were dominant in the previous analyses, but they really they formed at most a 10 percent or less contributor here.

A couple of factors that suggest the applicability of our models, the main steam line breaks are PWRs in general. First off, we've got intentionally conservative modeling. Even though we modeled the effects of operator action, the operators didn't act until after the failure time that was predicted in FAVOR.

if a failure is going to occur in a main steam line break, it's going to be dominated by the initial, very rapid, depressurization and the operators just can't act in time to safe that.

Another thing that speaks to the generality of these results is, again, we're in a conduction-limited situation. A main steam line break is a huge heat sink, and so there's absolutely no way that the cooling rate of the pressure vessel can keep up with that.

So the minor differences between the size of the main steam line and plant A to plant B to plant C just, while they influence the cooling rate of the water, they don't influence the cooling rate of the vessel.

So it's a minor contributor overall even though we've got a conservative model, and that minor contribution should scale very well from plant to plant to plant.

And in terms of the other transients, in all cases, other than the ones we've just talked about, in all cases, these other transients, a combination of a low probability of occurrence and/or a low consequence makes the contribution of through-wall cracking frequency somewhere between negligible and zero.

MEMBER ABDEL-KHALIK: Just for reference, what is the thermal time constant of the vessel wall?

MR. KIRK: I'm sorry?

MEMBER ABDEL-KHALIK: What is the thermal time constant of the vessel wall?

MR. KIRK: Meaning? Can you define that?

MEMBER ABDEL-KHALIK: L-squared over alpha, thickness-squared divided by the thermal diffusivity?

MEMBER CORRADINI: Long, long, thousands of seconds I'd bet.

MR. KIRK: Yes, it would have to be.

MEMBER CORRADINI: Alpha is about $2x10^{-7}$ meters squared per second.

MR. KIRK: Yes, yes.

MEMBER CORRADINI: I think approximately. 20 divided by 7000 divided by 1000.

MR. KIRK: Huge. This is a logical stop spot.

This is just a summary where we put the -- now, the curves for the three major transient, or the two major and one minor transient classes on the same graph versus embrittlement, blue is primary side stuck-open valves, red is the pipe breaks, and green is the main steam line break, which I note that if we actually had a realistic model, that green curve would either be much, much lower or, in fact, it would just disappear entirely.

So those are the three transient classes. We believe we have identified features that make these findings generic to all plants. We'll discuss that after the break.

Just one thing to point out here is that the primary side transients that are dominant, they're dominance shifts as embrittlement occurs. For low embrittlement levels up to about 230°F, the stuck-open valves that can later repressurize as driving the through-wall cracking frequencies you need for the less-brittle materials. You need that late-stage repressurization to cause a failure.

However, once you get up to embrittlement levels that approach the screen limit we're going to propose, the primary side pipe breaks start to dominate. And I think by the time you get to the limit, the primary side pipe breaks make up about 90 percent of the through-wall cracking frequency.

So with the Chairman's permission, we can stop at that point.

MEMBER CORRADINI: So is this universal for the red to be dominant as I get up to the frequency of concern?

MR. KIRK: Yes. I mean the trend makes sense. That is, you get more and more brittle material. It's initiation control. Once it initiates, it just goes.

MEMBER CORRADINI: Right. But I guess what I'm saying is, although the transients fall away --

MR. KIRK: Yes.

MEMBER CORRADINI: -- and it's because of the -- and the only thing is is some sort of intermediate primary side pipe breaks --

MR. KIRK: Yes.

MEMBER CORRADINI: -- and I might not see a reordering of this with the plants?

MR. KIRK: I wouldn't expect so.

MEMBER CORRADINI: Okay.

MEMBER MAYNARD: Quick question. You did all these transient analyses. You took into account the operator procedures, the different plant designs and everything. Did you find anything going through this to where the procedures actually created a bigger problem?

MR. KIRK: No. And, in fact, the influence of the -- of these three dominant transient classes, the only one where the procedures mattered even a little was the stuck-open valves. But where the differences in the procedures mattered was very early in the transient. Once you got to the late stage of the transient, after the valve had re-closed, but before the operator had met all their criteria for acting, the differences on in the transient, the different procedures and different training schedules provided, had all essentially melted away.

DR. SHACK: Time for a break until we'll make it 3:15.

(Whereupon, the foregoing matter went off the record at 3:00 p.m. and resumed at 3:18 p.m.)

DR. SHACK: Gentlemen, if we can come back into session. Mark has got his track shoes on.

MR. KIRK: Okay. So we have insight from the three detailed plant studies that only the most challenging transients contribute in any significant ways to the through-wall cracking frequency rules. So we wanted to expand our view to see how general we thought these conclusions were.

Three specific activities, first was informed by the baseline results. We wanted to determine if the plant-specific features that were expected to produce -- I'm sorry. We wanted to determine if medium- to large-break LOCAs, stuck-open valves, and main steam line break look the same in other plants. And I've got just the results on each of these.

After doing a detailed examination of five more high embrittlement PWRs, we decided that the only thing we really hadn't adequately covered was that the baseline model of stuck-open valves could underestimate the through-wall cracking frequency by about a factor of two-and-a-half. Really, only a factor low embrittlement levels because that's the embrittlement level which stuck-open valves dominates.

And, as you'll see, hopefully in 10 or 12 slides in 10 or 12 minutes if I'm optimistic, we've accounted for that in setting the references.

We spoke a little bit about the effective external initiating events like fires and earthquakes, and so on. And based on a coping analysis, it was determined that the effective external initiating events increasing the through-wall cracking frequency was not over a factor of two and that was judged to not be significant.

The third area we did both thermal hydraulic and PFM sensitivity studies with two purposes in mind. One was looking at different credible variants of the model to see if there were any changes that should be accounted for. And the second area was to see if there were cautions regarding the applicability of the base line results to all plants.

In the thermal hydraulic analysis, the conclusions were that there were no credible model changes to the RELAP model that we should consider and there were no cautions regarding the general applicability of these results.

All plants in the PFM analysis, we didn't find any credible model changes that changed anything significantly. However, we did have two cautions regarding general applicability. The two things were vessel wall thickness and screening limits for forgings, and I've got a few slides on each of those.

DR. SHACK: Just what were the nature of these sensitivity studies?

MR. KIRK: For example, in the PFM model, as you're well aware, perhaps painfully so, the NRC and the greater embrittlement damage community have not yet come to expert consensus on an embrittlement trend curve and I won't say any more.

So there are a variety of different embrittlement trend cruves on the table and they all fit the data pretty well. Where they differ is in their features outside of the fit database. One of the sensitivity studies we did was to plug in models different from the ones used that different sets of experts might prefer and we found out it changed the results, factor of one-and-a-half, factor of two, that sort of thing.

So in these studies we really tired to restrict our attention. I mean it's always possible to perform sensitivity studies and get large changes and results. I could postulate flaws that are bigger by a factor of ten.

DR. SHACK: No. Here you have a credible, alternate model.

MR. KIRK: So those are the sorts of things we looked.

But what we did find out in this examination is that there were two things, again, vessel wall thickness and treatment of flaws in forgings that we hadn't really adequately addressed.

So for vessel wall thickness, in fact, our first clue was in looking at a graph I showed you about five slides again, which was the variation of the conditional probability of through-wall cracking with break sides. Yes, if you go back to, it's the one that looks like that. It doesn't have it on here.

Anyway, I mean we made the argument, in looking at that graph, that once we got to big breaks, all plants should be similar because the thermal conductivity and the thickness was similar. And then we looked and we said, okay, well, Oconee and Palisades are dead on top of each other for the very large breaks, but Beaver Valley has a lower through-wall cracking frequency.

It was only then that we realized that -- I mean it was in the model, of course. The Beaver Valley vessel was about an eight-inch thick vessel. The other two vessels are about an eight-and-a-half-inch thick vessel. And if you look at the distribution of thicknesses throughout the entire plant population, certainly most PWRs are in the eight- to nine-inch thickness category.

But we have a few PWRs that are considerably thinner. And we've got Palo Verde vessels that are considerably thicker.

And we just did some analyses in FAVOR on using a 16-inch LOCA with a fixed wall size, and you see what you would expect to see, that is, thickness increases, then the level of thermal stress increases; therefore, the peak value of the $_{k}$ applied increases, and so, of course, the through-wall cracking frequency must increase when you apply the same challenge to progressively thicker and thicker vessels.

MEMBER ABDEL-KHALIK: If we go back to that figure that you referred to earlier --

MR. KIRK: Yes.

MEMBER ABDEL-KHALIK: -- where the probability as a function of break diameter --

MR. KIRK: Yes.

MEMBER ABDEL-KHALIK: -- where you gave the explanation that beyond a certain break size the vessel wall just doesn't cool fast enough?

MR. KIRK: Right.

MEMBER ABDEL-KHALIK: And I assume that those two data points that refer to the Beaver Valley plant are the ones that got you into this discussion?

MR. KIRK: Right.

MEMBER ABDEL-KHALIK: Could you, please, project that?

MR. KIRK: Yes. Number 15.

MEMBER ABDEL-KHALIK: So those two open squares are what got you into this?

MR. KIRK: No, actually, not because this is --

MEMBER ABDEL-KHALIK: Those two green squares?

MR. KIRK: Not those.

MEMBER ABDEL-KHALIK: Which ones, then?

MR. KIRK: Because down here, I mean, yes, there's Beaver Valley cold. I mean that's the difference between a cold and a hot leg break. But it was these out here because this is down.

MEMBER CORRADINI: I missed where you're pointing.

MR. KIRK: I'm sorry. Laser pointers don't work on shiny screens.

That one and that one.

These are certainly off the main trend.

But they're so low, they weren't particularly of concern. It was out here where the Oconee and Palisades results were lying dead on top of each other, and the Beaver Valley results were a little bit lower.

I mean on one sense this is a factor of like four or five. It's really not that much. But on the other hand, it didn't agree with our physical understanding that this point all vessels should be essentially the same.

Well, certainly the thermal conductivity was the same. But what became apparent is that this was the thickness effect used at the Beaver Valley vessel. It's about half-an-inch thinner than the other two vessels, and, therefore, lower thermal stresses, lower through-wall cracking frequency.

MEMBER ABDEL-KHALIK: But that doesn't negate the argument that the time constant is --

MR. KIRK: No, no.

MEMBER ABDEL-KHALIK: -- far longer than the transient time constant of the accident.

MR. KIRK: That's right.

MEMBER ABDEL-KHALIK: Because I mean looking at the calculation that my colleague here did, if you do the thermal time constant of the entire wall, that is very long. And what we're concerned about is probably the thermal time constant of the first quarter inch?

MR. KIRK: Yes.

MEMBER ABDEL-KHALIK: And that is not different for Beaver Valley or the other two plants.

MR. ELLIOT: This is just a thermal stress problem. That's what this is.

MEMBER ABDEL-KHALIK: Right, right. Okay. Say you have lower stress in this case?

MR. KIRK: Right.

MEMBER ABDEL-KHALIK: Okay.

MR. KIRK: Okay. So we can go back.

So what we did then was recognizing while certainly all of our results we repicked plants that had representative. So we felt fairly comfortable with that. And, certainly, the thinner-walled vessels would be conservatively limited by any results generated from our base line plant, so there wasn't much concern there.

But it was these three plants that had greater thickness that caused us some concer. And so we then did some more sensitivity studies, which are shown on the left-hand side.

MEMBER ABDEL-KHALIK: So, again, back to

the issue of the effective thickness. The most important part is sort of localized very close to the inside surface.

MR. KIRK: It is. It is.

MEMBER ABDEL-KHALIK: And, yet, on top of that, you're adding the overall hoop stress.

MR. KIRK: Yes, but the stress, and as a result the stress intensity factor that the vessel can generate, is a factor. I mean it's where all the action is in terms of the cracks that will get you is in the first quarter inch. But the stresses to which that first quarter inch is subjected is, indeed, influenced by the thickness.

DR. SHACK: But between 8 and 8.5, that seems like it ought to be --

MR. KIRK: Well, you see a more -- I mean okay. So here, 8 and 8.5, you know, that's a factor of two to three. But when you get up 8 to 11, you're getting up for something significant. And a leading indicator, I don't know. It's just something that didn't seem right. And when we probed into it a little bit more, we said, okay, if it was 8 to 8.5 that you were worried about, it probably wouldn't be worth worrying about.

But in recognition of the fact that we

have some thicker vessels, we thought it should be a factor in the analysis. So we ran several different classes of transients and found out that, yes, the through-wall cracking frequency goes up systematically as the thickness goes up. And you'll see this, now, reflected in yet another adjustment factor in the through-wall cracking frequency estimation equation that'll hopefully be coming up shortly.

So then the other area that we realized as we did our sensitivity studies that we hadn't covered as thoroughly as we needed had to do with plants with forgings, which perhaps isn't a very big surprise because up until now we haven't analyzed any forging plants.

So the main difference, of course, is the different flaw populations that you can get in forgings. Well, forgings have two different sorts of flaw populations. One is just the embedded flaws that are left over as a result of the forging process.

We commissioned a destructive examination study of x-vessel forging material at PNNL and discovered that the embedded flaws in forgings were pretty much the same size and same density as the embedded flaws and plates.

PARTICIPANT: Why would you expect it

different?

MR. KIRK: Yes, which you would expect based on the knowledge of the manufacturing process.

So on that basis we said, okay, well, if that's all we have to deal with, we're already done. We can just use our relationship between the reference temperature of the plates, although now it will be a reference temperature with forgings, and the through-wall cracking frequency from our base line analysis.

But, unfortunately, that's not the only population of flaws to which forgings can be subjected. In rare circumstances, there can also be sub-clad flaws in forgings. And sub-clad flaws arise due to a combination of the chemistry in some forgings and very high heat and put cladding, which generates stresses that tend to open cracks under the cladding layer. They occur perpendicular to the direction of the cladding, so they're axial now instead of circumferential, which puts them in the bad orientation.

And if they do occur, if there are conditions that exist where they do occur, they're very, very dense. They occur like once every millimeter or two, but they're shallow. They extend only to the depth of the heat-effected zone.

MEMBER CORRADINI: So the inner cladding of the vesse, somehow the way it bonds to the carbon steel creates some flaw?

MR. KIRK: Yes.

MEMBER CORRADINI: Differential expansion I assume, or that's incorrect?

MR. KIRK: Well, you've got metallurgical inhomogeneities that are being stressed on the surface of the forging.

MEMBER CORRADINI: But it's the way the cladding is put on?

MR. KIRK: Yes. It's the way the cladding is put on.

MR. ELLIOT: You have to have a very high heat input to cause this type of defect.

MEMBER CORRADINI: Okay. All right, I got it now.

MR. ELLIOT: If it's a low heat --

MEMBER ARMIJO: It's an intermediate alloy between the stainless and the ferritic. So it's on a very thin intermediate alloy plus a lot of stresses.

MEMBER ABDEL-KHALIK: The way they lay the clad is a very high heat input process.

MR. ELLIOT: Not all of them. Some of them are not. That's what we look for. We have a reg guide, 1.43, where we look at all these parameters and then we figure out whether that forging is susceptible to this or not.

DR. SHACK: Wouldn't forgings, by and large, be favorable chemistries though?

MR. ELLIOT: Yes, yes.

DR. SHACK: I mean people have learned at this point?

MR. ELLIOT: yes.

MR. KIRK: And, in fact, 1.43 came out in the mid 1970s. I mean this is a known problem and -- is a known problem/was a known problem -- and most of the vessels out there are compliant with 1.43. So this is a completeness step that we're taking. It's not seen to be a significant problem.

So what we did was we collected together the information that we could find in the literature to generate a sub-clad flaw distribution, but that sub-clad flaw distribution in FAVOR and generated the results. These are the results and they are quite a bit different in character than the previous curves you've seen --

DR. SHACK: But, again, Mark, how dense are these flaws?

MR. KIRK: A flaw every millimeter to two millimeters. They're everywhere.

MEMBER ARMIJO: Like uniform.

MR. KIRK: It's crazy.

DR. SHACK: Yes. But is FAVOR really applicable in that situation?

MR. KIRK: How so?

DR. SHACK: I mean it really is sort of based on -- aren't all the fracture mechanics solutions for cracks an infinite?

MR. KIRK: Yes, yes. So you're right and so, if anything, we've overestimated the -- by treating these very closely spaced flaws as --

DR. SHACK: Infinitely isolated.

MR. KIRK: -- if was the only one, yes, this is, by intention, a conservative analysis because, quite frankly, if the conditions were right for sub-clad cracking, I would doubt as to whether they'd be right everywhere in the vessel. But that's how we've simulated it to occur.

But this is a vessel that just -- now we've gone from thousands of cracks per vessel to millions of cracks per vessel and that's the key reason why the graph looks different. In the other graphs the through-wall cracking frequency went up at a much more gradual rate with increase in embrittlement. Whereas, here, essentially, you've got -- because these, even though there are lots of them, they are very small and very shallow flaws.

At low levels of embrittlement you don't have enough $_k$ applied to get them going at all. But once you get enough embrittlement, you no longer are playing the game of, oh, I've got high embrittlement, but I might not have a flaw there. If you have high embrittlement, you certainly do have a flaw there and that's why this is something that's very close to a step function.

MEMBER ARMIJO: But if you have multiple flaws right next to each other, the same loading is going to distribute and reduce the stress.

MR. KIRK: Yes, you are both absolutely right. So by treating them in the way that we have, it's very conservative.

MEMBER ABDEL-KHALIK: I guess just as a follow-up, I don't understand that in a sense that I do understand that if you have so many of them, then you get some stress relief by the presence of neighboring flaws.

But if you're modeling, it just looks at an individual flaw assuming that it's not impacted by neighboring flaws. Why would the flaw density affect the results?

MEMBER ARMIJO: The model won't see it. But, in reality, there --

DR. SHACK: No. He still has the same flaw density. It's just that the $_k$ on every one of those flaws is higher than it should be.

MR. KIRK: I have a very high --

DR. SHACK: He treats the millions.

MR. KIRK: I have a million flaws and I treat each of them as if there are no neighbors to share the load.

PARTICIPANT: He's certainly got a candidate once he gets to where he wants to be.

MR. KIRK: That's right. That's right.

So a conservative view of the occurrence of flaws, a conservative of the density of flaws, and a conservative treatment of their $_k$ applied values. And just to get a relationship so that we have a table that treats every condition, and what we found out in our regulatory analysis is that nobody is likely to transgress this condition any time soon.

PARTICIPANT: They're just not going to get to a high enough --

MEMBER ARMIJO: Even though it's highly conservative.

MR. KIRK: Even though it's highly conservative, forging vessels just don't have enough copper and nickel in them to get them up to this level.

MEMBER ARMIJO: Bit flaws.

MR. KIRK: Okay. So that was the end of the generalization studies. The wrap-up is we found three things that we needed to adjust our base line results to account for: vessel wall thickness, a minor fact of a stuck-open valves, and forgings.

So now here's the part where we try to take all of this and bundle it together into a method to get us to new reference temperatures and it's replaced candidate replacements for the historic 270 and 300°F values.

Like I said, we've emphasized here, the understanding we have suggests that with these three minor modifications we can apply our results from our base line plants to all PWRs.

So the basic idea here is that, and you're going to see the adjustment factors I think on the next slide, is that we can use our results -- these are our base line results remember that relate the embrittlement, axial weld materials, the embrittlement of plate, the embrittlement of the circ welds to a through-wall cracking frequency.

So the idea is we just sum these up. We say, okay, go out to your plant and tell me what these reference temperatures are. Based on these curves, I can calculate what my through-wall cracking frequency is, add it up, don't go above 10^{-6}. And, oh, by the way, remember we're adding 95th percentile values and comparing to the 10^{-6} limit.

Now, I think this is the only equation my colleagues let me keep in and I'm not going to dwell on it. But the idea is we're setting a limit on the total through-wall cracking frequency of 10^{-6}. It's a sum of contributions from axial welds, plates, circ welds and forgings. Here are the reference temperatures you put in. All the numbers are are just the parameters of those curve fits you saw in the previous pages.

But there are three parameters in here I want to call your attention to: the alpha, the beta, and the eta parameter. The alpha parameter is the adjustment for stuck-open valves. Beta is for vessel wall thickness, and eta is for sub-clad cracks.

It's all completely prescriptive. The only vessel-specific information that goes in here, which is determinable from our surveillance information and the information in the database are these four reference temperatures. So that's the equation.

The graphical depiction of the equation is shown here. So this is the three-dimensional graph. Axial weld reference temperature on one axis, plate reference temperature on another axis, circ weld reference temperature on another axis, and the surface you see is the ISO through-wall cracking frequency service at 10^{-6}.

So, basically, if you wanted to assess your plant relative to this, if the point that's assessing your plant is under the dome, everything's okay. If it's somewhere out here floating in space, you'll be talking to my colleague, Barry, very soon.

MEMBER ARMIJO: Mark?

MR. KIRK: Yes.

MEMBER ABDEL-KHALIK: That's really hard for me.

PARTICIPANT: How does under the dome? Explain that.

MR. KIRK: Okay. We've got now a series of equations that relate reference temperature to through-wall cracking frequency. So you put all those together and you say I'm going to limit this to 10^{-6}. What are the combinations of axial weld reference temperature, plate reference temperature, circ weld reference temperature that add up to 10^{-6}, and the dome, if you will, is just the combination. If you had a point that had those three specific values, you would add the 10^{-6}.

Now, obviously there's a number of ways to get there. You can have a really embrittled axial weld and an unembrittled plate, or different combinations.

To simplify this a little bit, we've said before that the circ welds don't contribute very much. So we thought, well, maybe three dimensions are hard to plot. Two dimensions I can do in Excel. So we said maybe if we take a slice through this at an acceptably high reference temperature for the circ weld, we can simplify it.

So we observed that based on our currently available plant data, the highest circ weld RT value that we'll get to in 60 years is 260°F. At that point the through-wall cracking frequency contribution to the circ weld is 10^{-10}. It's in the dirt.

So we said, okay. You know, we're good regulators, so we'll add a little margin to 10^{-10}.

We'll take it up to 10^{-8}. We get a circ weld limit of 312 and nobody's going to come anywhere near that, and that's this green line.

So now we can start to plot things in two dimensions with the implication that in two dimensions there's that circ weld limit of 312, which nobody's ever going to get near.

So if we go to two dimensions, now we've got an inner play of the axial weld reference temperature on the horizontal axis, the plate reference temperature on the vertical axis. I put a number of different through-wall cracking frequency lines or curves on here for illustration. But the key one is, of course, the 10^{-6} curve, which is our limit from Nathan that we talked about this morning.

And, also, on here, you can now plot individual points for different plate plants that are currently in service at the end of 60 years and you find the fortunate result that all those plants are inside the 10^{-6} surface.

The other things that I wanted to talk about here is that these limits, because these limits don't have a margin term, whereas, the old limits that Barry talked about had a margin term, you can't compare them numerically. You got to adjust the margin out.

But once you adjust the margin out, you find out that the RT limit for axial welds flaws exceeds the current limit by about 60°F. The temperature limit for plate flaws exceeds the current limit by about 150°F.

MEMBER CORRADINI: Make sure I understand. The term you're giving us here is a ΔT_{30} number.

MR. KIRK: No. It's --

PARTICIPANT: It's the margin term.

MR. KIRK: The margin, yes. Okay. You could take this as our estimate of the margin on the current limits. In other words, right now the limit on axial weld is 270.

What we're saying is you could take that up to 330, calculate RT_{NDT} irradiated the same way as you do now, and still be less than 10^{-6}. That's the implicit margin in the current limit is 60° on axial welds, 150° on plates, and virtually unlimite on circumferential welds.

MEMBER CORRADINI: So can I ask the question then about the three trianges that are 17° away from the red line?

MR. KIRK: Yes.

MEMBER CORRADINI: So those triangles are the outer bound of a grouping of calculations? I'm trying to understand what those triangles are.

MR. KIRK: Each triangle there is a plant.

MEMBER CORRADINI: Right.

MR. KIRK: Yes.

MEMBER CORRADINI: But a plant at what value of -- given all your calculation of the boxes that we got to this point, this is the upper bound value?

MR. KIRK: No.

MR. ELLIOT: What he's doing, the triangles are the actual plant data. It's copper, it's nickel, it's fluence. Everything we know about that plant, put it into the embrittlement equations that are in the rule, and this is where they wind up.

DR. SHACK: At 48 effective full-power years?

MR. KIRK: Yes.

MR. ELLIOT: Yes.

MEMBER CORRADINI: At what? I'm sorry.

MR. KIRK: At the end of its life you've got an 80 percentile --

(Simultaneous speakers.)

MEMBER CORRADINI: Sixty year lifetime, right.

MEMBER ARMIJO: Are these the same three plants that were the most limiting of the current rule?

MR. KIRK: No, no.

MEMBER ARMIJO: So they change?

MR. ELLIOT: And wait. This is a projection of neutron fluence that he's using. The actual plant could have had flux reduction and has a lot more than this. He's just giving you an estimate from the knowledge that he has about the fluence at 60 years. Everybody's okay.

MEMBER CORRADINI: Right. But I guess I'm still struggling with am I happy with 17°, am I unhappy with 17°? What's the thing?

So then if that's actual plant data with the triangles, then let me try it again.

The red line has been drawn with a calculation procedure such that that's the lower limit --

MR. ELLIOT: Yes.

MEMBER CORRADINI: -- not the best estimate?

MR. ELLIOT: No. That's the 95th percentile.

MEMBER CORRADINI: That's the 95th

percentil?

MR. ELLIOT: Yes.

MEMBER CORRADINI: Okay.

MR. KIRK: Yes, that's the 95th percentil.

MEMBER ABDEL-KHALIK: Now, the old rule didn't distinguish where you do the RT_{NDT} measurement, right?

MR. KIRK: Right.

MEMBER ABDEL-KHALIK: So if I were to apply the same philosophy to these results, the limiting value would be that RT maximum for the axial weld, right, which is the nearly vertical line, right?

MR. KIRK: Nearly vertical, yes. 270.

MEMBER ABDEL-KHALIK: Right. So I don't understand why you're saying that this is less -- or, the old rule is more conservative by 60°.

MR. KIRK: Because in this case the 270, is that right?

MEMBER ABDEL-KHALIK: Right.

MEMBER CORRADINI: 269.

MR. KIRK: 269. The 270 limit is applied, the plant points that are getting compared have no margin associated with them because all of the margin was accounted for in establishing the origin of the LOCAs.

MR. ELLIOT: We're talking about apples and oranges. Let me make one clear.

MEMBER ABDEL-KHALIK: Please.

MR. ELLIOT: In the beginning I told you that when we had the previous rule, the mean value for failure at $5x10^{-6}$ was 210. So he is getting a value of about -- for the mean value without the margin. In the old rule the mean value without margin was 210.

The equivalent here is 270, so there's a 60° difference.

MEMBER ABDEL-KHALIK: Right. But the regulatory --

MR. ELLIOT: That's what he was talking about.

MEMBER ABDEL-KHALIK: A regulatory screening --

MR. ELLIOT: Comparing mean value to mean value.

MEMBER ABDEL-KHALIK: Okay. The point I was trying to make is that the regulatory screening limit that was used in the old rule is pretty much the same as this.

MR. KIRK: That's correct. The numeric value is the same, but the calculational procedure that we asked the plants to go through to estimate their position relative to that limit is not the same.

MR. ELLIOT: We're not going to be asking to throw in the margin into the RT_{MAX} calculation because it's already accounted for in his analysis. While in the old rule, it wasn't accounted for, it was added on. We went through this this morning.

MEMBER CORRADINI: That's okay. It takes us time.

(Simultaneous speakers.)

MR. ELLIOT: We're going to go through it again if you want me to go through it again.

MEMBER ABDEL-KHALIK: No, I understand. But this is sort of essentially confirmation that whoever did this before did a good job.

MR. KIRK: Did a conservative job, yes.

One other distinction to make is, in the past, if somebody was coming up on their axial weld limit, the way they calculated their reference time -- well, they included a margin, which we've talked about, but they, also, were obligated to use the maximum fluence on the inner diameter of the vessel.

Whereas, what we say in the new rule is that if you're calculating the reference temperature for your axial weld, you use the fluence at the axial weld location, which could be -- we talked about the fluence truss, could be significantly less than the maximum.

MEMBER CORRADINI: That changes the positioning of the triancles.

MR. KIRK: That's right. That's right.

MEMBER CORRADINI: Some move up because they have had a higher fluence, some move down, some move around.

MR. KIRK: I think it's safe to say they all have to move down because in the past everybody was using the max fluence.

MEMBER ABDEL-KHALIK: This is the luck of the draw as to where people put the axial weld relative to the orientation of the core I guess.

MEMBER BROWN: Okay, to the neophyte, the new screening criteria will be 270. Is that what that number is?

MR. KIRK: Yes.

MEMBER BROWN: Okay. In the piece of paper you issued, and it's a single number?

MR. ELLIOT: No, no. He's just talking about the axial --

MEMBER BROWN: That's fine, an axial weld. That's fine.

MR. ELLIOT: We have all the screening

limits for everything else.

MEMBER BROWN: Okay. All right. All right. But in a paper you issued last year, you had, for the 50.61(a), there's a whole bunch of different screening criteria.

MR. KIRK: Yes.

MEMBER BROWN: I mean the numbers bounce all over the place from 312 to 530, axial weld 269, another axial weld.

MR. KIRK: If I may, Barry, it's all on here. What this is saying is the limit for axial weld is 270.

MR. ELLIOT: Next slide explains it best.

MR. KIRK: The limit for the axial weld is 270. The limit for the plate is 356. And then the other one that says there's a limit on --

PARTICIPANT: Both.

MR. KIRK: -- is just the slope of that line.

MEMBER BROWN: There's a 538 number in here.

MR. KIRK: Right. And that's just a reflection of the slope of that line. We're attempting to take a graph and put it in a table. That's all that's going on there.

MEMBER ARMIJO: Less than.

MR. KIRK: So that's just saying we had this curve, this continuous curve. Remember, it came from the 3D bubble. We took a slice through to create a 2D curve and then, instead of putting the curve in, we said, well, we'd like to express this in a tabular form, and so that gives us a limit on axial welds, a limit on plates, and a limit on the combination of the two so you don't let people get out too far.

Because if you just had the limit on axial welds projected up and the limit on plates applied independently, then somebody could be out here and that wouldn't be good, so you'd crop that off.

This graph also shows the effect of the functionality of a thickness. So this is for the 8-inch vessel range, this is for the three really thick vessels, and I put the Palo Verde vessels 1, 2 and 3 on there, and, obviously, they're fine, but their limits are much less. Instead of being 270, it's more like, looks like 225.

MEMBER ABDEL-KHALIK: Now that's what's sort of confusing to me. Because in the previous discussion you said that Beaver Valley has a thicker vessel and, therefore, those data points that you got before were lower than the other two plants.

MR. KIRK: Beaver Valley had a thinner vessel.

MEMBER ABDEL-KHALIK: A thinner vessel? Okay, sorry.

MR. ELLIOT: But our table has less than nine-and-a-half inches. So Beaver Valley, Palisades, Oconee are all on that table. Even though they may have different thicknesses, they still meet the table because we limit it to nine-and-a-half inches and less.

MEMBER ARMIJO: Mark, you're going to have to help me out. How do you get the 538 less than 538F on that slope there? What number do you add to what number and divide by what?

MR. KIRK: I'm trying to remember. That has to be the y-intercept of this line because you go down to RT_{MAX-AW} is zero and project that 45 diagonal up and that becomes 538 and the graph becomes--

MEMBER CORRADINI: So what you're saying is, can you just point to the upper knuckle? So you're saying the sum of the x and the y is 538 along that line? That's all you're saying, right?

MR. KIRK: Yes, exactly, exactly, yes.

MEMBER ARMIJO: It's a mid point of that diagonal?

(Simultaneous speakers.)

DR. SHACK: One at a time.

MEMBER CORRADINI: If RT_{MAX-AW} were zero, then RT_{MAX-PL} is 538. If RT_{MAX-PL} was zero, RT_{MAX-AW} would be 538 and the line drives? That's it.

MEMBER ARMIJO: Okay. So I got it now.

MR. KIRK: Right, those would be because the absolute values would be too big.

DR. SHACK: You missed the others.

MR. KIRK: Yes.

MR. KIRK: All three of those conditions have to be met to stay. All you're saying is that the tabular limits are saying you got to be inside this blue box with the lopped off corner. That's all. And the size of the blue box with the lopped off corner varies with thickness.

Then the next diagram is for forgings. Again, we plotted all the forging plants on there at the end of 60 years. The extent of the horizontal axis, the limit on circ welds is 312 where we point out by labeling on here that that's a limit that's actually a 10^{-8}.

Now, we don't expect anybody to set at 10^{-6}. We don't expect anybody to bet getting anywhere close to that limit. However, if they did and went over it, the obvious next thing for them to do would be to go back to the formulas and calculate what the actual through-wall cracking frequency was.

The two horizontal lines are the limit on the maximum reference temperature in the forging. The upper limit is for forgings without underclad flaws, so they get a higher limit. The lower limit is for forgings with underclad claws. However, again, what you can see is all the forgings are well within that limit.

So, really, there shouldn't be any need for proof or debate regarding whether a forging has underclad flaws or not because we previously discussed this limit was very conservatively set and all the forgings at least at the end of 60 years are well inside that limit as well.

MEMBER CORRADINI: And just to make sure, so if you could just pick some triangle up there high, some high triange? Okay. So in this case you're projecting out to the end of 48 full power years is approximately 60 years with some sort of operating flux history based on what you know now?

MR. KIRK: Right.

MEMBER CORRADINI: Okay. And they tend to march at what, sort of so we're talking like a couple of degrees per full power year?

MR. KIRK: Yes. They're going --

PARTICIPANT: But it's an expanding universe. They're going out from the origin.

MEMBER CORRADINI: The big bang.

PARTICIPANT: At about a degree Fahrenheit per year.

MEMBER CORRADINI: Per year, okay. That's what I was trying to understand. Thank you.

MR. KIRK: So, yes, they're 50° form the line, you've got maybe 50 years to go after 60.

MEMBER MAYNARD: Why did you go to 10^{-8} rather than 10^{-6}? Just because you could or was there --

MR. KIRK: Well, it was because we could and because the idea of putting a 3D graph in a regulation, while I voted for it, all my colleagues voted roundly against. It was just to try to simplify the equation.

MEMBER MAYNARD: Okay. That's fine.

MR. KIRK: And we could.

MEMBER MAYNARD: There's no technical reason why that should be? It's just that --

MR. KIRK: No, no, no, no, no.

MEMBER MAYNARD: -- it encompassed all the plants?

MR. KIRK: Yes. No, and we're certainly not saying that if for some reason circumferential welds got out to that level that we would apply a higher standard for the. It's just done for simplicity's sake.

And again, these graphs, they appears in our reports. The graphical representation --

DR. SHACK: Fifteen minutes, Mark.

MR. KIRK: We're going to do surveillance here I guess.

MEMBER ARMIJO: This is good stuff.

MR. KIRK: This is?

MEMBER ABDEL-KHALIK: Yes.

MR. KIRK: And the rest wasn't? Okay.

So a little bit of background. The current PTS rule requires that the embrittlement trend curve, the ΔT_{30} embrittlement trend curve be modified if credible plant-specific surveillance data is available.

Basically, if you have two or more surveillance points, you adjust the trend curve to go through those data. The rationale for doing that is to ensure that no plant or material-specific trends are missed, and, also, to prevent against extrapolation outside of the database.

We want to make sure we know that in the future we're going to be getting surveillance data points at higher fluence than the database that was used to calibrate the generic trend curve. So we want to make sure that the generic trend curve isn't missing high fluence trends that the surveillance data might reveal.

MEMBER ARMIJO: Why is that, Mark? You know, those high fluence specimens that have been put in just reactors, run up to high fluences.

MR. KIRK: Not in this case. We're talking only about --

MEMBER ARMIJO: I'm just wondering why you expect the high fluence effect when you did I assume to radiation of vessel materials out to measure their progress?

MR. KIRK: The fact is that the bulk of the available data for power reactors in the United States, have I put enough qualifiers on that, peters out at somewhere between three and four times 10^{-19}. Once you get out to higher fluences in our surveillance database, you really just don't have enough data to reliably calibrate a correlation.

The limited amount of data that we do have out at higher fluences, which is predominantly test reactor data or power reactor data from other countries, indicates that our correlation may be non-conservative in the high fluence regime, which is one of the key reasons why we want to include the surveillance chart.

MEMBER ARMIJO: Is that because there's a flux effect, the rate of which could be applied damage or perhaps actual damage?

MR. KIRK: This where, again, and the experts disagree. It could be a flux effect. It could be some other embrittlement mechanism kicking in that had a long incubation phase, is only now just starting. Really, right now, all we know is that we've got data that we can't trend well.

But we don't have the atom probe, the TEM, the microstructural characterization of those irradiations to ascribe the physical qualities to. Lots of people think lots of different things, but at this stage there is no proof.

MEMBER CORRADINI: You just haven't investigated or it's in progress?

MR. KIRK: The investigation is in progress. In fact, this is an issue that we -- I mean it's been around for years. But in the process of doing this work and working on reg guide 1.99, we sort of flagged it up to the technical community, and, in fact, next, the European Union is considering funding a project on high fluence effects in their next fiscal cycle and there's going to be an international meeting to discuss this held at the Belgium Research Institute in November of this year, and we're planning on participating in both projects, as well as doing our own work.

I mean this is a matter of great current interest because everybody is trying to go for license extension and so people will be looking at this.

MEMBER ARMIJO: You're going to keep this surveillance check?

MR. KIRK: And not only are we going to keep the surveillance check, we're going to require licensees to --

DR. SHACK: We know how you disposed of that public comment.

MR. KIRK: More columns in their Excel spreadsheet. This just shows --

MR. ELLIOT: He's going to explain why. This is important.

MR. KIRK: This just shows the population of plant-specific evidence that we have now. The pie slices, the little number one through eight shows the number of ΔT_{30} values that are available for a particular material. Whereas, the percentage shows the percent of the surveillance population that has that data set size.

So one of the take-aways here is that over half the population doesn't have that much surveillance data. So we're in many ways trying to discern generic trends based on very limited data. But I will point out that all the plants are, by definition, compliant with our current regulations because they're licensed to what the ASTM required when the plant was -- I can't remember when the construction permit was issued and when it was built. But, in any event, that means their held to the requirements of a long time ago when so many surveillance capsules might not have been needed.

So as we already talked about, the motivation for retaining the surveillance check is that in examining non-US data and test reactor data we find out that as we go to fluences above about $3x10^{19}$ our generic trend curve that's in the rule tends to begin to under predict in a significant way.

So right now we don't have the physical evidence or the scientific understanding to feel confident in picking a trend curve to represent that. So while we're working on that, we're retaining the plant-specific surveillance check as a protection.

MEMBER ABDEL-KHALIK: When you say under predict, what do you mean?

MR. KIRK: What I mean is perhaps the actual irradiation shift in a particular material is 200°C, but our trend curve would only predict 150°C.

MEMBER ABDEL-KHALIK: Which means it's non-conservative?

MR. KIRK: Non-conservative, yes, non-conservative in this case.

MEMBER STETKAR: But are those typically from high flux irradiations compared to power reactor irradiations?

MR. KIRK: Well, that's what you see here is the -- it's color-coded for high flux and low flux. So at least at put all the data on one plot level, we don't see a strong flux effect. But I assure you that flux effects are a topic of great current debate among interested parties.

Okay. So how we constructed the surveillance check, I'll blank out the one we decided not to use.

We tried to think about all the possible ways that surveillance data could deviate from the general trend and this is what we came up with.

In one case the surveillance data might just -- which is shown by the green points -- so this is ΔT_{30} embrittlement shift versus fluence. The red curve is schematic representation of the general curve. The green points are a schematic representation of individual surveillance measurements.

In one case the surveillance measurements might all just be high relative to the generic trend. In another case they might show different fluence dependencies. So they might agree initially, but then as fluence goes on, we get progressively more and more embrittlement in our surveillance data set than the general trend predicts.

And then the third case is where we might be cooking along well for a while, but then all of a sudden at the end you start to get a very significant deviation.

So we have put provisions in the alternate rule to check for all three of these possible types of deviation, which we've called in the rule the mean test, the slope test, and the outlier test. All three tests are required for all surveillance data sets that have three ΔT_{30} values or more.

The check is performed only to check against non-conservative predictions. If all tests are passed, we default to the generic ΔT_{30} trend curve. In other words, if the surveillance are close to the generic trend, we adopt the generic trend.

Whereas, if one test is failed -- I'm sorry. If at least one test is failed, the licensee, if they want to move forward with 10 CFR 50.61(a) submission, is required to submit the recommended treatment to the Director of NRR for approval.

The point here is that our approach, what we do is we provide a standard and prescriptive statistical test that everybody has to perform. So we recognize that statistics is well developed and, in this case, fairly standard. So there are standard and accepted procedures to assess the statistical significance of differences between individual data sets and models. However, there are no standard and accepted procedures to assess the practical importance of such differences.

For example, if you have data with very little noise, you can get statistically significant differences that might only be 1°F or 2°F from the generic trend, at which point I think our colleagues in the industry and our friends in regulation would say, no, what's one or two degrees.

So that's why we've left it to say you need to apply these statistical tests and do it exactly this way. But if you fail them, we haven't provided a correction procedure. We've asked the licensees to come forward and make a recommendation recognizing that the best recommended procedure may be different in different cases.

MEMBER ARMIJO: Mark, these are very small populations.

MR. KIRK: Yes.

MEMBER ARMIJO: So doing statistical analysis with one more data point on our already small population, how good is that?

MR. KIRK: Well, I guess we would argue it's better than not doing it.

MEMBER ABDEL-KHALIK: I agree with that. But I just don't want to.

MR. ELLIOT: we have to regulate conservatively. If we don't have enough data, then that's the approach. I mean it doesn't mean that their plant is not. That just means they have to come in here and explain their data.

We can't set up criteria. We can't tell

you whether it's one, A, B or D, and we don't know what the result is. Well, when they get a result that --

MEMBER ARMIJO: That looks odd.

MR. ELLIOT: -- odd, I mean you come in here and we'll decided what to do.

MR. KIRK: Yes. Obviously, there are technical issues with applying statistical tests to small populations. I'm trying to remember what Lee Abramsom told me. The tests aren't particularly powerful. They're likely, because there's large scatter and small data sets, the tests are likely to maybe not flag up things that should be flagged up.

But what is certain is if something is flagged up, it probably deserves attention.

So we've covered surveillance. Then the last issue, two minutes, has to do with inspection requirement, which we talked before about the flaw distribution model and that it's a major input when estimating the through-wall cracking frequency and that differences between the current flaw distribution model and the old flaw distribution model resulted in significant changes in the through-wall cracking frequency.

We've also discussed that, while we have a vastly better empirical basis for our flaw distribution than the models used in the 1980s, in the end it's still based on an examination of a limited material volume from a limited number of plants.

So based on, again, recommendations from yourselves, recommendations from the external review group, we felt that it was prudent to check or compare the flaw distribution model to vessel- specific walls that are detected by ISI.

And so, basically, what we asked the licensees to do, and, again, this is expressed in the form of a table in the rule, is to go out and query their ISI data and compare the flaw sizes and flaw densities that they derive from their ISI data to the flaw sizes and densities that we used in the FAVOR code.

If the comparison demonstrates that they have fewer flaws of smaller sizes that we assumed in the FAVOR code, then everything's good and they can go ahead and use the alternative rule. If not, then again, if the licensee wants to continue down that path, their obligated to make a special case to the Director of NRR.

MEMBER STETKAR: I need to ask something. I'm sorry.

MEMBER ARMIJO: The big flaws, I think they should be able to measure those. But I think the little ones are going to be very tough.

MR. KIRK: Yes. And, indeed, there were public comments in that regard.

MEMBER ARMIJO: You're asking them to do something that can't be done.

DR. SHACK: We'll be discussing that I think a little.

MR. KIRK: You'll get another cut.

DR. SHACK: We'll let John take his shot here.

MEMBER STETKAR: If I step way back from this, where the whole basis for this is a risk basis that we want to keep the frequency of through-wall cracks less than 10^{-6} per year, that's what we started to do, that frequency -- and we're regulating on a consequence here, and that is the conditional likelihood of failure.

I don't see anything in here that talks about variation in the frequency. For example, suppose my plant, and I've done extensive analysis on my plant and I've looked at a lot of things, has a frequency of small LOCAs and stuck-open relief valves that's five times higher or five times lower than the nominal frequency that was used as an input to all of this analysis.

I don't see how that effect any way, shape, or form my compliance with this rule. In other words, I don't see how I'm penalized for having a higher frequency of challenges or I get benefit of having a lower frequent, then the nominal frequency that was derived from 3.0, only three, risk assessments of limited scope of three specific plants using approximate models, and that bothers me a bit.

MEMBER BLEY: Yes.

MEMBER STETKAR: it just bothers me that -- in fact it bothers me more than a bit.

MR. DINSMORE: This is Steve Dinsmore from the NR PRA branch.

The alternative you're talking about would be to also put in the rule that you have to evaluate the frequency of these PTS events.

The alternative, you're kind of indicating would be to also put in the rule that they have to, to use this rule, they have to estimate the frequency of the different PTS sequences and compare that to the bounding frequencies used in the analysis, analogous to the way that these flaws are set up.

But I think what that generalization

study, what all those studies on all that work was intended to do was to look and see if there was really enough variation in the fleet of plants that are out there, that it would be necessary to ask them to do that work.

Because the PTS is a specific analysis. You can't just run your PRA. You have to do all these extra studies because of different end states.

So the generalization work concluded that it was not that important to check those frequencies. Whereas, the distribution of the flaw sizes is somewhat sensitive. If they've got a lot of flaws out there or fairly large flaws, then the rule is structured to have them check and make sure that they're bounded by the flaw sizes and we selected the flaw sizes instead of the frequencies.

MEMBER STETKAR: I think, one, assuming there's higher variability in the flaw sizes than in the frequencies?

MR. DINSMORE: Well, and the flaw sizes have more of an impact because you can never -- one big flaw and you might have an unacceptable through-wall cracking frequency. Whereas, if your SORV opens two times more often than this other plant, you're probably not going to get an unacceptable through-wall cracking frequency.

MEMBER STETKAR: No. A more realistic evaluation of the SORV opening, that's what I was trying to get at. It said that the risk assessments that went into the frequencies were vintage 2000/2001 with an approximate treatment of external events. Better analyses might show different frequencies, either higher or lower, and markedly different. Factors of five, for example.

MR. DINSMORE: Well, factors of five lower would not be disturbing.

MEMBER STETKAR: Factors of five lower would be to my benefit --

MR. DINSMORE: Yes.

MEMBER STETKAR: -- if I'm a licensee. Factors of five higher would be detrimental to me if I'm a licensee.

MR. DINSMORE: Well, no. You'd have to compare it to the bounding analysis. This doesn't allow you if you have fewer flaws to say I can get a higher fluence. It just says here's the bounding analysis and, if you're blow that, you're okay.

The same thing would have done with the frequencies. It wouldn't say, well, they're not supposed to calculate a through-wall cracking frequency for each plant. They're supposed to first compare themselves to some bounding analysis. And if they're below that, then they're fine.

MEMBER STETKAR: But the bounding analysis is based on an assumed frequency.

DR. SHACK: But, John, I mean your real concern is the non-conservatives. I mean if it's not --

MEMBER STETKAR: No. I'm thinking --

DR. SHACK: he can come in and ask for an analysis. He can perform the analysis. We're going to let him get away with the analysis -- without the analysis if he just accepts this results. To me, there's extra work, perhaps, on the licensee's part if he wants to take credit for that.

MEMBER STETKAR: If he wants to take credit for that, that's true.

DR. SHACK: But the only real concern is whether it could really be significantly non-conservative.

MEMBER STETKAR: In a regulatory sense, yes. But I'm going to try to think of both sides of the coin.

DR. SHACK: I think he's going to be so

happy to get this much relief, it'll be a long time before he worries about anything.

MR. DINSMORE: And, again, there was a generalization study that Mark mentioned. I guess there's new regs, there's lots of new regs. We can provide them to you.

In the generalization study, what they did is they took these -- from the detailed analysis they identified five general scenarios, which Mark was talking about as well, and then they chose five other plants, which they figured covered the full range of PWRs, and they compared the detailed analysis from the three plants to those five plants and determined there wasn't enough difference --

MEMBER STETKAR: In the vessels.

MR. DINSMORE: No, in the sequences, in the sequences.

MEMBER STETKAR: In the sequences or the frequencies? In the frequencies of the sequences?

MR. DINSMORE: Yes, because, again, the LOCAs. The LOCAs, it's obvious they're all the same because they all use the same frequency.

MEMBER STETKAR: Parts of the large LOCAs, it's obvious that large and medium LOCAs, it's obvious that it's all the same. However, there's a multiplier in this whole thing that says stuck-open valves. And the small LOCA frequencies are also different from plant to plant.

MR. DINSMORE: Well, the stuck-open valves again --

MEMBER STETKAR: Can be very plant specific.

MR. DINSMORE: Well, you usually need one or two valves to stick open I think.

MEMBER STETKAR: And that's why it has to be very plant specific.

MR. DINSMORE: Well, you can argue with the details of their analysis. But what they've said is that the frequencies are not dissimilar enough that it would have a great influence on the results.

The external event stuff, what they did there is they took three general classes of accidents. One of them was LOCAs, one of them was secondary and primary upsets and the other was secondary upsets. And they went and tried to figure out how external events would cause those and the fires caused the SRVs open or the PROVs to open and that was a higher contribution than from the internal events, but it still wasn't high enough to affect the results, the total results.

MEMBER STETKAR: I guess I'm not questioning specific details at the moment. I'm questioning a philosophy that's based on a risk metric, which is a frequency of through-wall cracking failure, and that's a risk. It's a frequency and a consequence. And we're regulating purely on the conditional consequence without any information about variations in that frequency.

We're looking at that potentially potential differences in the susceptibility, but with no information about differences in the frequency. Whereas, in general, it's the product of the two.

MR. DINSMORE: Yes, we could have done that, but we chose not to do that because we didn't think it was necessary.

MEMBER STETKAR: Okay. I guess I'd be interested to know.

MEMBER BLEY: Maybe you could point us to those generalization studies.

MR. DINSMORE: Or we can provide you copies.

MEMBER BLEY: We didn't hear enough about that I think to gain confidence.

MEMBER STETKAR: Okay. Thanks.

MR. DINSMORE: Do you want me to

distribute those to everybody or just?

MS. RODRIGUEZ: We can just get it through the ML number. We'll give Michael the ML number.

MEMBER ABDEL-KHALIK: Will your entire package, tools and methodology be made available to the licensees in case somebody wants to go through the process using their own data?

MEMBER STETKAR: In principle they could do that.

MR. KIRK: Yes. It's all in the public domain, the codes, the reports, everything.

MR. ELLIOT: We just want to be clear. We're setting regulatory limits. If plants meet the regulatory limit, if they demonstrate to us, they're done with this.

MEMBER ABDEL-KHALIK: Yes, I understand. But if somebody is borderline --

MR. ELLIOT: They don't have to go to any other text, or any other NUREGs, or anything like that. It's all in the rule.

MEMBER BLEY: That's why we're asking this kind of question.

(Laughter.)

MR. ELLIOT: I want to tell you where we're coming from. That's what we're trying to do.

We're trying to put out a piece of paper that that's the answer.

MEMBER STETKAR: I want to have confidence that that regulatory limit, indeed, is set at a certain point that it accounts for variations in the consequence into that equation. I want to have confidence that it, also, somehow accounts for variations in the frequency end of that.

MR. KIRK: It's certainly a fair question to ask. I mean there are hundreds of factors in engineering decisions in these models.

MEMBER STETKAR: Sure.

MR. KIRK: And it's a fair questions to say, okay, well, if you've got all that, you've asked licensees to specifically check or validate two of them. Why did you pick those two?

MEMBER STETKAR: Yes.

MEMBER MAYNARD: Just a point of clarification. This is an alternate PTS rule.

MR. KIRK: that's correct.

MEMBER MAYNARD: Does the other rule stay in the books?

MR. KIRK: That's right.

MEMBER MAYNARD: Okay. That's what I thought.

MEMBER BLEY: I just want to take you back to one thing, Mark, sorry.

I appreciated the reference to NUREG-1807 and it's really interesting. And Appendix A gives a really nice view of the validation of the linear elastic fracture mechanics and correlates that with experiments. But doesn't have a word about the FAVOR code and how it was validated.

MR. KIRK: Okay. That's these tests.

MEMBER BLEY: So if you can give us a reference to that I really would like that because that's the glue that holds this stuff together.

MR. KIRK: NUREG --

MEMBER BLEY: Don't guess again. Just get it.

MR. KIRK: Yes, we'll get it. We'll get it.

DR. SHACK: Can we move on, gentlemen, to give a chance to discuss the rule itself?

MR. KIRK: Okay.

DR. SHACK: Okay. Thank you very much, Mark. You can go drink to your heart's content.

(Laughter.)

MR. ELLIOT: He did a hell of a job. This is a very complex subject.

After he finished explaining all the complexity, I'm going to try to make it simply.

We took Mark's information and we put it onto the rule on October 3rd, 2007. We got back a lot of comments. As a result of all those comments, we put out a supplement and one of the issues in this supplement was the issue you were talking about. It is difficult to find the flaws that are that small. And we'll get back to that later on in my presentation. But I want to go through the rule and then we'll get back to the supplement.

PWR licensees can voluntarily choose to apply the requirements of the rule. If you don't go above the screening criteria, you're happy, you use the old rule. That's going to be for 90 percent, whatever. And we're talking about the other ten plants that need this to keep operating.

The discussions today are based upon the staff's present position on the rule. We're getting some more comments from this supplement. We may change at that time. I doubt it, but this is where we are today.

There are two analyses in the current rule. In the previous rule there was only one. There was the RT PTS, the embrittlement calculation. Today we're requiring an embrittlement estimate and the in-service flaw estimate.

The embrittlement estimate has two parts. It has a calculation and it also has, as Mark pointed out, is a surveillance data evaluation. That is a change, also, from the previous rule.

DR. SHACK: Are you going to respond to the public comment about putting that information in --

MR. ELLIOT: Yes, we're going to respond. Our position today is we're keeping it in.

DR. SHACK: Okay.

MR. ELLIOT: And we have a procedure for responding to public comment and we just haven't done it yet, but count on us, we'll do it.

MEMBER ARMIJO: Is the period for public comment over right now?

MR. ELLIOT: Yes.

MEMBER ARMIJO: Okay.

DR. SHACK: What's the rationale for keeping the embrittlement correlation in the rule rather than putting it as an NRC approved methodology.

MR. ELLIOT: I think what we decided there was that this is a -- everybody will have confidence that this is what is approvable and they know where the NRC stands. If we put it into some other document, I think they might be worried that we'll change that document.

DR. SHACK: You could with reg guide 1.99.

MR. ELLIOT: We don't have a reg guide 1.99 Rev. 3 yet and I'm not holding my breath for that one. But we want to get this one out.

DR. SHACK: If you can't settle on a reg guide, you now want to embed this thing in a rule?

MR. ELLIOT: We need to get this out because plants need it. We have enough checks in here so that if the embrittlement correlation isn't true, we'll find out about it.

The surveillance check-- at all points they can use this rule, we'll have surveillance data at higher fluency eventually. And then we'll be able to check through the surveillance data at that time. So plants will know what they need to do to keep operating. They'll need to keep their embrittlement down, have to keep track of their surveillance material, and they're going to have to have good ISI results.

MR. MIZUNO: This is Geary Mizuno from the Office of General Counsel. If I could respond to that because it's a combination of technical/regulatory considerations, as well as legal considerations that led to the rejection of the suggestion to remove the embrittlement correlation from the rule language, and, basically, can sum up the primary reasons from a regulatory standpoint as keeping the correlation in the rule provides for regulatory stability and predictability to both the NRC staff, as well as to licensees/applicants, and, also, provides transparency to the public, the general public.

They know that this is what the commission is going to be using to evaluate -- well, actually, licensees are going to be using to evaluate the adequacy of their reactor vessels, and if there is any need to address whether the reactor vessel is going to continue to function, that those criteria are established by rule and are consistently applied across the board to all licensees.

DR. SHACK: Of course, there's precedent for not putting it in the rule.

MR. MIZUNO: Absolutely.

DR. SHACK: It’s like 1961.

MR. MIZUNO: There is no reason regulatory or legal reason why the commission or the NRC staff could suggest a different approach. I mean in other situations there may be a different set of considerations in which flexibility and transparency, predictability, those kinds of things, weigh in a different fashion.

So, yes, it's possible, legally speaking, to do something else. But the legal consequence of not putting it into the rule in this particular situation was felt from, again, the standpoint of transparency, certainty, predictability would result in an adverse regulatory environment or less preferable regulatory environment and that's why the NRC staff ultimately decided that they would recommend to the commission that the environment correlation be maintained in the rule.

It's always a balancing between how much flexibility you want in doing things on a case-by-case basis versus giving more stability, predictability and, quite frankly, in the context of any hearing, being able for the NRC staff and for the licensee to rely upon the rule as something which the commission has adopted and, therefore, is not subject to challenge absent special circumstances, and with the commission approving that versus a situation where it's done on a case-by-case basis and it is subject to litigation.

MR. ELLIOT: That was a public comment and that's our answer.

MEMBER ARMIJO: These equations, there various equations go into the rule.

MR. ELLIOT: No, no. Those equations do not go in the rule.

MEMBER ARMIJO: Would they be in a reg guide somewhere?

MR. ELLIOT: No.

PARTICIPANT: Is this what you're talking about?

MR. ELLIOT: Yes. We're not putting those equations in the rule.

MR. KIRK: Hold on, hold on. Point of order. Those equations are not the embrittlement trend curve and they do not go in the rule.

MEMBER ARMIJO: Okay.

MR. KIRK: No, no. I'm sorry. The embrittlement trend curve correlations are in the reports. They're not in your slide chart.

MR. ELLIOT: So I'll continue on and tell you what's in the rule.

MEMBER ARMIJO: Okay. Good.

MR. ELLIOT: So there are two analyses. There's the embrittlement analyses and the in-service inspection analyses.

In any risk-informed analysis you want to consider the important -- evaluate the important assumptions. And in here is the flaw distribution and the density, which is a critical assumption. So we've decided to put it in the rule.

And we put a distribution -- Mark, show the distribution -- it's very similar to the distribution that was put into FAVOR except we made smaller increments to account for the reporting sizing requirements in the code. The ASME code reports sizes in 50,000s increments. So we put up a table with that increments built into it.

It's very simple. If you have less flaws and smaller flaws than are in the table, you can use the rest of the rule. If you don't, then you've got to come to the NRC and provide us justification. We'll get into that a little bit more in the next couple of slides.

What we're doing is we're building on the existing technology. The existing ASME code, we're using the existing ASME code requirements for inspection and qualification, and those are the requirements for the procedure.

Next slide.

The alternate rule contains flaw limits on the size and density. The flaw is within one inch of the clad-steel interface or 10 percent of the wall thickness, whichever is greater. The limit is more restrictive than the ASME code requirements. Some of the flaws that would be accepted by the code would not be accepted by our table.

It also requires licensees to determine if the flaws at the clad-steel interface have penetrated through the clad and opened into the inside surface. If you remember Mark's analysis, they had clad defects, but they didn't have any clad defects that penetrated into the steel and intersected an existing flaw defect.

To this day, we have never seen this except for one case in a BWR in its upper head. Quad Cities had a surface clad defect that penetrated and intersected at a subsurface sub-clad defect.

DR. SHACK: But they have a mechanism that you probably wouldn't expect to find in here.

MR. ELLIOT: We wouldn't expect to find it, but we're putting this requirement in. If you see a flaw at the clad-steel interface, we want you to check to see if it connected up with something in the clad. We have no way of inspecting the clad UT using ultrasonics. We can inspect the weld in the base material and we can see what it shows and then we're going to have to have some other alternative inspection if they have a defect at the clad-steel interface.

And then, finally, we have a requirement, and this is just for confirmation, we know the flaws that are created in three-eighths of the wall thickness contribute nothing. So it's just to confirm that you did look at it and that it met the ASME code requirements and we'll be satisfied with that. There's no real inspection there.

If the ISI limits on flaw size, density, and location are not met, quantitative or qualitative analysis can be submitted for NRC approval. And what we mean by that is if you get a flaw that succeeds the table and it's one flaw and it's not in a high fluence region, there's no reason to go through all of this all over again to try to do all the risk analysis all over again. It's probably not going to matter.

So this is going to be a case-by-case basis. Most of the time, it depends where the flaw is located, how big it is, how much it exceeds the criteria, how many of them there are, things like that will determine whether or not we want to do a full-blown quantitative analysis.

Next one.

That's where we stand on the ISI. We'll come back to it and discuss on the supplementary in a few minutes.

Analysis of the reference temperature from embrittlement, the projected RT_{MAX} values are calculated in accordance with the rule, including evaluating the effects of surveillance data. The separate RT_{MAX} valves are calculated for axial welds, circumferential welds, plates, and forgings.

All the rule does is compare the RT_{MAX} values to the screening limits provided in the rule, and Mark explained to you how the screening limits were calculated.

Screening limits contain a combination of RT_{MAX} values for plates, forgings and welds to insure that the through-wall cracking frequency for the entire vessel is below the risk limit. This is different than what was in the old rule. The old rule, each independent material was evaluated independently and separately and it wasn't a cumulative risk.

The screening criteria in the current rule is based on a limit of $1x10^{-6}$ per year, through-wall cracking frequency, which is a little bit lower than the old rule.

The RT_{MAX} is calculated for each belt line material, weld, plate, and forging. The RT_{MAX} is the sum of the other irradiated temperatures and the increase in the 30-foot pound temperature resulting from neutron irradiation. There's no margin and that's because, as Mark explained, the margin is accounted in the analysis.

For welds, the RT_{MAX} is the higher of the RT_{MAX} for the weld and the adjacent base material and that, as Mark also explained, is because we're concerned about the flaw that is in the fusion zone and that it could propagate whichever is the more limiting material, the weld or the plate. So we're limiting that.

Next one.

Now here's a comment. Rule contains a prescriptive methodology for calculating ΔT_{30}, which is based on the neutron fluence, the neutron flux, the copper, nickel, phosphorous and manganese content, the product form, cold leg temperature and vessel manufacturer, a very intricate model. And we don't have the basis in the rule, but Mark has a NUREG that explains how this embrittlement correlation was developed.

MEMBER CORRADINI: So just to ask, nothing about this has changed?

MR. ELLIOT: No, no, nothing has changed there.

MEMBER CORRADINI: So this methodology stays?

MR. ELLIOT: Yes.

MEMBER CORRADINI: Okay.

MEMBER BROWN: I thought this gave lower numbers. I thought you all changed the correlation.

MR. ELLIOT: We changed it from the -- there is a way of calculating embrittlement in 10 CFR 50.61, the old rule. We are not using that embrittlement correlation. We have a new which is totally contained in the rule. It answers the question of Bill asked was why do we put it in there. We could have put it in a reg guide and just say meet the reg guide or something else. We decided to put the entire calculation model in the rule.

MEMBER BROWN: I mean it results in lower RTs, doesn't it? Your limits are still fairly low, aren't they, 270, or 269 and 358s and stuff?

MR. KIRK: The correlation doesn't produce reference temperatures. It produces transition temperature shifts. I mean for any individual plant or material, if you compare the new correlation to the old correlation, there may be significant differences for any given material. But on average, if you look at the entire population, the new correlation isn't really that much different overall than the old correlation in terms of the total fleet perspective. But just how far you can go.

MR. KIRK: Yes. What's changed significantly is how far we allow you to go?

DR. SHACK: What do you mean how far we can go?

MR. KIRK: What our limit is, what your RT limit is.

MR. ELLIOT: If you look at the screening criteria, we say the screening criteria is 269 for axial welds in the alternate rule. The screening criteria in the old rule for axial weld was 270. You say, gee, they're identical practically. Except for one case, we are requiring people to put in margin. In the old rule, to determine if you were below the screening criteria, you had to put in a margin term.

Now, in the current rule, you don't put that margin in. That helps them in the sense that they don't have to add the margin in any more. They're using a mean value for embrittlement.

MEMBER BROWN: And the max number of that was, what, 60 before with some numbers lower in the circumstances?

MR. KIRK: The max of the margin is 60, yes.

MR. ELLIOT: Okay. The new embrittlement model was developed from a large database, and, therefore, we have confidence in its applicability to predict embrittlement. Because of the confidence in the model, we require the model to be used unless there is contrary plant-specific data.

In the old rule, 10 CFR 50.61, the plant-specific data replaces the model for calculating embrittlement. In the new rule you have to go through the surveillance checks before we're concerned.

The rule require licensee to utilize the methodology in the rule to calculate ΔT_{30} unless plant-specific data fails any of the surveillance data statistical text in the rule.

Next one.

The surveillance data is evaluated using three statistical tests to determine if the ΔT_{30} value calculated using the embrittlement correlation should be adjusted. That is, is it showing non-conservative results. After conservative results, we're happy. If you get non-conservative results from the statistical analysis, then we're not happy and you have to come see the NRC.

If the surveillance data fails any of the tests, an evaluation of the data and its impact on the proposed --

DR. SHACK: Now, they do submit all that data to your database, right, whenever they do a surveillance capsule?

MR. ELLIOT: Yes. The overall database may see nothing because there's no many data points. But if we have one plant that has material that is really relevant to its plant and it's showing a significant change, we're concerned about that plant. So we don't want to hide all that one plant behind all of the other data. That's the intent behind the surveillance check.

The rule does not contain a prescriptive methodology for calculating ΔT_{30} when plant-specific data is used. You're going to have to come in and propose. Again, it depends on which of the three models, A, B or D, that they failed will determine what kind of change in the model they might have to make.

If screening criteria cannot be satisfied, licensee must submit a safety analysis to determine -- and these two are basically the two same safety analyses that we required in the old rule. Hopefully, nobody would have to do this.

I also point out that, remember, Mark had that little -- he explained how we got the screening criteria with the vertical and the horizontal and the tangent. Well, there's a little part in that rule that we didn't take into account, that is -- and you could be above the combination and still be under the screening criteria.

Under this part of the rule, people could do that if they needed to do it. They would just take the formulas that Mark has talked about in plant-specific RT_{MAX} values and they could then demonstrate that they reached and still are 10^{-6} and the rule takes care of that.

That's that one.

MR. DINSMORE: This is Steve Dinsmore. Just real quick. If your plant is a lot better and you end up in this situation, you can use the frequencies in your PRAs and do you analysis and come in and demonstrate. So you still have that option to do that. Now you wouldn't know how to do it because you'd have this big study to base it upon.

So you could still use risk information -- you can still use the frequency information. It's just you don't have to until you reach the limits that are set in the rule.

MEMBER STETKAR: To get benefit.

MR. ELLIOT: Next one.

Now we'll come back to the supplemental proposed rule. There are three parts that we put in the supplement to modify the original alternate rule.

We determined that we originally had the rule applicable to all plants, and one of the comments was how do you know that, you don't even know what the plants are going to be like in the future. And they're right, we don't know what the plants are going to be like in the future.

What we do know is that it's applicable to all operating plants and plants of that type of design that we have now. So the rule is applicable to plants that have operating licenses and we included Watts Bar Unit 2 in the rule because they could come online.

We also are considering whether to include other partially constructed plants whose NSS design is similar to those of the operating plants. We haven't concluded that yet, but we're considering it.

MEMBER ARMIJO: But does that include a

certified design, like AP1000?

MR. ELLIOT: No, it does not. It only includes the ones that have designs similar to operating plants.

MEMBER CORRADINI: So if we can go back to that, though, I guess.

MEMBER ARMIJO: I guess what's puzzling me, is there not enough specification or control of in the certified design with respect to the vessel material, vessel fabrication that it wouldn't fit into this?

MR. ELLIOT: I think the people who are looking at the certified design have to answer that. I don't look at the certified design.

Let me just say this, people with the certified design could use the old rule. And the people with the certified design, I looked at it. They're limiting copper to 0.1 percent. They're not going to need this rule. Remember, we showed at the beginning of the data, the plants that are 10° from the rule, 20°. They've got to be 100° from the rule.

MEMBER ARMIJO: Okay. That's fair enough.

MR. HACKETT: If I could make a further comment that might be helpful with Barry's adding. This is Ed Hackett, ACRS staff.

The new plants are, a lot of them, maybe all of them are likely to use forgings, so you're probably going to eliminate belt line welds entirely. The fab process will probably eliminate the need for consideration of this type of approach entirely in all likelihood.

MR. ELLIOT: We just had to answer the comment. The more we thought about it was we really don't know every possible design, so we shouldn't include it in here.

Also, we thought about the surveillance data, checked the original proposed alternate rule, only had the mean test and we decided that higher fluences could be more of a problem than we thought, so we added the slope test and the outlier test, which is tests for plants with higher fluencies.

Now, the third item in the proposed and we put out in the supplement was the flaw sizing issue. One of the things we talked about was that for small flaws they probably can detect them, but there's going to be a very difficult time sizing them.

So we proposed, we put into the supplement that the NRC is considering whether to permit flaw sizes to be adjusted to account for the effects of sizing error when the estimated size and density in the RPV is compared to the size and density in the rule.

The reason for that is because small flaws, most of the time, are going to look, from the UT inspection, larger. So we would push a whole bunch of small flaws into another bin and we don't want that. We would want them to account for the actual size based upon the uncertainty in the error in the sizing. And we'll be discussing that.

But, presently, we plan on just allowing plants to take that into account. If they fail it, they have to take it into account and tell us how they adjusted -- you know, why it's acceptable.

MEMBER ARMIJO: What's the smallest size flaw that you're concerned about, 50 mil or 10 mil?

MR. ELLIOT: No. The smallest size is 0.075 to 0.125 in through-wall dimension.

DR. SHACK: That's considering. Are you going to permit or is this just out for comment?

MR. ELLIOT: Do you want to take that, Matt?

MR. MITCHELL: Again, Matt Mitchell, NRR. That was --

DR. SHACK: It doesn't sound like regulatory stability to me.

MR. ELLIOT: We haven't reached the end, concluded yet.

MR. MITCHELL: We put the concept out in the supplemental proposed rule for public comment. Now, I think our going-in position, I mean we put it out with thinking that this is a feature that we would expect to put into the final rule barring significant adverse public comment to including that provision or that allowance within the final rule.

So I would say we're biased toward putting it into the final rule, but we will still have to deal with the last set of public comments.

MR. ELLIOT: Okay, conclusion.

The proposed rule provides an alternative method for licensees to demonstrate that the risk from PTS is low throughout their extended operating period. The alternate rule is needed for reactive vessels that are projected to exceed the screening criteria in the current PTS rule prior to end of the first renewed license. There are a few that need it, right, you know, to continue their operation in the renewed license. And, also, that plants may need it for power uprates.

Next.

The conclusion is, remember motivation,

why we were doing this way back about 9:00? Staff analyses have removed unnecessary conservatism in the current PTS rule. Implementation fo the alternate rule will reduce the burden on the NRC and licensees and eliminates an unnecessary impediment to license renewal.

And then we've looked at all the operating plants, all operating reactors are projected to be below the alternate PTS screening criteria at the end of their first renewed license and should have adequate margins to permit power uprates.

That's where we are with the alternate rule.

MS. RODRIGUEZ: Do you have any questions before we move?

MEMBER BROWN: Yes. Why would you advertise the basis for doing something like this, removing conservatism because it reduces the burden on the NRC and licensees? Why isn't there a technical safe operation basis that's more -- I mean I would never tell anybody that I implemented this less conservative method because it made life easier for me and other people to operate.

MR. ELLIOT: We explained that it provides the comfort of a risk limit of 10^{-6} per reactor year.

MEMBER BROWN: But that's meaningless to me if I am somebody out there in the public domain that wants to fight this and I would walk up to somebody across the room table and say, geez, the only reason these guys are doing that, they established this arbitrary $1x10^{-6}$ with okay to break vessels and they're doing it because it's easier on them and lessens the burden on the licensee for continuing to operate.

I don't want to be sitting on this side and answering that question. The technical basis, I would couch this more in the terms of a technical, which you've presented I think pretty well.

DR. SHACK: But still, you have to agree that $1x10^{-6}$ is an acceptable limit.

MEMBER BROWN: No, that's fine. But establish it based on the context that the previous limit was not unacceptable, but was so overly conservative that drove unreasonable design or plant operations or modifications, et cetera, et cetera, and, therefore, you took an effort to go make this thing more reasonable and approached based on the knowledge base we have today.

The down side, the good side for you all is, yes, we're not going to have to evaluate 18 of these a year, or something like that, or over the next five years. But, really, I would never say this out in the public domain.

MR. MITCHELL: Let me see if I can --

MR. ELLIOT: I think we're trying on the first bullet to say that. Staff analyses have removed unnecessary --

MEMBER BROWN: I'd get rid of the second bullet. I would never say that. I'm not going to say any more. I just think you're shooting yourself in the foot if you give some of these groups that want to argue about this stuff. They'll just say, geez, you know, this is only to make it easier on people and so they're throwing away safety, okay, to make it easier on themselves.

MEMBER MAYNARD: Well, there is a stated policy by the commission to reduced unnecessary burden, where applicable, without any undue degradation to health and safety. So this isn't anything new and it is a state policy of the commission.

DR. SHACK: I mean and you are clearly allowing them to operate with more embrittled vessels, only that you have done is demonstrated that they can do that safetly.

MEMBER BROWN: Safety, but that's not the way the emphasis is put.

MEMBER BONACA: But for the way, I would say that the bases of the previous rule are obsolete. I mean they were simplistic for assumptions that were made that by today's standards are, you know, what we know for this rule.

MR. ELLIOT: We haven't reduced our safety standard. In fact, we made it be restrictive. It used to be $5x10^{-6}$ and we reduced it to $1x10^{-6}$.

MEMBER BONACA: I think it's more what Charlie's put together is the communication that you're giving there. I mean the way I see it, I appreciate this new rule because it has a technical basis that makes sense.

DR. SHACK: We all believe the numbers, right.

MEMBER BONACA: And the previous rule did not have the technical basis that made it reasonable I mean in many ways. The simple usse of forcing licensees to assume blow-downs, for example, without you want intervention without evaluation. The other is no technical basis for the old rule except very last, very conservative assumptions.

MR. MITCHELL: We do, the staff and NRR, and everything I think who's worked on this, does appreciate the committee's comment. And, certainly, if we didn't feel that there was, essentially, an airtight technical basis for what we're doing, we would not be promulgating this rule, and I think maybe we take that as a bit of a given that that message comes across, that we would not promulgate a rule that we were not confident in.

And, in addition to that, I would agree with Dr. Maynard's comment that the sense that you have on this particular slide, that comes at the end of a very long series of slides, that emphasizes our technical basis, is just, again, the notion that, from a principles of good regulation standpoint, we do not want to be putting regulations in place or we want to be providing -- we don't want to be putting unnecessary regulatory burden on the licensees.

So it certainly was not meant to be a bullet that overwhelmed or took the emphasis away from all the good, technical work that has been presented for the first, what, about nine hours of what we've put into this presentation. It was merely just paying homage to that fact that the principles of good regulation that we're applying here.

MEMBER BROWN: The word unnecessary relative to over conservatisms based on lack of detailed knowledge of material characteristics, et cetera, is a better way of phrasing it than unnecessary conservatisms. Overly conservative based on a lack, that's what it was based on 30 years ago. We had a lack of knowledge and so we set one-size-fits-all to cover a whole range of things, which impinges things.

You can do what you want. If you want to argue about this for another five years with the public, you can. I agree with what you're doing sort of, I guess. We haven't voted on it yet.

MEMBER BONACA: I mean the old rule would force retirement of a number of plants unnecessarily.

PARTICIPANT: There are some people would like that.

MEMBER BROWN: Because of over conservatism in the requirements.

MEMBER BONACA: It goes around. I mean it's not something that it says that you would have to retire plants that can operate for another 20 years safely because the requirements imposed are unreasonable.

MEMBER BROWN: Overly conservative?

MEMBER BONACA: Overly conservative.

MEMBER BROWN: As opposed to unnecessary conservatism?

MEMBER BONACA: Right.

MEMBER BROWN: I rest my case.

DR. SHACK: Have you examined or demonstrated the feasibility of demonstrating the allowable number of flaws in the welds? I mean does it take days --

MR. ELLIOT: We haven't looked at every possible --

DR. SHACK: -- phased array ultrasonics?

MR. ELLIOT: We haven't looked quantitatively at, if you get 20 flaws and one of them is larger than the limit and it's in a bad location, what that does. We could do that, you know, eventually, make sensitivity studies to see, you know, what distributions really are a problem.

We know that if you meet this table, the distribution is fine. We don't know how bad is bad. We just know this is fine.

MEMBER ARMIJO: But you're going to be using UT in all probability --

MR. ELLIOT: Yes.

MEMBER ARMIJO: -- going through a

stainless steel cladding, which itself is not homogeneous and perfect, through an interface and trying to detect, tiny, tiny, tiny little flaws near the surface with all sorts of stuff going on, I just think you've got to be very careful that you don't ask them to do something that nobody believes in.

DR. SHACK: If you need to do it, though, you need to do it.

MEMBER CORRADINI: You only need to do it if you fall outside the band.

MR. ELLIOT: No, no. You have to do it if you enter this -- to use this rule, you have to do the inspection. You've got to do the analysis of the inspection.

Everybody has to do the inspection. That goes with if you don't have this rule or not. Everybody has to do the inspection. That's an ASME code requirement. The NRC requires that.

What we are proposing here is just an alternative acceptance criteria that is in the code. We have an acceptance criteria for pressurized thermal shock.

MR. MITCHELL: Let me add, perhaps I'll address Dr. Armijo's question.

With the exception of the issue that Barry spoke about in terms of uncertainties and potentially an oversizing bias for the very smallest flaws, it's our understand that what we are asking for the licensees to do is not beyond the scope of existing technology that is being implemented to do ASME code examinations today under the PDI qualifications that are already in place.

So it's our understanding --

MEMBER ARMIJO: Puts my mind at ease that it's a practical thing, that it actually can be done.

MR. ELLIOT: We have a qualification procedure in the regulation that says how to qualify this type of inspection. Everybody has to do it whether they do the rule or not. What they didn't have to do was look at the results through the sizes that we are asking for. Now they're going to have to do that if they want to use this rule.

We'll see that happens.

MEMBER MAYNARD: That does relieve some of my anxiety about what do they have to do. However, I still believe that what you're going to come up with, especially on the density, is going to be showing much less density than what you because I don't think the capability to take some of these -- I think people are going to fall under the curb, especially for the density there, and I don't think they're going to find that many of the real small ones just because of the capability.

MEMBER ABDEL-KHALIK: But if they fall under the curb, then they're satisfied.

MEMBER MAYNARD: That's fine. As long as you're not asking them to do something above what the code requires from a capability standpoint.

MR. ELLIOT: We are following the code qualification procedure. We're not inventing anything new here.

MEMBER MAYNARD: And if you're doing that, that's fine.

DR. SHACK: Let me just sort of see schedules here. You're expecting to have a draft final rule in March?

MS. RODRIGUEZ: Basically, yes. The comment for the period already ended, it closed on September the 10th. And our next steps after we get out of this meeting is to evaluate the comments and start putting the responses together. And we're going to incorporate the comments on the supplemental proposed rule and on the proposed rule and we're going to put it on the final rule.

We're expecting that the commission will review the final rule in April of 2009. So that puts us to perform a full committee briefing around March time frame. I think we have that as a tentative date.

Once we get the -- I need to talk to Michael to see if we get the final word on that. But, tentatively, we'll be seeing you again in March and we'll be briefing you on the final rule.

DR. SHACK: When will be seeing all your responses to the public comments?

MS. RODRIGUEZ: In that package you're going to see it. You're going to see it all.

MR. ELLIOT: We've already discussed some of the more significant ones here.

DR. SHACK: Yes. Well, there's still the ones on table three.

MR. ELLIOT: The ISI?

MR. DOMES: That and the question of allowable numbers compared to the FAVOR calculations.

MR. ELLIOT: We explained that. That's where the allowing of the taking into size effect would account for that. If we allow to take into consideration the oversizing, then that would take care of their concern.

DR. SHACK: But wasn't there some concern that you were picking the allowable numbers based on failures rather than populations?

MR. ELLIOT: No. We're not basing it on failures.

DR. SHACK: Okay. That's a misinterpretation.

MR. ELLIOT: Yes.

MR. MITCHELL: And, also, just to clarify. Although you will notice that in the supplemental proposed rule that was published this year, we've only addressed certain specific points. The three issues that Barry mentioned.

From the first round of public comments that we received from the original proposed rule, we have already developed our answer and responses to every public comment that we received the first time around. We simply just did not publish them in the supplemental proposed rule. We focused it on the significant changes that we felt needed another round of public comment or which were new and had not been seen the first time around.

So there are changes, there are answers to the original public comments. We have those. Those will be put together as Veronica suggested in the final rule package.

MS. RODRIGUEZ: That is an excellent

point. Yes, thank you, Matt.

We have pretty much all the responses for the comments that we received on the proposed rule. We just to work with those that we received on the supplemental.

DR. SHACK: Are there any more questions?

MEMBER BROWN: Yes, I just had one briefly that talked about the three numbers, 269, 356, and 538 that you derived from your little chart. Where does the 312 for the circumferential weld pop up?

MR. ELLIOT: 312 is --

MS. RODRIGUEZ: Can you tell me that page?

MEMBER BROWN:It says circumferential weld.

MR. ELLIOT: Yes, that's where the ten-to-the-eight degrees that he had. He used 10^{-8}.

MEMBER BROWN: Is that what that number --

MR. ELLIOT: Yes.

MEMBER BROWN: Okay. I got it. Stop. I missed that. That's that plane that he cut us off.

MR. ELLIOT: Right.

MEMBER BROWN: Okay. Thanks. I got it. I'm trying to absorb too much on this stuff, okay. The neurons were not snapping.

MR. MITCHELL: I will offer the Committee one final point of clarification.

There has been a lot of discussion here at the end about the embrittlement model, and if you have the opportunity to pull out your supplemental proposed rule making package from this year, if you look in that, what we're talking about specifically are equations five, six, and seven, along with the associated definitions that are under that.

Those equations and the associated definitions are what we would have terms the embrittlement model that is in the new rule. So that's just to calibrate whenever you get a change to step back and go through it, that's what we're talking about.

DR. SHACK: Any further comments? Well, thank you very much. Thanks very much to the staff. It looks like Mark has already gone, but it was quite a presentation today, an impressive package, and thank you very much.

MEMBER ABDEL-KHALIK: I'd like to just make a comment.

First of all, great job. This is really a well done piece of work.

There are two things that I would like to just make sure they don't fall through the cracks. One of them is that we need to see the details of how the thermal hydraulic uncertainties were incorporated in this.

MR. ELLIOT: Thermal hydraulic uncertainties.

MEMBER ABDEL-KHALIK: Right, because that came through.

And the second thing is that whether or not a utility elects to adopt this new rule, I think it would be worthwhile if the lessons learned from this study would be given to those people in case there are any procedural implications. If they have to look and re-examine their current procedures to see if there are any necessary changes so that they wouldn't exacerbate this problem, I think that would be worthwhile, whether or not they elect to adopt this new rule.

MR. MITCHELL: We understand the comment. Some of us here at the side table were sort of discussing that as well as those comments came up earlier. I think part of our observation is that many of the lessons learned about managing pressurized thermal shock events were learned in the mid '80s, in the '90s, and that many of the procedures may already be informed in large part to offset or to combat those things that operators could do to make a situation worse.

However, we will make an effort to go back and see if there are any additional insights or lessons that we could promulgate and get out to the industry that might even help the matter further.

MEMBER ABDEL-KHALIK: I think that would be a good idea. Thank you.

DR. SHACK: Any more questions for the staff?

(No response.)

DR. SHACK: Just go quickly around the table. Michael, any other comments?

MEMBER CORRADINI: I don't have any other comments. I think the before-lunch thing, Said caught it that Mark had promised us, and whenever we rotate back through to understand how you fit in the uncertainties.

I guess I wanted to ask you, so, only at the time of March would a letter be generated for this, is that correct?

DR. SHACK: Yes. You know, we'll have some discussion. I don't see that there's any necessity for a letter unless we feel that there are real show-stoppers here.

MEMBER CORRADINI: But I think it's really well done work and thanks for explaining it.

MEMBER BROWN: No, I agree. For somebody who's not primarily materials or thermal hydraulic, that was very well presented and I almost understood everything you said.

DR. SHACK: Just on the take-aways, I mean we are going to get the report on the thermal hydraulic uncertainties and the five-plant generalization studies I take it, only it's in ADAMS. It's not a NUREG of any sort.

MS. RODRIGUEZ: Right. I think everything is in ADAMS.

DR. SHACK: Right. NUREGs are available other places.

MEMBER CORRADINI: You have to put us to it so we can get it.

MS. RODRIGUEZ: Right. I will, definitely. I will contact Michael.

MEMBER BLEY: And the other one was the -- I should say V&V, but --

MS. RODRIGUEZ: For the FAVOR.

MEMBER BLEY: The FAVOR of V&V, but that that's probably a NUREG again.

PARTICIPANT: Which one?

PARTICIPANT: The peer review of the FAVOR where people security your specification and replicated your results from that.

MS. RODRIGUEZ: Yes. I think I made notes of all the documents that we owe you.

MEMBER MAYNARD: Excellent briefing. I think a good product. I do believe that as we change or bring in new regulations that are relying on PRA, to me, I think consideration needs to be given to somebody that wants to use this alternate rule, they should do something to provide confidence that they fall within some of the assumptions for PRA.

I don't think they have to do a total PRA. But I think as part of the application it would be good to have something that just shows that their event frequency would be consistent with the rule development there. That's all.

MEMBER ABDEL-KHALIK: I've made my comments.

MEMBER BONACA: I think it's an outstanding piece of work and was also a great presentation. Thank you.

DR. SHACK: Dana?

MEMBER POWERS: I am very disappointed in the product relative to what was promised when it was initially proposed, which a rigorous exploration of the uncertainties here. And, instead, we have found that the phenomenological uncertainties are very discarded, hidden, obfuscated so that I don't have any understanding of the breadth and width of these points they get plotted on, unusual amplifications of three-dimensional maths.

That said, clearly, the agency has given a gift to the industry through its research program, maybe it's some help to the staff, but it is clearly a gift to the industry in this area.

DR. SHACK: Sam?

MEMBER ARMIJO: Well, I said earlier I thought it was a terrific piece of work. I read through the NUREGs. I tried to read through one, but they were very well written, very easy to understand, and very thorough.

It wasn't presented today, but I looked at those equations 5, 6, and 7, and the supplementary correlations that go with them, and some of these things are really-- five significant figures using very complex correlations and I just urge the staff to really double check that there aren't errors in some of these numbers.

(Laughter.)

MEMBER ARMIJO: I would never trust that

these were done right. But, all in all, I think it was a great presentation and a good piece of work.

MEMBER STETKAR: I don't have anything to add. I learned a lot. Thanks.

MEMBER BLEY: I liked it a lot. I appreciated the presentations very much and the breadth of knowledge that was displayed, and the answers.

I am a little disappointed on the side of it sounds like the things that concern me have been thought about and maybe done, but the trail wasn't completely clear, and I think those three things that we talked about are kind of key to this hanging together, the uncertainties, and some hydraulic model, that FAVOR properly integrates everything and treats the uncertainties, and that the generalization studies really are sufficient to generalize these three plant-specific PRAs to the fleet. And that's a hard thing to do, generally, with PRAs. That may be well justified, but it's important to see that.

DR. SHACK: Okay. Well, if that's the case, we're adjourned for the evening.

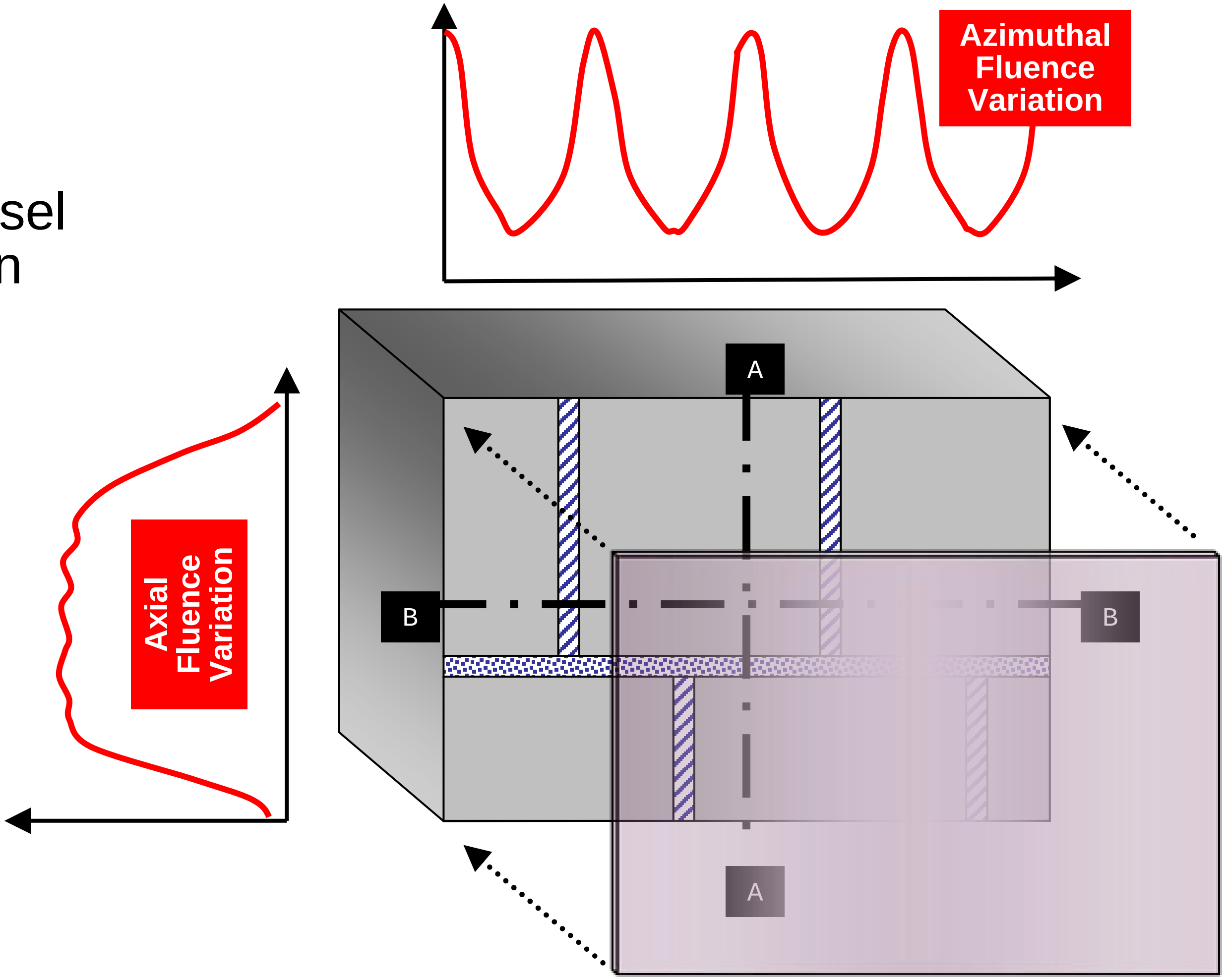
(Whereupon, the above-entitled matter was adjourned at 5:19 p.m.)

Baseline Results

Material Factors Controlling TWCF

- Distribution of
  - Flaws
  - Toughness

  varies widely thru vessel because of variation in
  - Fluence
  - Composition
- Fracture toughness at the flaw location(s) is needed to accurately correlate failure probability



Baseline Results

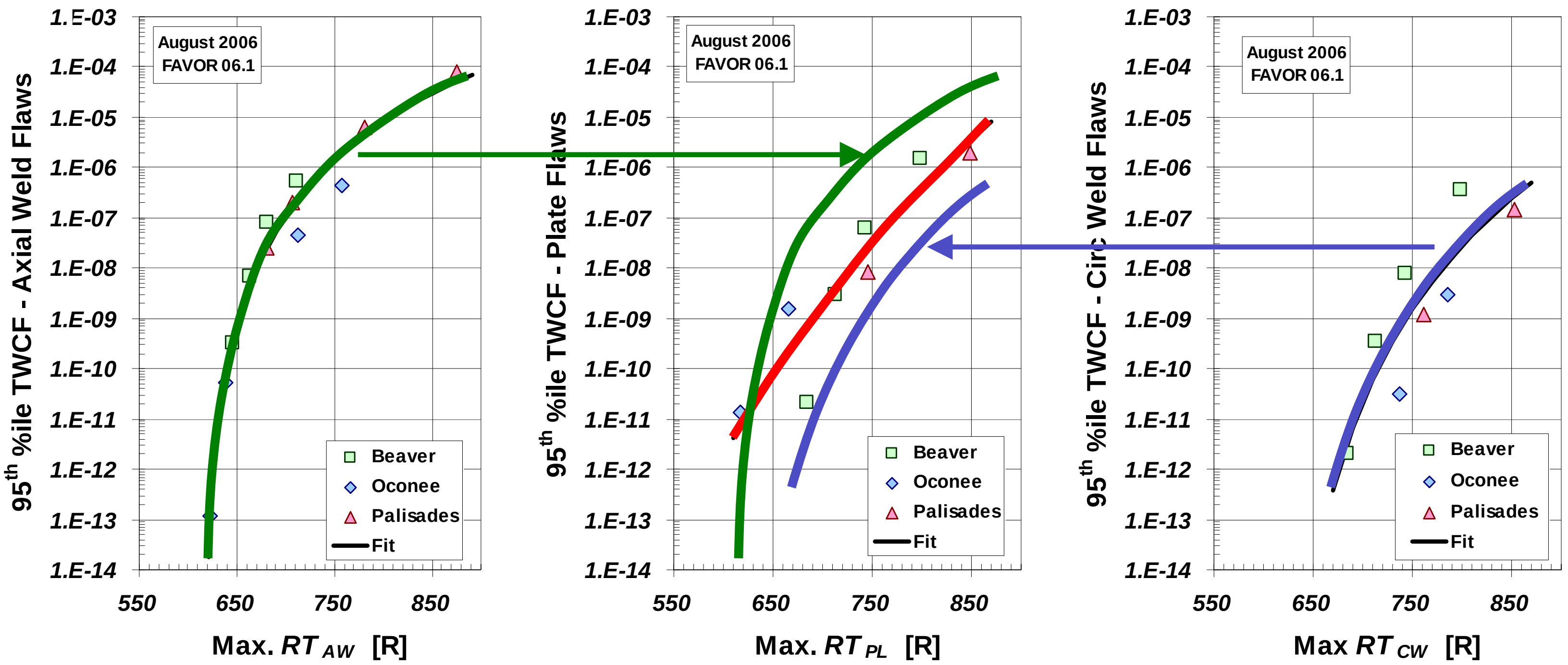
Reference Temperatures

RTs reflect the toughness at the locations of the different flaw populations

Metric	Flaw Location	Description	Depends on Properties of
$RT_{MAX\text{-}AW}$	on fusion line of axial welds	Max irradiated RT_{NDT} along axial weld fusion lines	Axial welds Plates
$RT_{MAX\text{-}CW}$	on fusion line of circ welds	Max irradiated RT_{NDT} along circ weld fusion lines	Circ welds Plates
$RT_{MAX\text{-}PL}$	in plates remote from welds	Max irradiated RT_{NDT} in any plate	Plates
$RT_{MAX\text{-}FO}$	In forgings remote from welds	Max irradiated RT_{NDT} in any forging	Forgings

Baseline Results

Effect of Flaw Distribution on TWCF



RT_{MAX-AW}

Maximum reference temperature along an axial weld seam

RT_{MAX-PL}

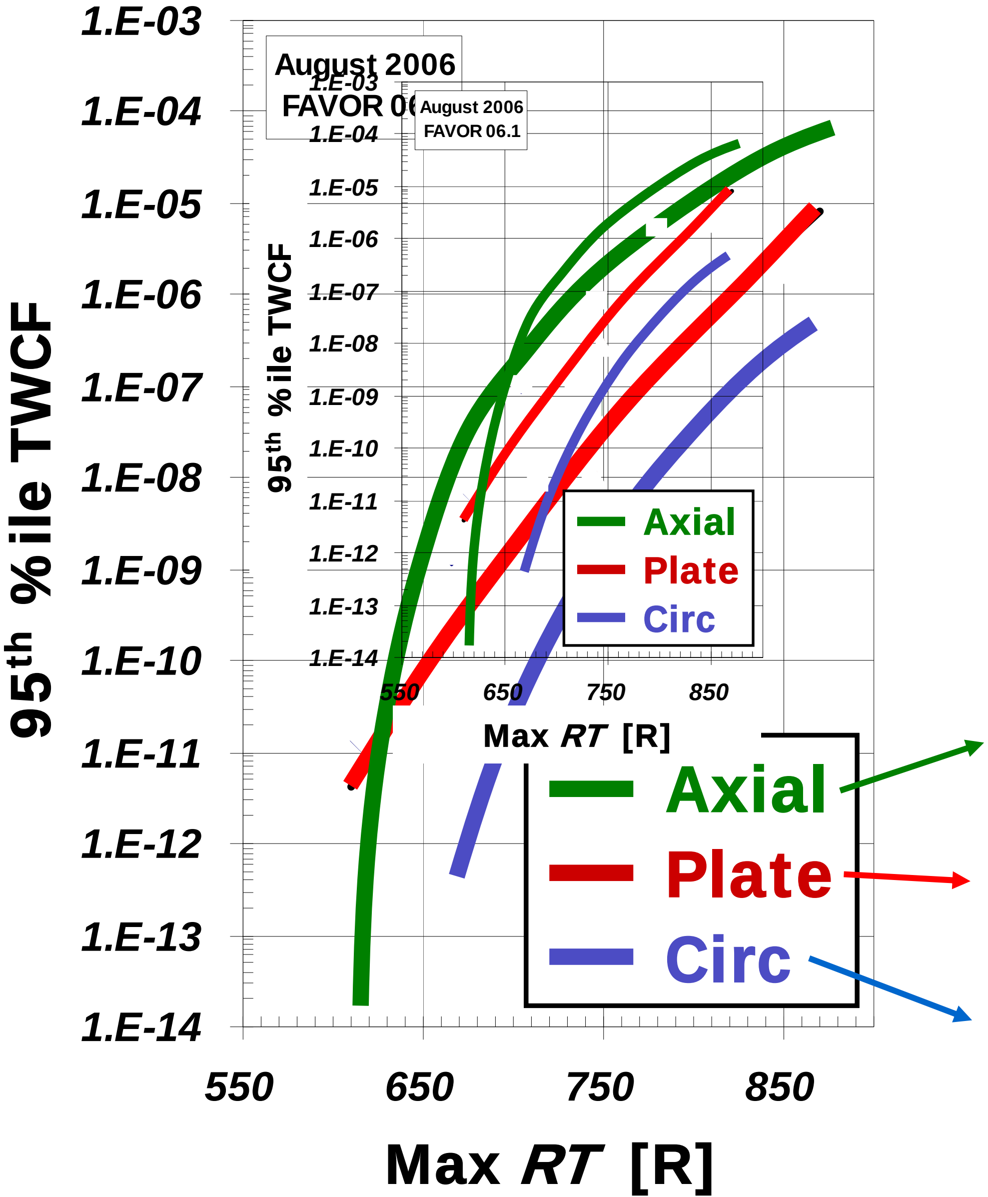
Maximum reference temperature within a plate

RT_{MAX-CW}

Maximum reference temperature along a circ weld seam

Baseline Results

Effect of Flaw Distribution on TWCF



Dominant

Minor

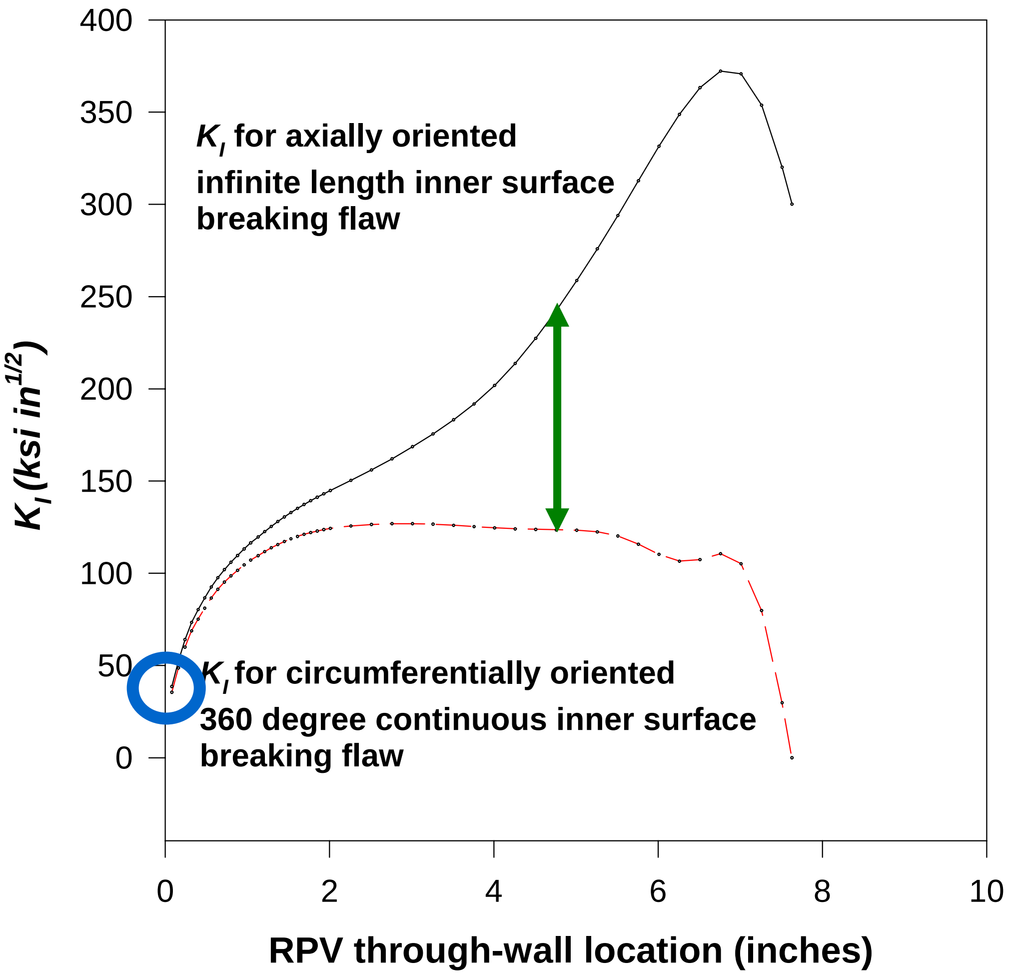
Negligible

Baseline Results

Flaw Orientation Effects

- ○ Axial and circumferential flaws have identical driving force to crack initiation

- ↕ Through-wall driving force variation makes axial flaws much more likely to fail the vessel than circumferential flaws



Baseline Results

Transients Controlling TWCF

🟧 **Dominant**	🟨 **Minor**	🟩 **Negligible**

Primary System Faults

- Pipe breaks
  - 🟧 Large
  - 🟧 Medium
  - 🟩 Small
- 🟧 Stuck open valves that later re-close
- 🟩 Feed and bleed

Secondary System Faults

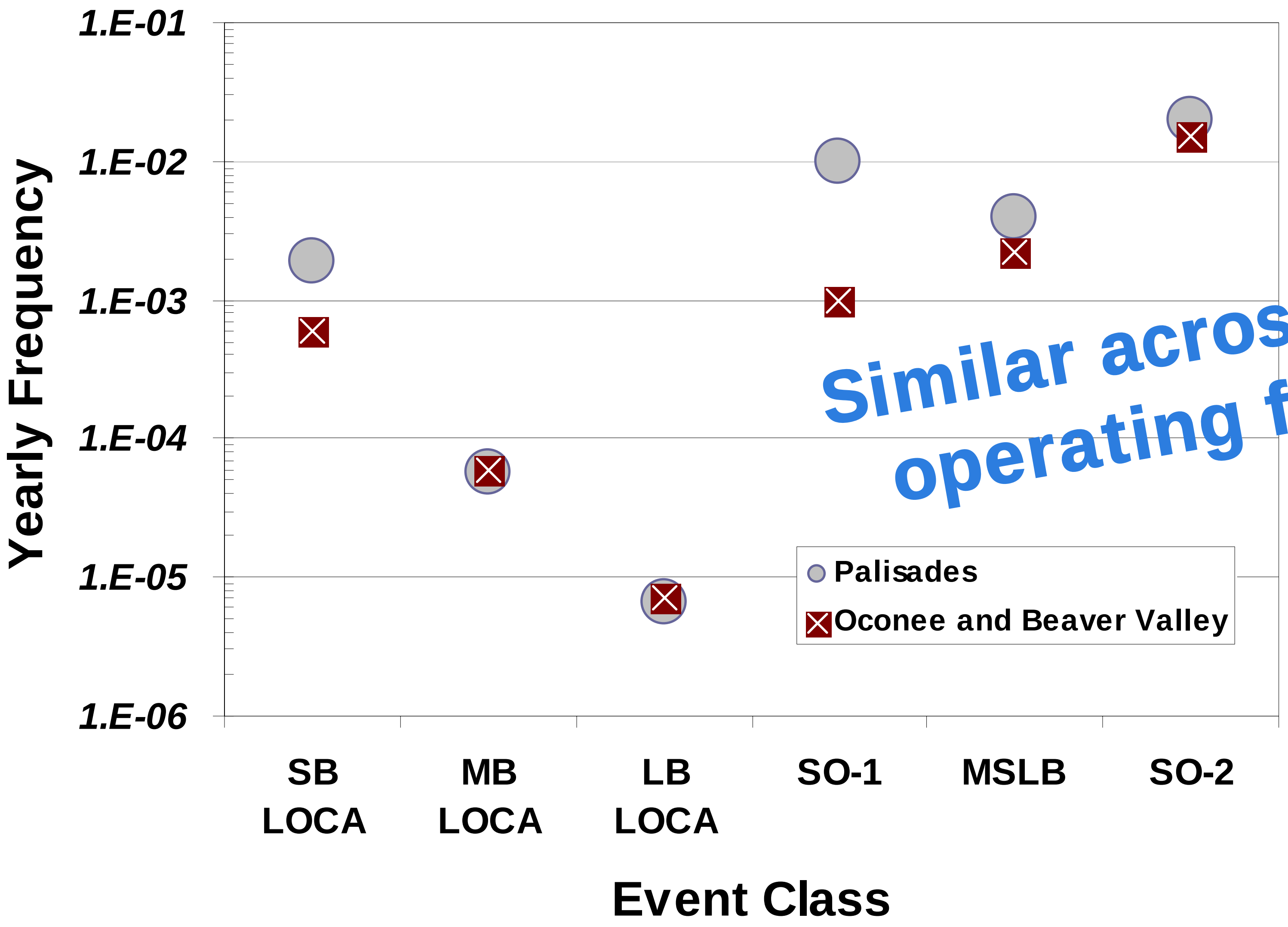
- 🟨 Main steam line break
- 🟨 Stuck open valves
- 🟩 Steam generator tube rupture
- 🟩 Pure overfeed

Importance depends on

- **Frequency of occurrence**
- **Severity if transient occurs**

Baseline Results

Transients Class Frequencies



Similar across the operating fleet

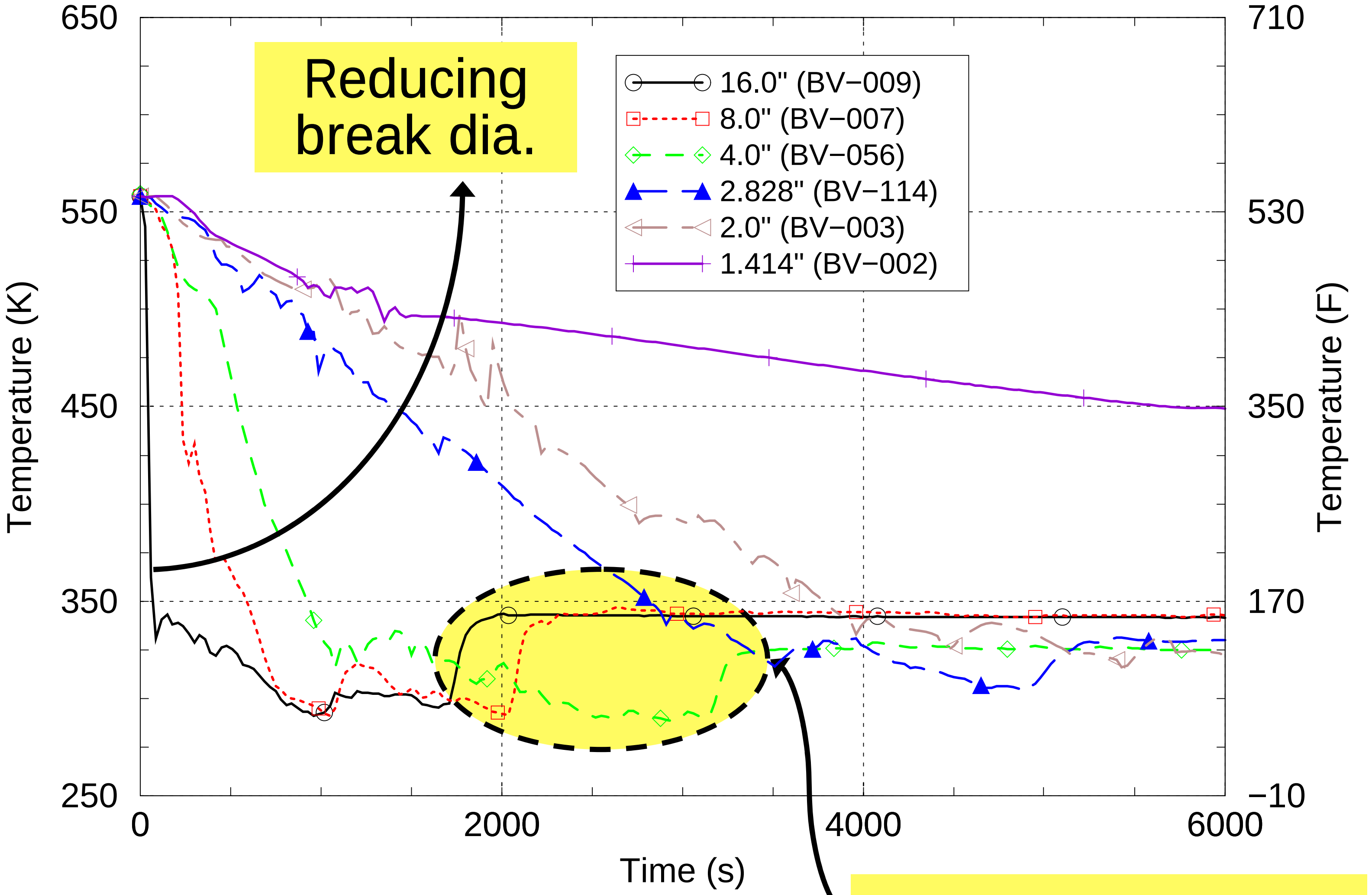
Baseline Results

Primary Side Pipe Breaks

- 2 cooling mechanisms
  - – Rapid depressurization causes rapid temperature drop
  - – Injection of colder ECC water
- No operator actions
  - – SI flow cannot compensate for diameters of ~2-in. and above
- Examine effect of … on plant response
  - – Break diameter
  - – Break location
  - – Season of the year

Baseline Results

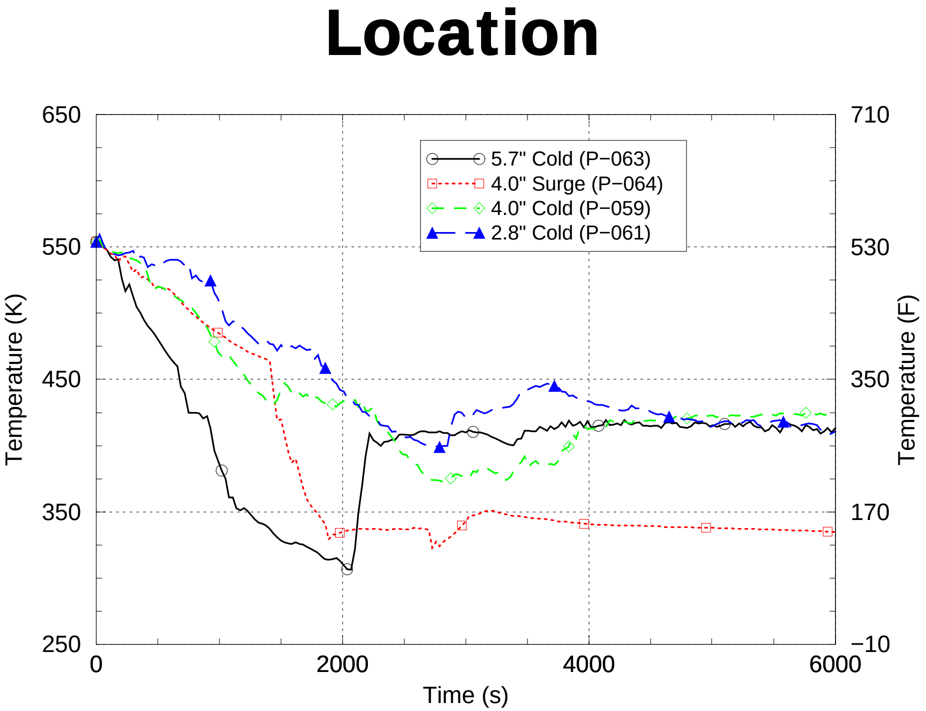
Break Diameter Effects

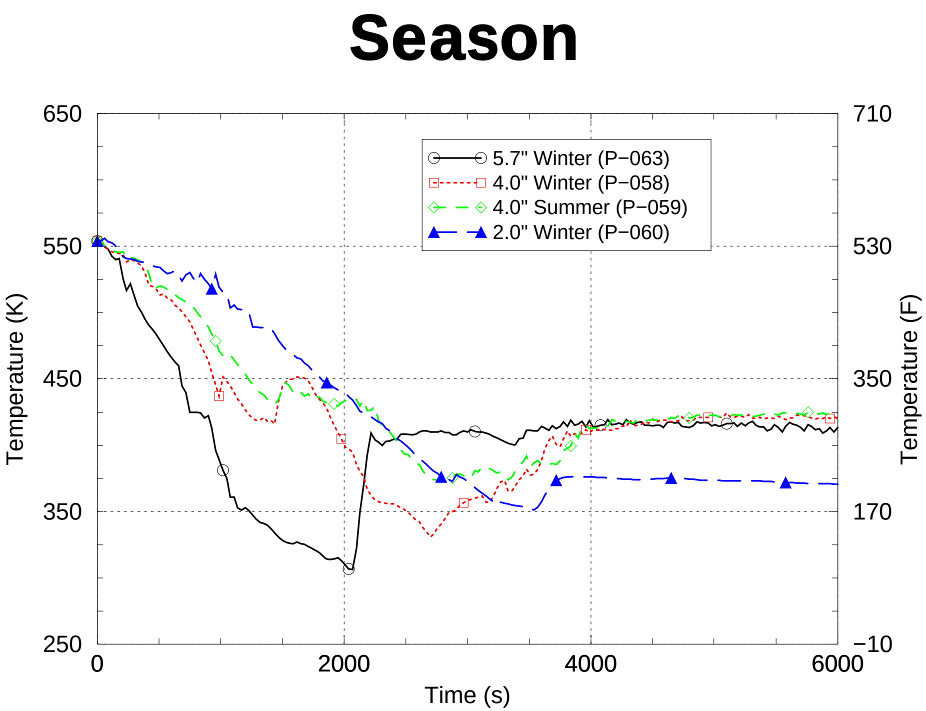


Switchover to sump

Baseline Results

Break Location and Seasonal Effects



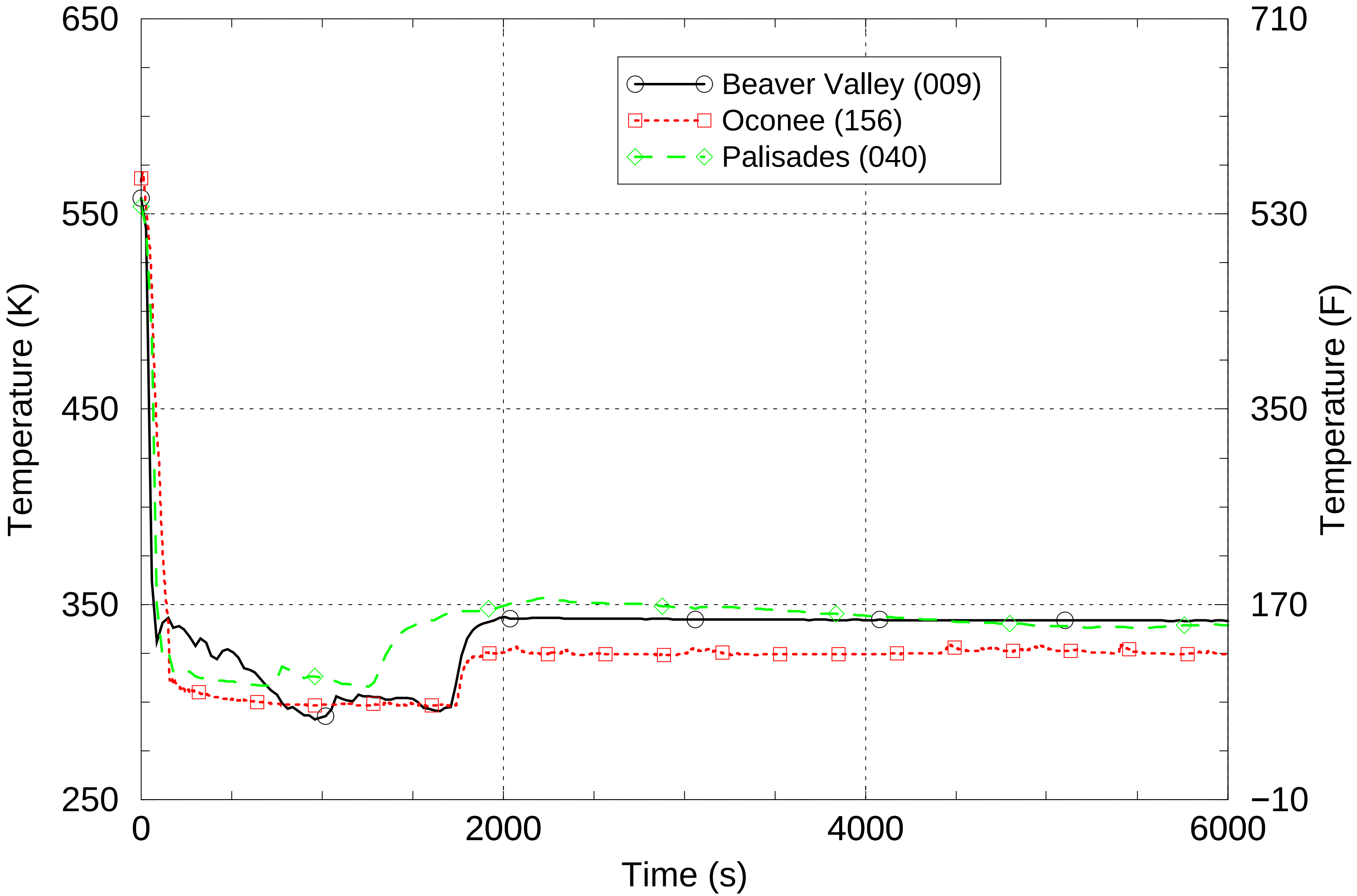


Cold line breaks and breaks in the Summer somewhat less severe, but are not out of break size order

Break Size is the Dominant Factor Controlling Transient Severity

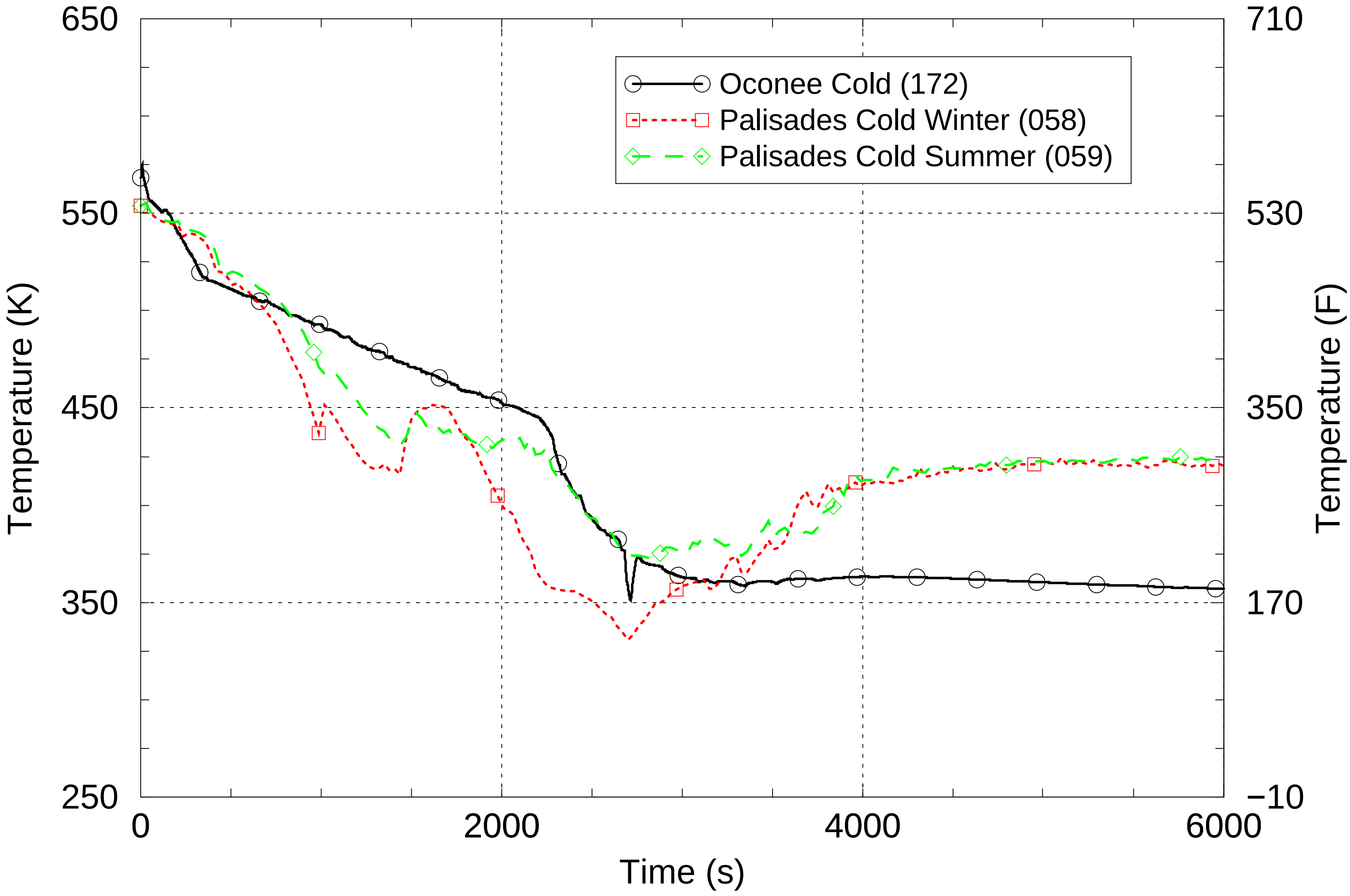
Baseline Results

Break - Plant Comparison: 16” Hot Leg



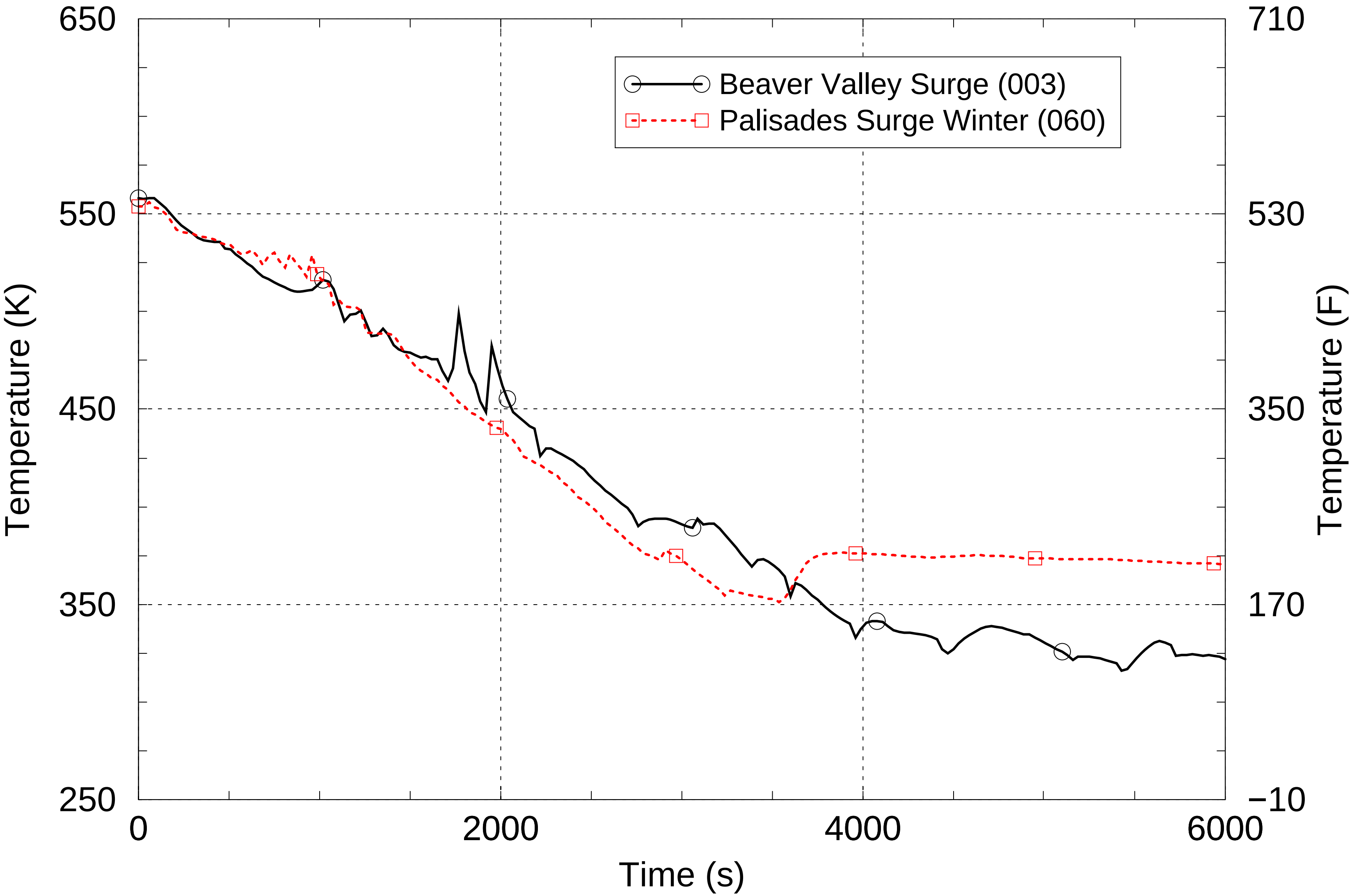
Baseline Results

Break - Plant Comparison: 4” Cold Leg



Baseline Results

Break - Plant Comparison: 2” Surge Line



Baseline Results

Primary Side Pipe Breaks

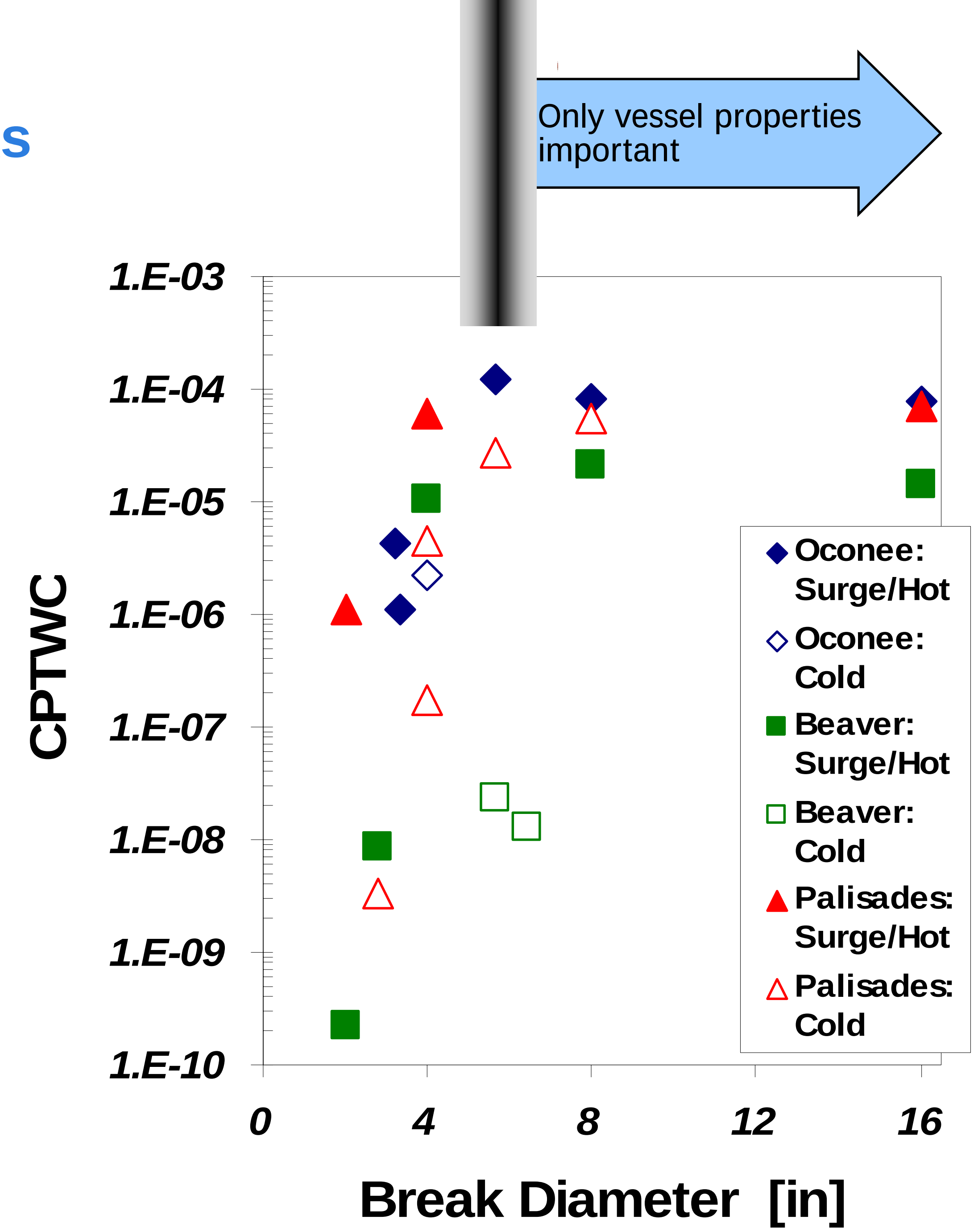
- **Larger diameter breaks pose consistent challenge from plant to plant**

  Steel vessel cannot cool as rapidly as depressurizing water

  “Conduction controlled”

  Thermal stresses controlled by thermal conductivity and vessel thickness only

  Details of transient unimportant



Only vessel properties important

Baseline Results

Primary Side Pipe Breaks

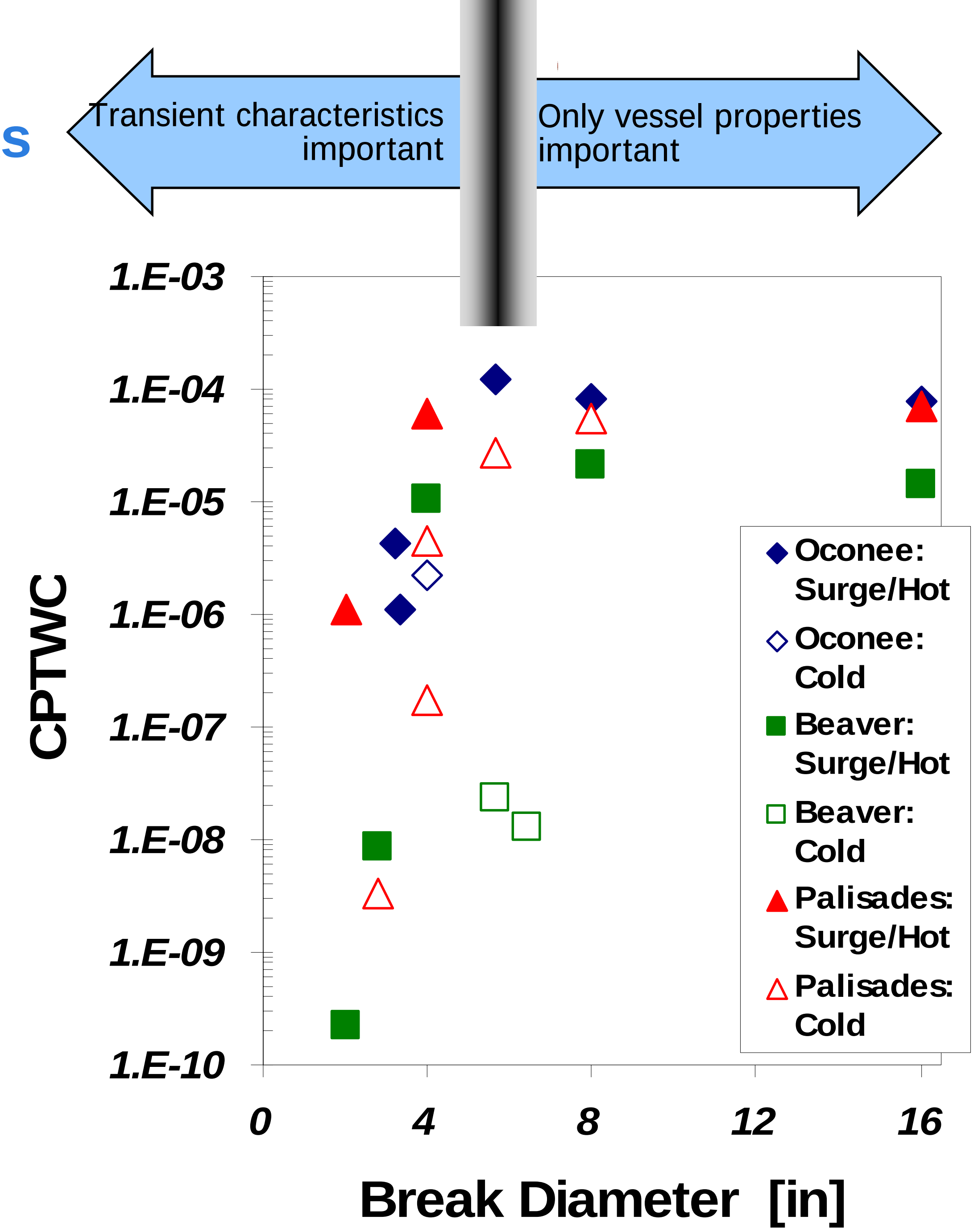
- **Smaller diameter breaks**

  Steel vessel can cool as rapidly as depressurizing water

  Thermal stresses influenced by RCS cooling rate

  Details of transient important

  CPTWC much lower than for larger diameter breaks



Transient characteristics important

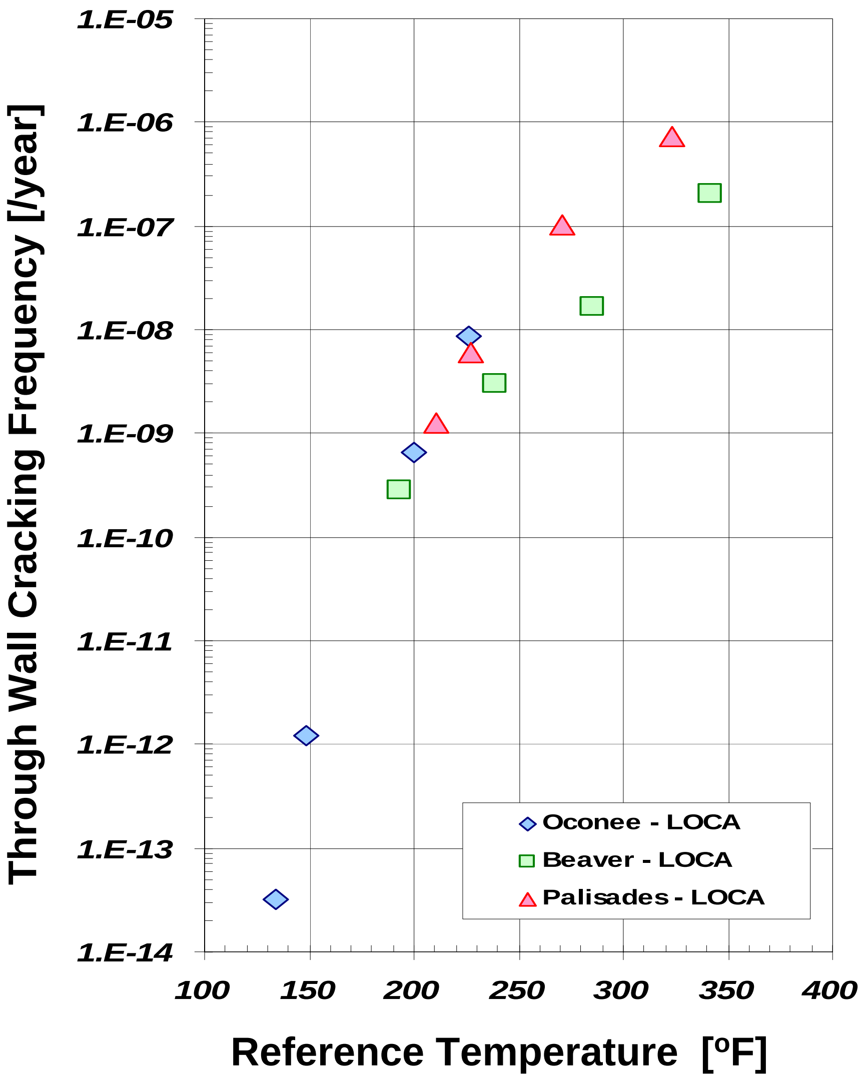
Only vessel properties important

Baseline Results

Primary Side Pipe Breaks

- Factors suggesting applicability of these results to PWRs in general
  - No influence of operator action
    - Failures occur very early in transient (< 20 min)
    - Operators must keep core covered
  - Large diameter pipe breaks (5" and above) dominate TWCF (70%)
  - 4" pipe breaks contribute the rest
  - < 4" diameter breaks contribution is negligible

Transients that dominate pipe break TWCF are the least influenced by plant-specific factors.



Baseline Results

Stuck-Open Primary Valves

- Begins with demand (real or false) on one or more SRVs
  - Open SRV depressurizes primary (rate equivalent to ~2” dia. pipe break)
  - ECC accelerates cooling by direct injection of cold water
  - Valve re-closes at a later time
  - Continued SI begins to refill the primary
    - Throttling criteria usually not satisfied because pressurizer level is low
  - Once pressurizer is full
    - Throttling criteria should be met
    - System will rapidly re-pressurize unless the operator throttles SI quickly

- Significant factors
  - Timing of valve reclosure
  - Power level at transient initiation
  - Timing of operator action to throttle charging

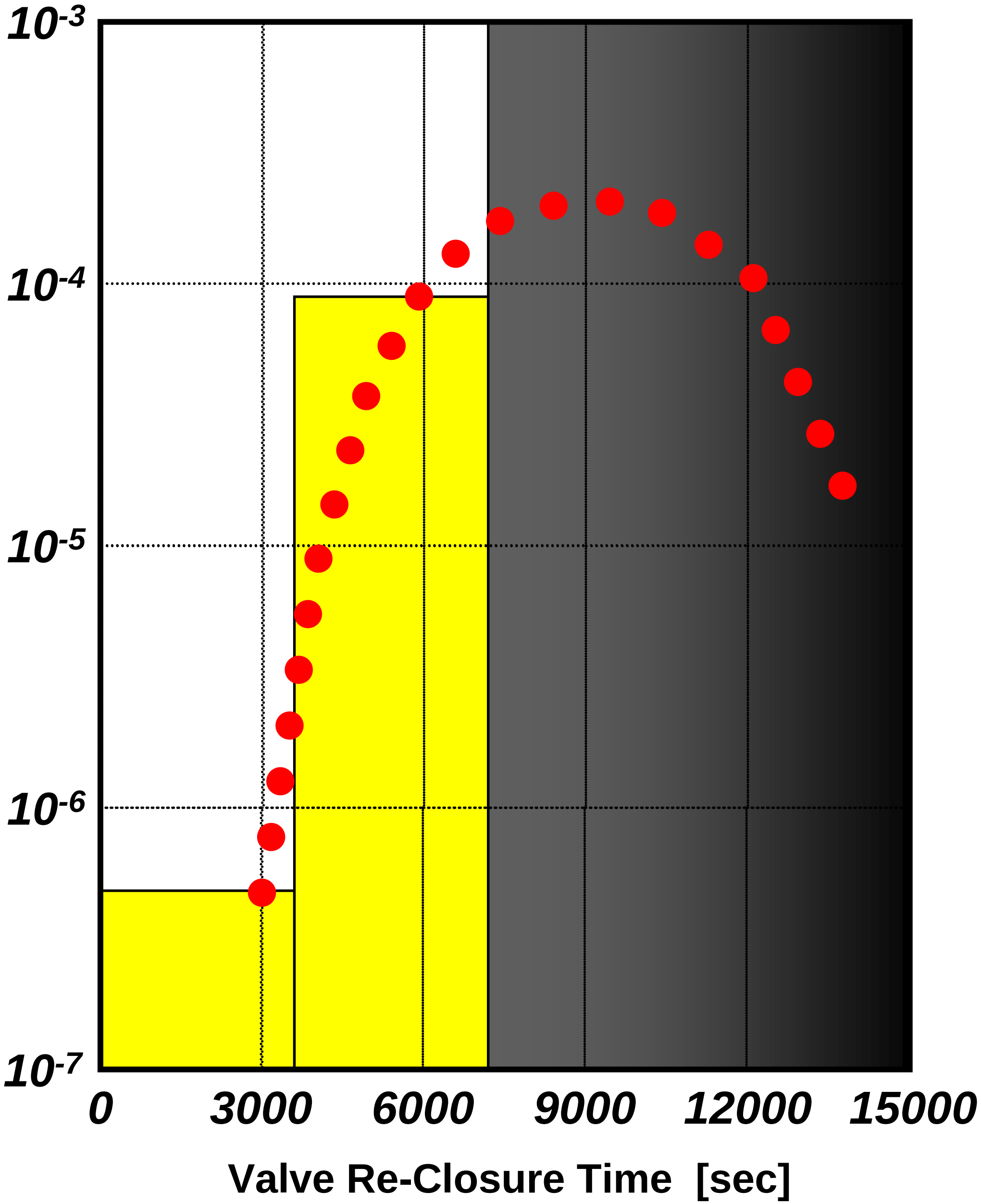
Baseline Results

Stuck-Open Primary Valves, Effect of Valve Reclosure Time

- Valve can re-close at any time after the transient begins
- Competing effects of thermal stress and minimum temperature at the time of re-pressurization produce a peak in the CPTWC
- After ~2hr (7200 sec) operators would initiate new procedures, changing the transient
- All valve re-closures < 2 hours discretized into 2 times:

  3000 seconds

  6000 seconds



Baseline Results

Stuck-Open Primary Valves Transients and Operator Actions

- Power level
  - Thermal shock more severe under HZP
    - vessel is not yet iso-thermal
- Timing of operator actions
  - Considered action at 1 and 10 minutes after throttling criteria were met
    - Throttling after 10 minutes never stops re-pressurization
    - Throttling after 1 minute

Stops re-pressurization under HZP (More effective under HZP due to lower system energy level)

Only delays re-pressurization under full power

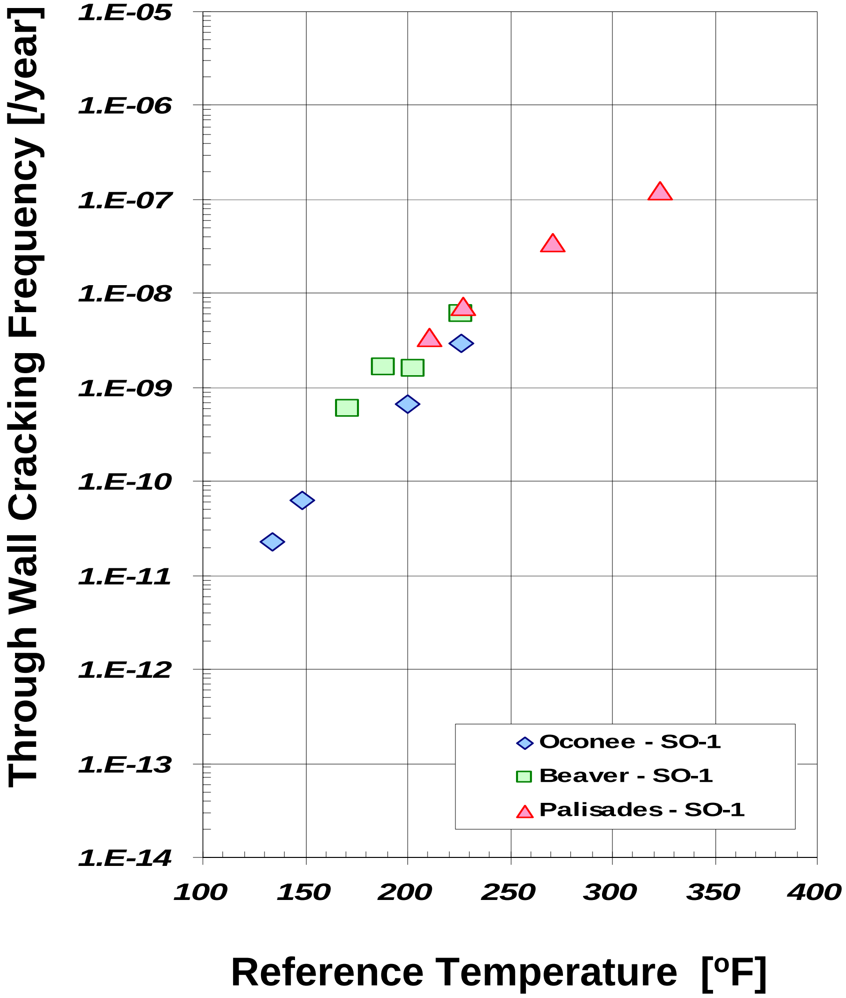
- Effect of operator action credit is minimal
  - “credited” with 1 minute throttling
    - <u>Oconee</u>: 68% of the time
    - <u>Beaver</u>: 40% of the time
    - <u>Palisades</u>: 0% of the time
  - Throttling only prevents re-pressurization at HZP
  - HZP accounts for only 20% of the transients

Baseline Results

Stuck-Open Primary Valves – Summary

- Factors suggesting applicability of these results to PWRs in general
  - Re-pressurization is a dominant factor influencing the transient severity
  - All PWRs have similar safety valve set-points
  - While reasonable and appropriate operator actions have been credited, the physical factors that control the severity of these transients limit the effect of these credits on the TWCF

Transient severity driven by system characteristics. Influence of operator action is small.



Baseline Results

Main Steam Line Breaks

- Rapid de-pressurization of affected generator through large (multiple ft^2) hole
  - Causes rapid temperature drop in the affected generator to the boiling point of water at the break location

- Temperature in the primary tracks that in the affected generator due to the large heat transfer area of the steam generator tubes
  - Rapid cooling shrinks the primary inventory, depressurizes the primary
  - Very rapid cooling

Baseline Results

Main Steam Line Breaks – Model

- Models include delayed operator actions
  - Allowing feed to the faulted generator for 30 minutes, or indefinitely
  - Throttling of HPI 30 or 60 minutes after allowed
- Models include exacerbating equipment failures
  - MSIVs fail to close
- Models include physically unrealistic minimum temperatures
  - Pressure buildup inside containment not modeled, so minimum temperatures are ~40^{o}F too low

Conservative treatment motivated by scoping calculations showing MSLB contributions small relative to LOCA and SO-1

Baseline Results

Main Steam Line Breaks

- Cooling rate is very rapid in the primary system – conduction limited conditions

- Failures, if they occur, happen between 10 and 15 minutes. Failures occur before any
  - Operator action credits
  - Effect of power level
  - Effect of break location

  become important to T, P, and h *vs*. t

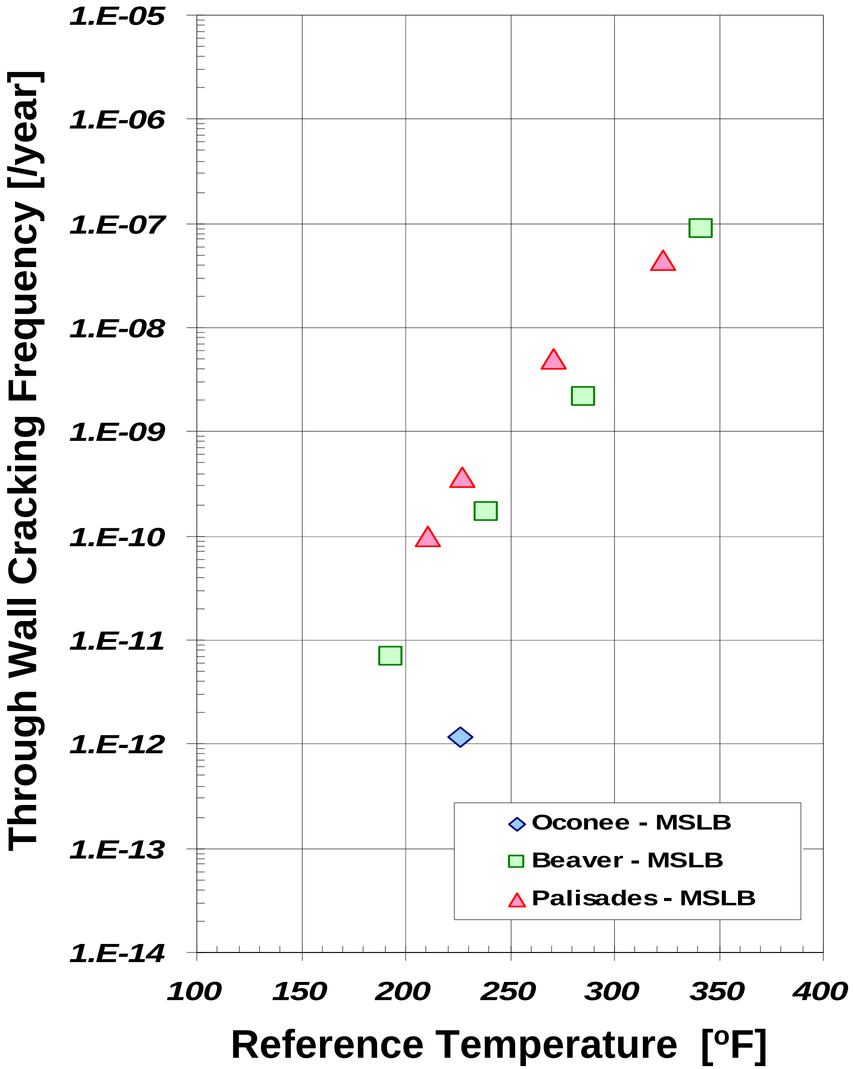
- Perceived dominance of MSLB in transients occurred in 1980s analysis because
  - Primary temperature allowed to fall below 212°F
  - LB LOCAs not modeled

Baseline Results

Main Steam Line Breaks

- Factors suggesting applicability of these results to PWRs in general
  - Intentionally conservative modeling
  - No effect of operator action credits
  - The rapid cool-down that controls vessel failure probability is in the conduction limited regime, mitigating plant-specific factors.

Big breaks …
Intentional conservatisms …
Failure probability still low!



Baseline Results

Other Transients

- Stuck open valves on the secondary side
- Pure overfeed
- Feed and bleed
- Steam generator tube rupture
- Mixed failure in primary and secondary system

- In all cases
  - Low probability of occurrence and
  - Low consequence

  combine to make the contributions of transients in these classes to TWCF
  - Negligible, or
  - Zero

Baseline Results

Effect of Transient Type on TWCF

- Primary side failures dominate risk (90% or more)

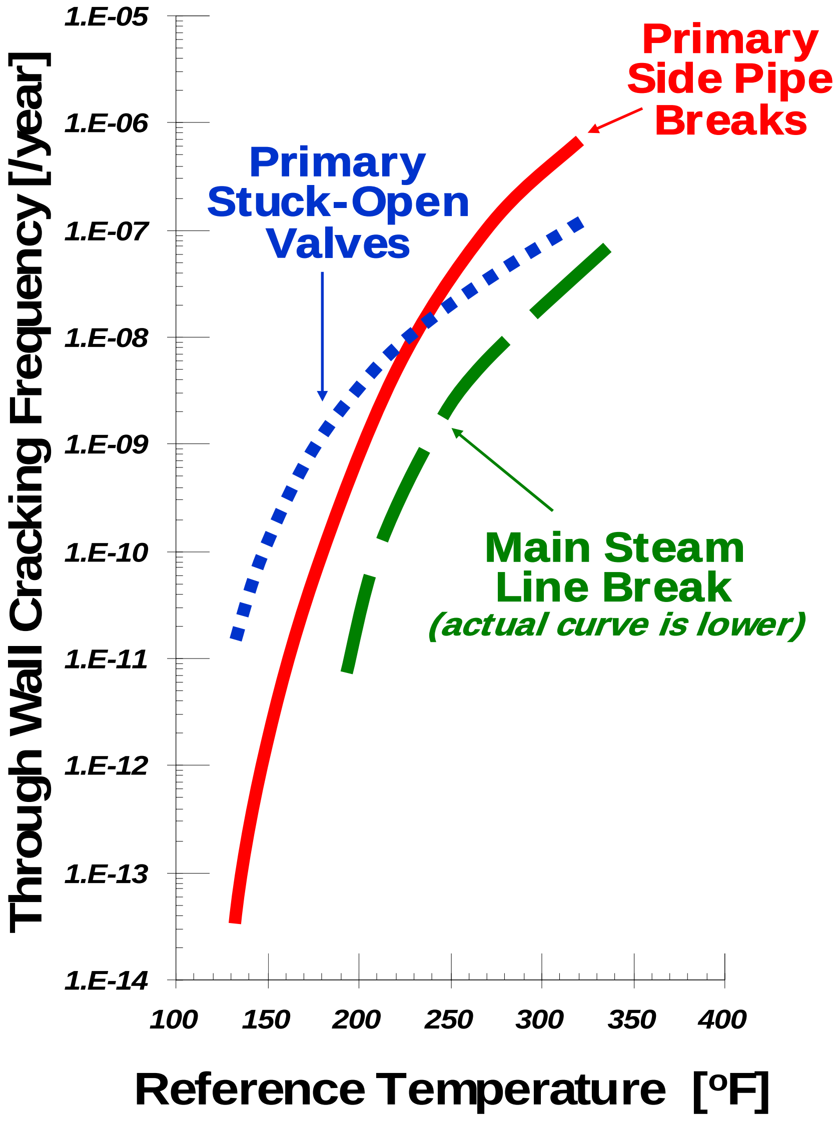
  Low embrittlement: stuck open valves that later re-close

  Higher embrittlement: medium and large diameter pipe breaks

- Secondary side failures

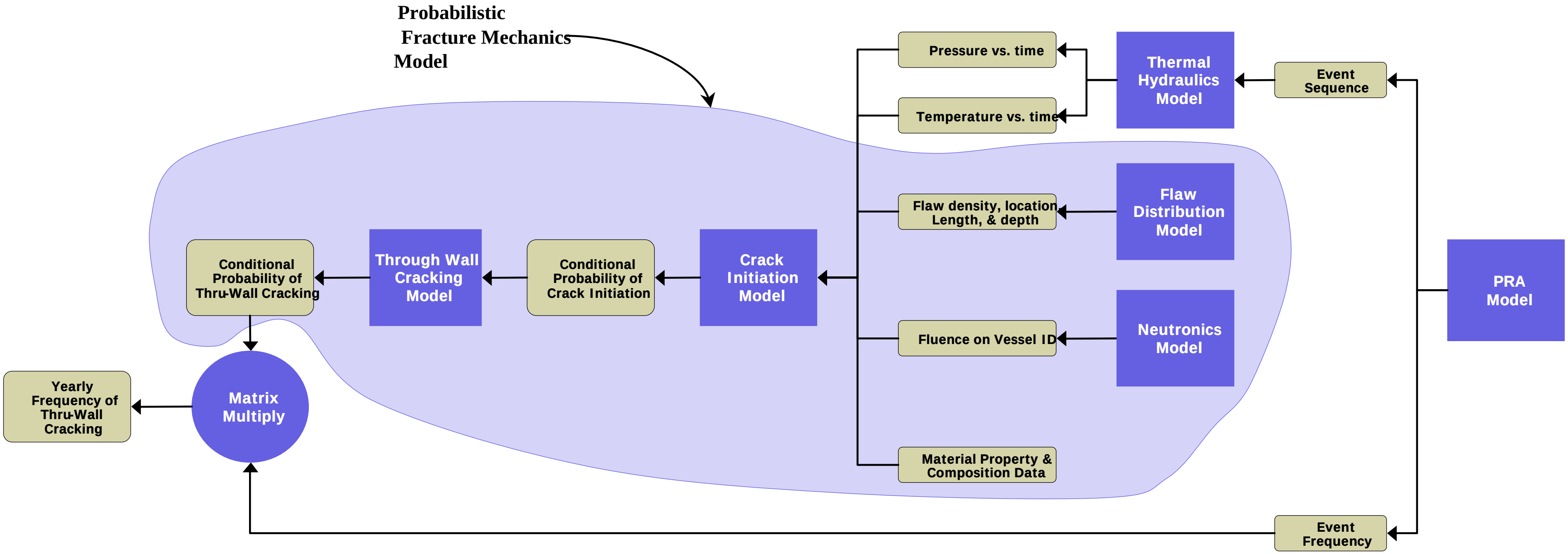
  Conservatively modeled main steam line breaks of much smaller consequence

  Actual contribution is ***less than*** estimated by our models



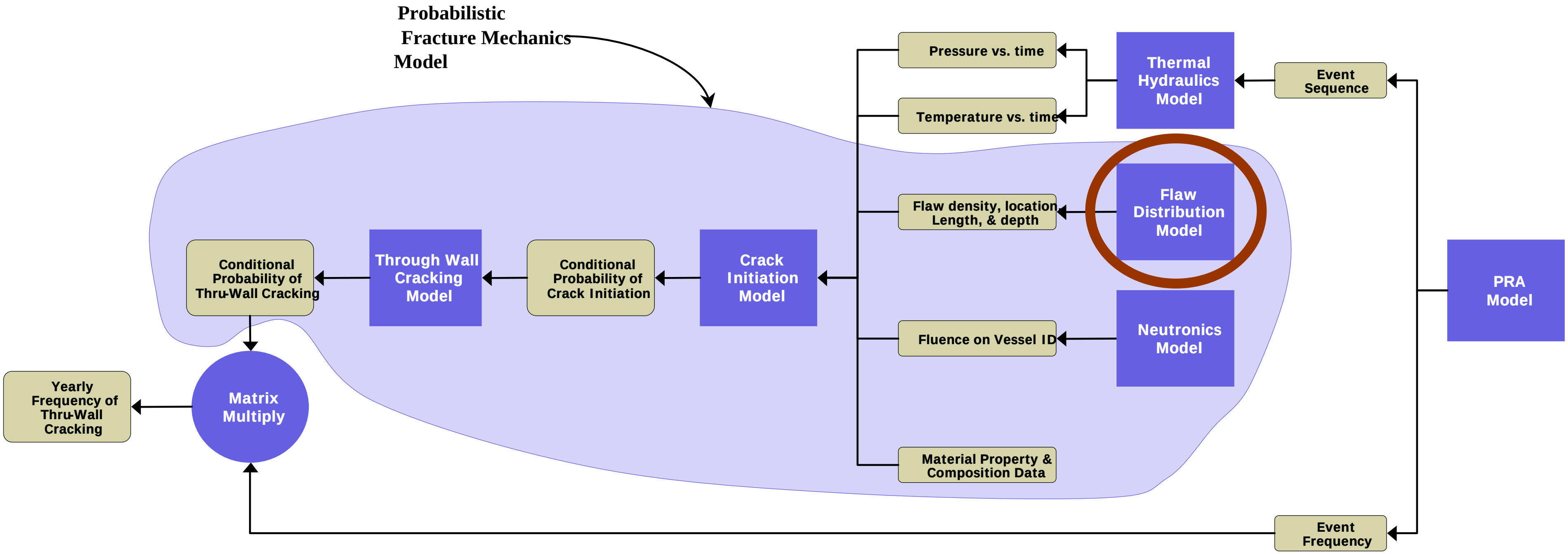
Probabilistic Fracture Mechanics

Model Details



Probabilistic Fracture Mechanics

Model – Flaw Distribution



Probabilistic Fracture Mechanics Model

Pressure vs. time

Temperature vs. time

Thermal Hydraulics Model

Event Sequence

Flaw density, location Length, & depth

Flaw Distribution Model

PRA Model

Conditional Probability of Thru-Wall Cracking

Through Wall Cracking Model

Conditional Probability of Crack Initiation

Crack Initiation Model

Fluence on Vessel ID

Neutronics Model

Yearly Frequency of Thru-Wall Cracking

Matrix Multiply

Material Property & Composition Data

Event Frequency

- Relative to previous analysis
  - Many more flaws
  - Flaws generally smaller
  - Flaws buried rather than on surface
  - Weld and cladding flaws have orientations tied to welding direction
- Flaw distribution used viewed as appropriate / conservative representation of the flaws in any PWR
  - Support of physical models
  - Adoption of systematically conservative judgments in the face of uncertainty

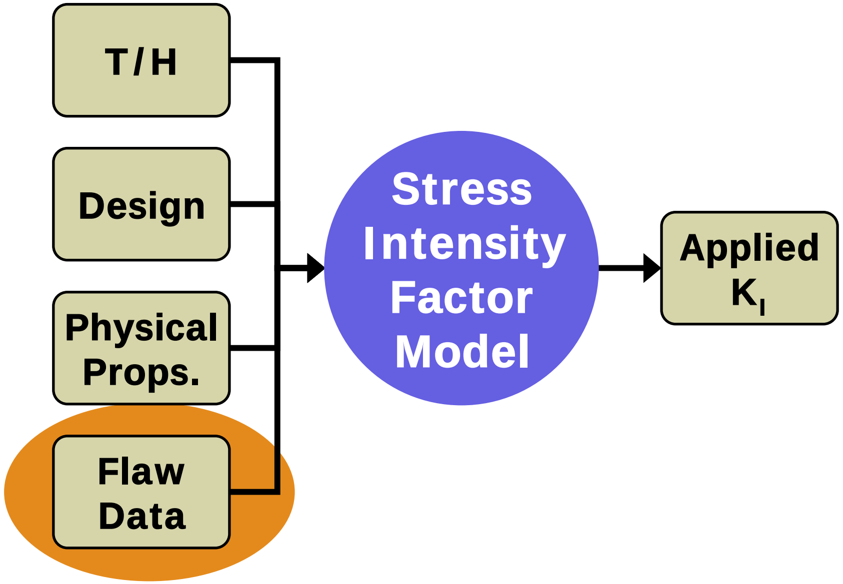
Probabilistic Fracture Mechanics

Model – Flaw Distribution

- Sources of data
  - Experimental
    - Destructive
    - Non-destructive
  - PRODIGAL model
  - Expert elicitation

- Developed distributions of flaws in
  - Fabrication welds
  - Repair welds
  - Cladding welds
  - Plate materials

- Each distribution includes
  - Flaw density
  - Flaw size
  - Flaw orientation
  - Flaw location



T/H

Design

Physical Props.

Flaw Data

Stress Intensity Factor Model

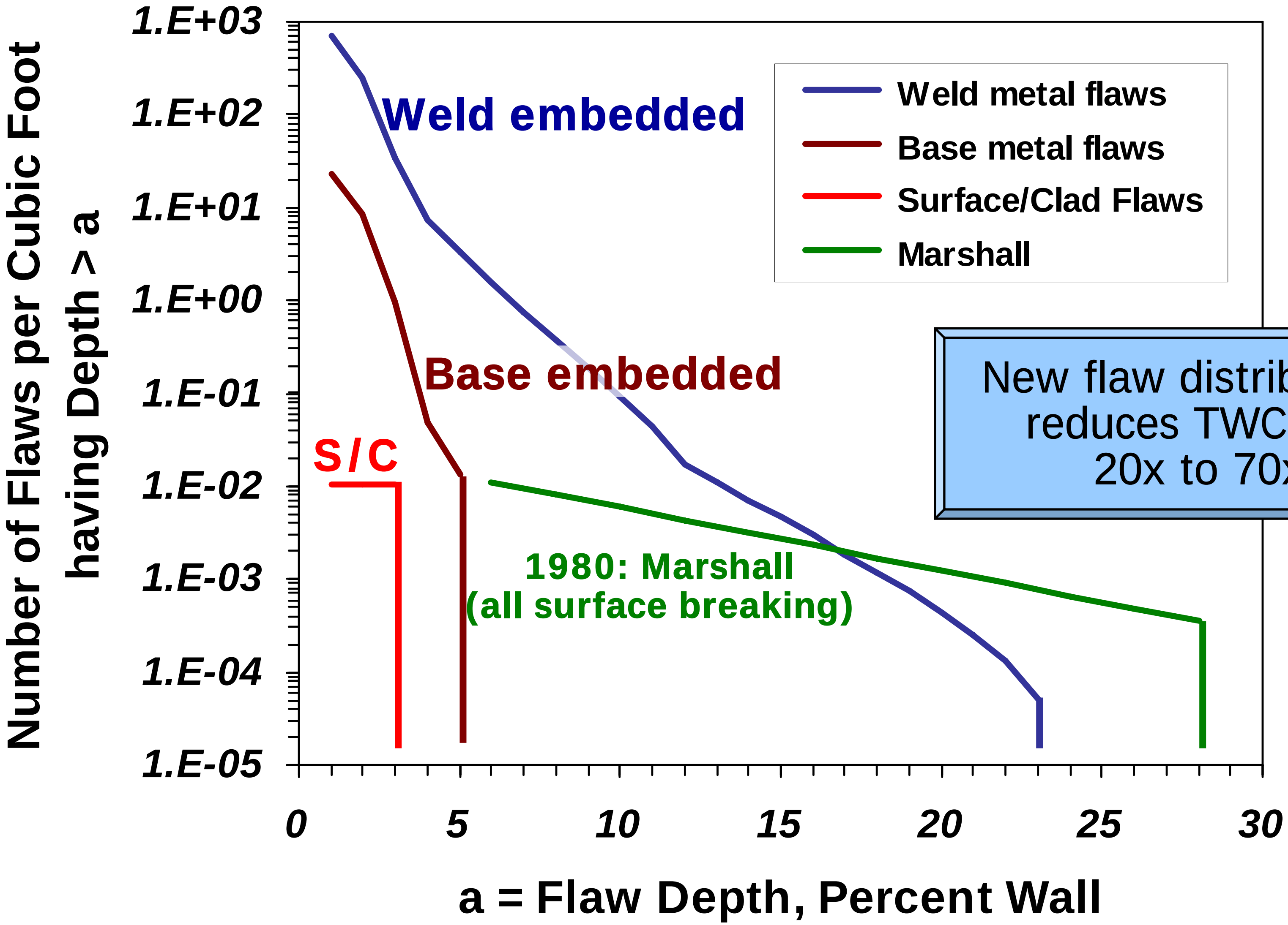
Applied K_I

Experimental Data Sources

	Weld	Plate	Clad
PVRUF	☑	☑	☑
Shoreham	☑	☑	
Hope Creek		☑	
River Bend		☑	

Probabilistic Fracture Mechanics

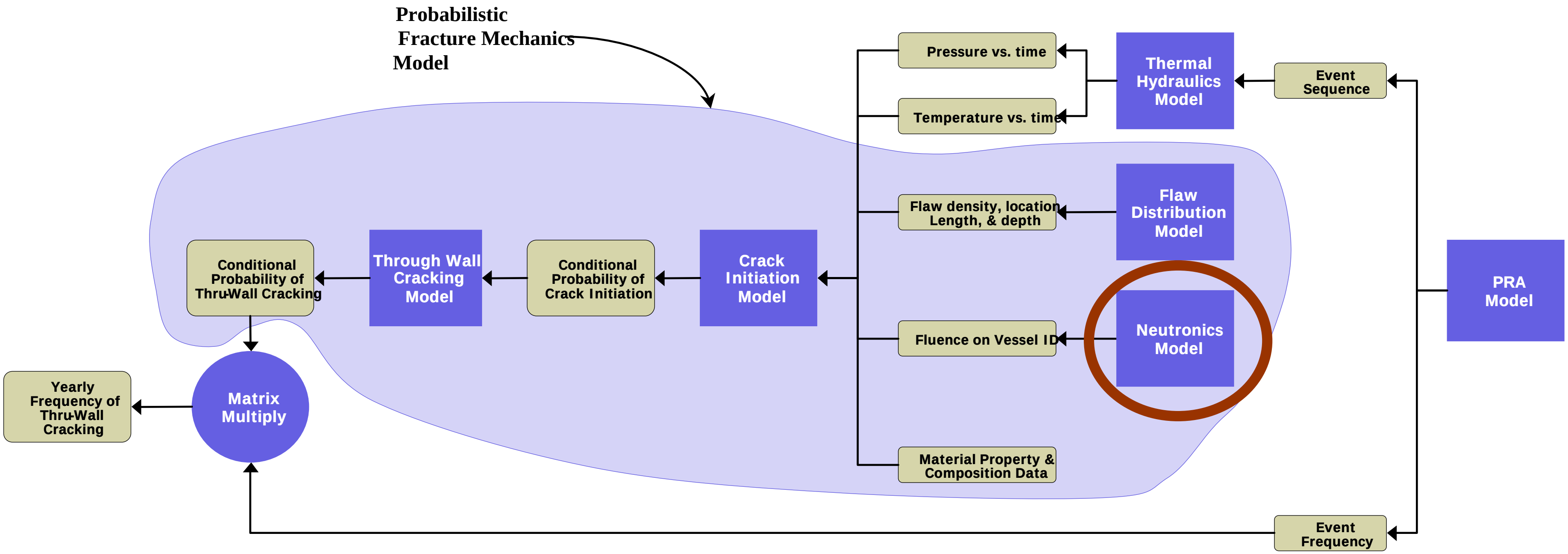
Model – Flaw Distribution



New flaw distribution reduces TWCF by 20x to 70x

Probabilistic Fracture Mechanics

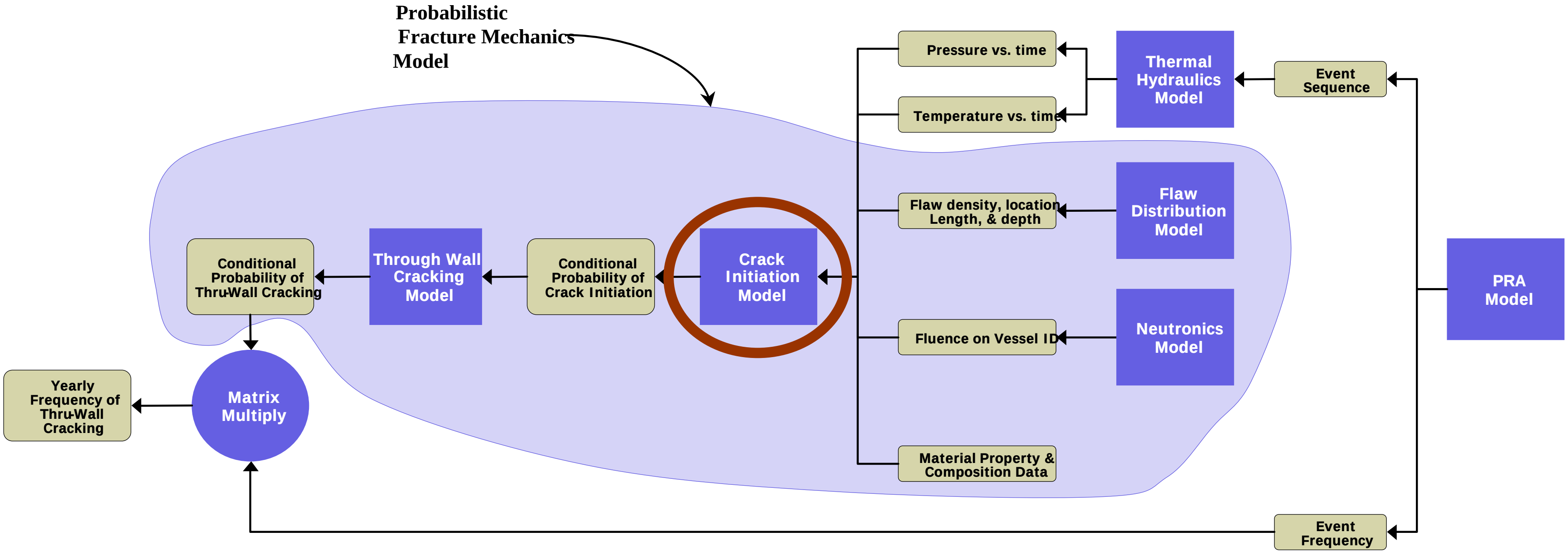
Model – Neutronics



- ID fluence estimated per Regulatory Guide 1.190 procedures
  - accounts for axial and azimuthal fluence variation
  - Much greater detail (less conservatism) than before
- Through-wall attenuation of radiation damage (fluence) still modeled conservatively using Regulatory Guide 1.99 procedures [EPRI MRP-65]

Probabilistic Fracture Mechanics

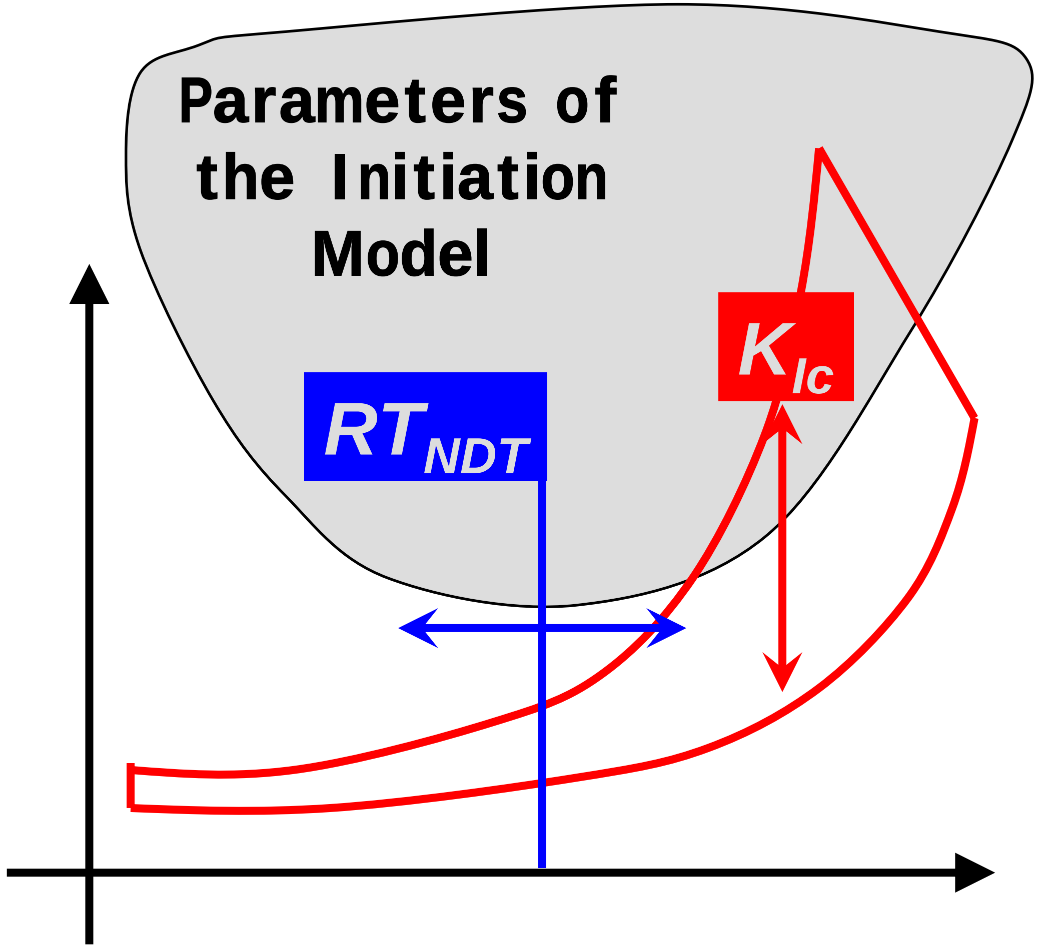
Model – Crack Initiation



- Material uncertainty modeled conservatively relative to plant-specific variability
- Conservative bias in RT_{NDT} removed, on average
- Aleatory uncertainty in initiation fracture resistance modeled
- Warm pre-stress effects accounted for
- Physically motivated irradiation shift model, converted to toughness shift

Probabilistic Fracture Mechanics

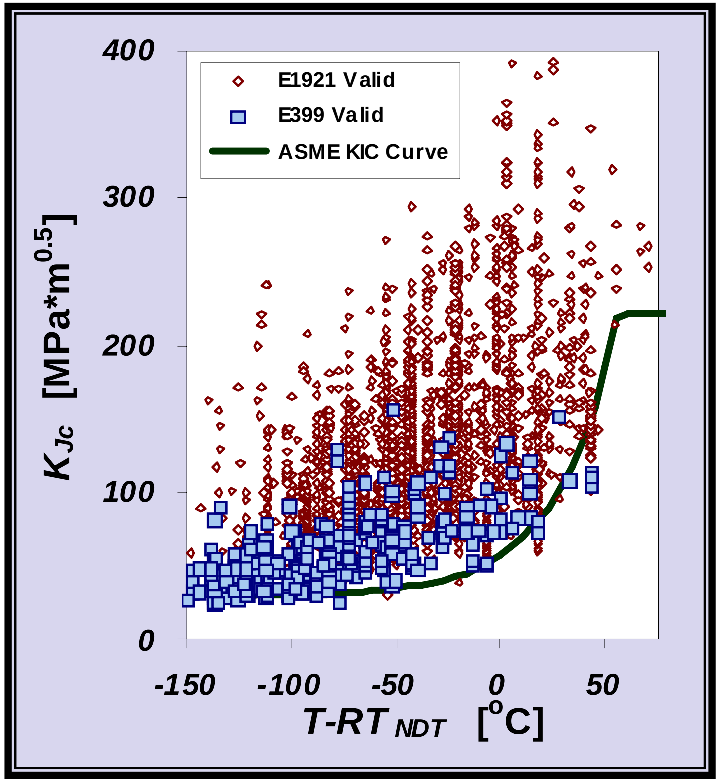
Model – Crack Initiation Fracture Toughness



Parameters of the Initiation Model

RT_{NDT}

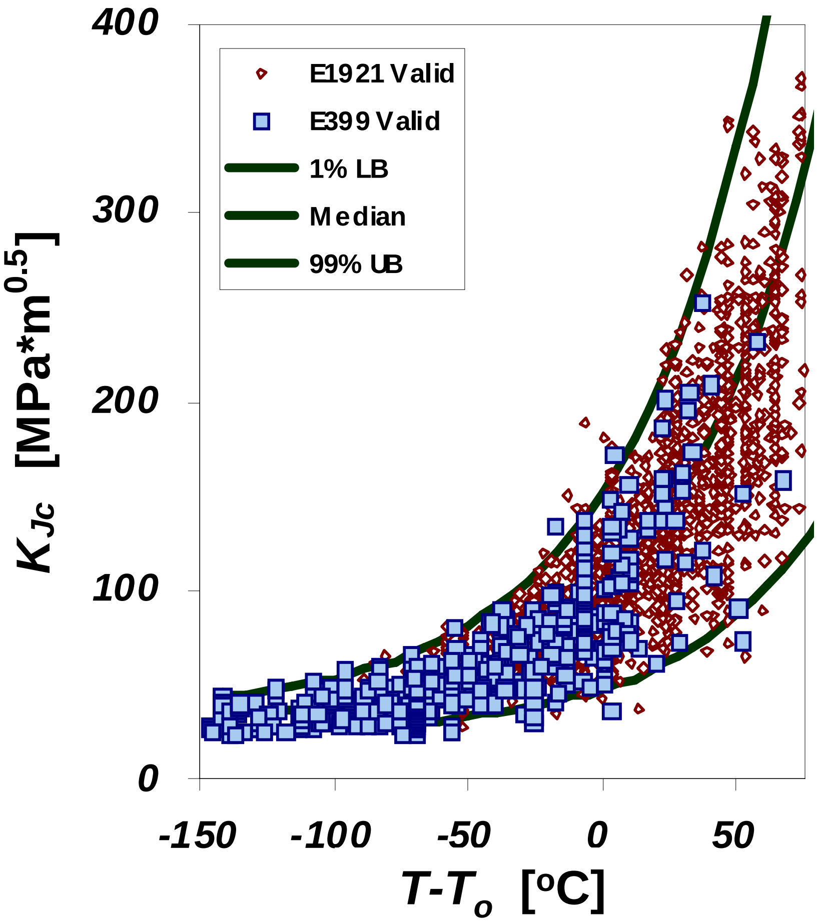
K_{Ic}



Probabilistic Fracture Mechanics

Model – Uncertainties

- Because of implicit conservative bias, fracture toughness models based on the RT_{NDT} index temperature contain a mix of
    - Epistemic uncertainty in RT_{NDT}, and
    - Aleatory uncertainty in K_{Ic}
- Use of the best-estimate Master Curve index temperature (T_o) effectively removes epistemic uncertainty, leaving only the aleatory uncertainties produced by material variability

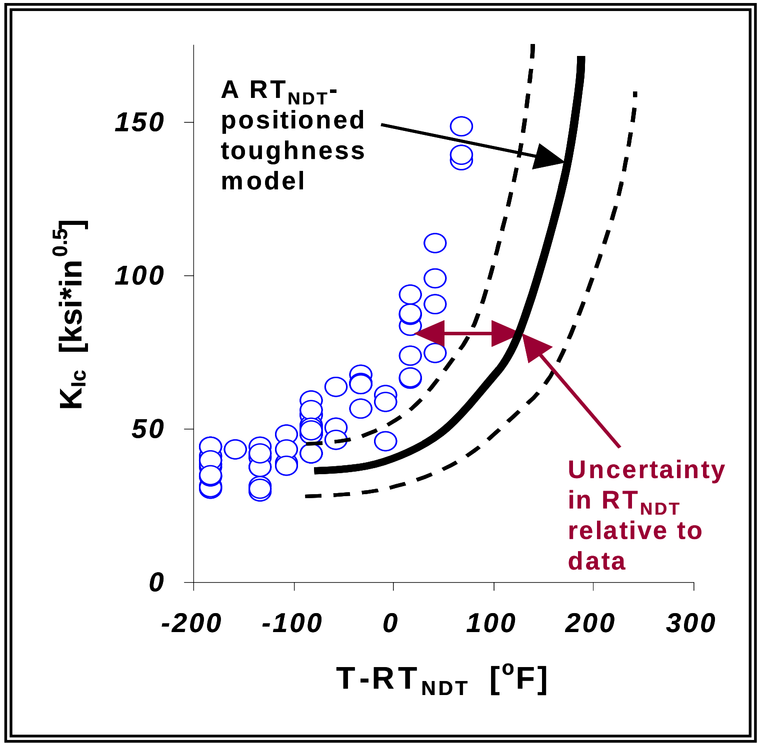


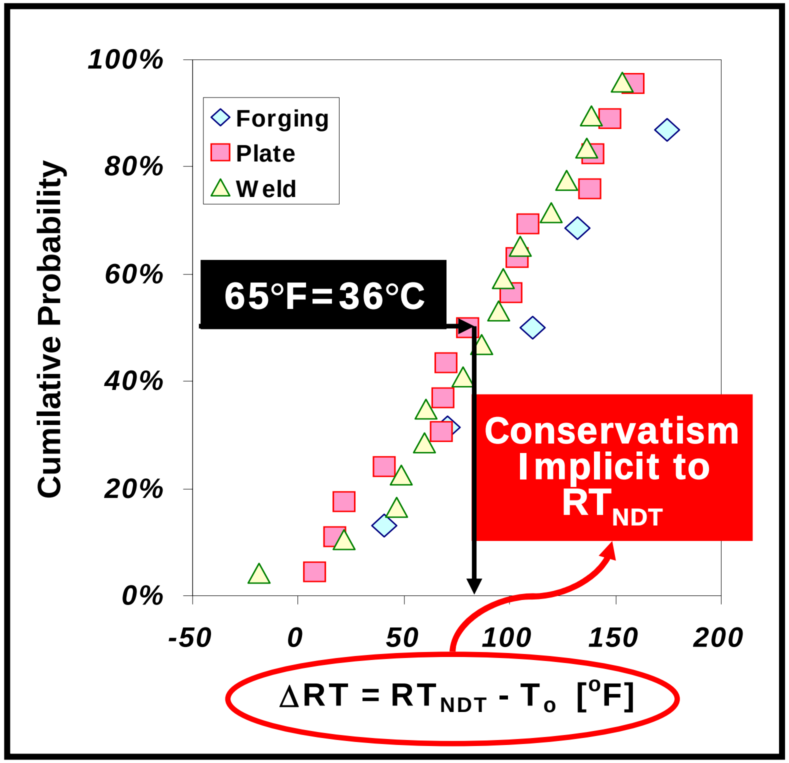
Probabilistic Fracture Mechanics

Model – Uncertainties

- Determine how far $RT_{NDT(U)}$ is from an accurate representation of measured toughness data
- T_o best represents the position of measured data

- Adjustment based on CDF of $\Delta RT = RT_{NDT(U)} - T_o$
- ΔRT accounts for epistemic uncertainties in ASME NB-2331 $RT_{NDT(U)}$ values

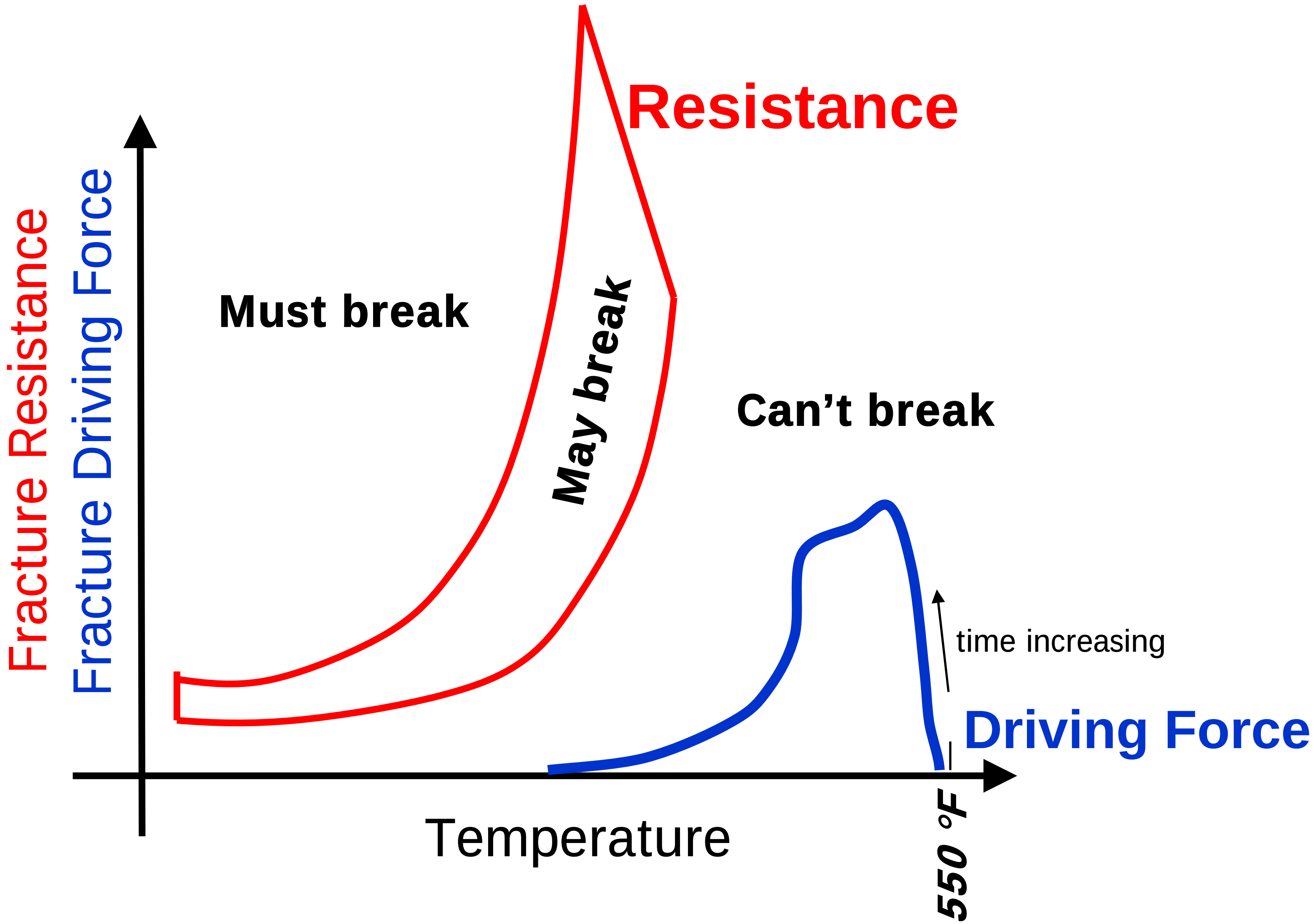




Probabilistic Fracture Mechanics

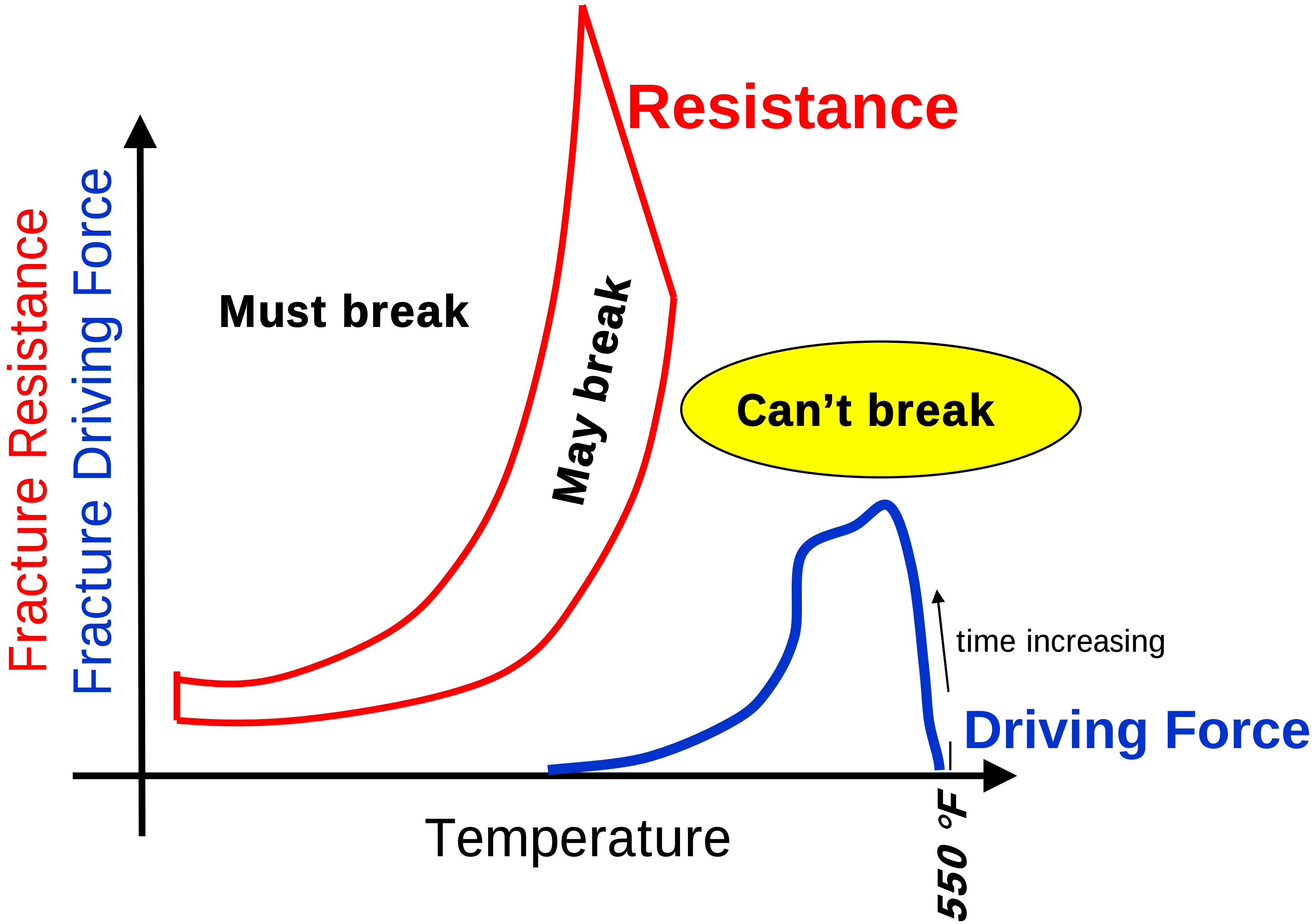
What is Warm Pre-Stress?

U.S.NRC
United States Nuclear Regulatory Commission
Protecting People and the Environment



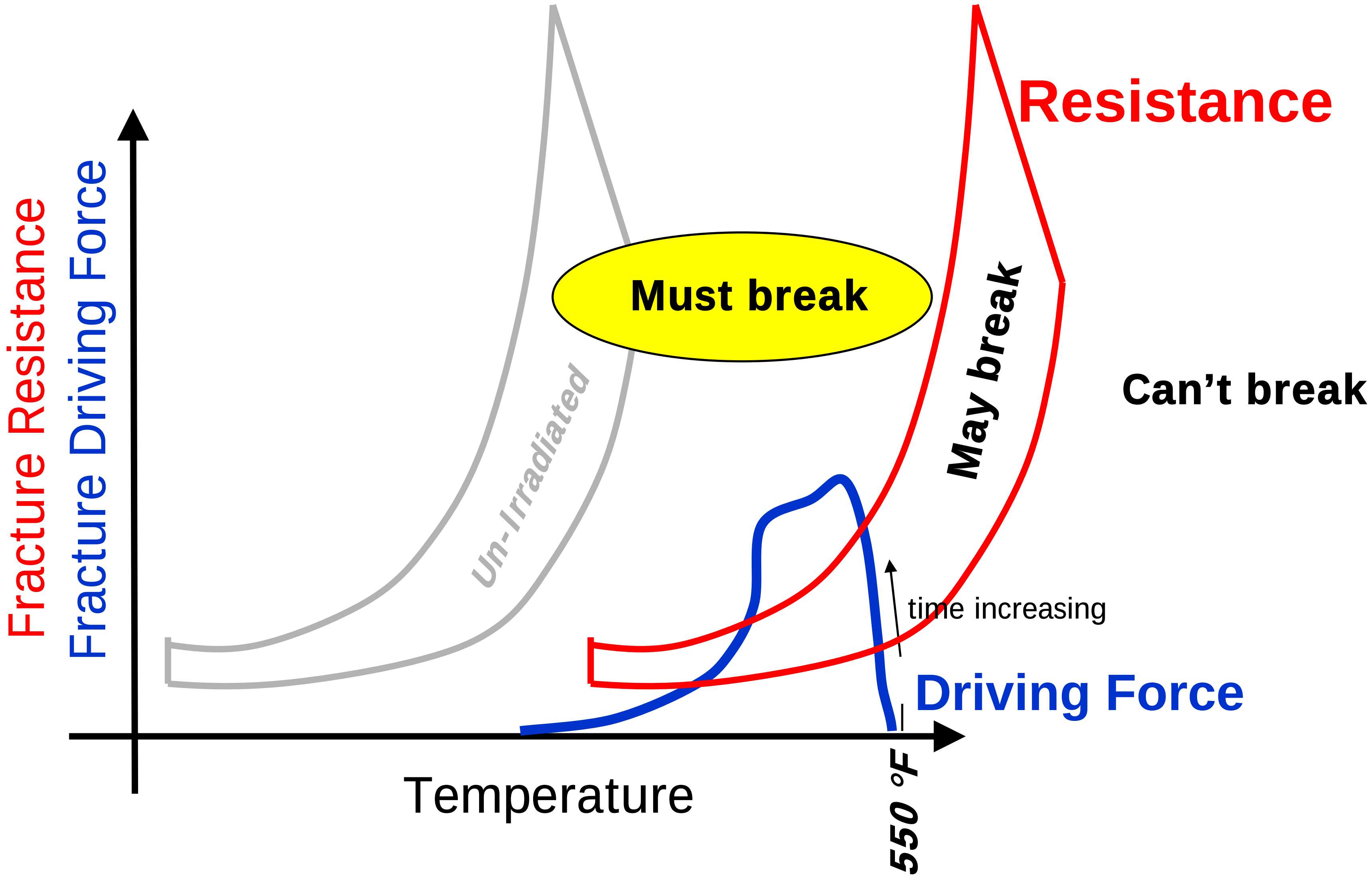
Probabilistic Fracture Mechanics

Warm Pre-Stress - No Irradiation



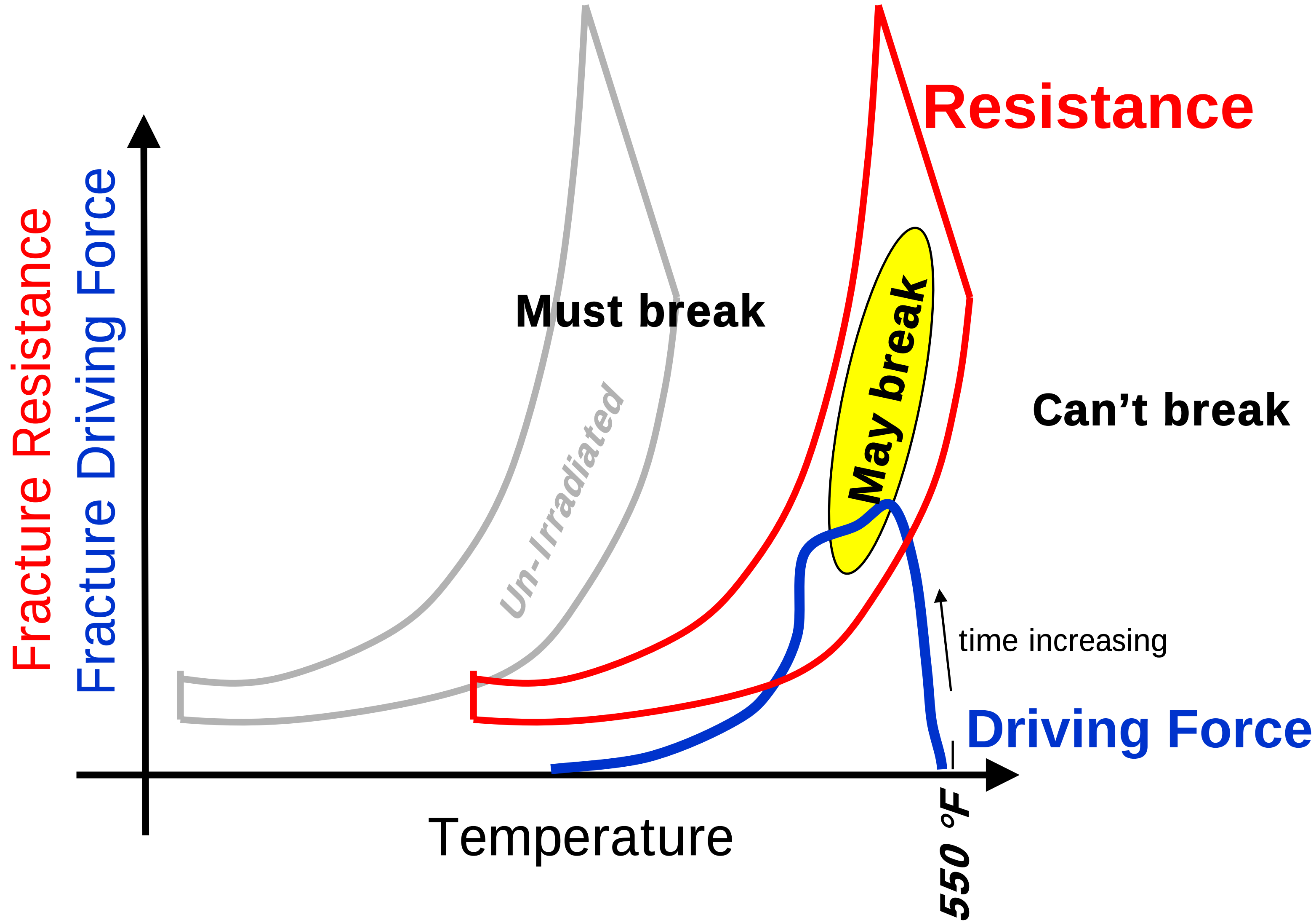
Probabilistic Fracture Mechanics

Warm Pre-Stress – High Irradiation



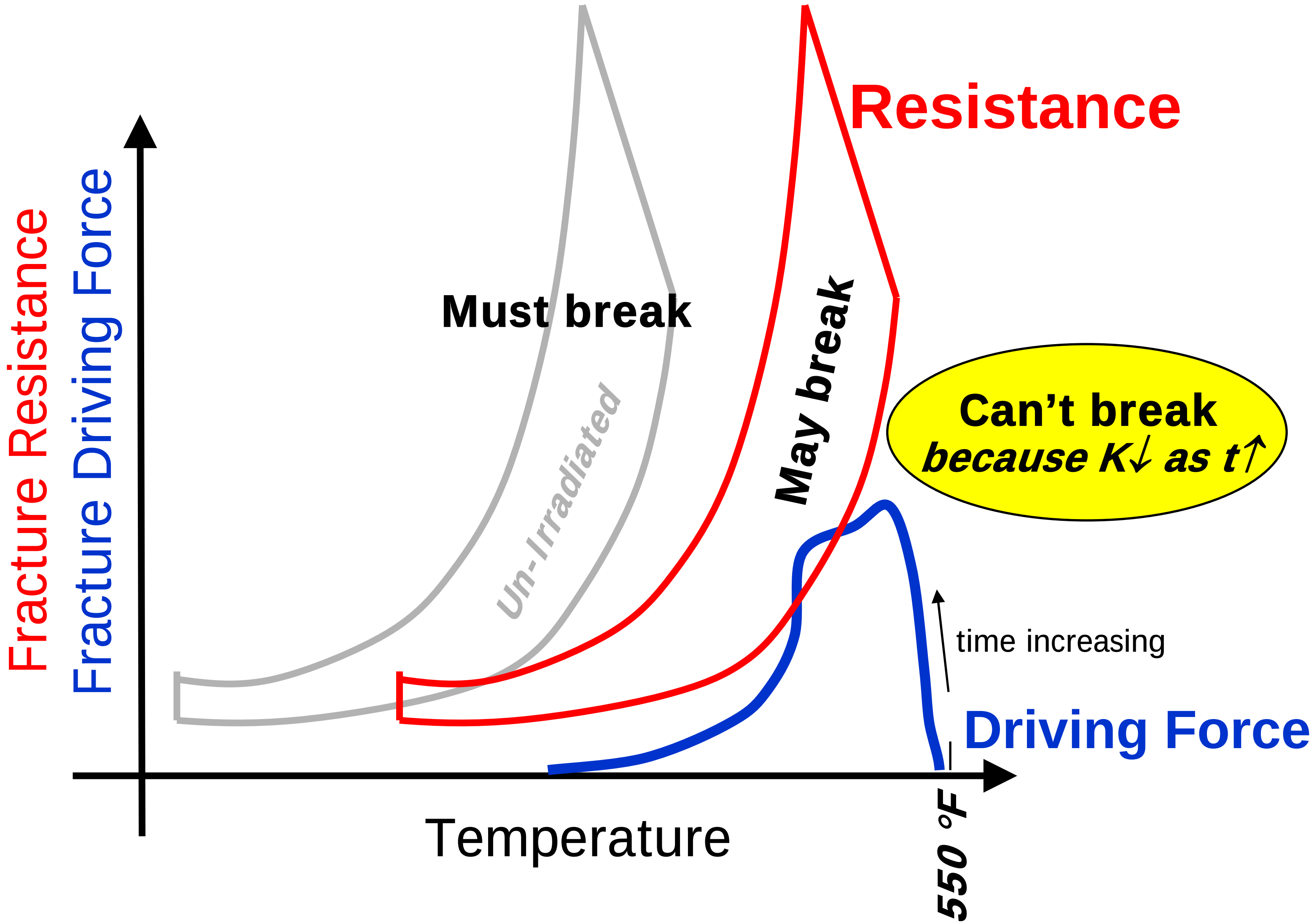
Probabilistic Fracture Mechanics

Warm Pre-Stress – Intermediate Embrittlement



Probabilistic Fracture Mechanics

Warm Pre-Stress – Intermediate Embrittlement



Probabilistic Fracture Mechanics

Warm Pre-Stress

- First noted in technical literature in 1963
- Mechanisms of WPS are well established
  - WPS plastic zones → immobile dislocations, these high load needed to yield (and fracture) at lower temperatures
  - Crack-tip blunting
  - Compressive residual stresses
- WPS **may be** active during LOCA transients depending upon
  - Specifics of the transient
  - Location of the crack in the vessel wall

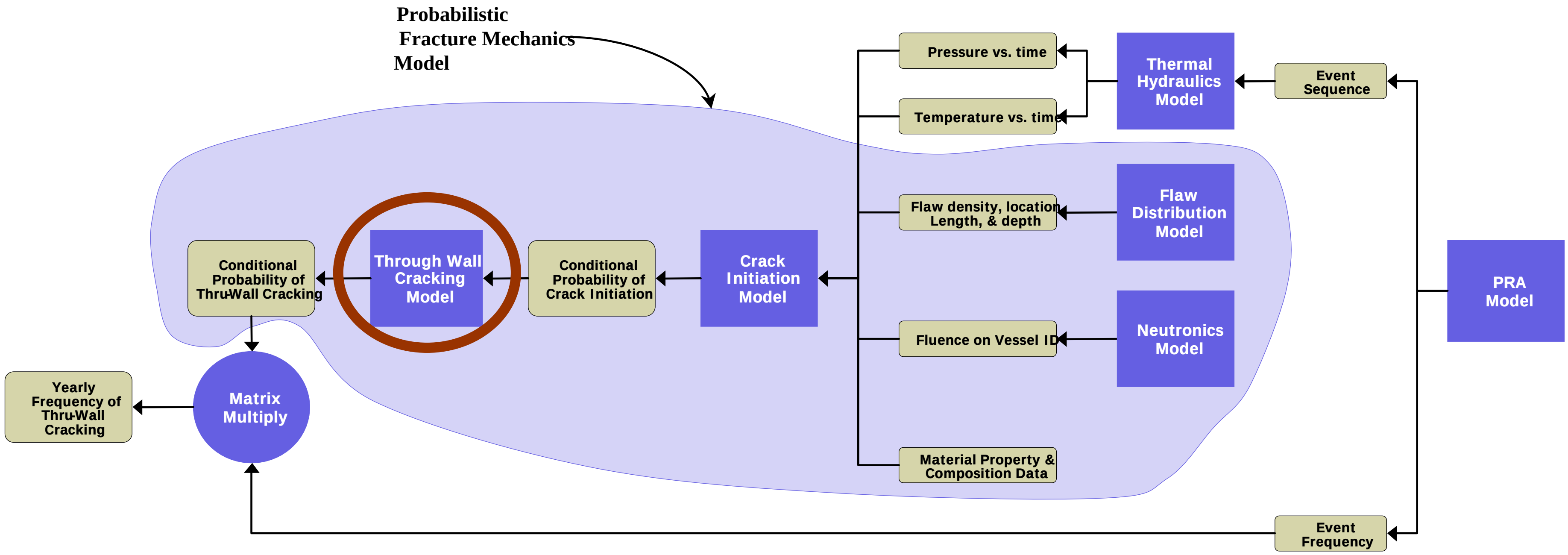
Probabilistic Fracture Mechanics

Warm Pre-Stress – Current Model

- WPS not previously credited in PTS assessments (circa-1980s) because
  - – PRA not sophisticated enough to account appropriately for all operator actions and inactions
    - Re-pressurization scenarios that would invalidate WPS may not have been modeled
  - – TH used idealized transients
    - Did not capture re-loadings that could invalidate WPS
- Current models eliminate both deficiencies, so WPS is credited
- Effect of WPS on results
  - – Very large for pipe break transients
  - – None for stuck open valve transients
  - – Integrated effect is ≈3-5x on TWCF

Probabilistic Fracture Mechanics

Model - Through Wall Cracking



- Effect of embrittlement on separation of arrest and initiation toughness curves modeled
- Aleatory uncertainty in arrest fracture resistance modeled
- Arrest toughness allowed to exceed 200 ksi√in
- Possibility of upper shelf failure allowed
- **Linkage of all toughness relationships accounted for**

Crack Initiation Toughness

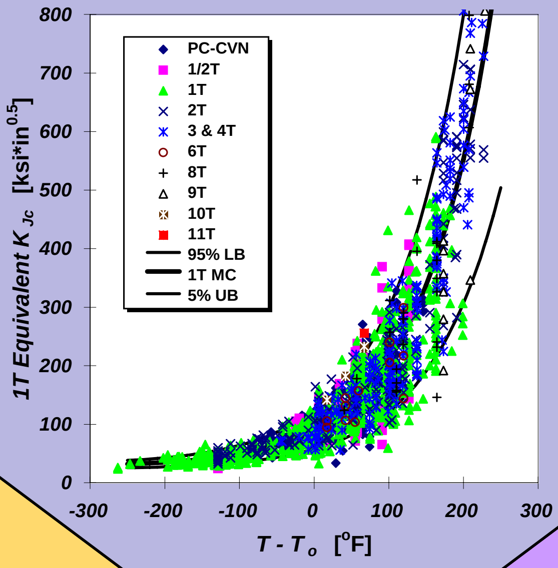
K_{Jc}

Crack Arrest Toughness

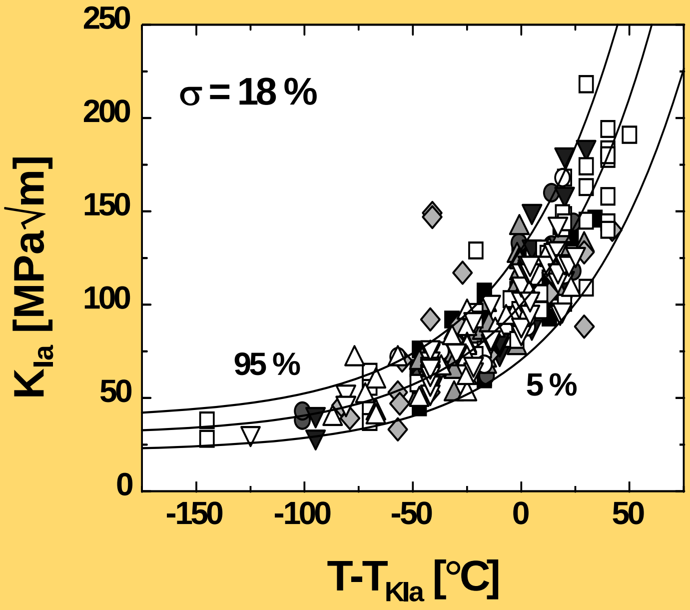
K_{Ia}

Upper Shelf Toughness

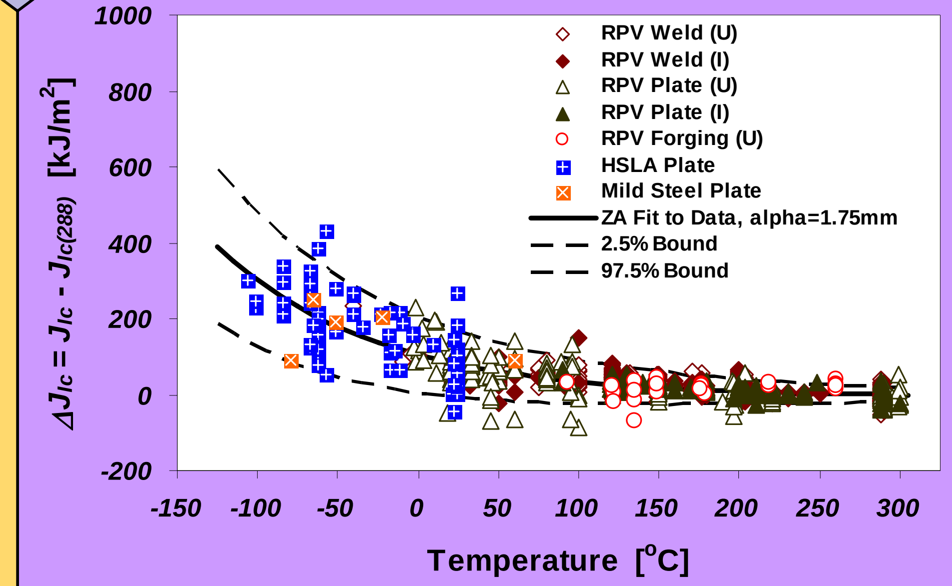
J_{Ic}



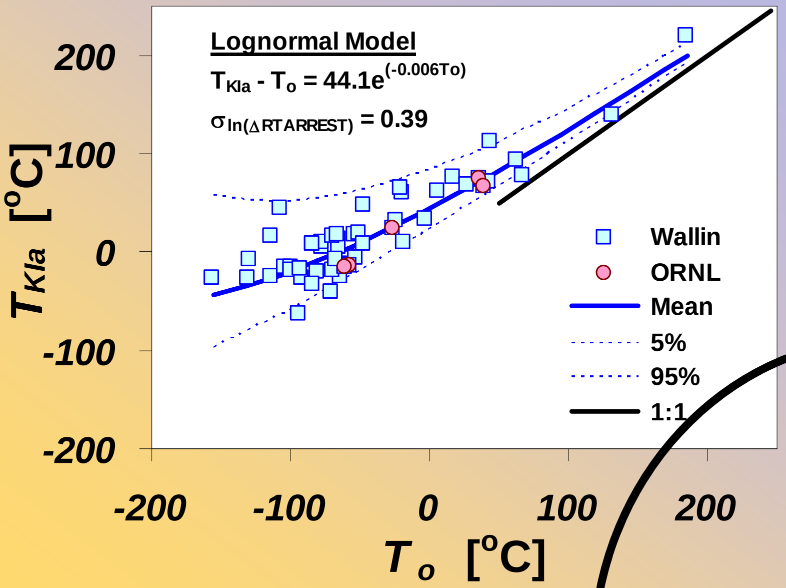
Crack Initiation Toughness:
K_{Jc} Master Curve
[Wallin]

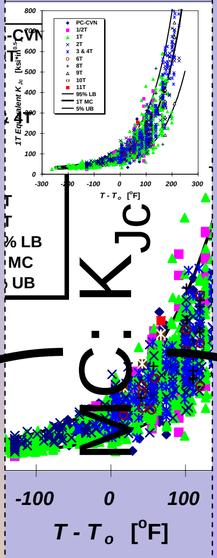


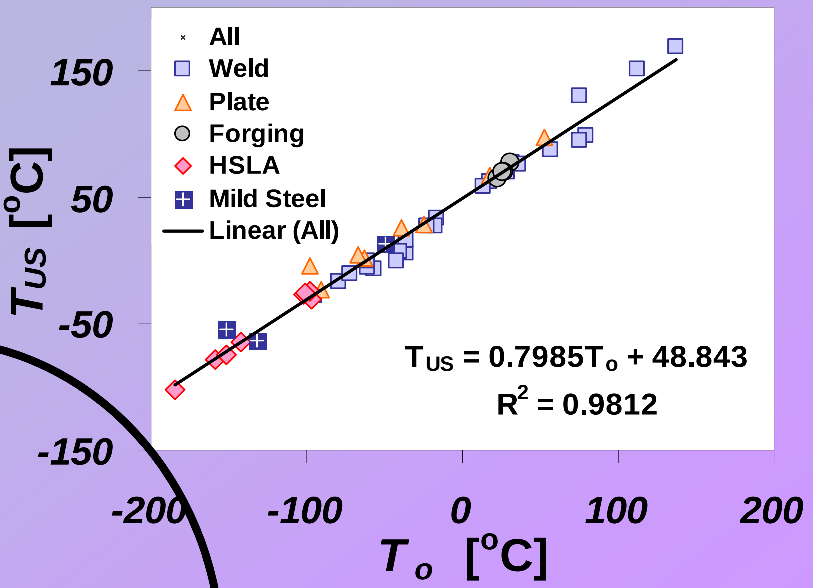
Crack Arrest Toughness:
K_{Ia} Master Curve [Wallin, Kirk]

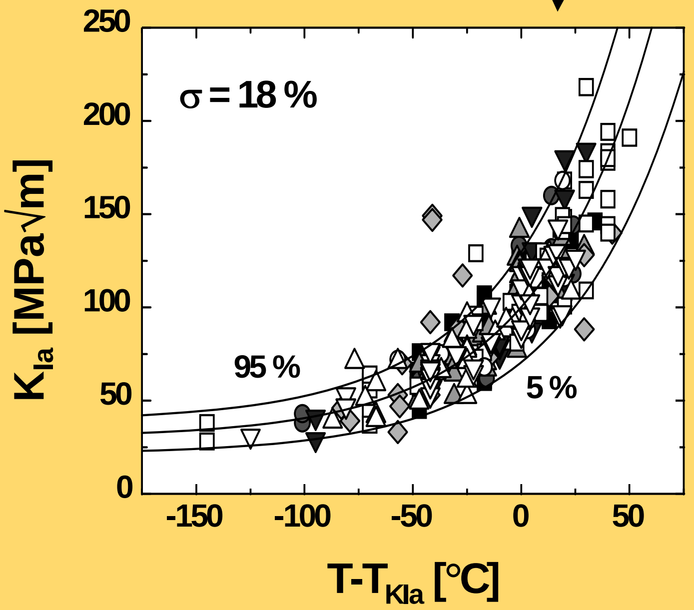


Upper Shelf Toughness:
J_{Ic} Master Curve [EricksonKirk]

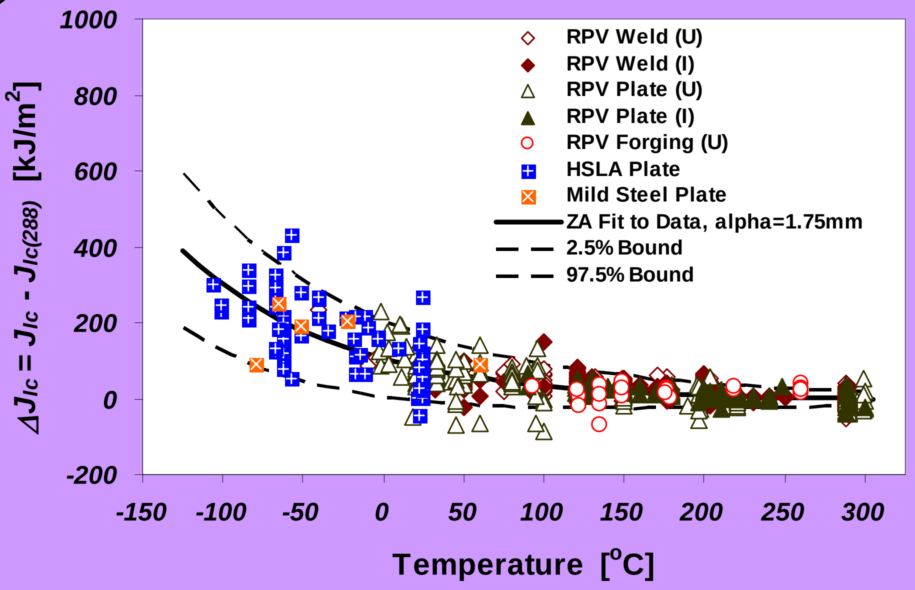








Crack Arrest Toughness:
K_{Ia} Master Curve [Wallin, Kirk]



Upper Shelf Toughness:
J_{Ic} Master Curve [EricksonKirk]

Probabilistic Fracture Mechanics

Example of Linked Toughness Distributions (Palisades)

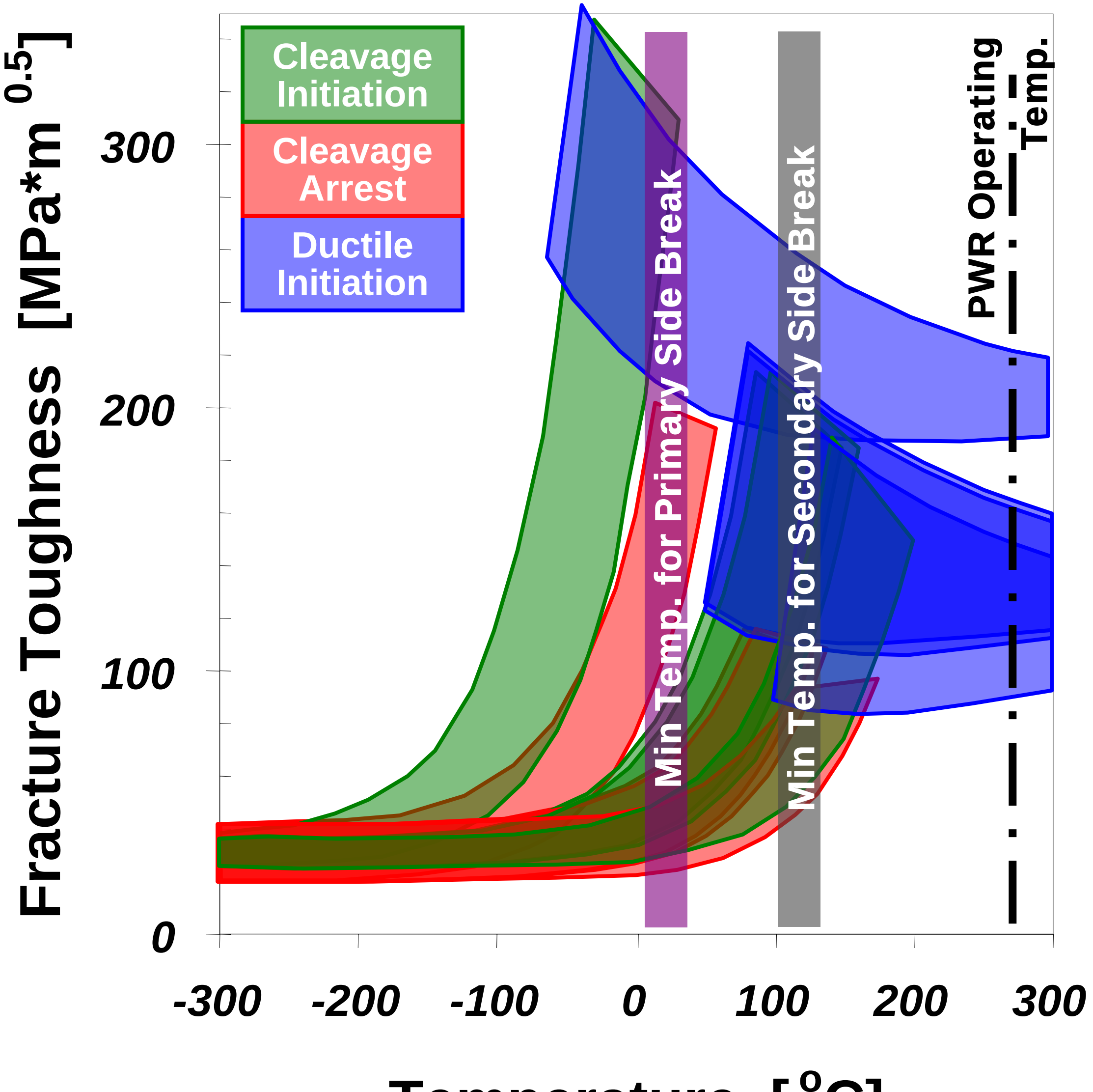
- **A reference temperature (RT) characterizes *all* of the toughness properties of interest**

  Cleavage crack initiation (transition)

  Stopping (arresting) a running cleavage crack

  Ductile crack initiation (upper shelf)

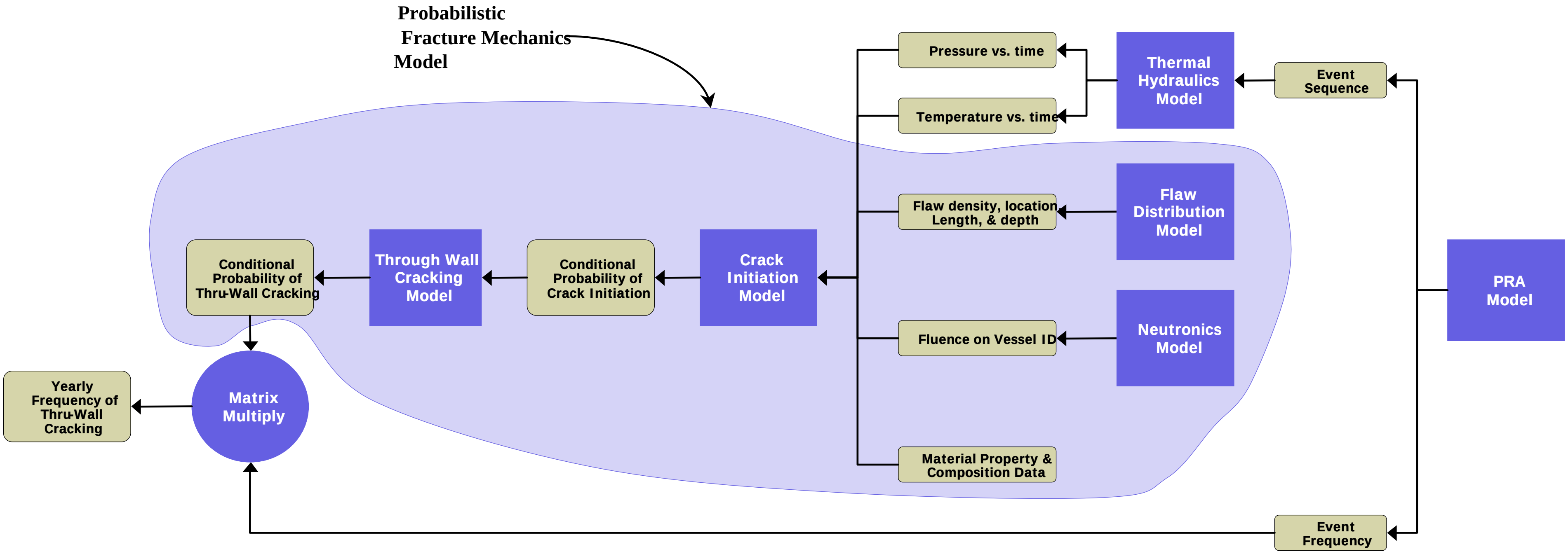
- **Toughness curves for the most embrittled axial weld in Palisades**
  - **At beginning of life**
  - **At 40 years**
  - **At 60 years**
  - **At TWCF ≈ 10^{-6} / year**



T_o= +126°C

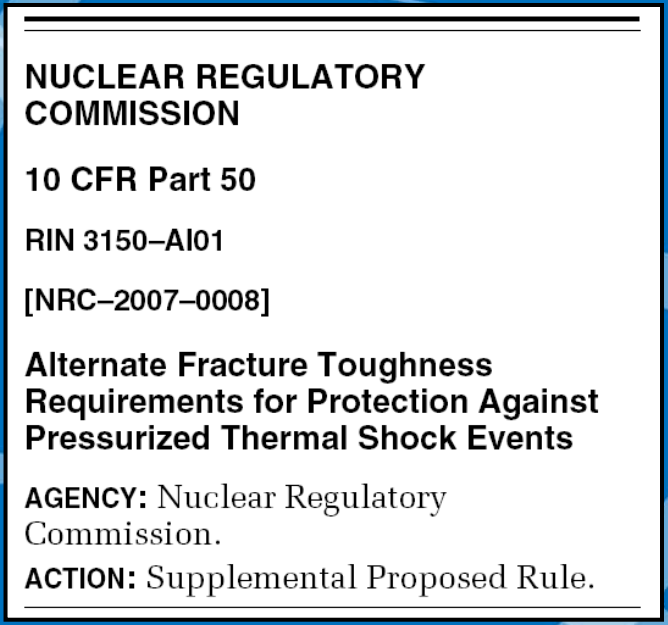
Probabilistic Fracture Mechanics

Model - Summary



- Significant improvements in most aspects of PFM model
  - Based on physical understanding of failure phenomena
  - Models calibrated to extensive data sets
- Conservatisms intentionally retained in model where state of knowledge did not permit improvement

Alternate Fracture Toughness Requirements for Protection against Pressurized Thermal Shock (PTS) Events Rule
(10 CFR 50.61a)



NUCLEAR REGULATORY COMMISSION

10 CFR Part 50

RIN 3150–AI01

[NRC–2007–0008]

Alternate Fracture Toughness Requirements for Protection Against Pressurized Thermal Shock Events

AGENCY: Nuclear Regulatory Commission.

ACTION: Supplemental Proposed Rule.

ACRS Subcommittee Meeting
October 1, 2008

U.S.NRC
United States Nuclear Regulatory Commission
Protecting People and the Environment

Rulemaking Working Group

Alternate PTS Rule

Barry Elliot	NRR/DCI
Matthew Mitchell	NRR/DCI
Stephen Dinsmore	NRR/DRA
Lambros Lois	NRR/DSS
Veronica Rodriguez	NRR/DPR
Mark EricksonKirk	RES/DE
Robert Hardies	RES/DE
Nihar Ray	NRO/DE
Geary Mizuno	OGC

Agenda

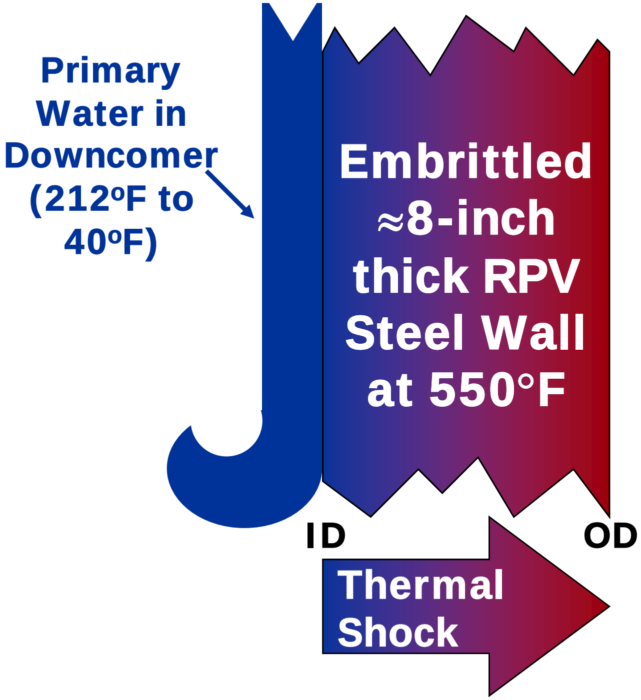
Alternate PTS Rule

- Discussion of Current PTS rule
  (10 CFR 50.61)
- Motivation for and Objective of Research
- Technical Basis for the Rulemaking
- Discussion of Alternate PTS rule
  (10 CFR 50.61a)

Current PTS Rule

Overview

- What is Pressurized Thermal Shock (PTS)?
  - Event that produces rapid cooldown from operating temperature, resulting in cold vessel temperatures with or without repressurization
  - Combined thermal and pressure stresses could induce fracture of the vessel if the vessel is embrittled



Primary Water in Downcomer (212°F to 40°F)

Embrittled ≈8-inch thick RPV Steel Wall at 550°F

ID

OD

Thermal Shock

Current PTS Rule

Overview

- PTS Rule
  - Sets limiting level (i.e., PTS screening criteria) of embrittlement, beyond which a plant may not operate without demonstrating that the risk of vessel failure is acceptably low
  - PTS screening criteria given in terms of a pressure vessel material indexing parameter, RT_{PTS}
- PTS screening criteria was developed from:
  - Likelihood of PTS event
  - Pressure and thermal stresses resulting from thermal hydraulic condition in the vessel during the event
  - Likelihood of pre-existing flaws in vessel
  - Vessel fracture resistance
- Current PTS rule based on 1980s technology

Current PTS Rule

Overview

- Promulgated in May 27, 1983; Amended in 1985, 1991 and 1996
- PTS rule requires PWR licensees to:
  - Demonstrate that projected values of RT_{PTS} meet the screening criteria in the rule at the end of license
  - Evaluate surveillance data as part of the process of determining RT_{PTS} values
  - If licensees cannot satisfy the screening criteria in the rule, licensees may submit a safety analysis to determine:
    - If plant modifications are necessary to prevent potential failure of the reactor pressure vessel (RPV)
    - If thermal annealing of the RPV will result in projected values of RT_{PTS} that meet the screening criteria

Current PTS Rule

Provisions

- Calculate RT_{PTS} value
  - $RT_{PTS} = RT_{NDT(U)} + \Delta T_{30} + \text{Margin}$
  - Treatment of plant-specific surveillance data
    - Plant-specific data used to determine ΔT_{30}, if data satisfies criteria in rule
    - Rule contains prescriptive methodology for calculating ΔT_{30}

- Compare RT_{PTS} value to regulatory screening limits of 270°F for axial welds, plates and forgings and 300°F for circumferential welds

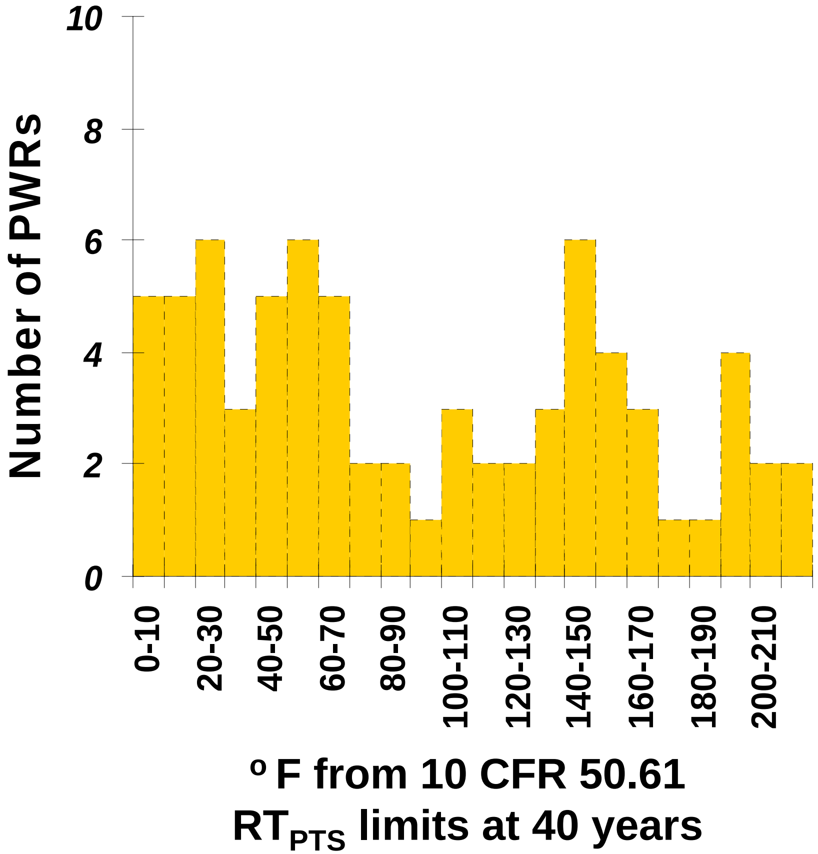
- Screening limits were based on a TWCF of 5×10^{-6} per reactor year (ry)

- No additional inspections beyond ASME Code requirements

Current PTS Rule

Impact on Licensees

- 40 years
  - All operating reactor vessels have RT_{PTS} values less than the PTS screening criteria in the current rule at the end of their 40 year license.
- 60 years
  - Approximately 10 reactor vessels may exceed the PTS screening criteria in the current rule at the projected end of their extended licenses



Current PTS Rule

Motivation and Objective of Alternate Rule

- Motivation
  - Demonstrate conservatism in the current PTS rule
- Consequences of unnecessarily conservative RT_{PTS} limits
  - Unnecessary burden on licensees and NRC
  - Unnecessary impediment to license renewal
- Objectives of research effort
  - Provide bases for rulemaking
  - Provide an alternative for licensees who cannot demonstrate compliance with the current rule through the end of their licensed operating period

Technical Basis

Presentation Overview

- Project background and motivation
- Reference temperature limits
  - Technical approach
  - Details of model
    - PRA
    - TH
    - PFM
    - Risk limit
  - Results of probabilistic calculations
  - Reference temperature (RT) limits and plant status
- Surveillance check
- Inspection requirement

Technical Basis

PTS Re-Evaluation Project Team

Broad Government and Industry Participation

United States Nuclear Regulatory Commission

EPRI

University of Maryland 1856

PNL

ISL

Duke Energy

Constellation Energy Group

Sandia National Laboratories

SAIC
An Employee-Owned Company

FRAMATOME

PEAI

Consumers Energy
Count on Us

UT-BATTELLE

ornl

INEEL

FirstEnergy

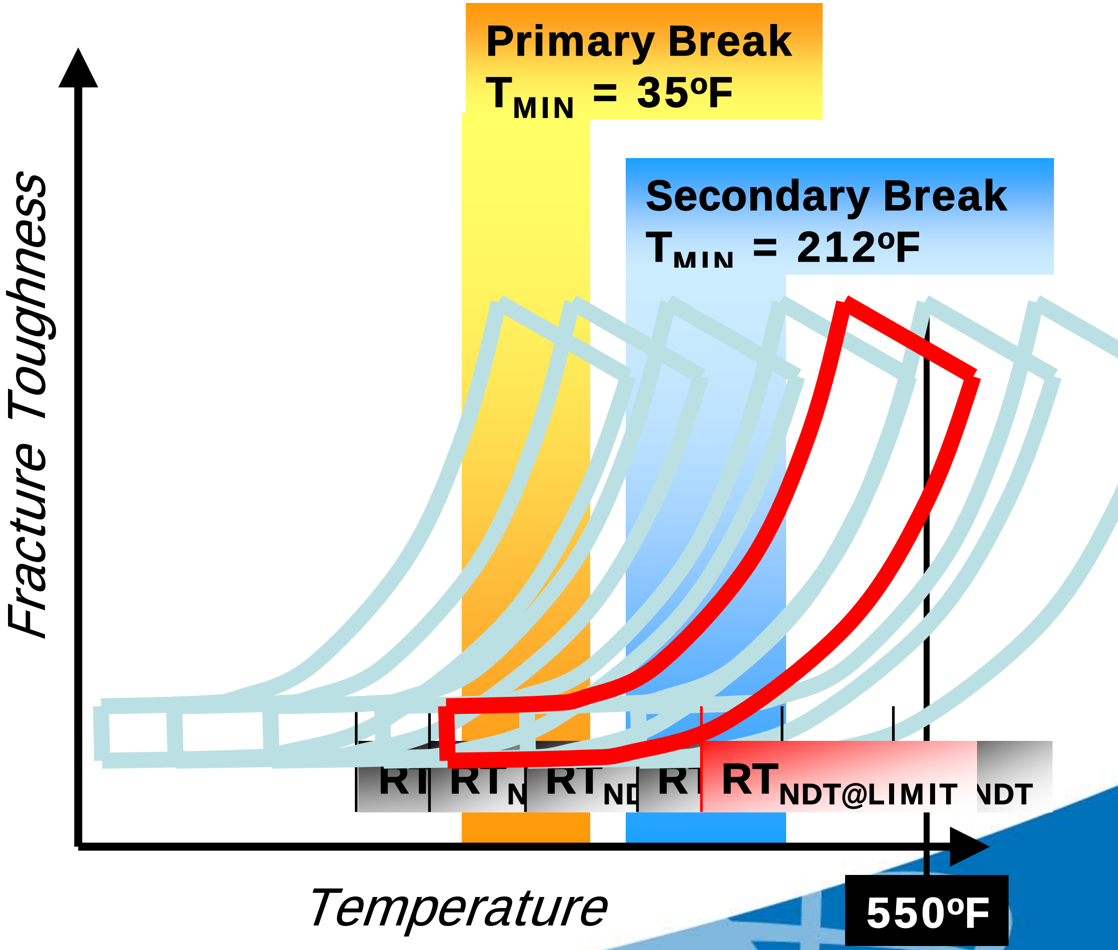
SARTREX

BROOKHAVEN
NATIONAL LABORATORY

Technical Basis

Current Rule Provisions

- Embrittlement monitored using irradiated-RT_{NDT} (10 CFR 50 App. H surveillance)
- If RT_{NDT} exceeds 300ºF or 270ºF before EOL:
  - Keep RT_{NDT} below limits
    - <u>Reduce Flux</u>: Reduce embrittlement rate
    - <u>Anneal</u>: De-embrittle the material (see RG 1.162)
  - Show higher RT_{NDT} is safe
    - <u>Analyze</u>: Plant specific analysis per RG 1.154



Technical Basis

Motivations for Rule Revision

Conservatisms suggest current RT_{NDT} limits are unnecessarily conservative

PRA

- Use of latest PRA/HRA data ⬇
- More refined binning ⬇
- Operator action credited ⬇
- Acts of commission considered ⬆
- External events considered ⬆
- Medium and large-break LOCAs considered ⬆

TH

- Many more TH sequences modeled ⬇
- TH code improved ⬇

PFM

- Significant conservative bias in toughness model removed ⬇
- Spatial variation in fluence recognized ⬇
- Most flaws now embedded rather than on the surface, also smaller ⬇
- Material region dependent embrittlement props. ⬇
- Non-conservatisms in arrest and embrittlement models removed ⬆

⬇ Change reduces risk

⬆ Change increases risk

Technical Basis

Project Sequence

- Establish motivation
- Develop risk-informed modeling approach
  - Risk limit
  - Development and integration of many models (PRA/HRA, TH, PFM)
  - Establish RT limits
    - Analysis of 3 “baseline,” or “detailed study,” plants
    - Generalization
  - Debate, vetting, and acceptance of results
- Rulemaking – established need, or not, for plant-specific checks on applicability
    - PRA/HRA/TH
    - Surveillance
    - Flaw distribution

Technical Basis

Risk Limit

U.S.NRC
United States Nuclear Regulatory Commission
Protecting People and the Environment

51 FR 28044, *Safety Goal Policy Statement (1986)*

SECY-00-0077, *Modifications to Safety Goal Policy Statement*

Regulatory Guide 1.174

10 CFR 50.61a

Voluntary Alternative Pressurized Thermal Shock Rule

QHOs < 0.1% of the total public risk (prompt & latent)

CDF < 1x10^{-4}/ry

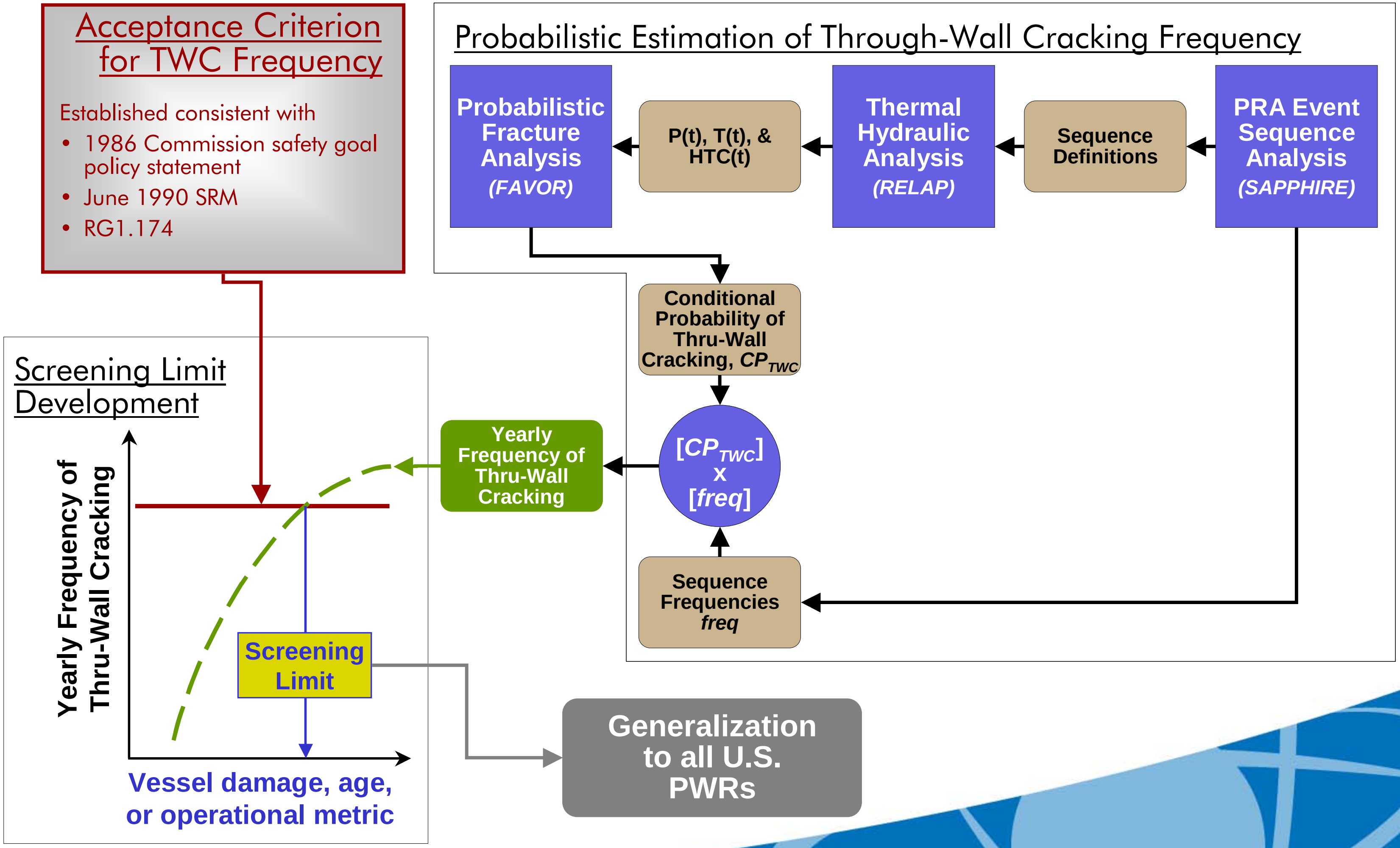
CDF & QHO limits for generic decisions

	Mean	Δ-Mean
CDF	10^{-4}/ry	10^{-5}/ry
LERF	10^{-5}/ry	10^{-6}/ry

- Accident sequence progression study shows that through-wall cracking rarely leads to LERF
- Conservatively assumes equivalence of LERF and the yearly through-wall cracking frequency (TWCF) of the reactor pressure vessel
- **Tolerable limit on TWCF established as 10^{-6}/ry**

Technical Basis

Overall Model



Acceptance Criterion for TWC Frequency

Established consistent with

- 1986 Commission safety goal policy statement
- June 1990 SRM
- RG1.174

Probabilistic Estimation of Through-Wall Cracking Frequency

Probabilistic Fracture Analysis *(FAVOR)*

P(t), T(t), & HTC(t)

Thermal Hydraulic Analysis *(RELAP)*

Sequence Definitions

PRA Event Sequence Analysis *(SAPPHIRE)*

Conditional Probability of Thru-Wall Cracking, CP_{TWC}

$[CP_{TWC}]$ x $[freq]$

Sequence Frequencies ***freq***

Yearly Frequency of Thru-Wall Cracking

Screening Limit Development

Generalization to all U.S. PWRs

Technical Basis

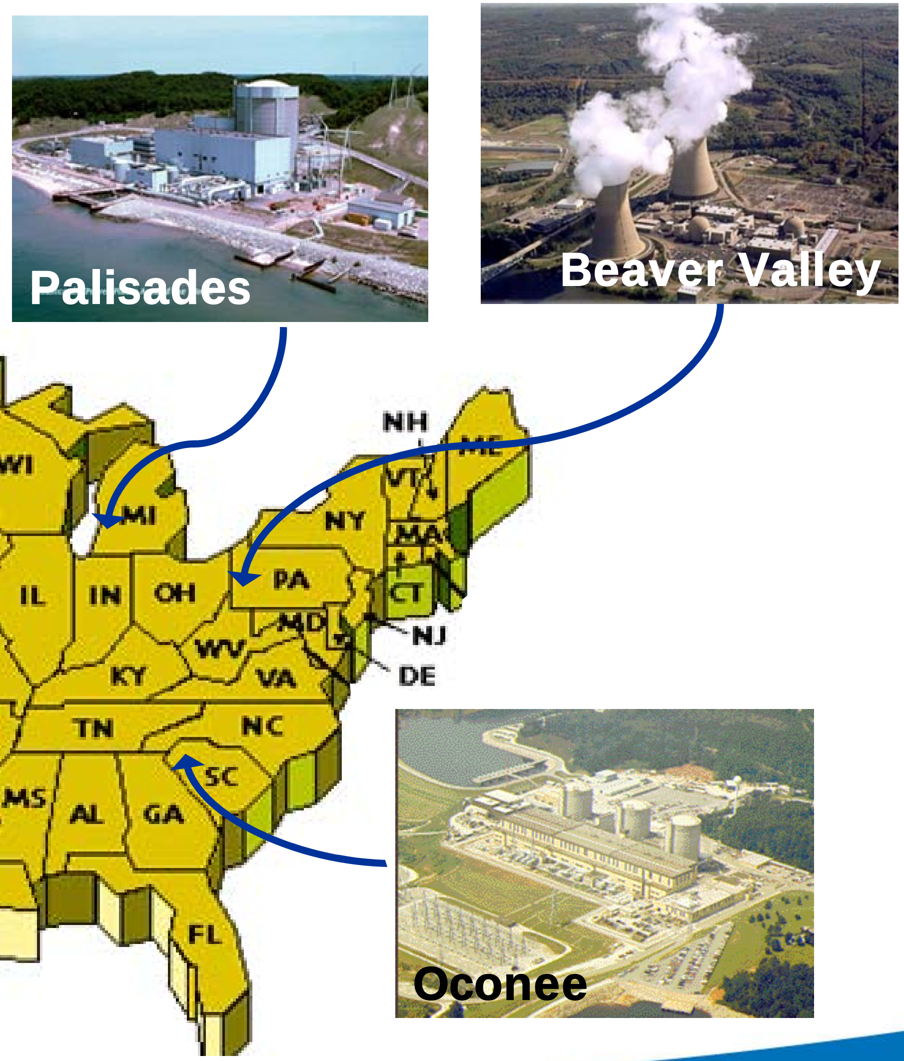
Treatment of Uncertainties

- Systematic treatment of uncertainties
  - Comprehensive process makes uncertainties visible, improves comprehensiveness of model
- All uncertainties classified (aleatory *vs*. epistemic)
- All uncertainties treated
  - Some were ***numerically quantified***
    - Used data, physical models, expert opinions to support quantification
  - Some were ***accounted for*** by the structure of the model
    - Discretization of reality (a continuum), and decisions about what parts of continuum to discretize more
    - Intentional conservatisms left in the model

“As far as the laws of mathematics refer to reality, they are not certain; as far as they are certain, they do not refer to reality.”
A.Einstein

Technical Basis

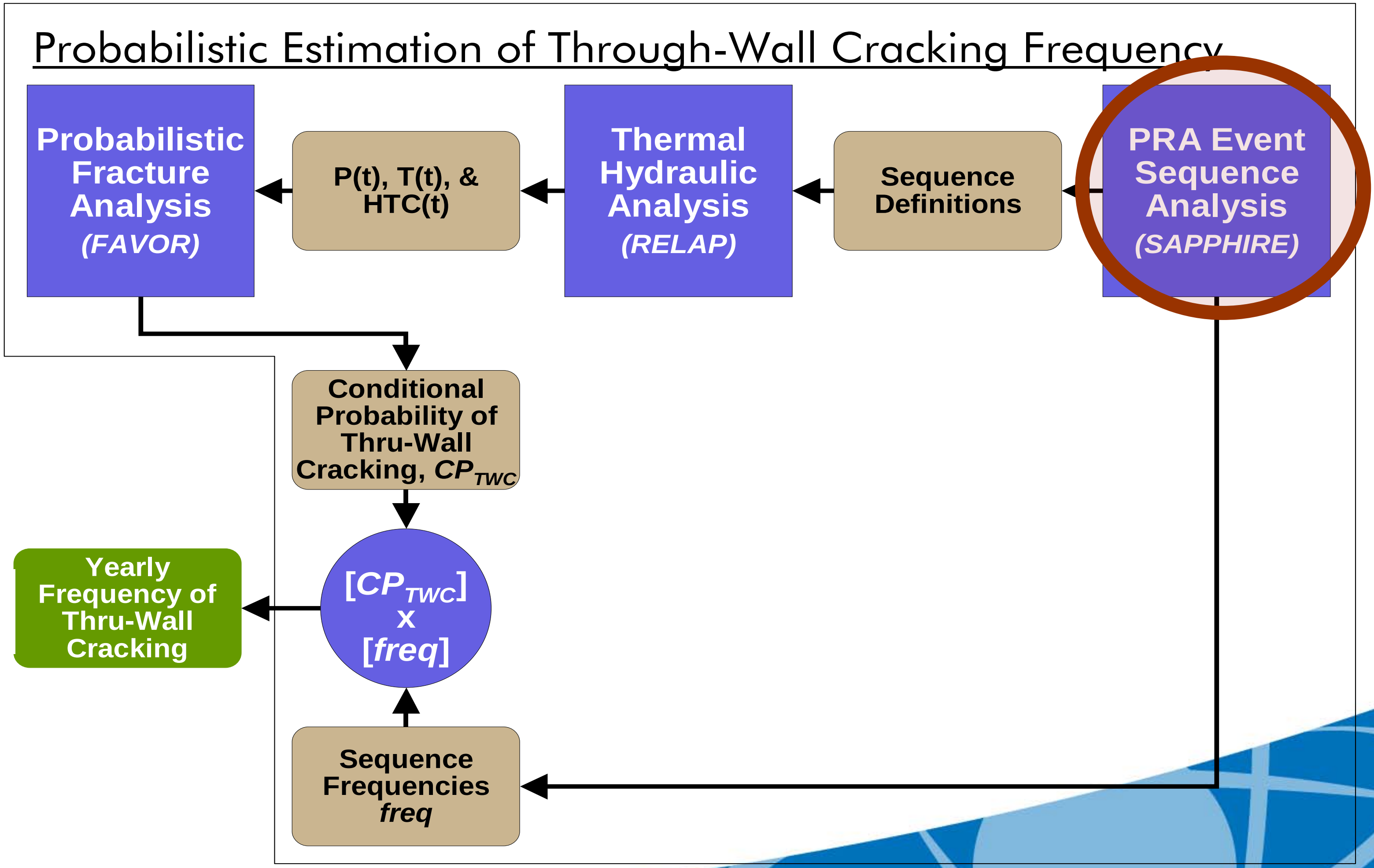
Detailed Study Plants (Baseline)



- Detailed analysis of 3 PWRs
  - All PWR manufacturers
    - 1 Westinghouse
    - 1 CE
    - 1 B&W
  - 1 plant from original (1980s) PTS study
  - 2 plants very close to the current PTS screening criteria
- Generalization to all PWRs
  - Characteristics of materials and transients that dominate failure frequencies
  - Examination of 5 more high embrittlement PWRs

Technical Basis

Details of the Model Components



Probabilistic Estimation of Through-Wall Cracking Frequency

Probabilistic Fracture Analysis *(FAVOR)*

P(t), T(t), & HTC(t)

Thermal Hydraulic Analysis *(RELAP)*

Sequence Definitions

PRA Event Sequence Analysis *(SAPPHIRE)*

Conditional Probability of Thru-Wall Cracking, CP_{TWC}

$[CP_{TWC}] \times [freq]$

Yearly Frequency of Thru-Wall Cracking

Sequence Frequencies *freq*

Technical Basis

PRA Event Sequence Analysis - Goals

- Define universe of potential PTS overcooling sequences
  - Based primarily on event tree construction
  - Sequences represented by:
    - an initiating event (disruption of normal plant operation such as a turbine trip, LOCA…) and
    - equipment and operator responses (successes and failures) that lead to overcooling
- Bin sequences, and select representative sequences from each bin for TH analysis
- Estimate the bin frequencies, including uncertainties

Technical Basis

PRA Event Sequence Analysis – Info Sources

- LER review (1980-2000)
  - 128 more significant events
  - Secondary overfeeds → minor overcooling, some actual/potential loss of secondary pressure control events
  - Operator influences can be important
- Began with previous PTS PRAs (~late 80s: Oconee, Beaver Valley, Robinson, Calvert Cliffs)
- Generic initiator frequency and probability data: represents industry-wide experience (e.g., NUREG/CRs 5750 and 5500, NUREG-1829 LOCA Frequencies)
- Plant specific information for the 3 detailed study plants
  - Interactions and review by plant personnel / experts
  - Operating procedures and plant design
  - Existing PRA documentation
  - Observed simulator exercises

Technical Basis

PRA Event Sequence Analysis – Model

Initiators – Full and HZP

- **LOCAs:** Small, Medium, Large
- **Transients**
  - reactor-turbine trip
  - 2 loss of bus
  - loss of instrument air
  - loss of main condenser/main feedwater
  - loss of offsite power (including station blackout)
- **Other**
  - steam generator tube rupture
  - steam line break: small, large

Equipment Functions

- **Primary Integrity:** PORV and block valve, SRVs, RCS as break source, consideration of pressurizer spray/heaters
- **Secondary Pressure:** steam lines as break source, TBVs and associated block valves, MSSRVs, consideration of turbine stop/control valves
- **Secondary Feed:** main feed, emergency feed, condensate
- **Primary Flow / Pressure:** reactor coolant pumps, HPI/charging, consideration of core flood tanks/low pressure injection, vent valves

Technical Basis

PRA Event Sequence Analysis – Operator Actions

Primary Integrity Control	Secondary Pressure Control	Secondary Feed Control	Primary Pressure/ Flow Control
Operator fails to isolate an isolable LOCA in a timely manner	Operator fails to isolate a depressurization condition in a timely manner	Operator fails to stop/throttle or properly align feed in a timely manner	Operator does not properly control cooling and throttle/terminate injection to control RCS pressure
	Operator isolates when not needed	Operator feeds wrong (i.e., affected) SG	Operator trips RCPs when not appropriate and/or fails to restore them when desirable
Operator induces a LOCA that induces/enhances a cooldown	Operator isolates wrong path/SG	Operator stops/throttles feed when inappropriate	Operator does not provide sufficient injection or fails to trip RCPs appropriately
	Operator creates an excess steam demand		

Technical Basis

PRA Event Sequence Analysis – Plant-Specific Models

- Oconee and Beaver Valley
  - Models constructed by NRC contractors with input from industry representatives
  - Oconee (1st model) – very detailed
  - Beaver Valley (2nd model) – less detailed because low significance bins were eliminated
- Palisades (3rd model)
  - Model constructed by licensee, modified slightly based on insights from Oconee and Beaver Valley

Technical Basis

PRA Event Sequence Analysis – PRA Uncertainty

- Two general classes of uncertainty
  - Aleatory uncertainties are ***implicit*** to the model used
    - How event sequences were modeled
    - How event sequences were binned
    - How representative sequence from each bin was selected
  - Epistemic uncertainties are ***explicit*** and quantified
    - The frequency of each modeled scenario

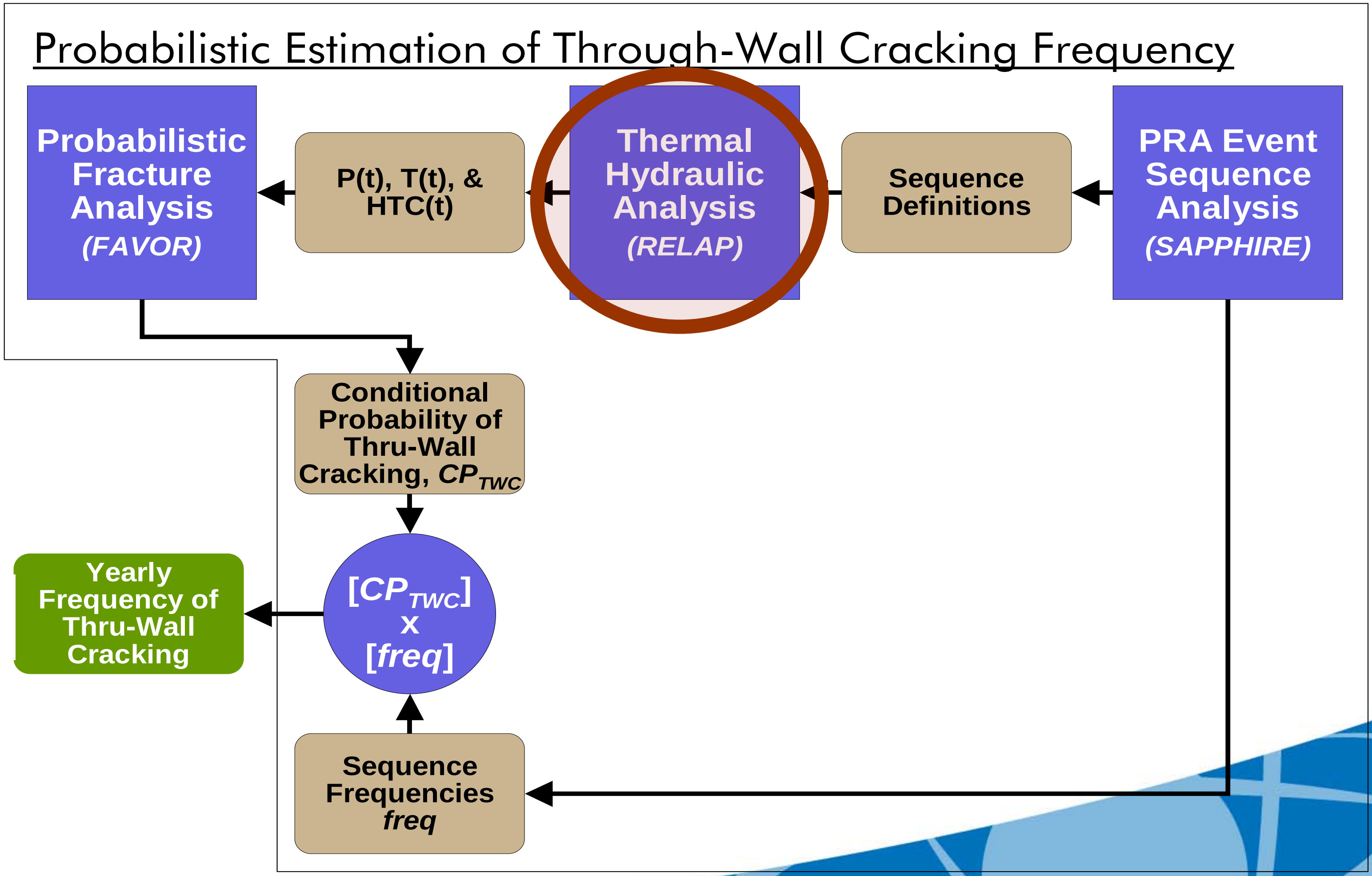
Technical Basis

PRA Event Sequence Analysis – Model Refinement

Difference Between Current PRA Analyses and the PRA Analyses that Supported 10 CFR 50.61		Effect on Risk
Refinement of Detail Considered by the Analysis	Slight expansion of the types of sequences and initiators considered (e.g. HZP, medium – large ∅ primary pipe breaks)	↑
	Slight expansion of support systems both as initiators and as dependencies affecting equipment response	↑
	Less gross binning of TH sequences	↓
	External initiating events considered as potential PTS precursors	↑
Treatment of Operator Actions	Credit for operator actions is based on detailed consideration of numerous factors associated with the modeled sequences, on simulator observations, on the latest procedures and relevant training, and on numerous discussions with operating and training staffs. Detrimental acts of commission are also considered.	↑ & ↓
	A greater number of discrete operator action times are considered.	↓
Use of New Data	Includes the latest industry-wide (and some plant-specific) data for initiating event frequencies, equipment failure probabilities, and common-cause considerations.	↓

Technical Basis

Details of the Model Components



Probabilistic Estimation of Through-Wall Cracking Frequency

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Thermal Hydraulic Analysis *(RELAP)*

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Conditional Probability of Thru-Wall Cracking, CP_{TWC}

Yearly Frequency of Thru-Wall Cracking

$[CP_{TWC}]$ x $[freq]$

Sequence Frequencies *freq*

Technical Basis

Thermal Hydraulic Analysis - Assumptions

- The RELAP code provides an appropriate and accurate representation of conditions in the downcomer
  - Overall for the transient conditions modeled
  - No plumes or thermal streaming of significance

- The temporal variation of P, T, and h for a single transient can be appropriately used to represent an entire bin (containing many transients) to the PFM analysis

Technical Basis

Thermal Hydraulic Analysis – RELAP Accuracy

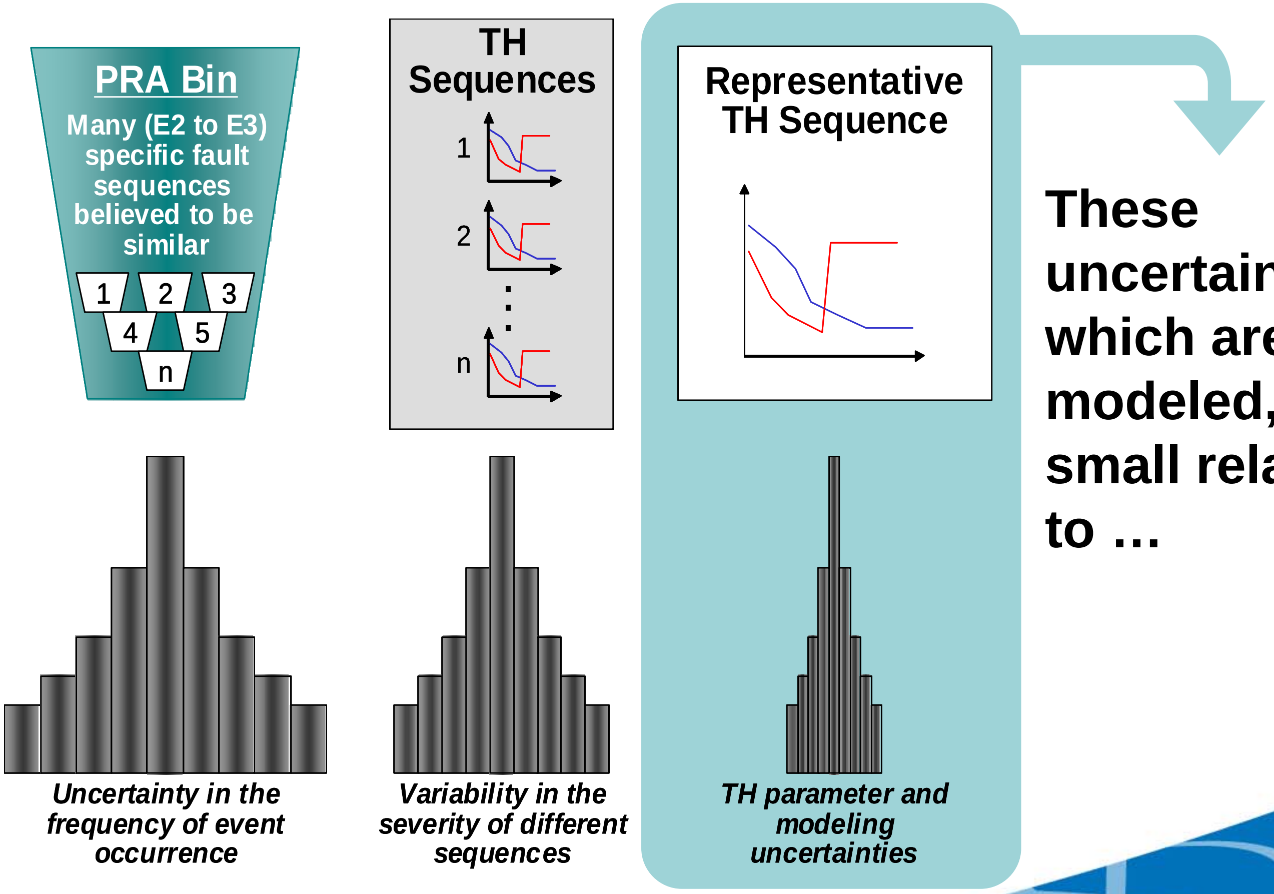
- RELAP5 predictions compared with experiments
  - Fletcher, Prelewicz, and Arcieri, NUREG/CR-6857
  - Tests performed at a wide range of facilities
    - Integral systems tests
    - Separate effects tests
  - Assessments attest to general accuracy of RELAP5 in modeling downcomer conditions during PTS

Plumes addressed, no impact on results

- **Experimental data**
  - Significant cold leg stratification
  - Mixing dissipates plumes before they reach the downcomer
    - < 10 °C: Integral-systems tests
    - < 20 °C: Separate-effects tests
  - Integral-systems tests provide most realistic model of full scale RPV
    - 3D representation of downcomer allows interaction among multiple plumes
- **FAVOR sensitivity studies**
  - Used far stronger plumes than seen experimentally (40-80 °C)
  - Plumes, if present, only increase axial stresses
    - Therefore, only increases driving force on circumferential flaws
    - Therefore, virtually no effect on TWCF

Technical Basis

Thermal Hydraulic Analysis – Bin Representation



Uncertainty in the frequency of event occurrence

Variability in the severity of different sequences

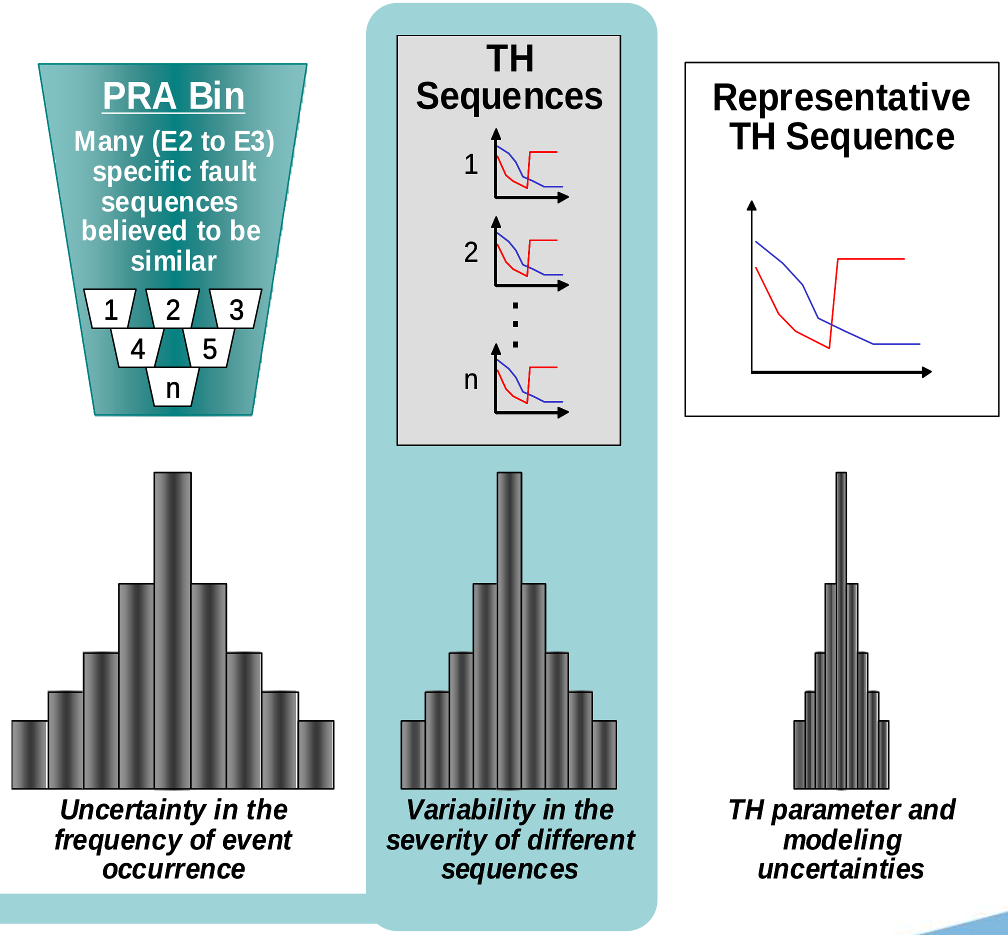
TH parameter and modeling uncertainties

These uncertainties, which are not modeled, are small relative to …

Technical Basis

Thermal Hydraulic Analysis – Bin Representation

these uncertainties, which are implicit to a binned representation of PTS challenge, and to …



Uncertainty in the frequency of event occurrence

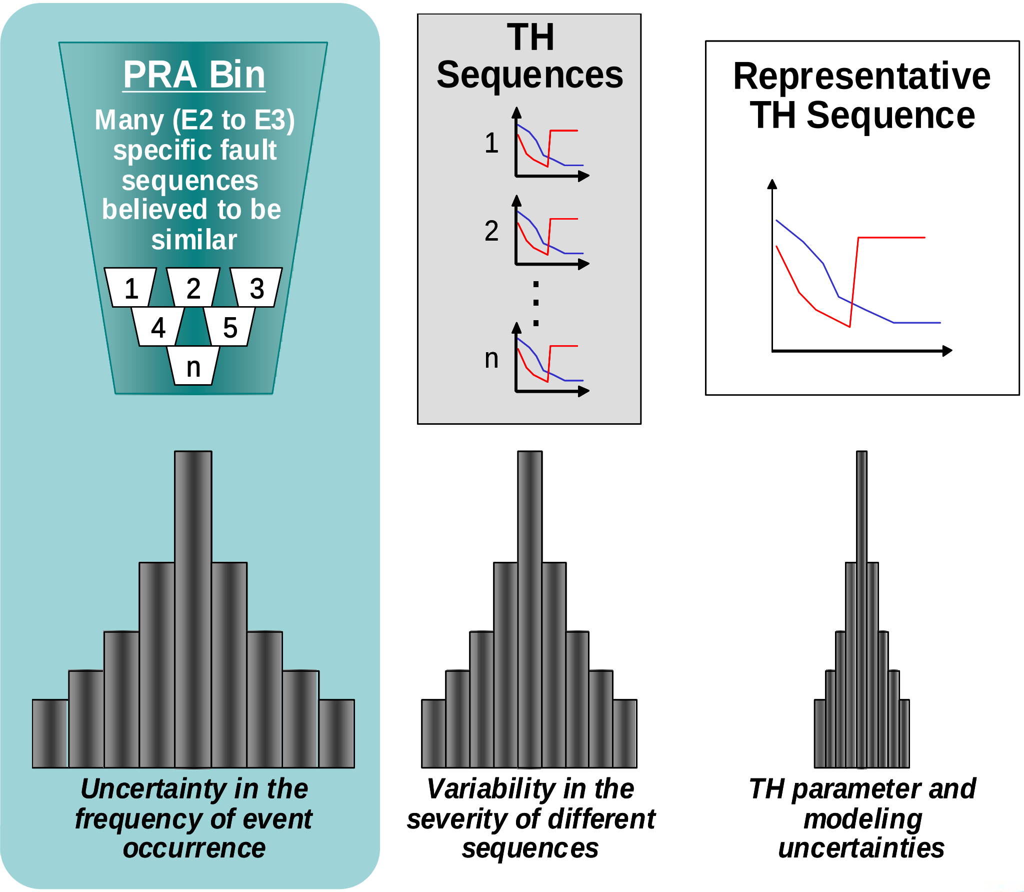
Variability in the severity of different sequences

TH parameter and modeling uncertainties

Technical Basis

Thermal Hydraulic Analysis – Bin Representation

these uncertainties, which are larger still, and are modeled.



PRA Bin

Many (E2 to E3) specific fault sequences believed to be similar

TH Sequences

Representative TH Sequence

Uncertainty in the frequency of event occurrence

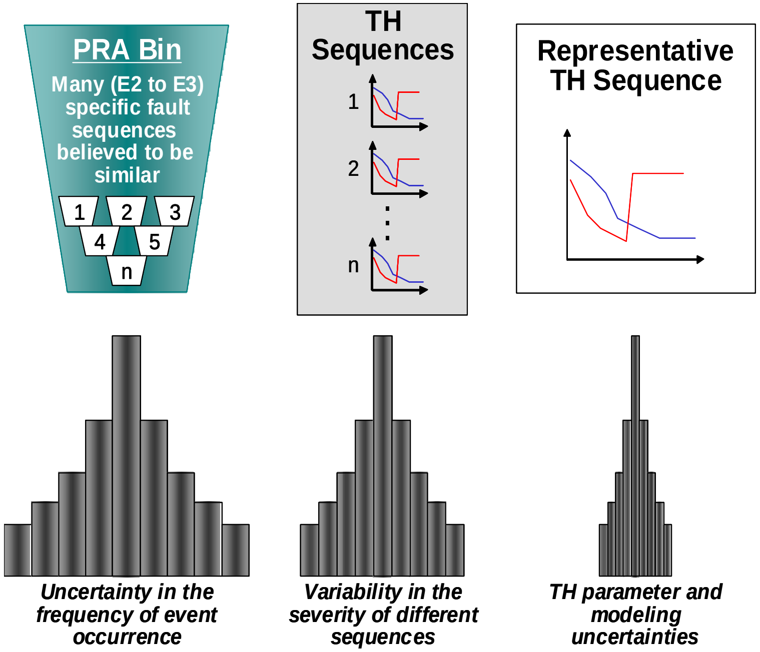
Variability in the severity of different sequences

TH parameter and modeling uncertainties

Technical Basis

Thermal Hydraulic Analysis – Bin Representation

- Even though TH uncertainties have not been explicitly modeled, they have been addressed
  - Much smaller than bin uncertainties and frequencies
  - Model building process includes bin subdivision ... ensures that discretization of PTS challenge does not impact answer
- Inaccuracies in RELAP5 predictions relative to experiments shown to be small relative to PRA bin uncertainty
  - *Even if the TH model was more accurate, the accuracy of the TWCF values predicted by FAVOR would not improve*



PRA Bin

Many (E2 to E3) specific fault sequences believed to be similar

TH Sequences

Representative TH Sequence

Uncertainty in the frequency of event occurrence

Variability in the severity of different sequences

TH parameter and modeling uncertainties

Technical Basis

Thermal Hydraulic Analysis – RELAP5

RELAP5/MOD 3.2.2g

- Models the coupled behavior of RCS, core, secondary systems, and control systems
- Simultaneously solves conservation equations of mass, energy, momentum
- Non-homogeneous (liquid and vapor can flow at different velocities)
- Non-equilibrium (liquid and vapor can exist at different temperatures)
- Models trip and control functions

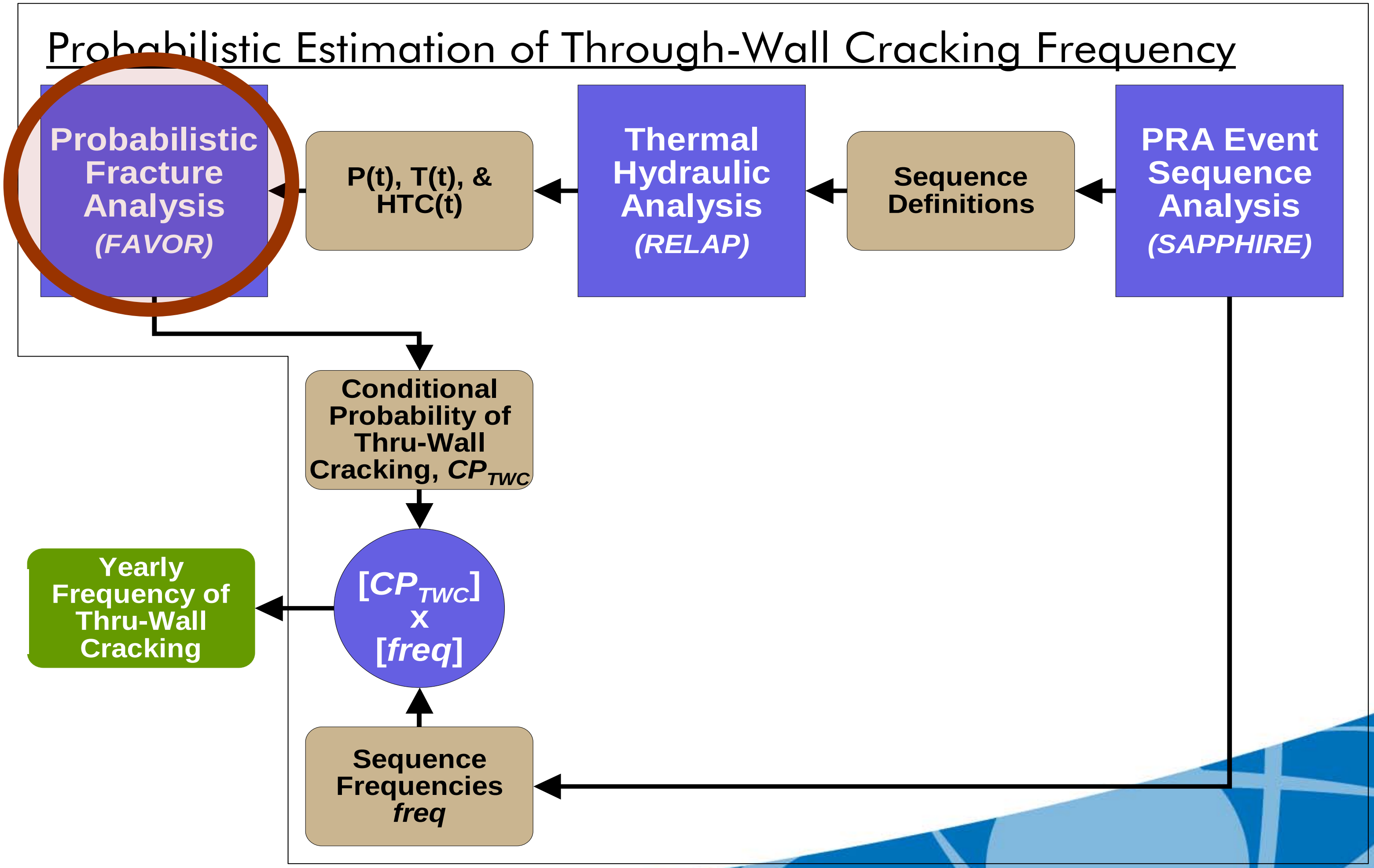
Building a RELAP5 Model

- Discretize physical system into a network of fluid cells connected by junctions
- Select flow models appropriate for different parts of the system
- Establish
    - Initial conditions
    - Thermo-physical properties
    - Time step information

to represent PRA-specified transients

Technical Basis

Details of the Model Components



Probabilistic Estimation of Through-Wall Cracking Frequency

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$[CP_{TWC}] \times [freq]$

Yearly Frequency of Thru-Wall Cracking

Sequence Frequencies *freq*

Technical Basis

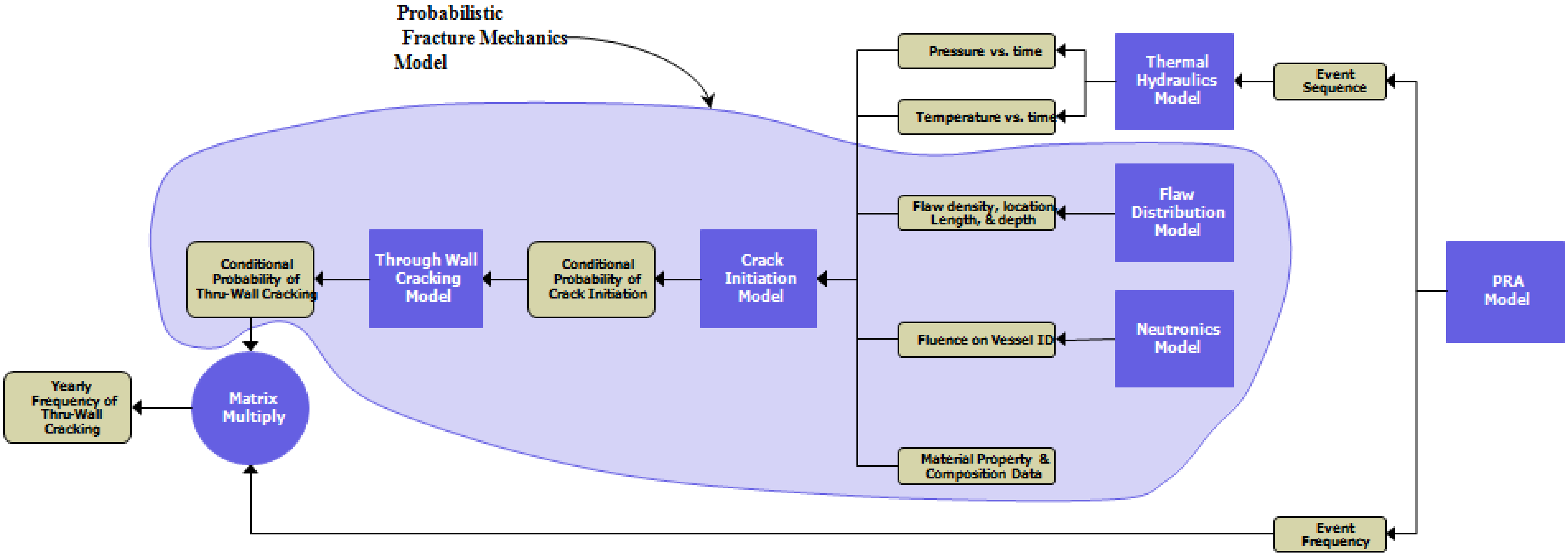
Probabilistic Fracture Mechanics - Assumptions

- A linear elastic fracture mechanics model is appropriate
  - Plastic zone << structural dimensions
  - Demonstrated by large scale tests (ORNL and worldwide)
- Sub-critical crack growth is negligible
  - Environmental mechanisms
  - Cyclic loading (fatigue)
- *A priori* elimination of certain contributors to TWCF
  - Flaws (deeper than $3/8 \cdot t_{WALL}$)
  - Transients (T_{MIN} > 400 °F)

appropriateness of both confirmed *a posteriori*

Technical Basis

Probabilistic Fracture Mechanics - Model



Jump to Details

Technical Basis

Baseline Results

- Sources of information
  - – Chapter 8 of NUREG-1806
  - – NUREG-1874
- Will discuss
  - – Material features that dominate TWCF
  - – Transient classes that dominate TWCF

Jump to Details

Technical Basis

Generalization Results

- Informed by baseline results, determined if plant-specific features were expected to produce significant changes in 5 other PWRs with high embrittlement

- Effect of external initiating events

- TH and PFM sensitivity studies
  - Are there credible model changes that should be accounted for?
  - Are there cautions to the applicability of the baseline results to all plants?

Technical Basis

Generalization Results

- Informed by baseline results, determined if plant-specific features were expected to produce significant changes in 5 other PWRs with high embrittlement
- Effect of external initiating events
- TH and PFM sensitivity studies
  - Are there credible model changes that should be accounted for?
  - Are there cautions to the applicability of the baseline results to all plants?

- The baseline model of stuck open valves may underestimate TWCF by about 2.5x
  - Only a factor at low embrittlement levels
  - Accounted for when RT limits are estimated

Technical Basis

Generalization Results

- Informed by baseline results, determined if plant-specific features were expected to produce significant changes in 5 other PWRs with high embrittlement

- Effect of external initiating events

- PTS due to external initiating events is not significant

- TH and PFM sensitivity studies
  - Are there credible model changes that should be accounted for?
  - Are there cautions to the applicability of the baseline results to all plants?

Technical Basis

Generalization Results

- Informed by baseline results, determined if plant-specific features were expected to produce significant changes in 5 other PWRs with high embrittlement

- Effect of external initiating events

- TH and PFM sensitivity studies
    - Are there credible model changes that should be accounted for?
    - Are there cautions to the applicability of the baseline results to all plants?

TH

- No credible model changes
- No cautions regarding general applicability

Technical Basis

Generalization Results

- Informed by baseline results, determined if plant-specific features were expected to produce significant changes in 5 other PWRs with high embrittlement

- Effect of external initiating events

- TH and PFM sensitivity studies
  - Are there credible model changes that should be accounted for?
  - Are there cautions to the applicability of the baseline results to all plants?

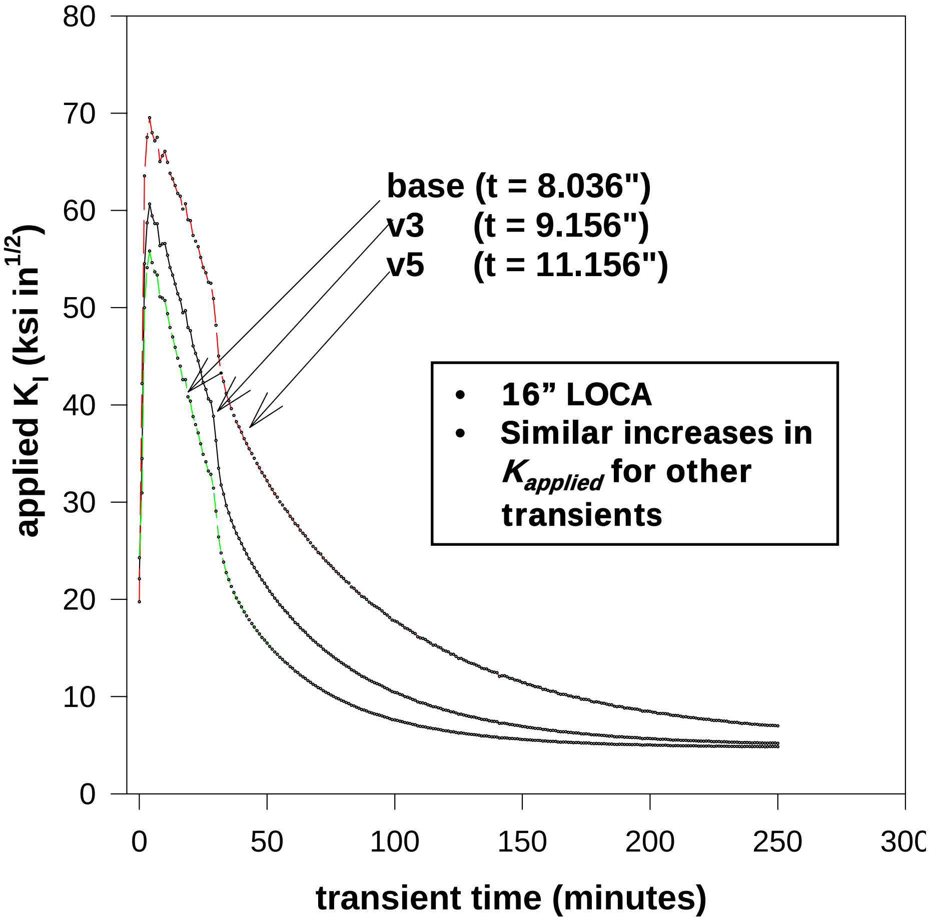
PFM

- No credible model changes
- Two cautions regarding general applicability
  - Vessel wall thickness
  - Screening limits for forgings

Further analysis performed to address both deficiencies. Results considered when RT-screening limits were established.

Technical Basis

Generalization Results–Thickness Effect



Beaver Valley transient 9

- 16” LOCA
- Similar increases in $K_{applied}$ for other transients

thickness increases →

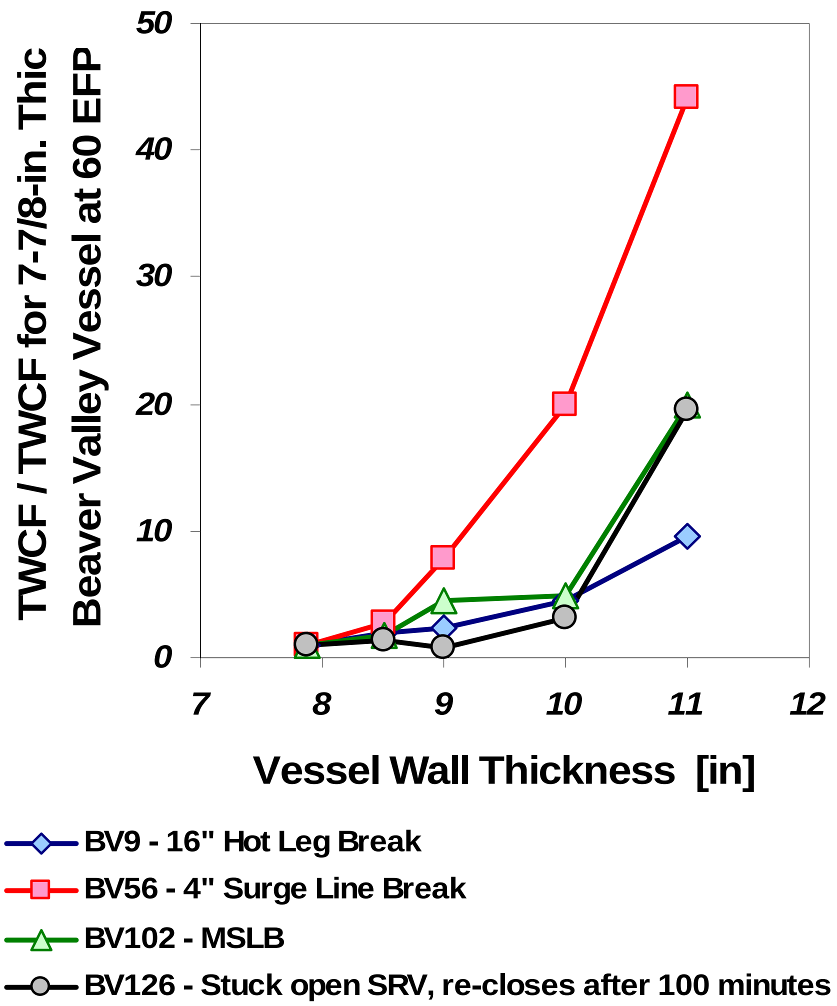
thermal stress increases →

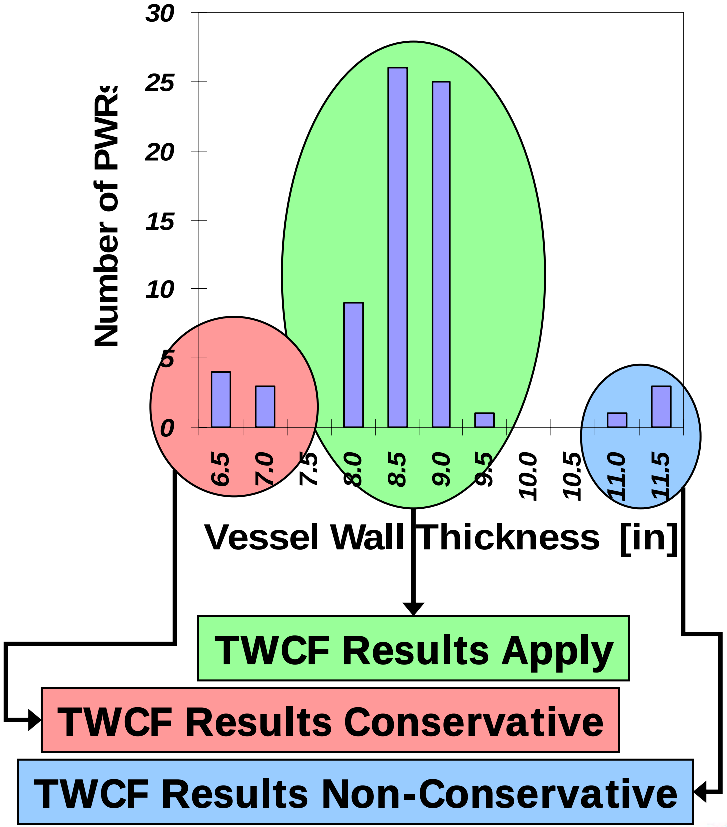
$K_{APPLIED}$ increases →

TWCF increases

Technical Basis

Generalization Results–Thickness Effect





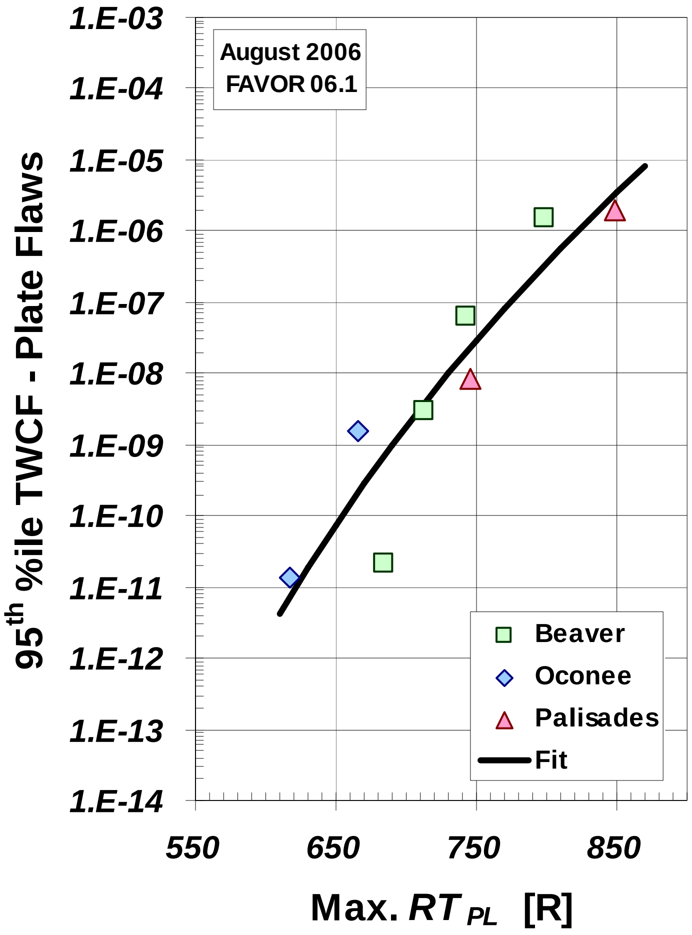
TWCF Results Apply

TWCF Results Conservative

TWCF Results Non-Conservative

Technical Basis

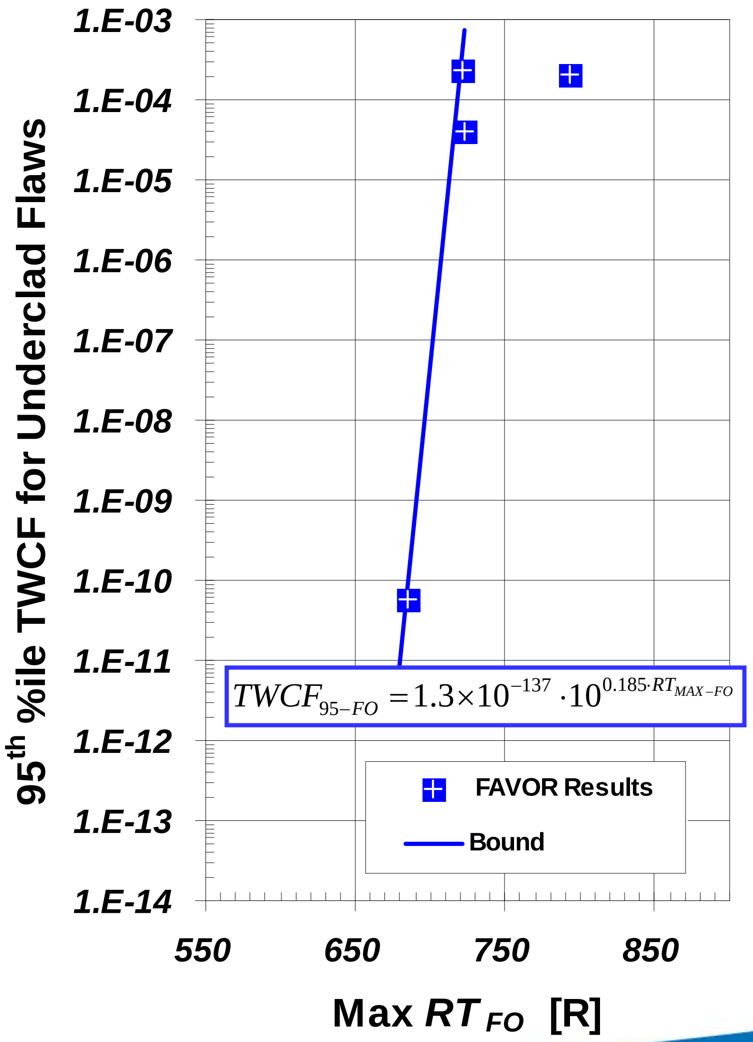
Generalization Results – Plants with Forgings



- Different flaw populations than plates or welds
  - Embedded flaws
    - Destructive evidence demonstrates similarity with embedded flaws in plates
    - Therefore use TWCF *vs*. $RT_{MAX\text{-}PL}$ relationship from baseline studies

Technical Basis

Generalization Results – Plants with Forgings



$$TWCF_{95-FO} = 1.3\times10^{-137}\cdot10^{0.185\cdot RT_{MAX-FO}}$$

- Different flaw populations than plates or welds
  - Sub-clad flaws
    - Forgings compliant with RG 1.43 should be have no sub-clad flaws
    - Occurrence depends on composition and on weld heat input
    - If sub-clad cracks occur they are
      - Perpendicular to the direction of cladding
      - Very dense
      - Extend to HAZ depth
    - Used conservative flaw distribution to quantify effect

Technical Basis

RT Limits – Overview

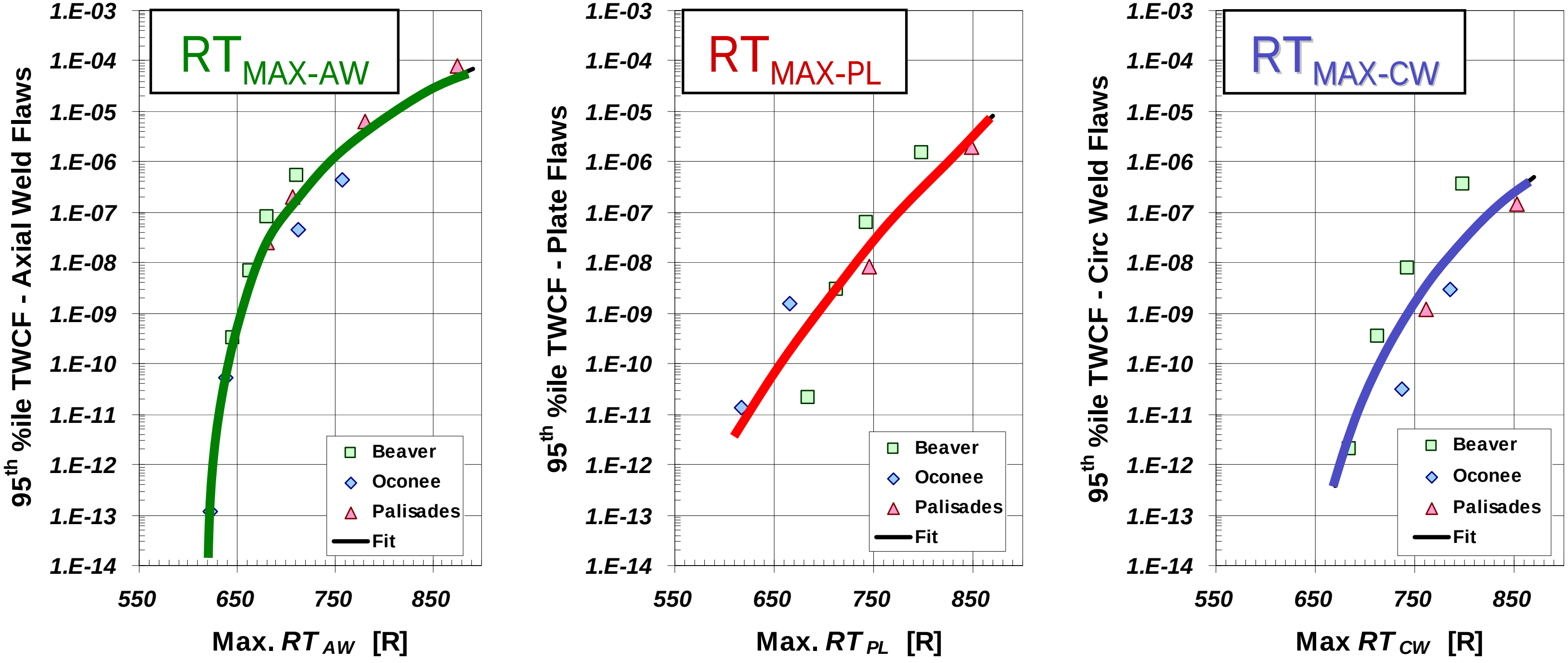
- Baseline plant results apply, with a few minor modifications, to all operating U.S. PWRs
  - Limited transient classes dominate TWCFs
    - 90%
      - Medium – large diameter primary-side pipe breaks
      - Stuck open primary valves that later re-close
    - < 10%
      - Main steam line breaks

  The characteristics of these severe transient classes are consistent across the PWR fleet

- **This understanding suggests that we may use baseline results to establish RT-based screening limits for *all* PWRs**

Technical Basis

RT Limits – Basic Idea



- Estimate total TWCF based on embrittlement level (based on RT_{MAX-AW}, RT_{MAX-PL}, and RT_{MAX-CW})
- Limit total TWCF to 10^{-6}

Technical Basis

RT Limits – Math

$$TWCF_{95-TOTAL} = 10^{-6} = \begin{bmatrix} \alpha_{AW} \cdot TWCF_{95-AW} + \\ \alpha_{PL} \cdot TWCF_{95-PL} + \\ \alpha_{CW} \cdot TWCF_{95-CW} + \\ \alpha_{FO} \cdot TWCF_{95-FO} \end{bmatrix}$$

$$TWCF_{95-AW} = \exp\{5.5198 \cdot \ln(RT_{MAX-AW} - 616) - 40.542\} \cdot \beta$$

$$TWCF_{95-PL} = \exp\{23.737 \cdot \ln(RT_{MAX-PL} - 300) - 162.38\} \cdot \beta$$

$$TWCF_{95-CW} = \exp\{9.1363 \cdot \ln(RT_{MAX-CW} - 616) - 65.066\} \cdot \beta$$

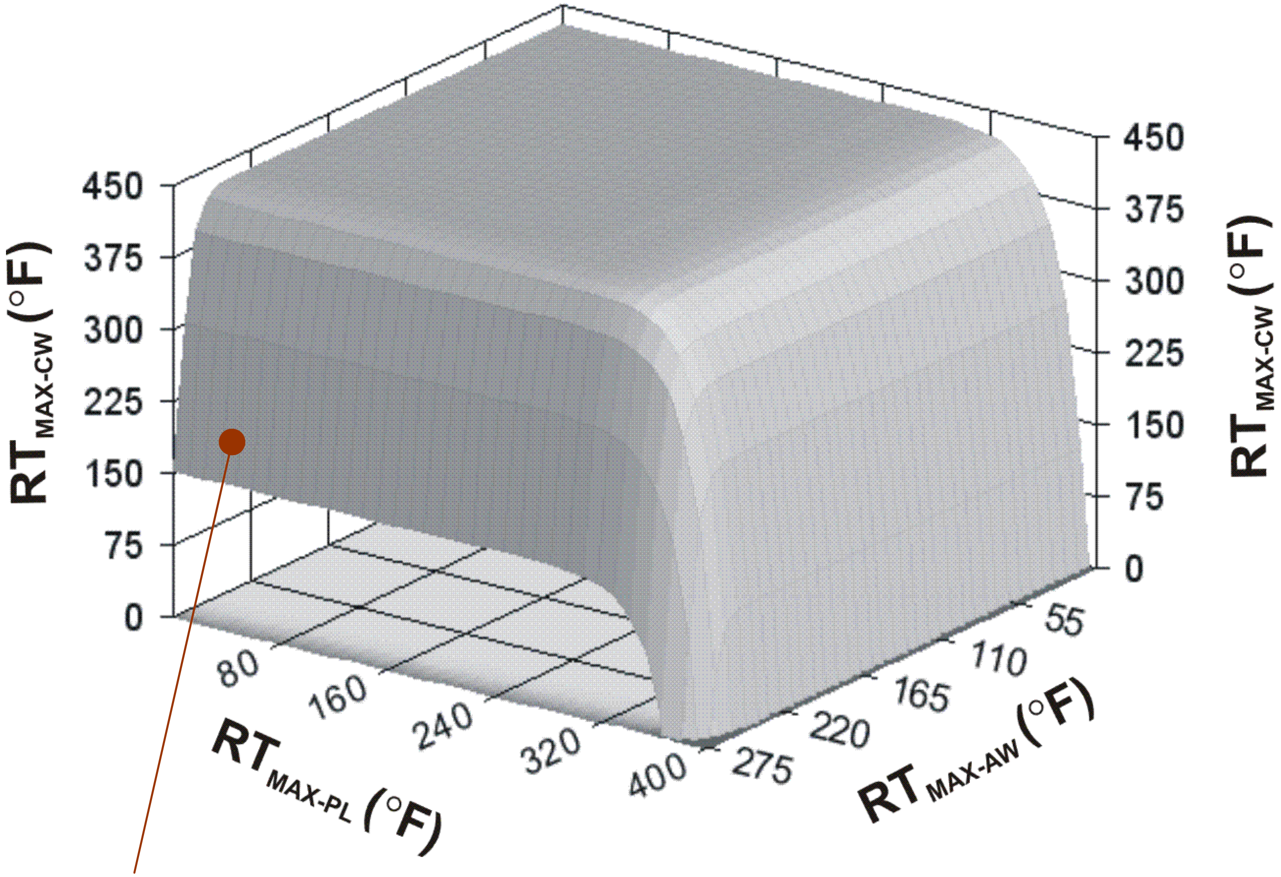
$$TWCF_{95-FO} = \exp\{23.737 \cdot \ln(RT_{MAX-FO} - 300) - 162.38\} \cdot \beta + \eta \cdot \{1.3 \times 10^{-137} \cdot 10^{0.185 \cdot RT_{MAX-FO}}\} \cdot \beta$$

Factor	Condition	Equation
Stuck-Open Valves α	$RT_{MAX-xx} \leq 625R$	$\alpha_{xx} = 2.5$
	$625R < RT_{MAX-xx} < 875R$	$\alpha_{xx} = 2.5 - \frac{1.5}{250}(RT_{MAX-xx} - 625)$
	$RT_{MAX-xx} \geq 875R$	$\alpha_{xx} = 1$
Vessel Thickness β	$T_{WALL} \leq 9\frac{1}{2}$ -in	$\beta = 1$
	$9\frac{1}{2} < T_{WALL} < 11\frac{1}{2}$ -in	$\beta = 1 + 8 \cdot (T_{WALL} - 9\frac{1}{2})$
	$T_{WALL} \geq 11\frac{1}{2}$ -in	$\beta = 17$
Sub-Clad Cracks η	Forging is compliant with Regulatory Guide 1.43	$\eta = 0$
	Forging not compliant with Regulatory Guide 1.43	$\eta = 1$

Vessel-specific information

Technical Basis

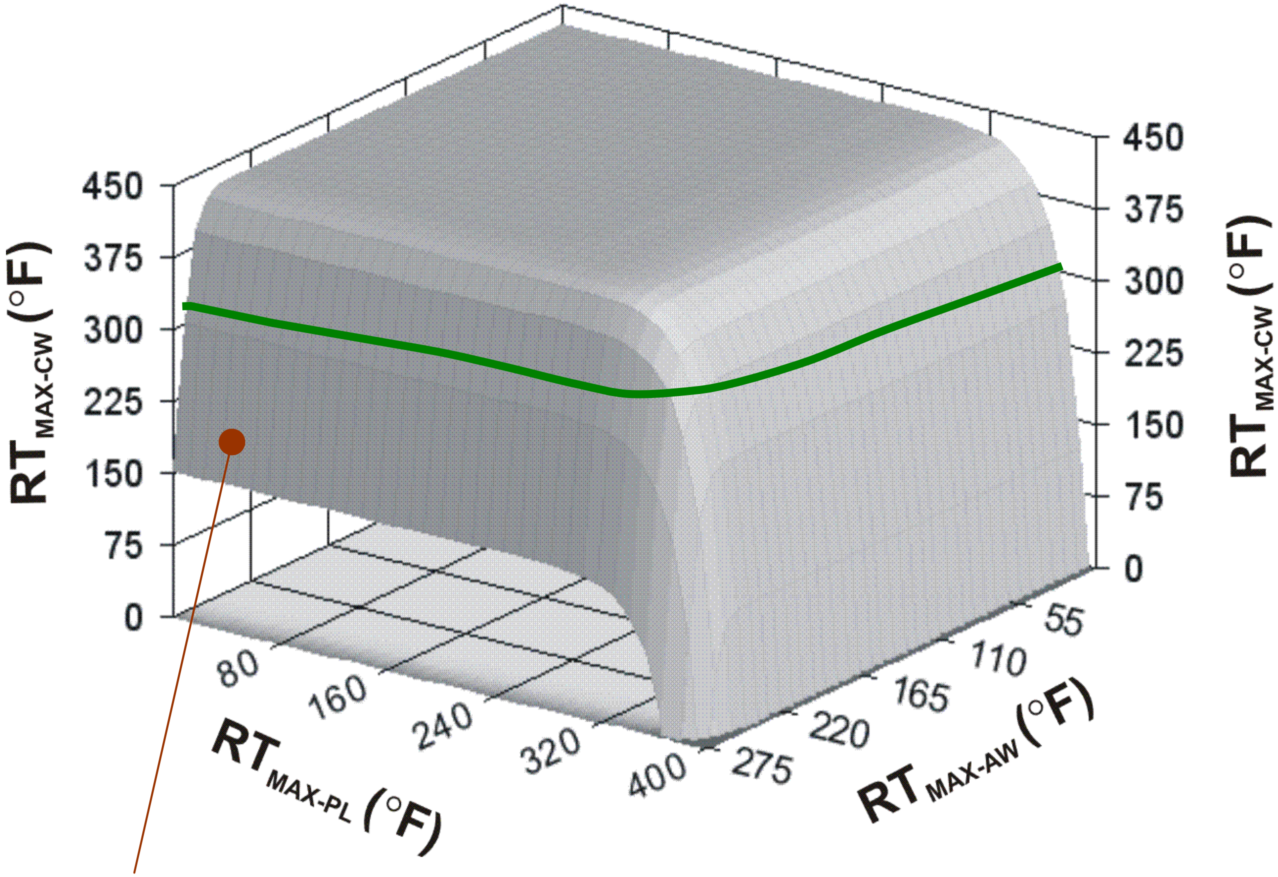
RT Limits – 3D Diagram for Plate Plants



On this surface TWCF = 10^{-6}/ry

Technical Basis

RT Limits – 3D Diagram for Plate Plants

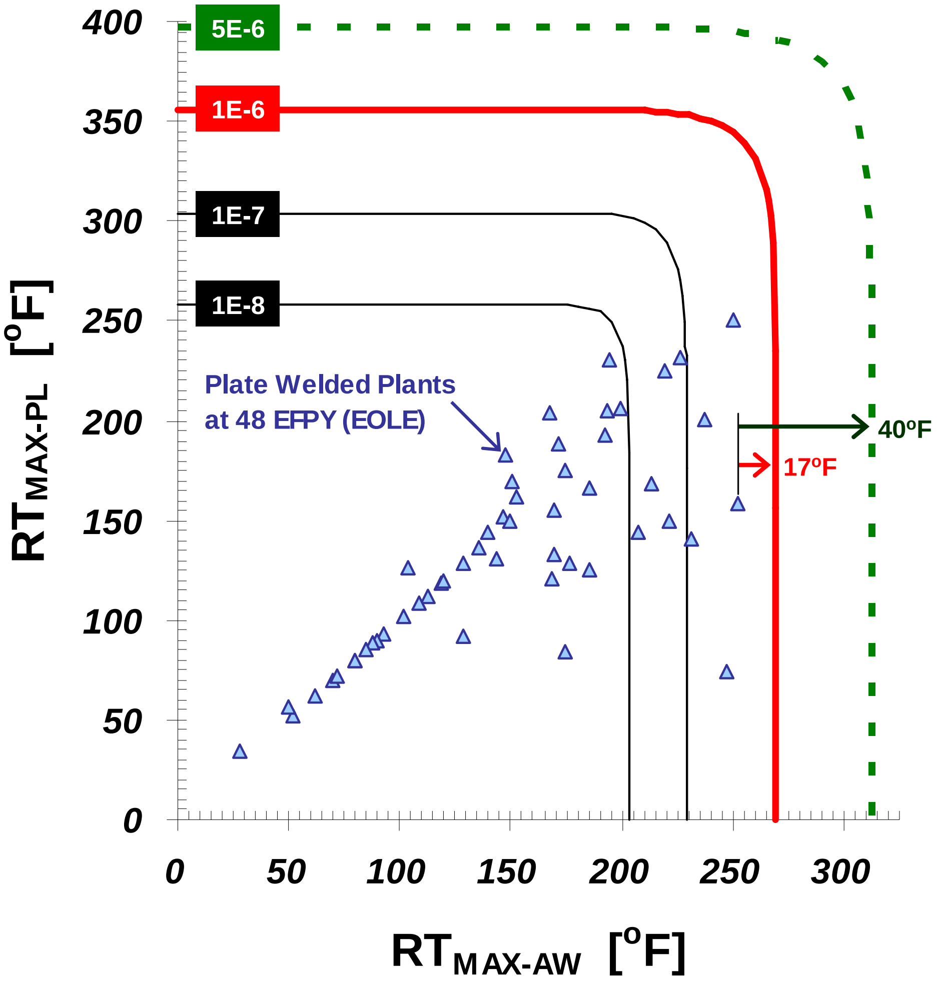


On this surface TWCF = 10^{-6}/ry

- Highest RT_{MAX-CW} at 60 years is 260°F, at which $TWCF_{CW} = 10^{-10}$
- To simplify surface into a plane for the assessment of plate plants, take a cutting plane at 312°F ($TWCF_{CW} = 10^{-8}$)

Technical Basis

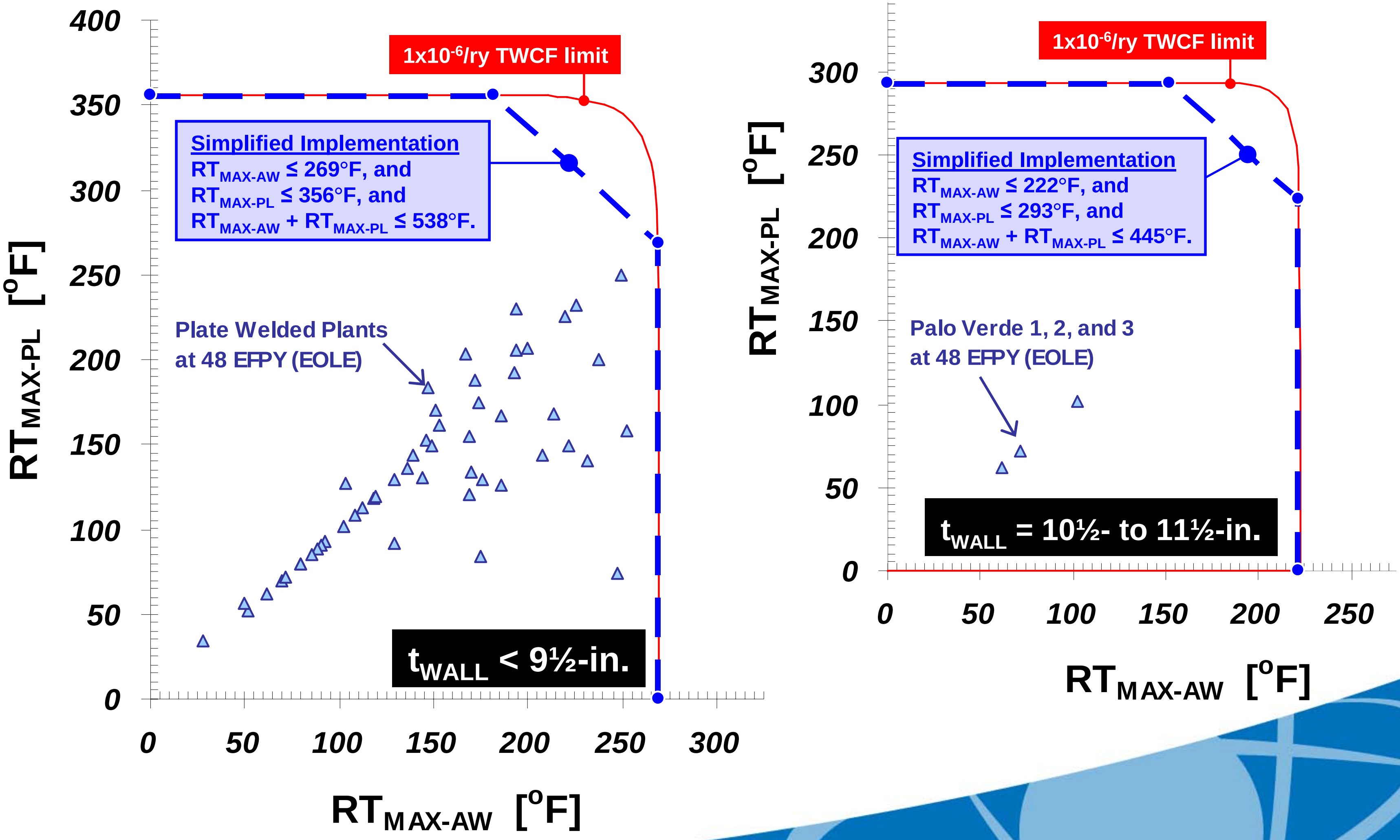
RT Limits – 2D Diagram for Plate Plants



- RT limit for axial weld flaws exceeds current limit by about 60°F
  - Depends on axial weld and plate properties
- RT limit for plate flaws exceeds current limit by about 150°F
  - Depends on plate properties only
- RT limit on circumferential weld flaws is not restrictive
  - Depends on circumferential weld and plate properties
- All plants well within 10^{-6} locus even after 60 years of operation

Technical Basis

RT Limits – Implementation for Plate Plants



1×10^{-6}/ry TWCF limit

Simplified Implementation
$RT_{MAX\text{-}AW}$ ≤ 269°F, and
$RT_{MAX\text{-}PL}$ ≤ 356°F, and
$RT_{MAX\text{-}AW} + RT_{MAX\text{-}PL}$ ≤ 538°F.

Plate Welded Plants
at 48 EFPY (EOLE)

t_{WALL} < 9½-in.

1×10^{-6}/ry TWCF limit

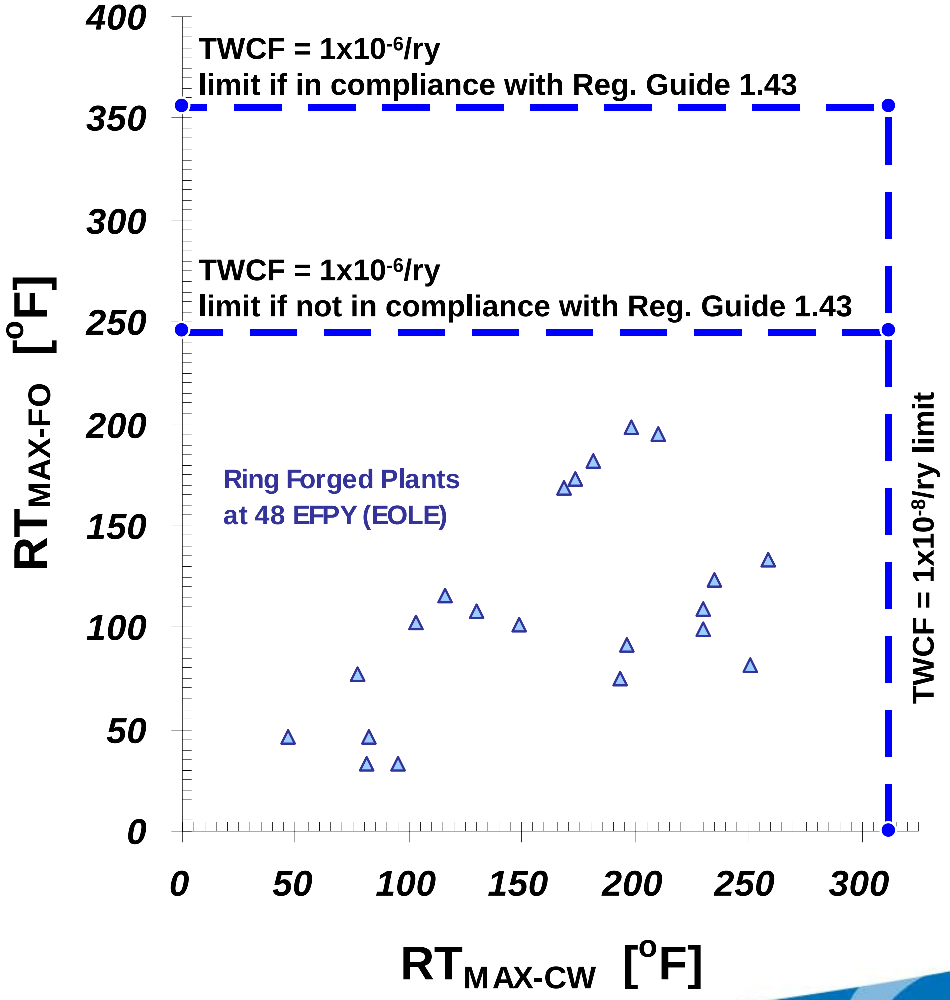
Simplified Implementation
$RT_{MAX\text{-}AW}$ ≤ 222°F, and
$RT_{MAX\text{-}PL}$ ≤ 293°F, and
$RT_{MAX\text{-}AW} + RT_{MAX\text{-}PL}$ ≤ 445°F.

Palo Verde 1, 2, and 3
at 48 EFPY (EOLE)

t_{WALL} = 10½- to 11½-in.

Technical Basis

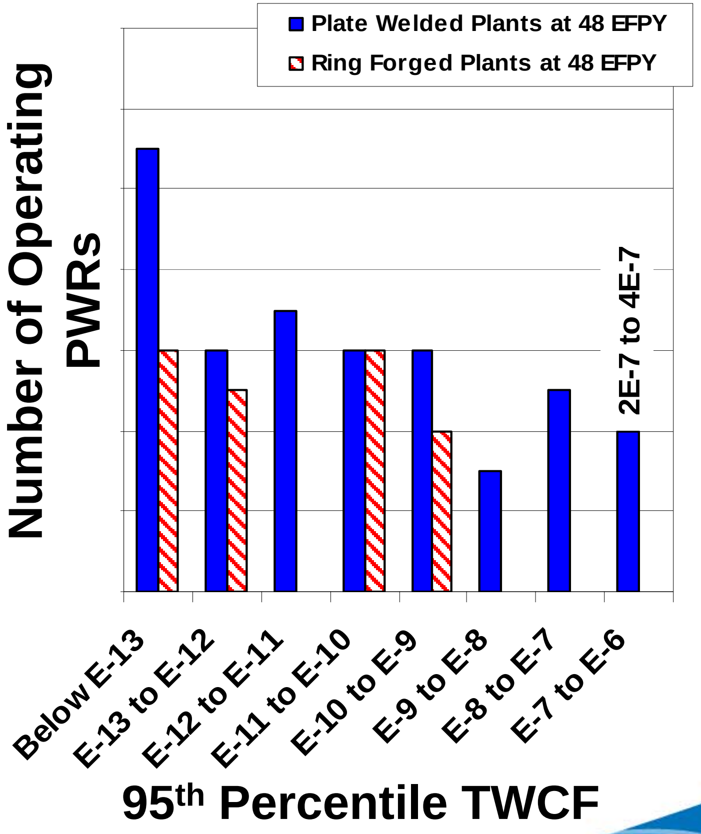
RT Limits – Implementation for Forgings



Technical Basis

RT Limits – Summary

- New limits expressed in two equivalent forms
  - <u>Limits on TWCF</u>: 10^{-6}/ry
  - <u>Limits on RT</u>: Considerably less restrictive than current rule limits
- Limits apply to all currently operating U.S. PWRs
- All plants assessable based only on available materials and fluence information
- All PWRs meet limits, even through 60 years of operation



Technical Basis

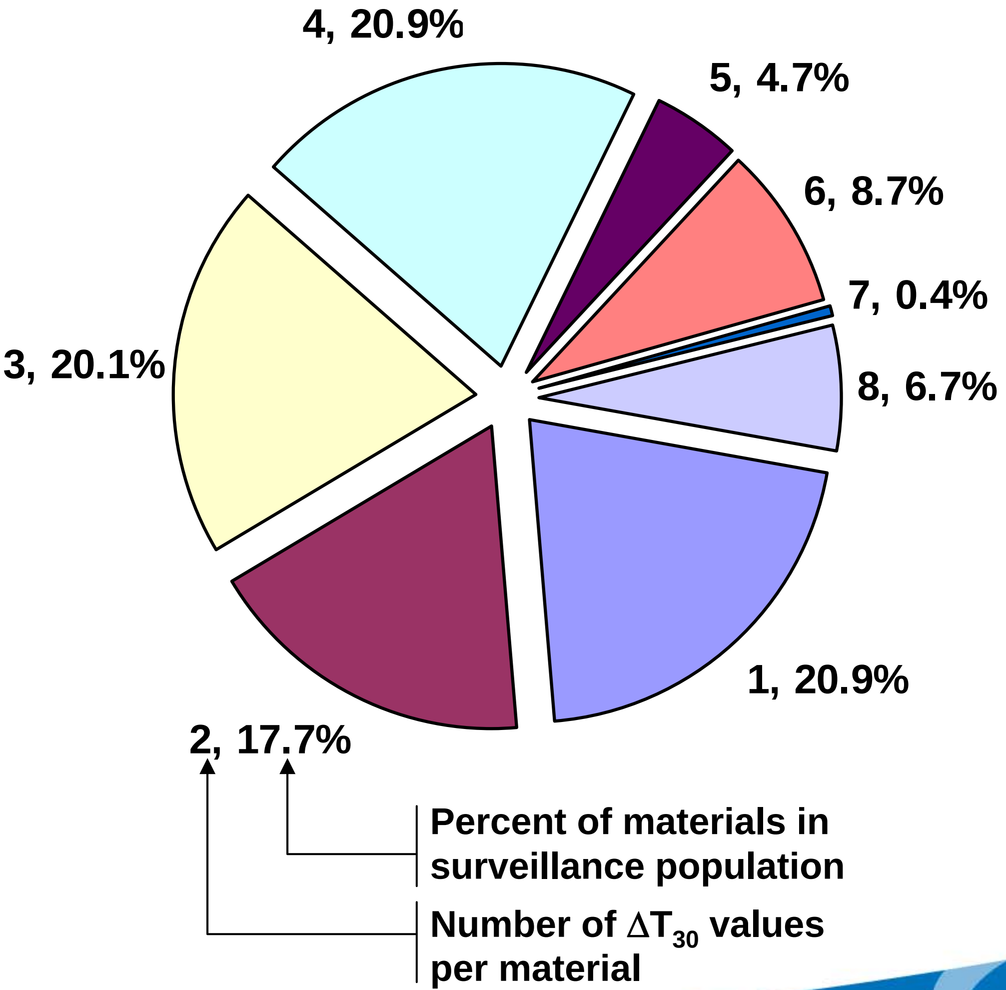
Surveillance Check (Current Rule)

- 10 CFR 50.61 requires that the generic ΔT_{30} embrittlement trend curve be modified if credible plant-specific surveillance data is available

- Rationale
  - Ensure that no plant- or material-specific trends are missed
  - Protects against extrapolation outside of the database used to calibrate the generic ΔT_{30} embrittlement trend curve

Technical Basis

Surveillance Check (Data Available)



Percent of materials in surveillance population

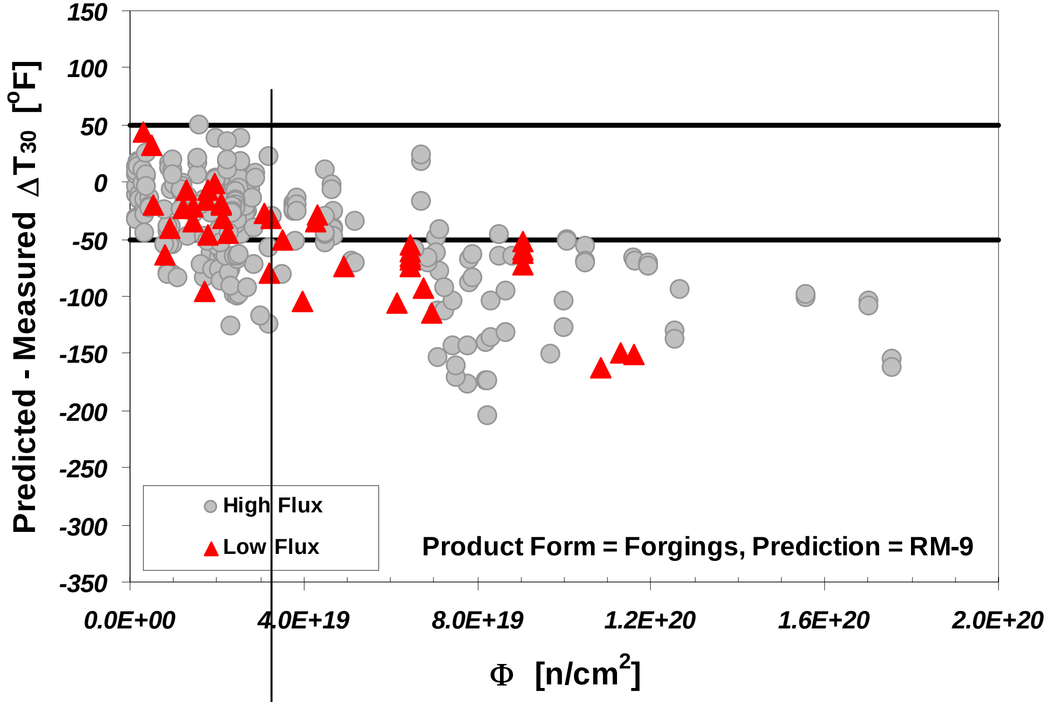
Number of ΔT_{30} values per material

- Only limited observations of ΔT_{30} are currently available
  - Compliant with
    - 10 CFR 50 Appendix H
    - ASTM E185

Technical Basis

Surveillance Check (Alternate Rule)

- Surveillance check is retained in alternate rule
- Rationale:
    1. Ensure that no plant- or material-specific trends are missed
    2. Protection against extrapolation outside of the database used to calibrate the generic ΔT_{30} embrittlement trend curve
- Retention of check motivated mostly by #2 at this time



Generic trend curve under-predicts (non-U.S.) ΔT_{30} data at high fluences

Technical Basis

Surveillance Check (Alternate Rule)

Type A

(measurements uniformly offset from ETC)

Type B

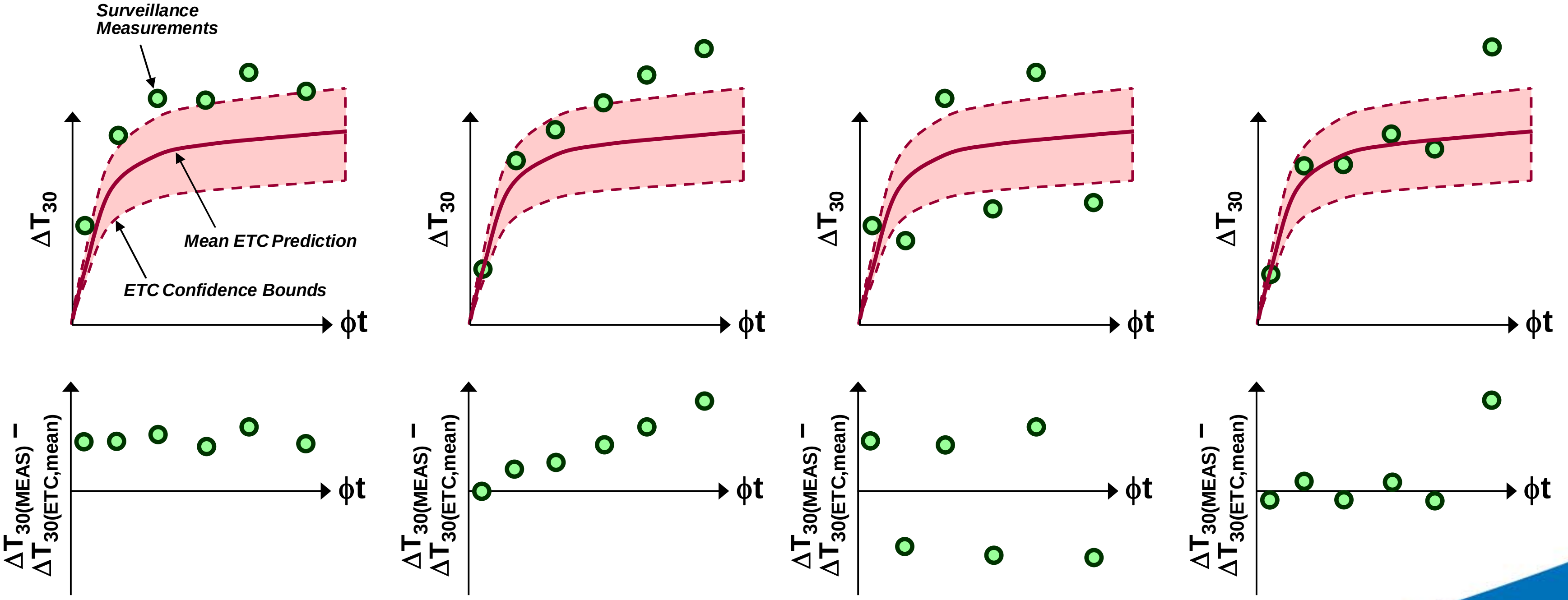
(measurements diverge from ETC; different fluence trend)

Type C

(measurements have more uncertainty than ETC calibration data)

Type D

(one measurement offset from ETC)



Scope of check expanded in Alternate Rule

Technical Basis

Surveillance Check (Alternate Rule)

Type A

(measurements uniformly offset from ETC)

Type B

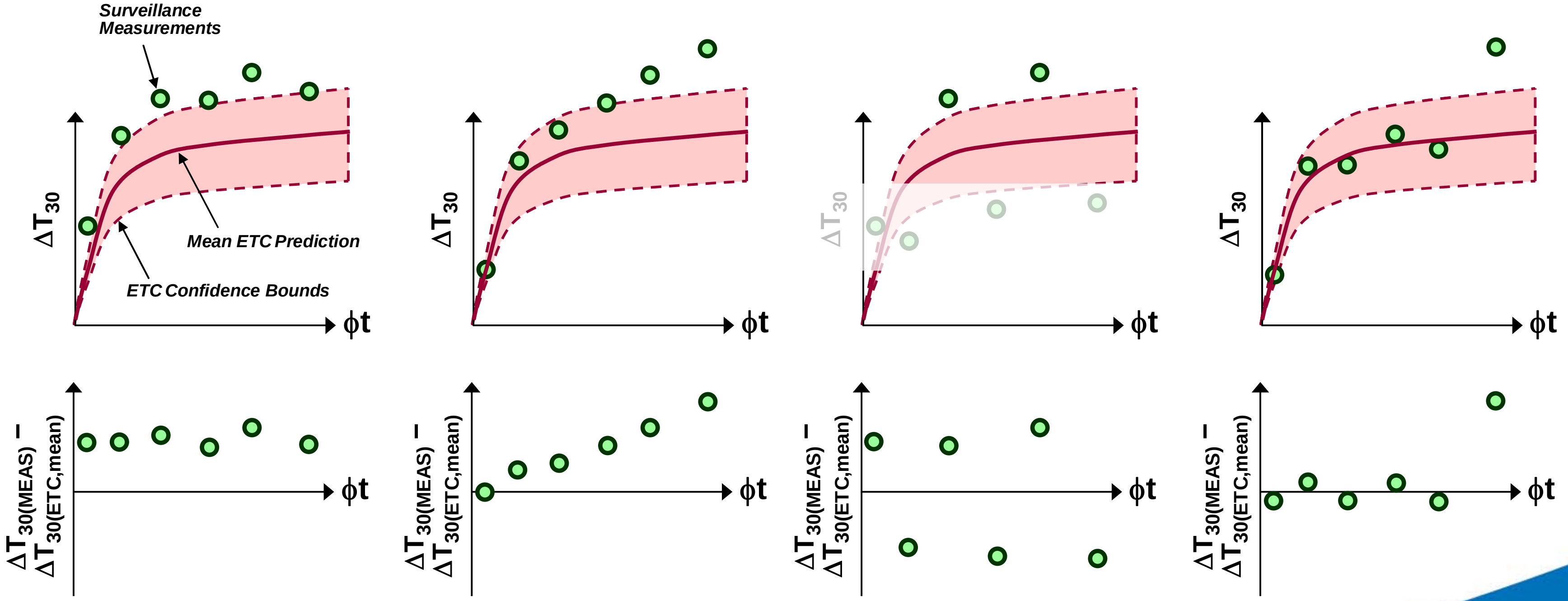
(measurements diverge from ETC; different fluence trend)

Type C

(measurements have more uncertainty than ETC calibration data)

Type D

(one measurement offset from ETC)



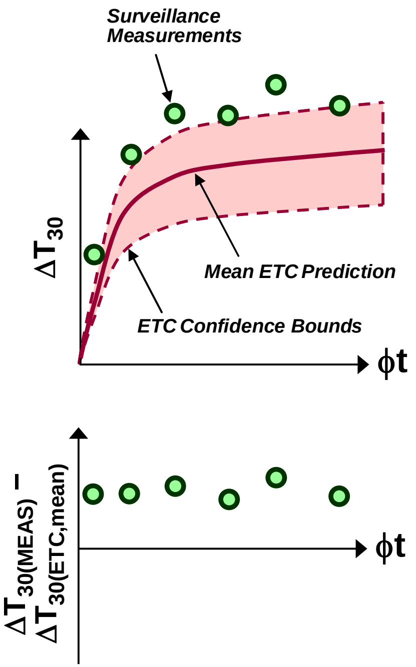
Only types A, B, and D adopted

Technical Basis

Surveillance Check (Alternate Rule)

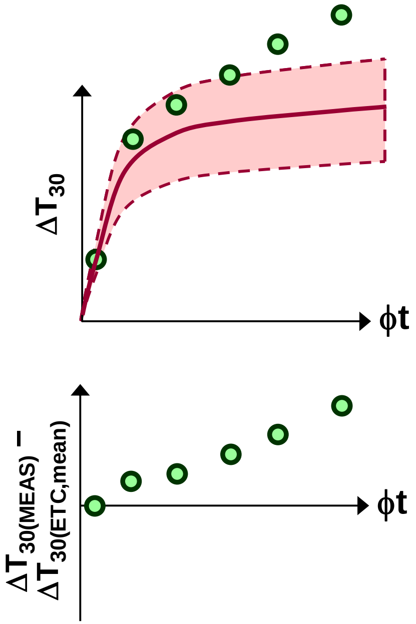
Type A

Mean Test



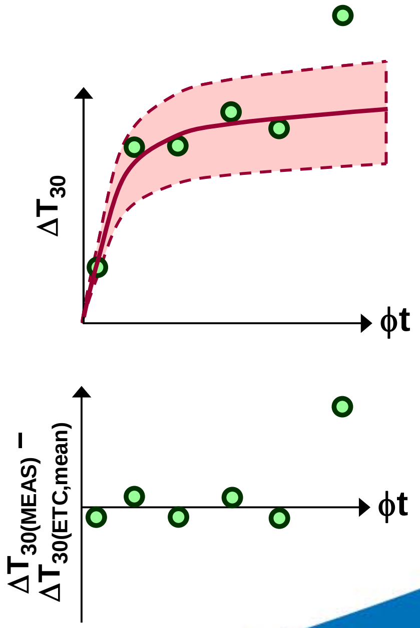
Type B

Slope Test



Type D

Outlier Test



Only types A, B, and D adopted

Technical Basis

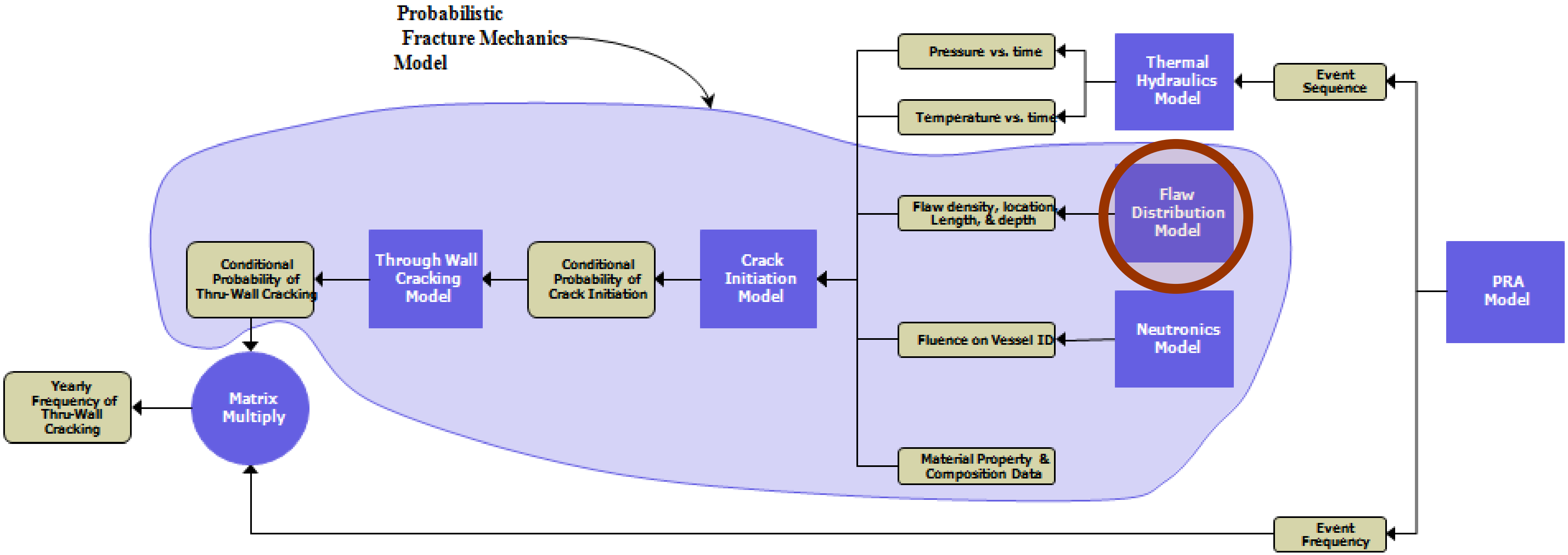
Surveillance Check (Alternate Rule) - Implementation

- Mean, slope, and outlier tests are required for all surveillance data sets with 3 or more ΔT_{30} values
  - Check only for non-conservative predictions
  - <u>All tests passed</u>: Use generic ΔT_{30} values
  - <u>1 test failed</u>: Submit recommended treatment to Director of NRR for approval
- Approach recognizes that
  - Standard and accepted procedures **to assess the statistical significance** of differences between individual data sets and models exist
  - Standard and accepted procedures **to assess the practical importance** of such differences are not available

Technical Basis

Inspection Requirements

- Flaw distribution model is a major input when estimating the TWCF



Technical Basis

Inspection Requirements

- Flaw distribution:
  - Size
  - Density

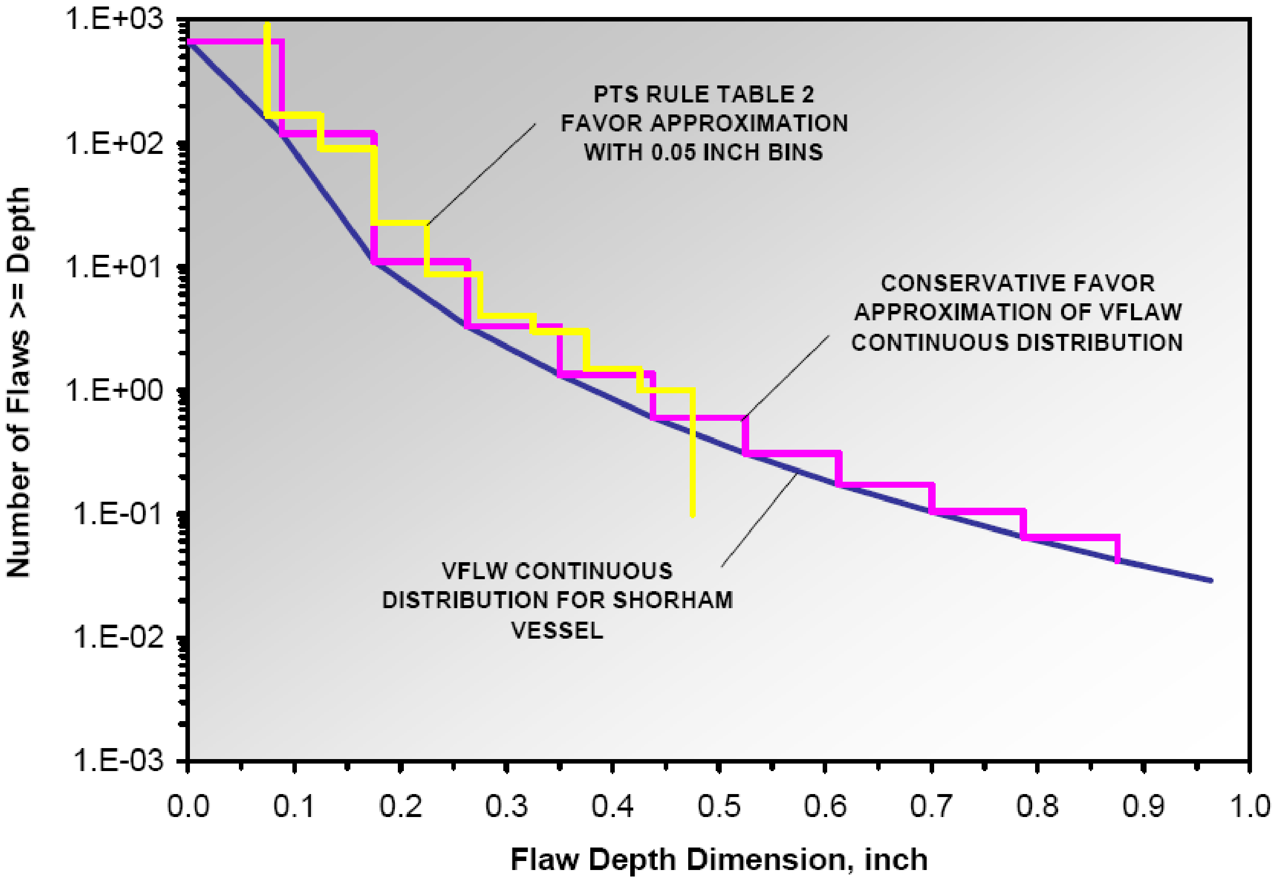
  effects significantly the predicted TWCF

- Empirical basis for distribution
  - Vastly better than for models used in 1980s
  - Still based on an examination of limited material

- Prudent to check (compare) flaw distribution model to vessel-specific flaws detected by ISI

Technical Basis

Development of Flaw Tables (Alternate Rule)



Data from destructive evaluation of vessels at PNNL used as input to FAVOR, and as basis for flaw tables in current rule.

Alternate PTS Rule

Overview

- Published in the *Federal Register*
  - Proposed Rule: October 3, 2007
  - Supplemental Proposed Rule: August 11, 2008

- PWR licensees can voluntarily choose to apply the requirements of this rule

Alternate PTS Rule

Entry Condition – ISI Data Assessment

- Analysis of ISI data
  - Determine if flaws in the RPV beltline are within the limits of the rule
    - Yes: Rule applies
    - No: Demonstrate that the flaws do not result in an unacceptable risk of RPV failure
- Incorporates volumetric examination methods and procedures required by the ASME Code

Alternate PTS Rule

Entry Condition – ISI Data Assessment

- Alternate rule:
  - Contains flaw limits on size and density for flaws within 1” of the clad/steel interface or 10% of the wall thickness, whichever is greater
    - Limit more restrictive than ASME Code requirement
  - Requires licensees to determine if flaws at clad-steel interface have penetrated through clad and open to inside surface
    - No ASME Code requirement to perform this inspection
  - Requires licensees to confirm that flaws between clad/steel interface and 3/8 of the wall thickness meet ASME Code requirements

- If ISI limits on flaw size, density and location are not met, a quantitative or qualitative analysis can be submitted for NRC approval

Alternate PTS Rule

Required Analyses

- Analysis of reference temperature (embrittlement)
  - Projected RT_{MAX} values are calculated in accordance with the rule including evaluating the effect of surveillance data
  - Separate RT_{MAX} values are calculated for axial welds, circumferential welds, plates and forgings
- Compare RT_{MAX} values to screening limits provided in the rule
  - Screening limits account for effects of uncertainties; therefore, margin is <u>not</u> included in the RT_{MAX} calculation
  - Screening limits contain combination of RT_{MAX} values for plates, forgings and welds to ensure TWCF for the entire vessel is below the risk limit
- Screening criteria in the rule is expected to limit the TWCF to 10^{-6}/ry or less

Alternate PTS Rule

Calculation of RT_{MAX}

- RT_{MAX} is calculated for each beltline weld, plate and forging

- RT_{MAX} is the sum of the unirradiated reference temperature and the increase in the 30 ft-lb temperature (ΔT_{30}) resulting from neutron irradiation

- For welds, the RT_{MAX} is the higher of the RT_{MAX} for the weld and the adjacent base material (plate or forging)

Alternate PTS Rule

Revised Embrittlement Trend Curve

- Rule contains a prescriptive methodology for calculating ΔT_{30} which is based on its neutron fluence, neutron flux, Cu, Ni, P, Mn content, product form, cold leg temperature and vessel manufacturer

- Rule requires licensees to utilize the methodology in the rule to calculate ΔT_{30} unless plant-specific data fails any of the surveillance data statistical tests in the rule.

Alternate PTS Rule

Plant-Specific Surveillance Data

- Evaluated using three statistical tests (mean test, slope test, and outlier test) to determine if the ΔT_{30} values calculated using the embrittlement correlation should be adjusted

- If surveillance data fails any of the tests, an evaluation of the data and its impact on the proposed ΔT_{30} and the proposed RT_{MAX} values is required

- Rule does not contain a prescriptive methodology for calculating ΔT_{30} when plant-specific data is used

Alternate PTS Rule

Required Analyses

- If screening criteria cannot be satisfied, licensees must submit a safety analysis to determine:
  - What, if any, modifications to equipment, systems, and operations are necessary to prevent potential failure of the RPV as a result of PTS events
  - Whether thermally annealing the RPV will result in projected values of RT_{MAX} for all RPV beltline materials at the end of license that meet the screening criteria

Alternate PTS Rule

Required Analyses

- Supplemental proposed rule
  - Applicability
    - Limited to PWRs with operating licensees issued prior to the effective date of the final rule, and Watts Bar Unit 2
  - Surveillance data check
    - Added slope and outlier test to identify whether data at higher neutron fluence levels suggest an embrittlement rate greater than that described in the rule
  - NDE uncertainty
    - NRC considering whether to permit flaw sizes to be adjusted to account for the effects of sizing error when the estimated flaw size and density in the RPV beltline is compared to the size and density limits

Alternate PTS Rule

Conclusion

- Proposed rule provides an alternate method for licensees to demonstrate that the risk from PTS is low throughout their extended operating period
- The alternate PTS rule is needed for
  - reactor vessels that are projected to exceed the screening criteria in the current PTS rule prior to the end of their first renewed licenses
  - reactor vessels that are projected to be below the screening criteria in the current PTS rule through the end of their first renewed licenses, but which may request power uprates

Alternate PTS Rule

Conclusion

- Staff analyses have removed unnecessary conservatisms in the current PTS rule
- Implementation of the alternate rule will reduce the burden on the NRC and licensees and eliminates an unnecessary impediment to license renewal
- All operating reactors are projected to be below the alternate PTS rule screening criteria at the end of their first renewed licenses and should have adequate margins to permit power uprates

Alternate PTS Rule

Schedule

- Comment period for supplemental proposed rule closed
  - September 10, 2008
  - NRC currently evaluating comments received
- Commission review of Final Rule
  - April 2009
- Publish Final Rule
  - July 2009

Alternate Fracture Toughness Requirements for Protection against Pressurized Thermal Shock (PTS) Events Rule
(10 CFR 50.61a)

NUCLEAR REGULATORY COMMISSION

10 CFR Part 50

RIN 3150–AI01

[NRC–2007–0008]

Alternate Fracture Toughness Requirements for Protection Against Pressurized Thermal Shock Events

AGENCY: Nuclear Regulatory Commission.

ACTION: Supplemental Proposed Rule.

ACRS Subcommittee Meeting
October 1, 2008

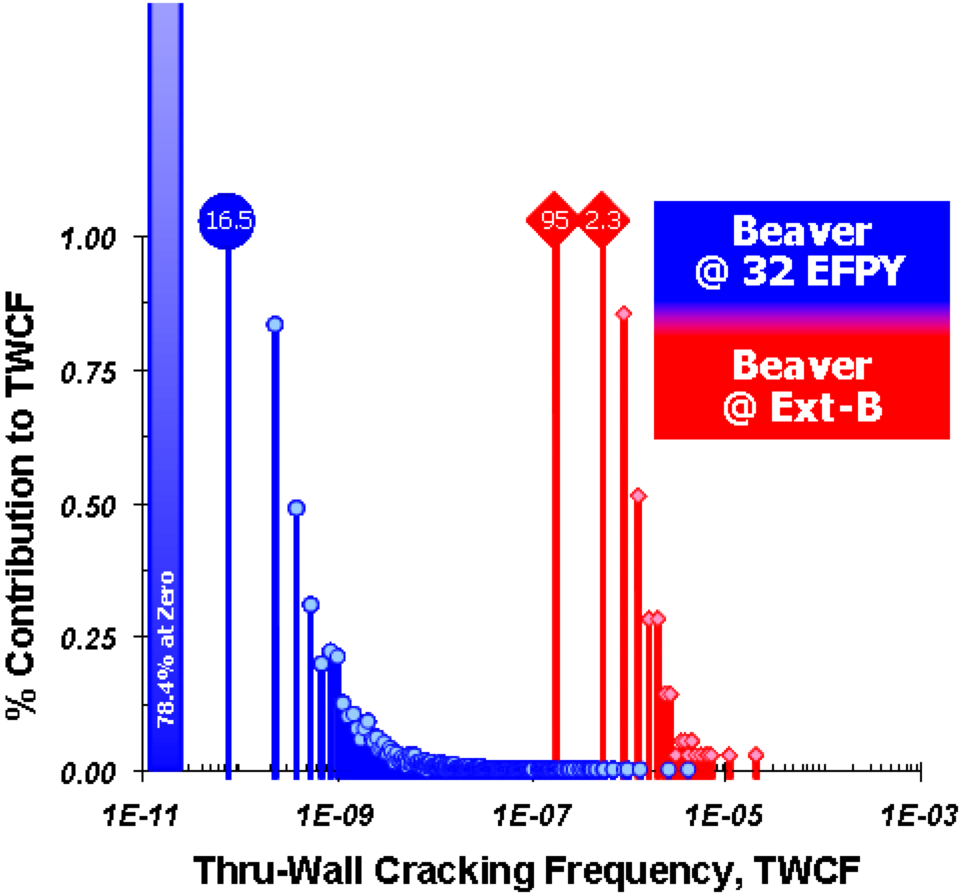
U.S.NRC
United States Nuclear Regulatory Commission
Protecting People and the Environment

Current PTS Rule

Plant	Limiting Material	RT_{PTS} Value per Current PTS Rule (°F)	RT_{MAX} Value per Voluntary PTS Rule (°F)	Difference between PTS screening criteria and RT_{PTS} value per Voluntary PTS Rule (°F)
Beaver Valley 1	Plate	**290**	212	144
Beaver Valley 1	Plate + Axial Weld	------	424	114
Palisades	Axial Weld	**287**	220	49
Palisades	Plate + Axial Weld	------	408	130
Palisades	Circumferential Weld	**302**	215	97
Point Beach 2	Circumferential Weld	**315**	245	67
Three Mile Island 1	Axial Weld	**289**	187	82
Three Mile Island 1	Plate + Axial Weld	------	271	267
Three Mile Island 1	Circumferential Weld	**316**	198	114
Indian Point 3	Plate	**280**	249	107
Indian Point 3	Plate + Axial Weld	-------	498	40
Salem 1	Axial Weld	**278**	234	35
Salem 1	Plate + Axial Weld	-----	468	70
Surry 1	Axial weld	269	195	74
Surry 1	Plate + Axial weld	-----	353	185
Kewaunee	Circumferential Weld	296	252	60
Point Beach 1	Circumferential Weld	299	243	69
Turkey Point 3	Circumferential Weld	297	233	79
Oconee 2	Circumferential Weld	297	191	121

Technical Basis

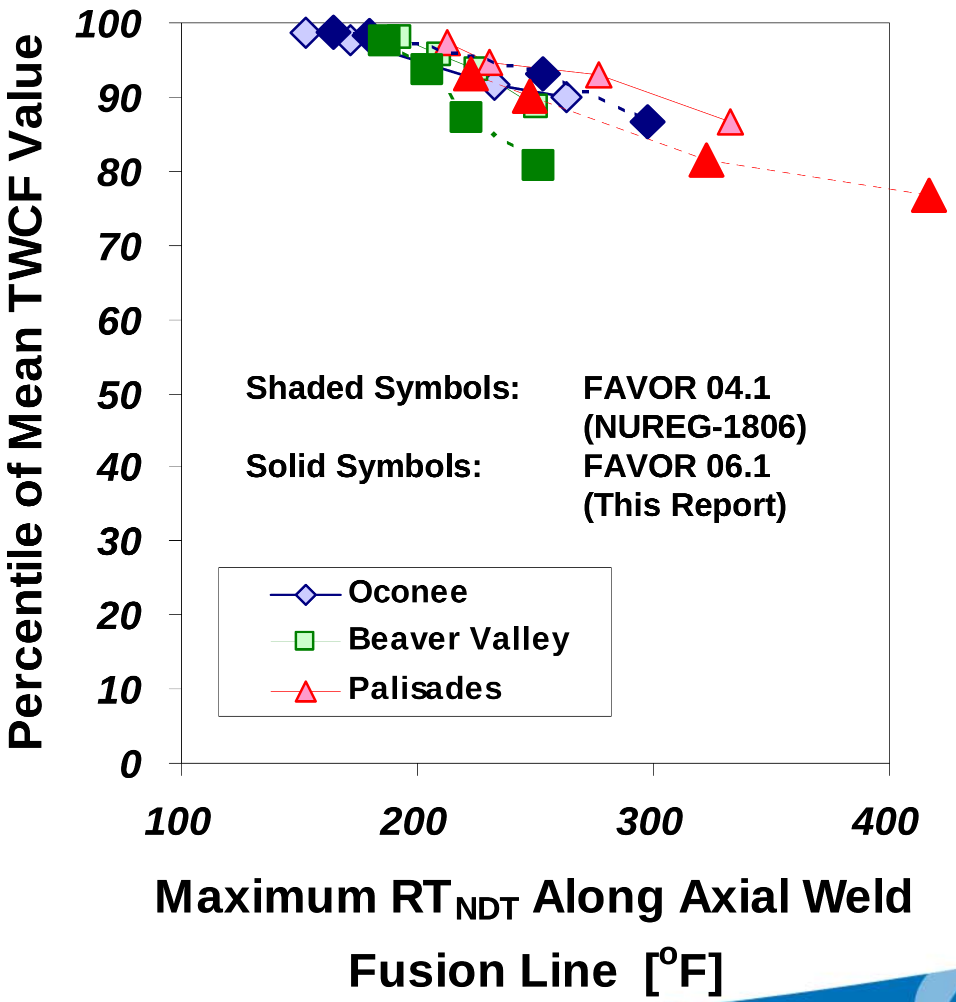
How Uncertainties Impact the Results



- Distribution of TWCF highly skewed toward zero

Technical Basis

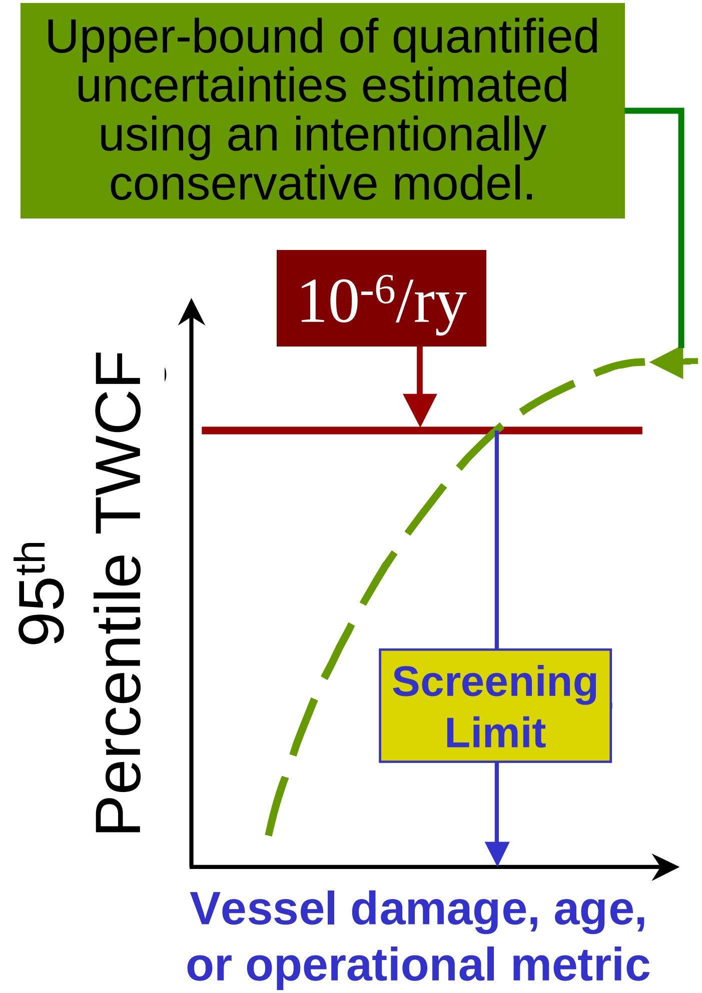
How Uncertainties Impact the Results



- Distribution of TWCF highly skewed toward zero
- Mean of distribution corresponds to a
  - High percentile that
  - Changes systematically with embrittlement

Technical Basis

How Uncertainties Impact the Results



- Distribution of TWCF highly skewed toward zero
- Mean of distribution corresponds to a
  - High percentile that
  - Changes systematically with embrittlement
- 95th percentile of TWCF used to establish RT-limits

Technical Basis

Incompleteness Uncertainty Addressed

- Reviews
  - Process for model building included reviews and vetting by the development team
  - Peer review of model components in many professional journal articles
  - V&V of computer codes used
  - Explicit reviews performed by
    - ACRS
    - Independent expert panel
    - NRR / NRO
    - Public comments

- Known conservatisms left in models and input data
- Upper bounds (95th percentile) used to establish screening limits

- Method of implementation: 10 CFR 50.61a establishes a low-probability screening limit that triggers compensatory measures

Technical Basis

Surveillance Check (Alternate Rule)

- All surveillance data reported through ≈2003 checked in ML801290654 relative to mean, slope, and outlier tests

Plant Name	Product Form	Number of ΔT_{30} Values	Heat Fails these Deviation Tests		
			A Mean Test	B Slope Test	D Outlier Test
San Onofre 3	Plate	3	FAIL		FAIL
D.C. Cook 2	Plate	8	FAIL		
Beaver Valley 1	Plate	8	FAIL		FAIL
Callaway	Weld	4	FAIL		FAIL
Surry 1	Weld	3	FAIL		FAIL
Indian Point 2	Plate	3	FAIL		
Sequoyah 1	Forging	8	FAIL		
Sequoyah 1	Weld	4	FAIL		
Sequoyah 1	Weld	4			FAIL

Alternate PTS Rule

Screening Criteria

Table 1 – PTS Screening Criteria

Product Form and RT_{MAX-X} Values	RT_{MAX-X} Limits [°F] for Different Vessel Wall Thicknesses[6] (T_{WALL})		
	$T_{WALL} \leq 9.5$in.	9.5in. $< T_{WALL} \leq 10.5$in.	10.5in. $< T_{WALL} \leq 11.5$in.
Axial Weld, RT_{MAX-AW}	269	230	222
Plate, RT_{MAX-PL}	356	305	293
Forging without underclad cracks, RT_{MAX-FO}	356	305	293
Axial Weld and Plate, $RT_{MAX-AW} + RT_{MAX-PL}$	538	476	445
Circumferential Weld, RT_{MAX-CW}[7]	312	277	269
Forging with underclad cracks, RT_{MAX-FO}	246	241	239

[6] Wall thickness is the beltline wall thickness including the clad thickness.

[7] RT_{PTS} limits contributes 1×10^{-8} per reactor year to the reactor vessel TWCF.

Alternate PTS Rule

Flaw Size and Density Limits

Table 2 - Allowable Number of Flaws in Welds

Through Wall Extent, TWE [in.]		Maximum number of flaws per 1000-inches of weld length in the inspection volume that are greater than or equal to TWE_{MIN} and less than TWE_{MAX}
TWE_{MIN}	TWE_{MAX}	
0	0.075	No Limit
0.075	0.475	166.70
0.125	0.475	90.80
0.175	0.475	22.82
0.225	0.475	8.66
0.275	0.475	4.01
0.325	0.475	3.01
0.375	0.475	1.49
0.425	0.475	1.00
0.475	Infinite	0.00

(similar format, different numbers, for plate flaws)