DR. RANSOM: Well, one thing along that line I didn't understand, reading this document it seemed like often times you were feeding the FAVOR the average of wall temperature over maybe periods like 10,000 seconds.

And I don't quite understand. Maybe I misunderstood something, but it seemed like often times you were extracting out of the RELAP five runs an average wall temperature over a long period of time.

MR. BESSETTE: No, actually what we feed FAVOR are, or what we have fed FAVOR is points every 30 seconds. What that 10,000 second you are referring to is like a screening step that University of Maryland used in looking at the results.

DR. RANSOM: What, you go through a preliminary kind of screening and then --

MR. BESSETTE: Preliminary, yes.

DR. RANSOM: -- select the worst --

MR. BESSETTE: Yes, it was just used, so it was just used as a screening step. It was never fed into FAVOR. What we feed into FAVOR is 30 second intervals.

DR. WALLIS: Well, I think, David,

actually the picture is much better than it may appear from the questioning. I think probably there is a really good case that you have. It just needs to be presented in a more convincing way. That's all.

MR. HACKETT: I think, Dr. Wallis, we agree. I don't think we are prepared to go into that in detail today, obviously, in terms of FAVOR. In your proposal to maybe, you have been through these before with the comparisons, say with ROSA and MIST.

DR. WALLIS: They don't determine anything.

MR. HACKETT: Maybe we could just go to the PFM --

DR. WALLIS: I mean you see that there are curves and yes there are some wiggles are not explained, but we don't know what that means.

MR. HACKETT: We do not right now.

DR. WALLIS: And the problem with the NUREG is that at the end of Section 3.1 it says that assessment results confirm the applicability of RELAP V to analyze PTS transients. Well, yeah, that's okay.

And to establish the validity of uncertainty studies. Now there's no uncertainty study presented, so I don't know what that means. Because I don't, I don't know what's being established as valid because there is no uncertainty study in the report.

So, that's the basic problem we have, I think. And maybe you can clear that up?

MR. HACKETT: No, I think what that boils down to is a fairly major take away for us. And, as I stated earlier, this is by no means a final product at this point. And that was one of the, the sensitivity analyses that needs to be, that needs to be further explored and finalized.

So, what I would propose at this point, since we don't want to waste the Committee's time in that regard, these are results that have been shared --

DR. WALLIS: Well, maybe this is like Number 40 or something that's good. I mean it's talking about differences between RELAP V and experiment. What are the kinds of errors. That is actually, is that something that's new?

MR. BESSETTE: Yes, that's just something we did after the December 11th meeting.

DR. WALLIS: Does that help us then with this conversation?

MR. BESSETTE: To some extent. It's not, I would say again, it's not, it can't be the final word because the final word is only obtained after you run these results through FAVOR.

But what this says, though, is that, in terms of stand alone RELAP assessment, you get very good agreement between RELAP and the data for these principle parameters.

So in one case, for example, for ROSA AP-CL-09, you have a bias of zero with a substandard deviation. So you say --

DR. WALLIS: It could be zero even if you have a huge variation.

MR. BESSETTE: That's true. That's where the standard deviation comes in.

DR. RANSOM: Are these means over time? These are means over time?

MR. BESSETTE: This is over the time of the whole transient. So, basically what this says is I can't conceive of doing any better than this with a thermal hydraulic code.

DR. WALLIS: The question is, is it good enough?

DR. RANSOM: Well, I think, too, there may be confusion in the report between sensitivity and uncertainty. You know, I think you did some sensitivity studies to see how much variation you would expect in the parameters.

But that doesn't necessarily answer these questions with regard to uncertainty. That's more of a probabilistic question.

MR. BESSETTE: Well, so, you know, the final, when you get, when you see the final answers you get from FAVOR with the mean and some 95th percentile, those incorporate, quote, the thermal hydraulic uncertainties.

This thermal hydraulic uncertainties are in that uncertainty bin. How do we get these thermal hydraulic uncertainties is, like I said, we went through a PIRT process and we did ranging of the most important parameters and the physical models to generate discreet RELAP predictions which are then fed individually through FAVOR and generate a distribution of probability of vessel failure.

DR. RANSOM: By ranging, you mean that these were the ranges of uncertainty in those parameters?

MR. BESSETTE: Yes.

DR. WALLIS: Well, I think we may be giving you a difficult time about something which actually has very little influence on the final answer. But I don't know that.

MR. HACKETT: I think the questions that have been posed are fair and ones we have to pursue. And particularly with regard to these variations in rate of change in the temperature feeding into the FAVOR code.

That's a take away for us and we'd been working on that prior to this. But we need to come back to the Committee next time around, whenever that is, with, you know, a more definitive answer in that regard.

What I was going to propose is Mark just mentioned to me here, we have five or six more slides to go through on the overall process for probabilistic fracture mechanics, and then we might be at a good break point.

I'd propose that to the Chairman, if that's reasonable we'll proceed that way.

DR. SHACK: That's fine.

DR. BANERJEE: Can we also request a thermal hydraulic uncertainty analysis at some point? We did that before.

MR. HACKETT: Absolutely.

DR. RANSOM: Well, one thing that I'm --

MR. BESSETTE: It's difficult to tell you definitively about thermal hydraulic uncertainties in a stand alone basis. Because you can only tell if they're important, they are of relative important after you get the FAVOR output.

MR. HACKETT: Well, that's pretty much true of most every variable in the project.

DR. SHACK: I think what you need is a clearer explanation of how you've incorporated your thermal hydraulic uncertainties into the FAVOR analysis, because I think they are there.

MR. BESSETTE: They are there.

DR. SHACK: You're just not doing a very good job of making clear to us that they are.

MR. ROSENTHAL: In the sense that you've ranged variables within sequences and you've run hundreds of sequences.

DR. SHACK: What I think you need to do is to show that the ranging that you've done sort of covers, you know, we need to see some of those outputs to show that they would, they give you differences in slopes, differences in temperatures.

You've got some that, some of the ranging is sort of parametric things that just cover, but then you've got other things that cover model uncertainty. I think you have to show us just how much difference those have made.

DR. WALLIS: Maybe it's effect on K-applied and it's trivial.

DR. RANSOM: Well, one thing I think that, I know I was always fairly uncertain about before when I heard these results is the ability of, say, a code like RELAP 5 to predict the heat transfer coefficient.

I mean these are pretty hard things to predict very accurately, which presumably would affect the thermal transient. But the analysis like shown in this University of Maryland report, shows that the BL number is high enough that really the heat transfer coefficient is immaterial.

It's really the thermal diffusion in the wall that's important. And that takes a lot of the uncertainty out of the ability. And the only thing you really are left with is the pressure and temperature. And so I think you can capitalize on that.

DR. WALLIS: And you have to ask whether a very big temperature gradient for a relatively short time is going to be a big action grading a crack or not. Because that's the kind of thing that does happen when you compare RELAP with experiment.

DR. KIRK: Probabilistic fracture mechanics in six slides or so. Okay, all, we all know the PRA goes through TH and then comes into PFM. To expand PFM a little bit more in terms of what's inside the box, and again, of course in the report it goes into even greater detail.

The thermal hydraulic pressure and temperature and indeed heat transfer coefficient is passed in to what we've called an embrittlement and crack initiation model.

Other major inputs to that model include the flaw distribution, which describes the density of the flaws throughout the material. Their locations. Their orientation with respect to the vessel major axes, length, depth and so on.

DR. WALLIS: Are you going to talk about that today later?

DR. KIRK: In one slide.

DR. WALLIS: In one slide. Because that flow is a big actor and it's a big change from what you did before.

DR. KIRK: Yes, absolutely. And we can go into more details in one slide, certainly. Another input is the fluence and its variation around the vessel. And, of course, the material properties and composition information.

All of that goes into the crack initiation model and we predict out of that the conditional probability that a crack will initiate. It then goes into an arrest model and we perform a through-wall crack initiation run arrest, re-initiation re-arrest and so on, until either the crack stops through the end of the transient or we break the vessel.

That gives us a conditional probability through all cracking which, again, we just simply --

DR. WALLIS: How frequently does it stop in the middle of the wall?

DR. KIRK: Quite a bit.

DR. WALLIS: Quite a bit.

DR. KIRK: The separation between conditional probability of initiation and conditional probability of failure, order of merit is about an order of magnitude. So only about ten percent, and of course that varies transient by transient.

But only about, in bulk, only about ten percent of the cracks make it through.

DR. WALLIS: This may save you from some of the rapid, local transients. You may start a crack and then you just stop again.

DR. KIRK: Yes, yes.

DR. FORD: Mark, on that item, this was brought up at one of our earlier meetings. Do we have a good factual basis for the fluence attenuation through thickness of the wall that will impact on crack arrest?

DR. KIRK: The, actually I'm going to look straight at Stan Rosinski from EPRI, who is hiding from me now. Because Stan heard your comment at an earlier meeting and actually, recently, well recently, last summer EPRI published a very nice report on attenuation, it's influence on the embrittlement function and so on.

And I'll give you my short summary because I read it recently. Is that the attenuation function in Reg Guide 1.99, Rev. 2, while certainly I think we would all agree we would like to see a better physical and databases for it, is about the best we have right now.

And it's certainly not way out of bounds and I think is generally viewed as being conservative. And that review was conducted by Colin English of AEA. Who else was an author, Stan? Stan?

MR. ROSINSKI: Yes, this is Stan Rosinski from EPRI. Colin English was one of the main reviewers, but we also utilized information in that report that was performed by Ray Nicholson of the UK as well, from the Atomic Energy Authority.

DR. KIRK: The other thing to point out is that, so we've adopted, quite independently of the EPRI report, but we adopted the Reg Guide 1.99, Rev. 2, attenuation function. And I think if you ask me for a technical basis for choosing that, I'm going to reference the EPRI Report because it is indeed very good and I learned a lot.

I think the other thing is important in a PTS context to recognize that the flaws that get you are within ten percent of the inner diameter, within the first ten percent of the thickness.

And within that range, the attenuation function doesn't really make that big a contribution. However, if we get to ever discussing heat up and cool down limits in Appendix G, where you have to attenuate, or at least now notionally you attenuate to the quarter-T and three quarter-T, it makes a heck a lot of difference.

So, I think, it's certainly a factor. But in PTS, because of, because of where the flaws reside it's not as big a factor.

DR. FORD: Okay, so there are data to support whatever algorithm you have?

DR. KIRK: Yes.

DR. WALLIS: Now, Mark, can I ask you

about the stainless steel liner? Isn't there a stainless steel liner in these vessels?

DR. KIRK: That's correct.

DR. WALLIS: And all this discussion is about the vessel, the flaw distribution in the main steel of the vessel?

DR. KIRK: Yes.

DR. WALLIS: But in a transient, the stainless steel liner undergoes transients, does it crack?

DR. KIRK: The, okay, a couple of things to say. The stainless steel liner is included in our analysis in several senses. There is a residual stress distribution due to the weld overlay that's incorporated into our analysis.

There are stresses caused by the differential thermal expansion of the stainless steel relative to the ferritic steel that are also incorporate into our analysis. If a flaw is completely buried in the stainless steel, we don't calculate its influence --

DR. WALLIS: The stainless steel is bonded to the, weld to --

DR. KIRK: Weld overlay, yeah.

DR. WALLIS: Isn't there a source of flaws in that weld overlay?

DR. KIRK: Yes, indeed there is, and those are incorporated. Yes. The major contribution of the stainless steel is it's the only origin of surface cracks in our analyses. Because the flaw distribution work performed by PNNL showed that the only, well, they actually never really found a flaw that was all the way through.

They found, I think, one flaw that was 50 percent and one flaw that was 70 percent of the way through the stainless steel liner. And those were lack of inner run fusion between the weld beads:

And so, now here is, I'll reveal a buried conservatism in the analysis, to spite the fact that we haven't observed one, we took that as evidence that there is a non-negligible probability that you could get a lack of inner run fusion defect between two adjacent weld beads in the stainless steel cladding and that that could produce a surface-breaking defect in the vessel.

And those are indeed the only surface-breaking defects that are incorporated in it. Even though they are circumferential, where they are included they do make a small contribution to the conditional probability vessel failure on the order of five percent.

DR. WALLIS: Are you going to talk about the surface-breaking defect, the lack of surface-breaking defect from any other cause?

DR. KIRK: Yes, there's the, again, the work on flaw distribution found that there's no, well, first off, there's no empirical basis whatsoever for a surface-breaking defect. Nobody has found one.

Moreover, the work found that there was no physical basis for a surface-breaking defect save the lack of inner run fusion between --

DR. WALLIS: Is it because of the way the vessel is made, it only has flaws inside and not on the surface?

DR. KIRK: If they are on the surface of the ferritic steel, they will have been overlaid and therefore will now be buried --

DR. WALLIS: Or they've been removed in some way.

DR. KIRK: Yes.

DR. FORD: The point is, Mark, you just said you have in fact taken into account a surface-breaking defect.

DR. KIRK: Yes, yes indeed.

DR. FORD: And it happens to be from the

austenitic.

DR. KIRK: Yes.

DR. FORD: So, and it doesn't really matter whether it's in the austenitic or ferritic.

DR. KIRK: Well, the defect is assumed to fully penetrate the austenitic cladding and so its tip is in the ferritic material. And so it's treated as if it's in the ferritic steel.

DR. FORD: Okay, so you have done that?

DR. KIRK: Yeah, yeah.

DR. RANSOM: The experimental data that's used, that was taken at Oak Ridge on thermal stress and vessels, are those clad in the same way so they were typical of reactor wall?

DR. KIRK: I'm sorry, you've lost me. Could you repeat that?

DR. RANSOM: Well, the thick-walled vessel experiments that were made at Oak Ridge for thermal shock.

DR. KIRK: Right, right, yes.

DR. RANSOM: Were those, did they have typical clad walls like this vessel?

DR. KIRK: No, but our thermal stresses don't come from those analyses. Our thermal stresses are calculated from the thermal hydraulic and the conduction equation, yeah.

DR. RANSOM: Sure. But on the other hand, some of this nil ductility data came from those experiments, didn't it?

DR. KIRK: The NDT data comes from material-specific tests on each individual plant, and also laboratory experiments, yes. I'm afraid I'm not answering your question.

DR. WALLIS: It didn't come from Oak Ridge, the experiments. It comes from individual plant tests.

DR. KIRK: It comes from -- the data -- okay.

DR. RANSOM: Well, how were those vessel test used? Just to verify the models?

MR. HACKETT: It comes from, Mark is right. It comes from a variety of sources. When you're looking at in the, early on today we had the discussion about the regulatory application of this.

In regulatory sense, all of the plants have, by virtue of NRC's Generic Letter 92-01, have had to report their data that applies to this situation in terms of RT_{NDT}, fluence affects, limiting materials.

In addition to that, the NRC Research

Office, over many years past now, conducted confirmatory tests at Oak Ridge and other locations to say prototypically in a lab what would happen.

You know, I have this material, I applied this thermal shock to this scaled vessel, and what, how is, what sort of crack behavior or material behavior am I going to see. So they were intended to be confirmatory tests.

DR. KIRK: The, to answer the question you just asked, the vessel tests that were conducted at Oak Ridge were really used to validate that linear lasting fracture mechanics is an appropriate technology to apply to pressurized thermal shock situations. So a prototypical experiment.

DR. RANSOM: The type of flaw and things like that, that they, some of them I think they actually made flaws in the wall.

DR. KIRK: In all cases, yeah.

DR. RANSOM: But they may not have been typical of what you might find in a reactor?

DR. KIRK: No, those were laboratory generated flaws. The characterization of flaws that are typical of what you would find in a reactor came out of the flaw distribution work that was conducted at the Pacific Northwest National Lab where they, both non-destructive and destructively evaluated primarily welds, but also have done works on plates, forgings and the stainless steel liner that we were just talking about.

This is the summary slide on probabilistic fracture mechanics. And in particular we're focusing on the changes made in this analysis relative to the analysis that was used to establish the current rules on pressurized thermal shock.

I'll go through this and --

DR. WALLIS: Mark, I'm sorry, I've got to ask you about the presentation in this NUREG.

DR. KIRK: Yes.

DR. WALLIS: When you start reading and there's nothing about heat transfer, there's nothing about thermal transients and stress distribution in the wall. There's nothing about how thermal shock occurs.

And you never, you get the impression that you're never going to find out. And then you have to get to an obscure discussion in the middle of the discussion which is entitled Oak Ridge experiments to find out that, yes, someone does actually investigate crack driving forces and how it propagates through the wall.

So within the context of Oak Ridge experiments. Put that out front and say we really understand how cracks propagate and arrest. And give that theory some prominence in the report instead of hiding in this discussion of the Oak Ridge tests, which someone might just skip over.

DR. KIRK: Yes. Okay.

DR. WALLIS: I got much more reassured when I saw, yes, someone does understand these things.

DR. KIRK: And they actually were co-oped on the report. That must have been very reassuring. Again, here on the slide, and we've had full day discussions with this Committee on PFM, so I don't want to, unless you ask, revisit all that.

But I did want to focus on the major changes and then I've got a slide each on the ones that make the most difference. We'll start at the end with flaws, since we've been discussing that.

Our statistical distributions of flaws where we indeed do a count for our uncertainty or lack of complete knowledge in the flaw distribution. First off, it's based on significantly more data than was available before.

As we've already pointed, also, most, and by most I mean like 98 percent of the flaws are now embedded rather than surface flaws. And that's a major difference. However, there are many more flaws than there were before.

Our models now have a flaw density that is scaled to either the volume of the material or the area of the weld, as appropriate to the flaw type. And that results in somewhere on the order of two to six thousand flaws being simulated in each and every vessel.

That can be contrasted with the six flaws that were simulated in every vessel in the original PTS work.

DR. SHACK: Mark, do you know from a sensitivity study, just how much, you know, there's this quoted factor of 20 and 70 for the difference. How much of that is due to the fact that you don't have everything stuffed on the surface?

Is really the difference in the sizes less important than the fact that they're not surface-breaking anymore?

DR. KIRK: I'll ask Terry if he knows the answer to that question. My gut feel is yes, but I don't have a calculation to back that up.

MR. DICKSON: Terry Dickson, Oak Ridge National Laboratory. The simple answer is no. We, when I did that sensitivity analysis the, paper that you are referencing, I just bundled it together and did the analysis.

DR. SHACK: Everything this is in there.

MR. DICKSON: Yeah, yeah.

DR. SHACK: So you don't know independently --

MR. DICKSON: No.

DR. SHACK: -- how much is just due to the fact that they are not surface breaking any more.

MR. DICKSON: No, no. But my intuition would say that the surface breaking was the major, the dominant contributor. But I can't absolutely say for sure, because I didn't do the analysis.

DR. KIRK: Maybe there's another sensitivity study.

MR. DICKSON: There you go.

DR. KIRK: Certainly it would keep Mr. Strosnider happy.

DR. SHACK: Well, I think, in a sense, you know, there is less uncertainty in knowing that the flaws aren't all sitting on the surface than there is in the flaw size distribution.

DR. KIRK: That's right, that's right.

DR. SHACK: So if you could show that the location of the flaw really drives this all, then it's a warm feeling.

DR. KIRK: That's a good point. That's a good point. Also, one thing that, sub-bullet under flaws that isn't on the slide, but when we get to discussing embrittlement metrics will be very important, is the understanding both empirically and from an understanding of the physics of flaw formation, that the flaws, the big flaws here are of course the weld flaws.

The flaws associated with welds. And our inspection have revealed that most of those flaws, like on the order of 95 to 98 percent are fusion line flaws. And so that gives us a lot of information about the orientation of the flaws.

So axial welds may only have axial flaws. Circumferential welds may only have circumferential flaws. And as a preview, this is going to lead to a considerable diminution of the importance of the level of embrittlement of the circumferential weld, because it may only have circumferential flaws.

So that one piece of evidence, which again is empirical, but backed up very easily by an understanding of how flaws form in welds, is an extremely important insight.

DR. WALLIS: Mark, on flaws, I'm reading from your report. It says it was decided to adopt for further calculations flaw density space only on observations of the Shoreham vessel.

Now, I just wonder how typical a Shoreham vessel is. And vessels are made by different manufacturers, different welders actually weld these welds that are the source of many of the flaws.

DR. KIRK: Yes, yes. That's a very good point. The decision to adopt the flaw distribution from the Shoreham vessel as effectively the flaw distribution in every vessel was driven by the fact that we had basically two flaw distributions.

One from our Shoreham inspections, one from PV Ruff, and that the Shoreham was the worst of the two. It had, by and large, larger flaws and more of them. However, it's just a factual statement at this time.

We don't have a model that enables us to say how that would relate to any other vessel.

DR. WALLIS: But if flaws are caused by welding --

DR. KIRK: Yes.

DR. WALLIS: -- is welding really something, is that reproducible between one welder and another welder?

MR. HACKETT: A couple of comments we could make there. In the case of the large welds and the reactor vessels, probably the answer is yes. Particularly within the range of a manufacturer because these are automated processes.

In that case it would be submerged arc welding. Good and bad then, if something were to go wrong it would go wrong everywhere. But the good news is that it is a highly controlled process through nuclear fabrication QA.

And chances are, and everything we've seen says they are very well made. And to go beyond that, if you wanted to, again, this whole notion of where we have data and where we have to extrapolate, we do have a code, an expert code that comes to us from Rolls Royce in the UK called PRODIGAL.

That's basically a weld expert code. That if you're looking at I've got this particular weld process or I even have a welder laying it down a certain way and I want to see, in terms of a multi-pass weld, like goes into these vessels, what sort of defect distribution would I expect.

We do have a program that can predict those kinds of distributions. And we have run simulations with that code versus the data, and again, we get some pretty good comparisons. As Mark is indicating, the best data we have is from Shoreham.

But then of course we've had discussions with Jack Strosnider and others internally over how well that represents all vessels.

MR. ROSEN: The BWR vessel.

DR. KIRK: Exactly. So you do have, we've sampled a limited amount of welds. It's the best data that we have. There are obviously miles of welds probably that are in vessels in this country and worldwide.

So you're obviously, you know, having to adjust for that, you know, and you should do it in uncertainty space.

DR. WALLIS: Well, at least you know there is a variation because PV Ruff and Shoreham don't have the same distributions.

MR. HACKETT: Yes, right.

DR. KIRK: And they were in fact the same manufacturer.

DR. WALLIS: How big is that difference?

DR. KIRK: I'd have to go back to the data. I don't remember.

DR. WALLIS: Well, you're claiming one of them is typical of all and then you've got another one that's different. What should I conclude?

DR. KIRK: Well, the, the, maybe I've been a little cavalier in my statement. The, the, in some ways the distributional characteristics were established using both data sets, but the density, it was the density, I'm sorry, I misspoke.

DR. WALLIS: Yeah, the density is the one you relied on Shoreham for.

DR. KIRK: That's right.

DR. SHACK: Just another detail. Why don't the, the percentile, you have the Figure 2.18 where you have the small flaws and there's not a neat spread in the percentiles. The curves are actually different shapes as I go through.

You know, the other flaws, you know, when I go to the fifth percentile to the 95th, I get exactly what I think, you know. The flaws sort of go smoothly. And here the percentiles interchange the shapes. How did that come out?

DR. KIRK: I'll have to take a bye on that one, I don't know.

MR. HACKETT: I don't have a good answer to that either, Bill. We'll have to take that away and get back with you. One more comment I'd make just to the welding in general.

Of course, these types of realizations for fabricators and welding engineers have gone into this type of construction for a long time. So there is the realization in that case that in terms of welding, very often the worst case you get into is the root passes of welds.

And in a lot of cases in these vessel fabrication issues, the root passes are in the center of the wall. So they are in one of the more benign, it's not the case everywhere. But in a lot of cases the submerged arc welding is done such that the root pass is actually in the center of the vessel which is, vessel wall which is about one of the most benign places you're going to have it, you know, for this type of scenario.

DR. KIRK: And moreover it's ground out.

MR. HACKETT: That's right.

DR. KIRK: In areas other than flaws, in fluence we've used the calculational methodology expressed in our NUREG Guide. And the major change in our representation of fluence, relative to how we represented it before, is we recognized the spatial variation in fluence whereas previous analyses assumed that the maximum fluence existed throughout the vessel which is an obvious over conservatism.

In the area of toughness, we've made the bold leap to recognize that RT_{NDT} is a conservative representation of the index temperature, not the index temperature itself. And not a precise representation of toughness.

So we've statistically removed that conservative bias. We've also adopted a model describing the aleatory nature of toughness, uncertainty and both crack arrest and crack initiation.

Our embrittlement model is referenced to both toughness data and a physical understanding of the factors that cause embrittlement. So we've got a correlation with a much better empirical basis than before and some physical basis.

And also the slight bias, the slight differences between Sharpy shift and toughness shift have been eliminated, although that was not a major factor. Just to emphasize, you know, the question always comes back of how big are the green arrows?

And has been widely recognized, we don't have a complete answer on that, but I would like to point out that some of the arrows are bigger than others. And the one related to removal of the systematic conservative bias in RT_{NDT}, is indeed a pretty big arrow on the, on the graph on the bottom of this slide.

It quantifies that bias and shows that, in general, or on average I should say, RT_{NDT} is 65 degrees Fahrenheit higher than the true transition temperature. But that varies over quite a large range.

DR. WALLIS: Isn't that because T-zero is really a best estimate, as opposed to trying to understand how to correlate this toughness. I mean RT_{NDT} is an ASME conservative bounding sort of curve that's for design purposes. It's a different purpose altogether.

DR. KIRK: That's right. That's absolutely right.

DR. WALLIS: That doesn't come out in the introduction. And you want it to read that, and it says RT_{NDT} is a way to characterize toughness. It's not. It's really a way to conservatively describe toughness. It's quite different from trying to really predict what it is.

DR. KIRK: Yeah, yeah. But in fact, and you're right and that can be, can certainly be better described. But the difference here is more than just the difference between a bounding curve and a best estimate curve.

DR. WALLIS: You get a couple of these T-zeros and RT_{NDT}'s and all your criteria and methods to be based on an effective or modified or somehow done something with RT_{NDT}. And yet when it comes to the effect of radiation on embrittlement, in your Appendix, the effect is an effect on T-zero.

I don't understand how you translate the T-zero effect that you are predicting from embrittlement on to your RT_{NDT} frame work for analyzing common PTS. But that comes much later. But again --

DR. KIRK: Well, that comes from a --

DR. WALLIS: When you've got two different variables meaning different things but they are sort of correlated with each other.

DR. KIRK: Yeah, that's comes, the shift used in the RT_{NDT} model has always been the shift in the 30 foot pound sharpy transition.

DR. WALLIS: So that's the connection, that's the connection.

DR. KIRK: That's the connection.

DR. WALLIS: So you calculate your delta T-zero and then you get a delta TR-30.

DR. KIRK: That's correct.

DR. WALLIS: And then you go, that step is not, I think specifically brought out in that Appendix. It just says how you modified it to zero.

DR. KIRK: Okay.

DR. SHACK: Just a note on your presentation in Section 2314, you're very careful to put the epistemic air in the initial RD_{NDT}, but then the irradiation model is presented deterministically.

DR. KIRK: That's correct.

DR. WALLIS: I see, the irradiation was even more confusing because it says randomly select something and that's your best estimate. I couldn't quite understand that at all. How do we get these details to you? Do we send them our comments or what?

MR. HACKETT: That was one of the reasons for the request for the letter, not to over, put over much burden in Committee.

DR. WALLIS: A letter -- give you a hundred different comments on a report.

MR. HACKETT: We'd be happy to take those anyway you feel is most appropriate. In one-on-one sessions or anything.

DR. KIRK: E-mail, marked up copy.

DR. FORD: Mark, one of the questions that came out again in one of the earlier meetings was this question about the Eason correlation for the composition effects.

DR. KIRK: Yes.

DR. FORD: How happy do you feel about that? I mean if you have relationships where the correlation factor is pretty well zero, how do you, how do you put that into an uncertainty model.

DR. KIRK: I'm sorry, I was overwhelmed by your question about how I felt about it. So could try again and I'll try to recover.

DR. FORD: Well, the uncertainty that you have associated with the Eason correlation and the composition effects.

DR. KIRK: yes.

DR. FORD: How overwhelming are those on your end result? I get the feeling that it doesn't really matter too much. As scientists we can't really put too much faith in these correlations.

But in the end, is your answer, in the end it doesn't really matter?

DR. KIRK: Is, is, I'm sorry, is your question still, is your question, does the specifics of the embrittlement correlation matter much to the answer?

DR. FORD: Correct.

DR. KIRK: I don't think so, but I haven't proved that yet.

DR. FORD: Okay.

DR. KIRK: And the reason that I don't think so is that to get anywhere near, it might monkey around with the relationship between through-wall cracking frequency and RT_{NDT} whatever you want to call it, at lower levels of embrittlement when you're not on the flat part of the embrittlement curve.

But once you get up to any type of yearly frequency that anybody cares about, I would believe that the, the materials that are getting you and the cracks that are getting you are so embrittled that you can pick this correlation, you can pick the new ASTM correlation, and it's not going to make a huge difference.

DR. FORD: Okay.

MR. HACKETT: And I'll just add, that's not to say at all that there isn't, wasn't or isn't still significant controversy over the elements of that model. And I think our colleagues here from the industry would, you know, we could have a day-long session on that at least on the elements that go into that and their significance or lack of it.

DR. KIRK: Yeah. And to just be complete, so that Stan doesn't jump out of his skin, it should also be pointed out that while I've now, based on, in response to your questions, pooh-poohed the importance of either getting the attenuation function very precisely right, or getting the embrittlement correlation very precisely right in PTS.

You know, when screening at a yearly limit that's relevant to a regulatory agency. Both of those things are of the utmost importance when setting operational limits. And so when we, as we start looking at risk informing Appendix G, those are going to be very key issues.

And a good point from Dr. Wallis about comparing Rt_{NDT} to T-zero and one is a lower bound and one is a best estimate. So we can certainly tighten that up. Having said that, this correction represents at least an order of magnitude in the yearly through-wall cracking frequency.

The flaws themselves, we've already quoted the factor of 20 to 70. And there are many differences between the old Marshall flaw distribution and our current one. One thing, of course, is that our new distribution has many more flaws, but they are all smaller, they are mostly buried and that the weld flaws are along the fusion lines.

Those combine to make a very significant effect.

DR. WALLIS: This, to the uninitiated, looks impressive. I mean you've got probability, which is not all that small, having a ten percent wall flaw?

DR. KIRK: Yes.

DR. WALLIS: What do you mean by flaw there? It's a crack? It's an absence of bonding between --

DR. KIRK: Yeah, see, everything here has been modeled.

DR. WALLIS: I want to ask you what a crack is, because I once asked a Ph.D. student what a, in his final presentation, what a crack was, and he couldn't tell me. So, --

DR. KIRK: The absence of metal?

DR. WALLIS: No, no, defining what a real crack is, is not easy.

(Laughter.)

DR. KIRK: And anything else my mother told me not to say in public.

DR. WALLIS: What's in the flaw that there's nothing, there has got to be something in there. It says it's a space with nothing there?

MR. HACKETT: This is another one of those, this is probably another area of some buried conservatism and the fact, as Mark said, these are modeled as fracture mechanics sharp flaws.

DR. WALLIS: So ideally, they are the worst thing you could think of, or something?

MR. HACKETT: They would be, they would be, what they are is fatigue cracks in laboratory specimens. And so they are very sharp.

DR. WALLIS: So they have a leading edge which really accentuates the stress distribution around that.

MR. HACKETT: That's correct. When in all actuality, if they are weld flaws, they are very unlikely to look like that.

DR. WALLIS: And they don't run into other flaws or anything like that. Nothing gets complicated. You get the worst possible thing.

DR. KIRK: That's right.

DR. WALLIS: It's like a sword going through.

DR. KIRK: The conversion between the data that was taken and it's mathematical representation has been to assume that everything is, as Ed said, a fatigue crack or anatomically sharp crack which is, you know, clearly everything is not that and so there's, you know, there is a buried conservatism or a buried margin.

Having said that, you know, this improvement, again, is a significant factor in driving the through-wall cracking frequencies. This we've mentioned before and is indeed is something we haven't quantified, but you can see from the variation of fluence around the vessel, particularly azimuthally, that only very limited regions of the vessel experience the peak fluence where you would have the very high levels of embrittlement.

And if by, so by representing the vessel in a realistic way, we stay away from being so grossly conservative.

DR. WALLIS: And the thermal hydraulic analysis gets based on the fluid being well mixed by the time it gets to the 24 inches --

DR. KIRK: That's correct. That's correct. So we've got a, essentially a, well, I'm not sure how you do that. We have a fluence model that's 2-D planar, if you will. It wraps all the way around the vessel and gets attenuated through the vessel.

But that's combined with a 1-D TH model and a 1-D fracture mechanics model. Another, again, unquantified, but I feel very comfortable in saying major change from the past is previously we modeled the whole vessel as being made out of the most embrittled material.

Which, except in the case of Beaver Valley, is almost invariably a weld. And so in the past we represented the whole vessel as being made out of a material that in reality only represented about less than five percent of the vessels total --

DR. WALLIS: That does make a big difference.

DR. KIRK: Yeah. There, you know, in the list and even not on the list, there were many other changes in the fracture mechanics model, but I wanted to emphasize those because those are the, you know, those are the big arrows.

And the everything else is just being systematic about your process. So unless there are further questions --

DR. SHACK: It's time for lunch.

DR. KIRK: -- we can break for lunch.

MR. ROSEN: Let me ask one quick one. What's the big azimuthal variation of the fluence the result of?

DR. KIRK: That comes from the

differential and spacing of the fuel bundles relative to the, relative to the ID of the RPV. It's a checkerboard pattern. The fuel bundles are about like that and so at some places they might be only that far from the ID.

And in other places they might be that far. And you get an awful lot of attenuation of the neutron fluence through the water.

DR. RANSOM: What does this mean to these plants that have been upgraded by trying to flatten the flux profile, you know, throughout the core. I think we asked the question at that time and we were told that vessel cracking was not really an issue.

But fluence will be higher on the wall.

DR. KIRK: Yeah, and that would factor in, if somebody has done that, that would factor into their analysis and influence their surveillance program and so it would change the, quote/unquote, RT_{NDT} metric that they'd used to assess their vessel.

MR. BESSETTE: You know plants used to, they used to look for, try to get a fairly flat profile. If it have PTS importance, like 20 years, 15 years ago, they went to more of a peak profile. Now they may go back to a flatter again.

DR. SHACK: Okay, we'll come back at 1:25

then. And, Mark, one of the causalities might be the screening limit. It seems to me that's more speculative at this point, that's not really fundamental to the presentation.

DR. KIRK: Okay.

DR. SHACK: So, we'll probably have, we'll devote an hour to the plant-specific and I want to make sure we protect at least an hour to discuss the acceptance criteria and such. So we'll sort of run the individual analyses up until we have an hour left and then we'll go to the acceptance criteria.

DR. KIRK: Okay.

(Whereupon, the foregoing matter went off the record at 12:25 p.m. and went back on the record at 1:30 p.m.)

AFTERNOON SESSION

1:30 p.m.

DR. SHACK: It's time to come back into session.

DR. KIRK: We will try to present a somewhat abbreviated walk-through of our comments on plant-specific results. Now this might not quite track with what you've got in your slide packet.

To outline the discussion I will talk about, well maybe we won't. No, we won't talk about that. We won't talk about the plant-specific features and inputs, that's all detailed in the report.

We will discuss the estimated yearly through-wall cracking frequency in terms of both the values and the characteristics of the distributions of through-wall cracking frequency. We'll discuss both the transients and the material features that make up the dominant contributors to the through-wall cracking frequency.

And that will be the focus of Mark's in the next hour. This is the first presentation of the actual through-wall cracking frequency results. Just to orient everyone, we've tried to adopt a consistent format so that you don't have to keep reading the symbols from slide to slide.

Oconee will always be in blue, Beaver will always be in green and Palisades will always be in red. At the, during this phases of the presentation we're going to present all of the results regressed versus effect of full power years.

And defer discussion of RT_{NDT} since we've already acknowledged that RT_{NDT} is confusing until later in the presentation, if we get there. Suffice it to say, effect of full power years corresponds to how long the plant has been operating.

So longer operation, higher degrees of embrittlement. On the left-hand side of your screen you see one way of representing the distribution of the through-wall cracking frequencies

We've represented the fifth and 95th percentile, the median and means, with the means in the larger filled symbols. We've taken as our free variable in this analysis the years of operation in the plant.

And do to the low level or irradiation sensitivity of some of these materials, we've had to take the plants out to what I think everybody would agree to ridiculously long lifetime, in order to get mean through-wall cracking frequencies up in the E minus five, E minus six region.

Obviously, in principle, you can muck around with any of the variables in the analysis. For example, in the original PTS analysis a complete fictitious plan called H.B. Robinson Hypo was created by draining up very high copper numbers.

We felt it was less ambiguous just to increase the time variable. In any event, the main take away from this slide is that over any currently anticipated operational lifetime, the estimated through-wall cracking frequencies for these plants is very, very small.

At end of currently anticipated license extension or 60 years, the through-wall cracking frequency values range in the minus nine to minus eight region. And of course, as we've pointed and continue to point out, two of these plants, namely Beaver and Palisades, are among the most embrittled in current operation.

So at the end of any reasonably expected operating lifetime, we are way below the E minus five, E minus six type reactor vessel failure frequency criteria that have been considered.

I'd just like to take a moment to point out, on the left-hand side we showed the bounds of the distribution that we draw the mean or the median estimates from. I'd just like to take a moment to point out that these distributions have some characteristics that's common to all of our results.

Specifically the distributions of through-wall cracking frequency that come from propagating all of the uncertainties through the analysis. And this is now the amalgam of the PRA uncertainties, the thermal hydraulics uncertainties and the PFM uncertainties.

We get distributions that are both skewed and that most of the weight in the histogram is down at very low or in fact zero probabilities of failure, and they are very broad. Where greater than three orders of magnitude separate the fifth and 95th percentiles.

And the point that I would like the Committee to take away from this is these characteristics of the distribution, that they are skewed and broad, is not a mistake and not the consequence of any limited state of knowledge on the part of any of these models.

It's in fact a very natural consequence of the physics of cleavage fracture that results in absolute minima of K_{1c} and K_{1a}.. And so you've got, if you look at the distribution that's shown here in blue for Beaver Valley, 32 effective full power years.

And the bar on the graph that goes off the screen, which I realize is a little hard to read, but it represents that almost 80 percent of the simulations for Beaver, which is an embrittled plant, or currently thought to be an embrittled plant, at 32 effective full power years.

Almost 80 percent of the simulations result in absolutely zero probability of failure. Not a very small number with lots of leading zeros, but zero. And that's because the combination of the transient severity, the flaw size and the embrittlement wasn't enough to get the applied K above the minimum of the K_{1c} distribution.

And so there is just not, it's just simply not going to fail. As you increase the embrittlement in any of these plants, of course you get to the situation where the zero probability of failure goes away. But still the distribution is heavily skewed towards the low end.

DR. KRESS: You know what I'd take away from these curves?

DR. KIRK: What's that?

DR. KRESS: That I can quit worrying about PTS and we don't even need a rule or anything. 'Just

--

DR. KIRK: Can we end the briefing now?

DR. KRESS: Just forget about it.

(Laughter.)

DR. KIRK: Well, that's, it's much less, I mean obviously, as Dr. Shack pointed out, there is a need for the Committee to understand the procedure. But assuming the procedure is right, the consequence of the analysis, the PTS, is much less troubling than we thought it was.

So that's how all the distributions --

DR. SHACK: Until you get out to 200 years.

DR. KIRK: Yeah.

(Laughter.)

DR. KIRK: I'll be much older then. Also, one thing to just remember through the rest of the presentation is that because the distribution, or as a consequence of the fact that the distributions are this heavily skewed toward the low end, we've been plotting mean values, just as an order of merit.

However, in these distributions the mean in the 95th percentile approximately coincide. This slide speaks to what transients dominate through-wall cracking frequencies. And we've already sort of tipped our hand on this, in that for Westinghouse and CE Design Plants, LOCAs are the dominate contributor to risk.

In Beaver Valley, LOCAs are essentially everything. In Palisades they represent about 80 percent of the total through-wall cracking frequency. In B&W PWRs, due to the once-through stream generator design, we see that stuck open valves on the primary side are also dominate contributors to through-wall cracking frequency and in fact make up the bulk of the through-wall cracking frequency at low levels of embrittlement.

And as we discussed this morning, failures on the secondary side, including stuck open valves on the secondary side, like the stuck open atmospheric dump valve and certainly the main steam line break.

While they were dominate before, are not dominate now. And we'll now have a slide or two on each of these to explore the transient types in a little more detail. But, before we get there, this slide I call the Ashok slide because we made in response to a question asked us by Dr. Thadani.

And he said, well, that's great that the through-wall cracking frequencies are so low, but how is it made up. And of course, at least notionally, the through-wall cracking frequency is a product of how often things happen.

The initiating event frequency and the probability of failure occurring if the event initiates. And of course what one would like to see in an environment where you hedge your bets and don't want to believe entirely on any one thing.

As if there's some rough balance between the two. And when we look at the dominant classes of events and compare the initiating event frequency and the conditional probability of failure mean values, we find out that that's the case.

That for most of the dominant events, there's a rough balance and that these two figures are within an order to magnitude. So, it's not like we're getting low failure probabilities, it's not like was have extremely likely events, but our models predict that they don't matter.

Or the reverse. We've got extremely unlikely events, but if the event happens it's the end of the world. We do have a balance between these two figures.

Now getting back to the transients that dominate, as I already discussed, LOCAs are important in all three plants and dominate in the CE

Westinghouse-type plants. And, as we've said before, since these are the dominant contributors, therefore the dominant contributors to uncertainty in the total numbers, and so we discussed that a little bit.

There is at least three orders of magnitude uncertainty in these through-wall cracking frequencies, and in fact more orders of magnitude at lower embrittlements because at lower embrittlements you get many, many cases where you've got zero probability of failure.

At least two of those orders of magnitude come from the uncertainty in the LOCA frequencies, as we already discussed.

And the remainder to the uncertainty is largely attributable to the PFM on certain days, with about one order of magnitude for the flaw distribution and one order of magnitude for the RT_{NDT} bias adjustment that we discussed this morning.

And again, to reiterate what was discussed previously, especially for the medium to large break LOCAs, which are themselves dominating these contributors, operator actions do not really play a significant role.

There is not much an operator can do in response to a LOCA. This graph, I'll apologize to the non-fracture mechanists in the room because this is one of those inverse double normalized fracture geek plots.

The horizontal axis is the temperature at the crack tip normalized to the RT_{NDT} or to the index temperature. And I've turned the axis around intentionally, so you go from high temperatures on the left to low temperatures on the right, so as to make the X axis a quasi-time scale.

So you can think of time as at least approximately increasing as you move from left to right on the graph. The vertical axis is the ratio of the applied K to the minimum of the toughness distribution.

And what we've tried to do is, at least it's hard for me to look at probabilities of failure and gain a lot of insight. It was a lot o more instructive to look at just one crack, in all vessels, under equal embrittlement conditions and compare the dominant transients.

That's what this plot attempts to do for the LOCAs. And a couple of things to point out is first off, again, as we pointed out, until you get $K_{applied}$ above K_{Ic} there is absolutely no probability of failure.

So you can basically ignore all of the parts of these plots that fall below unity on the Y axis. And the second thing to point out, as we discussed in detail at a briefing, I guess it was about this time last year, the conditional probability of initiation exactly and the conditional probability of failure, at least approximately, scales with just one point on each of these curves.

That being the maximum of the $K_{applied}$ to $K_{1c(min)}$. So it's the maximum on the graphs that are important, and the message that I'd like everybody to take away from this is looking at LOCAs, which are the dominant contributors to risk, and at least in two out of the three plants that we've looked at there's a remarkable similarity in the level of challenge produced to the vessel by LOCAs in the different plants.

There's not huge plant-to-plant dependencies that we're seeing in terms of fracture driving force. Moving on to the stuck open valves on the primary side that reclose later. Stuck open, these formed a contribution to the through-wall cracking frequency in all of the plants.

However, it was really an important contribution only in the B&W plant, and that occurred due to the greater tendency to decouple the reactor coolant system from the secondary due to the B&W steam generator design.

There are more uncertainties to deal with in this type of analysis. Specifically the degree of valve opening which was modeled in the PRA as a split fraction for valve openings of interest.

Of course, when the valve recloses is important because that's when you get your pressure spike. And that was modeled as, Alan, correct me if I'm wrong, after 3,000 seconds, 6,000 seconds or never.

And, of course, the operator actions in these type of scenarios do play a key role. Looking again at a comparison of, this is now a comparison of these type of transients. It came up as being risk dominant, which our definition is, contributes greater than one percent of the total through-wall cracking frequency.

A comparison of stuck open primary side valves that reclose later between the three plants. And again we see Oconee, the peaks in these transients for Oconee produces a little bit higher crack driving force than in Beaver and Palisades, but not a heck of a lot.

So again, there's a fair degree of similarity in the level of operational challenge between the three plants, as you see in the blocks in purple. I think it's purple. Well, blue on your screen.

In blue, there are some differences in the initiating event frequencies that are plant-specific and have been taken into account. And then the third one we wanted to point out is to discuss the non-dominance of the main steam line break transient or the secondary side transients in general.

Our analyses, as you see here, it's at best a five percent contributor and in often cases in less, and in most cases less. And in fact in Oconee they didn't even come up on radar at all.

So, since they were important before, the obvious question is why? And as I suggested before, there are really three reasons for this, and I'm going to try to go through them in rough rank order of importance.

The first is that in our analysis, and we've made points about this earlier, our binning has not been nearly as gross as in earlier work. In our current work we separate large breaks from small breaks, from different valve opening scenarios.

Whereas before everything might have been binned together with the main steam line break.. And so grossly overestimate the significance of those transients. The second point, which I'll have a slide on in a moment, is just the point that these transients, if you compare them for the same crack, for the same embrittlement, are just simply not as severe as a LOCA.

They don't generate the high crack driving forces that the LOCAs do, which are dominant now because we've included them. And then the third thing is, yes, it's appropriate to admit that the credits that we've given for operator actions have helped to mitigate the severity of the secondary side events, because the operator does have influence over the degree of over cooling.

However, again, as Alan said before, we would have had to have been grossly wrong to turn these from five percent to 50 percent contributors. It has certainly been the feeling of the people that have conducted the analysis that if we, and this is again probably a ripe area for a formal sensitivity study, but that even if you assumed stupid operator actions, you wouldn't do more than double this contribution.

The next graph, yes, makes the point that even if the event occurs, the main steam line breaks are just simply not as severe as the LOCAs. Again, the thing to focus on in this graph are the peak values.

And this is, this has been done for same crack, same level of embrittlement. So it's a head-to-head comparison. And the main steam line breaks just don't get, don't generate the $K_{applied}$ values that the LOCAs do.

And the other thing, I think, and Alan can probably help me out with this, that's relevant to point out, is that the, there are, I think, four or five different curves on there on the main steam line break that represent different combinations of operator action, operator inaction, that we included in our analysis.

And you can see that all the curves essentially peak at about the same $K_{applied}$ so even that variation of operator action that we've included in our analysis is not making a significant difference in terms of the degree of challenge of the main steam line break.

And then, again, you've seen this type of presentation before. Just a comparison of the level of fracture driving force severity for the secondary side transients is relatively equal between the two plants.

They are peaking at fairly similar values. Moving on to materials considerations, we find that the plot is on the vertical axis, the percent contribution to yearly through-wall cracking frequency plotted versus the EFPY.

And we see that the axial cracks in axial welds are the things that dominant the through-wall cracking frequency. They are responsible for 90 percent or more of the through-wall cracking frequency.

And that means that the important material metric is, or I should say are, the material properties that could be associated with those cracks. So that's either going to be the RT_{NDT} of the axial weld or the RT_{NDT} of the plate, because those are the two materials that sit on either side of an axial crack and an axial weld.

Conversely, the circumferential cracks and circumferential welds play a very minor role. That would be the bottom half of this graph that I haven't shown. That they've never been responsible for more than ten percent of the through-wall cracking frequency.

So consequently, the properties of the circ welds or the forgings that the circ welds join, while they make limited contributions to the vessels resistance or perhaps lack of resistance to PTS, they are just not major players.

And then the third point is that the cracks in plates and forgings that are remote from the weld fusion lines, that are out in the bulk of the material, are just simply too small to play a role.

They have sizes that cap out around five percent of the through-wall dimension of the vessel, as opposed to 25 percent for the weld fusion line flaws. And those flaws are, those flaws subjected to these thermal hydraulic transients are just not big enough to generate any substantial crack driving force.

So these considerations, if we get to it, are going to be major factors in telling us how to construct a physically appropriate RT_{NDT} metric.

DR. SHACK: What happened to the rest of the Beaver for later in life? Why does it disappear at 100 years?

DR. KIRK: We didn't do an analysis beyond 100 years. At a, we stopped, obviously we had an inconsistent number of years. The consistent thing was we stopped running these analyses when we got total through-wall cracking in the E minus five, E minus six range.

And so no big surprise that you get there a lot sooner with Beaver and Palisades than you do with Oconee. So to summarize the findings of the plant-specific analyses. Again, the major take away is that the through-wall cracking frequency that occurs as a consequence of PTS, is low over any currently anticipated operating lifetime.

On the operational side, LOCAs and stuck open valves on the primary side dominant the PTS challenge. And breaks on the secondary side are insignificant contributors. And also, and this is an important point, holding all material factors constant, the operational challenge, in the way we modeled these plants, is reasonably consistent between the three plants.

Both measured in terms of the probability of the challenge occurring and the fracture challenge assuming, or the fracture probability assuming that challenge occurs.

From the materials side, the observation that nearly all of the weld flaws occur in the weld fusion line, the axial weld cracks therefore dominant the through-wall cracking frequency, so it's the properties that could be associated with axial weld cracks.

The axial weld toughness or the plate properties that are going to dominate the RT_{NDT} metric and circ welds make a minor contribution. So that's the really quick run through. If you have any questions, we can --

DR. FORD: Yes, could I come back to the materials composition. I noticed that on some of your initial slides, you were showing that Oconee was less susceptible, all other things being equal, in terms of operational changes.

It was more resistant, rather, than Beaver Valley and Palisades, which is the order you'd expect from the current way of doing it. Which is dominated by the materials influence inputs.

DR. KIRK: Yes.

DR. FORD: Do I take away that the materials composition effects are still an important part, but they are overlaid by these operational aspects, stuck open valves? Am I putting it clearly enough? I'm still worried about this materials composition.

DR. KIRK: I'd say it a little bit differently and see if you like this. Is that we've included the, of course we spent a considerable amount of time trying to find a way to get appropriate distributions for copper and nickel phosphorus and so on.

And the model we finally adopted was to use the values in the Arvid database, which have been docketed by the Licensees, as the mean values for all those distributions. And then we construct, and construct the distributions around them.

We constructed the distributions based on essentially all the data we could find on copper and nickel and phosphorus distribution in the literature, which included some detailed work that was done be EPRI years ago, some detail work that was done in Japan, and a number of other sources that don't come to mind right now.

But the level of material uncertainty that's been represented in these calculations has been drawn from essentially all available information on material availability in RPV steels. So I guess the way I would characterize it, is it's just not going to get any worse than that.

If any, if a specific plant were to come in, say Palisades, who spent a considerable amount of time measuring their material variability. They certainly have a greater state of knowledge regarding their material, their specific material, than was represented in these analyses because we use generic data and assume that the variability possible in any one weld was characteristic of the variability possible in all welds.

DR. FORD: Okay, let me just put it in another, replay back what I heard from you. What you're saying is don't get worried about the trendlines that are coming out of the Eason correlations. Forget those. If you just look at the worst, the worst it can affect you is not going to have any big affect on these results --

DR. KIRK: The worst, yeah. The worst that it could affect you is already in these results. So anything that's better would only tend to shrink the distributions, and well now, here's unsubstantiated sensitivity study opinion.

My guess is it's not going to influence them very much. Beaus I mean as materials people we look at distributions of copper and go, oh, my God. You know, that's really bad. And then Alan tells me, well, I've got a two order of magnitude certainty on how frequently this event occurs and all of a sudden I feel a lot better about what I know about copper.

DR. FORD: Okay. I have another question.

DR. KIRK: Okay.

DR. FORD: In your Executive Summary, you say that it's a blind statement, and without quoting it verbatim, it essentially says no PTS problem for all plants. I think you used all plants, all PWRs.

Based on the analysis for these three plants, you then go on in your main document here, the applicability of these analyses to all plants. You, is that the next --

DR. KIRK: Well, I wasn't planning on doing this is detail, but it's a question you asked.

DR. FORD: It is based solely on you look at the worst plants, five more extra plants and you say, well, what's different between those plants and these three plants and essentially there is nothing.

DR. KIRK: I'm thinking, I mean you're right, the statement in the Executive Summary was perhaps getting a bit ahead of ourselves in terms of, your know, rigorous drawing of conclusions from scientific information.

But I think the insights that have come

out as we've started to delve into this a little more, again getting back to those $K_{applied}$ parts, show that the, the level of operational challenge is remarkably consistent between the plants.

And what we've been able to do is to feedback our understandings about the level of challenge that these scenarios present and we fed that back to Alan and Donnie as they go forward to the other five plants to basically inquire, I mean do you, for example, do you have a LOCA that's going to be worse than this?

And since, I mean I think we need to do a little bit finer level thinking about the B&W plants because their operator actions are important. But it's quite frankly for me difficult to envision that, you know, an eight inch break in one plant is profoundly different than an eight inch break in another plant.

And so I just, that needs to be expressed better and more clearly, certainly. But it just doesn't seem, with LOCAs dominating the way they do, the plant-to-plant variability on the operational side, is going to be a significant factor.

DR. FORD: And then these other five plants, Fort Calhoun and the other four, they will be tackled in not quite maybe this rigor. I'm sorry I'm being --

DR. KIRK: No, that's fine. I was planning on omitting this, but we do appear to have time. The, what we've called the generalization step involves trying to take our insights from the three and a half plant analyses that we've done so far and then interrogate other plants to see if we expect them to be considerably worse.

And the strategy taken here was to take all the plants and rank them in terms of irradiation susceptibility. And specifically what that means is we took unirradiated RT_{NDT} , we added the Eason embrittlement shift at 32 EFPY.

We took out circ welds, based on the insight that circ welds don't contribute much, and then we ranked the plants from highest to lowest. And when we did that, Salem, in fact, came up as slightly more embrittled than Beaver Valley.

So basically what we did is we took the top five plants that we hadn't looked at and said, okay, these plants, based on our understanding, we believe to have the greatest level of materials challenge.

So now we want to go out operationally and see do they both have the highest materials challenge and somehow have a greater level of operational challenge than we had seen in these other three plants.

If we have both of those exacerbating factors, then we would conclude that, oh, well, perhaps there is something that we haven't, there is something that is outside of our current model that we haven't included that we need to.

If, however, we see that, you know, at the very least the highest five embrittlement plants that we haven't included have operational challenges that we believe to be equal to or less than what we've seen before, then we've reached the conclusion that, yes, these results should be applicable to remaining plants.

Not to represent them as a best estimate, but I think one would at least represent them as being of value. So that's something that's ongoing. Alan can talk to the status of that. We've drawn up a series of questions that is drawn out of our insights from what things are important and what things aren't important to basically ask that question.

To see if there's any operational challenge in any oaf these plants that is somehow more severe than something we haven't, than things that we've seen before.

MR. LEITCH: Is that effort in any way prioritized. I just noticed that I may not know the exact order in which plants are coming up for license renewal, but I think Fort Calhoun is quite soon.

DR. KIRK: Yes, it is.

MR. LEITCH: I think it's in-house at the moment and we're scheduled to review it in May or something like that.

DR. KIRK: Yes, Fort Calhoun has been in on a number of different occasions. The other ones, it's been prioritized only in the sense that those are the five that we picked that were the highest level of embrittlement.

We didn't pick it on the basis of who was coming up soonest. I don't know if there's any relationship there at all. If there aren't further questions on this part, we can go to the part on reactor vessel failure frequency.

DR. SHACK: Mark, just refresh my, if I go by initiation rather than through-wall crack, what do I, how much do I jump these curves?

DR. KIRK: It's about an order of magnitude.

DR. SHACK: It's about an order of magnitude.

DR. SIU: What I'm passing around is a segment of the action progression of entry which we're going to talk about in the discussion. And I'm passing it around just because I'm afraid the slides may not show very well.

And in your printed copy it almost certainly doesn't show because there is an animation and some of the blocks in the animation cover the actual tree. Given that we are actually ahead of schedule now, after that blinding presentation, we can just go ahead and take the hour? Okay.

Okay, I'm going to talk --

DR. SHACK: You could even cover the criterion.

DR. SIU: Yeah, actually I think that would be a good thing, quite honestly. I'm going to talk about the reactor vessel failure frequency criterion that we have done some analysis to establish what a reasonable value might be for that criterion.

We've tried to be a little bit careful and not express this as a risk acceptance criterion, because clearly we're not computing risk, although we're trying to inform the establishment of this criterion using discussions of risk.

And if that connection isn't clear later on, I'm sure we'll have questions on them. I'll point out a couple of things. This criterion plays a role in the current version of the rule in two ways.

First, it supports the establishment of embrittlement criteria, and those are the RT_{NDT} criteria that are currently in the rule. And furthermore, it provides an acceptance criterion in case a plant does an safety analysis and needs to compare, have a metric defining the level of PTS risk.

And the current value, as you know, is the five times ten to minus six per reactor year that's currently specified in Reg Guide 1.154. So there are two roles that this particular criterion plays.

What I'm going to report on is a limited scope activity that we've performed. And, just as a reminder, clearly the amount of time we are spending on this work is way out of proportion to the actual effort expended.

We spent a tremendous effort of looking at plant-specific, through-wall crack frequencies. What we're going to talk about here is very much a scoping study, just to get a sense of what an appropriate acceptance criterion could be.

And I'll get to the reason why in a little bit. Mark has already shown you this graphic that says how we might develop screening limits for embrittlement based on the establishment of an RVFF criterion.

Again, this is a notional slide. Our expectation is that the actual establishment of those limits would be done in a risk informed manner, and not a risk based manner. Nevertheless, of course, the risk information, again, informs that process.

And Mark is going to talk to how that risk information can be used, a little bit later. Okay. We covered some of these things already, I believe in the July briefing of the committee.

The activities were performed. Obviously, we had to identify options regarding criteria, and those were document in SECY-02-0092. We did perform a scoping study looking at the post-vessel accident progression.

It's largely a qualitative study, as you'll see. However, we did do some limited calculations, thermal hydraulic and structural, and Dave Bessette will talk a little bit to that.

We also reviewed the results of the pilot plant calculations to look at the energy of the system at the time of reactor pressure vessel failure. So we were trying to use these calculations to inform the judgments that underlie qualitative analysis.

We've, I mentioned the SECY paper already. We met with ACRS in July. We've had public meetings in October and just recently the end of January, talking about what we've done.

And the results, of course, are documented in Chapter 5 of the draft NUREG. I'll point out that the focus of this is on acceptability of certain levels of PTS risk. So although we acknowledge, as you've seen in the previous presentation that the PTS risk is probably very small, that particular fact didn't necessarily factor in very much with our effort.

Other than to say that we shouldn't spend a whole of time working real hard on the acceptance criterion issue. The principles that we applied in developing options. Again, we reported to the Committee on this back in July.

We wanted to be consistent with the intent of the original PTS rule. So the principles involved, keeping the risk associated with PTS at a low level, and keeping the relative contribution of PTS risk small compared to the risks associated with other sources.

We also, of course, wanted to bring in whatever thoughts had come about since the promulgation of the PTS rule in the ❻80's, with whatever risk informing issues have occurred since then.

Principally the Reg Guide 1.174 and Option 3 work. So we tried to make sure we were consistent with those, as we develop the options. These are the same options that we proposed to the Committee, so these were specifically in the SECY paper.

And Dr. Wallis isn't here, but the top, in terms of a definition of the reactor vessel failure frequency, we considered two options. The first one is essentially the through-wall crack, TWCF.

That's the current definition of reactor vessel failure frequency and so that was an actual option to consider. We did look at, very briefly, the issue or the possibility of adopting a definition based on the crack initiation frequency.

And I'll get you our conclusion on that in a second. We looked at three possible numerical limits for the acceptance value for RVFF. Those were the three that you see here.

DR. KRESS: I see only two there.

DR. SIU: I'm sorry?

DR. KRESS: I only see two there.

DR. SIU: No, the acceptance limits and numerical values?

DR. KRESS: I see, yes. Sorry. I was reading my slide there, I couldn't read it.

DR. SIU: Okay. And then of course in your letter back to us you suggested that there might be a fourth option, which is acceptance value significantly lower than the ten to the minus six.

So, getting to that point, after we met with the Committee, there were a number of discussions. Some naturally involved budget. And the decision was, and this is where the notion of the low PTS risk comes into play.

Expecting that the results were going to show that the risk was low, we decided not to spend a whole lot of effort on this particular task, the acceptance criterion tasks and spend most of our resources on making sure we had a good handle on the through-wall crack frequency for the pilot plants.

So, again, you'll see that we've done a scoping study and nothing more. And we're not pretending that this is a detailed analysis. We, of course, got the letter from ACRS indicating that we should base our considerations in terms of LERF.

That we should consider the possibility of something significantly larger than those underlying the current LERF criteria. And we could either start with a Level 3 PRA and work our way back to an acceptance criteria for reactor vessel failure or we should adopt a frequency-based approach just to assure that the frequency of failure of the vessel is very low.

In the letter you also expressed the expectation that the, whatever criterion we came up with would be significantly less than any of the options we proposed in the SECY.

I think the key point on this is in the quotation in the middle of the page. Whether air oxidation phenomena, and I would add large early release would be a likely outcome of a PTS event. And we've spent most of our time trying to investigate whether that's indeed the case.

Okay, just very quickly. On the first set of options regarding the definition of reactor vessel failure frequency, we stated in the SECY, I believe, the expectation that we'd come out with this conclusion and we still hold to it.

We believe that we should be defining

reactor vessel failure frequency in terms of TWCF. We believe that for two reasons. One, from a risk-informed standpoint TWCF is a more direct indicator of risk than in crack initiation frequency.

Now the counter argument to that, of course, might be, well, there are significant uncertainties in the prediction of crack arrest versus crack initiation. And I think our conclusion is that the current technology for predicting crack arrest is reasonably robust. And Mark will talk to that point.

DR. KIRK: Yes, I'd just like to make a few points on this slide. One is the graph that's already on the slide illustrates that when we compare K_{1a} data generated using ordinary laboratory experiments conducted as per ASTM standards, and that being just shown by the red data bounds.

Compare that with crack arrest data inferred from scaled vessel experiments, either the thermal shock experiments, the pressurized thermal shock experiments conducted at Oak Ridge and some of the experiments that have been conducted overseas.

We find both the same temperature dependency as well as the same distribution or similar distribution as is found in our laboratory experiments. So we've got a reasonable agreement, we feel, between specimen and structure data.

And also to point out that the uncertainty bounds shown there for K_{1a} are, if anything, a little narrower than the uncertainty bounds on, that are characteristic of K_{1c}. Let me find that on an empirical basis and also can anticipate it physically.

Also looking to how well we can predict the results of a run arrest event in a structure. We can reference back to, in a structure that we're interested in, we can reference back to the thermal shock experiments that were conducted at Oak Ridge, where we started with a thick wall cylinder and I forgot to show the holes.

But there is a hole in there that was heated up and then we filled it up with LN2, which of course generated a very severe thermal shock in the vessel. And after that a crack propagated from the ID out towards the OD.

And on the graph that's now on the screen, I've just shown the results of one of these experiments. Thermal shock experiment 5a. And shown hoe reasonable the prediction is. And the vertical axis was shown the percent of the vessel wall that was effectively cut by force excessive crack jumps.

And just make the point that using K_{1c} and

K_{1a} data within an LEFM model in a similar way to the way that FAVOR does the probabilistic calculations, we get a reasonable prediction of these experimental results.

DR. SIU: And that's all we have to say on the definition of reactor vessel failure frequency. So, if there are no other questions, I can go on. Okay, the rest of this discussion will be on the numerical criterion value.

And, again, we identified three options and really considered four, including the one suggested by the Committee. The key questions we were asking basically have to do with whether there is a margin between the occurrence of a through-wall crack and core damage.

If there is margin between the occurrence of the through-wall crack and a large early release. And should a large early release occur, associated with the PTS scenario, would the release characteristics of that be significantly different than what we consider risk significant events.

Our approach, we had identified a number of issues in SECY-02-0092. These were based on work done a little while ago by Idaho National Engineering Laboratory. We took, this was largely on the in-house reinvestigation of those issues, where we asked what do we know about the progression of events past the reactor vessel failure. And refine that list.

Just define. What are the things that we should be considering? We developed an accident progression event tree. This APET, as I'll refer to it, was not really intended to serve as a computation tool, although it can be used as such.

But really to identify issues. What's the progression of events. What's the context within which we should be evaluating the likelihood of events. So, in particular, you'll see in that APET, which we have a reduced version in the report.

What you would consider to be aleatory issues, such as the operation of containment spray, and you've also got epistemic issues, such as what's the force association with the crack opening.

Presumably, of course, in the latter case you could calculations to show what those forces are. We haven't done anything detailed along those lines but we've got some limited calculations to indicate what the forces might be.

We evaluated our current state of knowledge regarding these issues, focusing on the pilot plants that were addressed in the main study.

But we also took a quick look at some of the plants considered in the generalization portion and Mark had shown you that chart with the plants identified in color there.

And another important part of the context is whether the PTS changes that accident progression significantly. The point was to argue whether core damage or large early release could occur following a PTS event, but does it occur in a way and with likelihood significantly different than what you might find in other risk-significant accident scenarios.

DR. KRESS: What was your criteria for deciding whether or not to get a large scale air oxidation? Where does that show up on this event tree?

DR. SIU: Okay, well, I'll show you actually a the tail end here. This is the unreadable graphic, so don't bother. This is the one that is actually in the report. The next slide I'm just going to walk you through the top events in the event tree, so hopefully it will be a little bit more visible.

This, and then we'll have a similar animation for an event tree that shows the key sequences. A couple of things I want to point out with this event tree. First of all, the top events largely correspond to the issues that we, the technical issues that we'd identified.

And those technical issues are the ones that we've listed in the report. And it's a little bit different than the list of issues we had in SECY-02-0092. Another thing to note is that I've indicated here with yellow and red, two different classes of scenarios of interest.

The yellow scenarios are the ones where we thing that core damage is possible. Where large scale air oxidation is possible, and where the containment spray is operating therefore there could be a release but it wouldn't be a scrubbed release.

The red indicates the scenarios where containment spray is not operating, so you have the possibility of a large early release and large scale air oxidation for most of the scenarios that we looked at in the tree.

Large scale air oxidation and large early release are not synonymous, but for many of the scenarios the essentially occurred, we judged that they would occur at the same time or for the same scenario.

Another point I want to make here, we have ten scenarios, this tree has 200 scenarios in total.

Ten of those scenarios involve, what you would, involve the yellow kind of line. In other words, the scrubbed release.

And ten of them involve the red line, the unscrubbed release. Not all of them are equal in likelihood. In the report we identified the four scenarios we thought were the most important in terms of probability.

And I'll actually talk to those a little bit later in the presentation. Okay, this is the slightly blown up version of the tree. It reads a little bit better. Not perfectly, but again I'll just walk you through the tree.

First of all, of course, you start with PTS event. As Mark indicated, you can enter this tree with LOCA events. You can enter with stuck open relief valves that later reclose. So basically a low pressure event or a high pressure event.

But in both cases you'd be entering where the system has cooled somewhat, before you challenge the reactor vessel. And I'll talk to that a little bit later. The next branch deals with crack orientation. Whether the crack is axial or circumferential.

And as Mark indicated, again, there is

about a 90/10 split there. Ninety percent axial and ten percent circumferential. The next question we asked was how far does the crack extend.

We didn't do any new work ourselves, we referred back to an old Pacific Northwest Laboratory study on NUREG/CR-4483, I believe was the number. That's the one that we referred to in the report.

And some, that report documents an analysis that looked at the extension of cracks. And they considered whether the crack would extend to the circumferential welds, and I'm talking about the axial cracks, of course.

Whether it would go beyond the circumferential welds and whether it would turn the corner at a circumferential weld and continue on. And not so clear here, well, okay, I'll get to it a little bit later.

Clearly if the crack turns, if an axial crack turns the corner and continues, there is a possibility of arrest or continuation. And we had both of those possibilities in the tree. For circumferential welds, cracks, of course you still have the possibility of arrest or continuation.

So, again, these just identify the possibilities. We're not, in general, talking about likelihoods, yet. I'll talk to likelihoods a little bit later. There were certain hole sizes associated with these crack extensions and, again, we were relying on the old study to give us an indication there.

For the arrested cracks the size range was from zero to ten square inches. For cracks that would extend to the, beyond the circ welds, the range was from ten square inches to 1,000 square inches.

And we broke that up into two categories, a medium hole and large hole. And then we also allowed for a possibility of a catastrophic release and basically again the whole reactor vessel opening, should the crack turn the corner and go all the way around. So we did not discount that.

We didn't have, well, there are various opinions about the likelihood of that. We don't have an analysis to show us yet what would happen in that situation. We looked at blow down forces associated with these holes.

And, again, allowing for the possibility that the blow down forces are either roughly corresponding to design basis LOCA forces or even less, that's the upper branch. Or the possibility that the forces are significantly greater than design basis LOCA loads.

And that plays a significant role later on when we talk about dependencies. We asked the question as to where the containment is isolated. Clearly, if you have large forces on the piping, you might ask about, whether or not penetrations are affected. So we allow for that question here.

We ask if sprays are working. If the, if there's a large hole in the vessel, does the fuel get relocated outside of the vessel or does I t stay within the reactor pressure vessel, so that was a possibility that we asked about.

We asked if emergency core cooling continues to run. And we emphasize continues to run, because it was running prior to the reactor pressure vessel, or you wouldn't be in the PTS event.

And then we asked if the reactor cavity is flooded. Or is the cavity designed such that the water level coming out of the vessel would be expected to rise above the level of the fuel, which would be a cooling mechanism.

To answer your question, Dr. Kress, we looked at each of those scenarios and we decided, depending on whether ECCS was working and whether we had cooling, obviously, if you don't have cooling it would lead to core damage.

If you had large early release, we would consider that if containment in isolation was failed. And for air oxidation, we didn't think that that was possible or likely for some of the smaller holes in the reactor vessel.

And that's just based on considerations of the flow path for errors. But for the larger holes we didn't discount it. We simply said it could happen.

DR. KRESS: So the only containment failure you have is isolation, failure to isolate?

DR. SIU: That's the direct, that's right, that's the direct failure of containment. We have some calculations on pressurized to show why that's a reasonable thing. Yeah, that's basically what we did. Okay. All systems assessments, we were very concerned about dependencies between events here because that's what, dependencies between top events would lead you to any reasonable likelihood of the larger early release and so forth.

So we investigated whether there was characteristics of these scenarios that could lead to knock on affects. So we talked about plant systems. That refers to, for example the state of power at the time of the event.

And again, this is a situation where things are running prior to the reactor pressure vessel failure. This is very different than many of the severe accidents where station blackout is a major concern.

We asked questions whether the RPV, the reactor pressure vessel could move, given the forces on the vessel and given the time over which the forces would be operating. We asked questions about whether missiles from the failure of the reactor pressure vessel could to lead to failure of other systems, such as the containment spray.

And we also asked whether the fuel could be moved as a result of this kind of event. What we're going to talk about are some of the calculations that, again, inform the judgments that we made in the study.

I'll give an overview here and then I'll turn it over to Dave Bessette to talk about some of the TH calcs. But just to remind everybody what were the conditions at the time of the reactor pressure vessel failure.

And again, this is an analysis that assumes that the through-wall crack has occurred. And that's just, we're focusing on the conditional aspects of the scenario. First of all, power is available. We're not in a station blackout.

So systems that are not directly affected mechanically from the event, or affected say by other mechanisms, should work with high reliability. You were talking about independent hardware failures to lead to the loss of systems.

Now systems have been running at this time. So there, any probability that the failed to run would say that they would stop and the operators aren't able to restore the systems.

We're entering with LOCA events and stuck open safety relief valves.

In the LOCA events, of course, the reactor cooling system has been cooling and depressurizing for a while. In the case of the medium LOCA, the estimates for the time of failure of the reactor pressure vessel, and this is based on examination of the FAVOR calculations.

We're talking some 15 or 30 minutes after the initiation of the event. These times are indexed, by the way, to the 40 EFPY, effective full power year results. For large LOCA, things happen more quickly, of course, but still reactor pressure vessel failure occurs minutes after the occurrence of the LOCA.

And this has an effect on the thermal hydraulic state when we challenge the vessel. For the stuck open and safety relief valves, the system is at pressure, perhaps 2,400 PSI or thereabouts. But the pressure vessel failure is predicted to occur between 60 and 120 minutes after the trip.

So the system has been cooling for a while before the reactor pressure vessel is predicted to fail. With that, Dave is going to show some calculational results. Do you want to switch chairs?

MR. BESSETTE: What we did was to total up the primary system energy for all the PTS significant transients, that is to all the transients that contribute one percent or more to the total probability of failure.

So this is the plot for all the Oconee transients. If you remember, Oconee had a lot of contribution from events. There was a stuck open pressurizer safety valve that recloses. And most typically we took a reclosure time of 6,000 seconds.

The LOCA event that show up is this transient here. For LOCAs, the vessel failure time is typically about 1,000 seconds or thereabouts. Whereas the stuck open SRV cases typically fail around 7,000 seconds.

This is the initial primary system energy and power. And this dotted horizontal line if the energy of a primary system that was filled with 212 degree water. So basically this is a, you might say is a zero reference point for blow down potential.

So you can see when vessels fail for a LOCA-type event, basically there's no blow down potential. For the stuck open SRV cases it's perhaps, you're dealing with roughly, effectively one-third of the initial system energy.

This is the same plot for Palisades. These are the LOCAs and these stuck open SRVs, so you can have some idea of the blow down potential at the time the vessel fails.

DR. RANSOM: Is that based on the energy of the amount of the water still in the vessel?

MR. BESSETTE: This is so, these plots are the total primary system energy, includes both water and steam.

DR. KRESS: This is enthalpy.

MR. BESSETTE: Enthalpy, that's right, enthalpy.

DR. BANERJEE: Oh, it doesn't include the metal and fuel?

MR. BESSETTE: It does not include the

metal structures, no. Basically for a blow down, I mean there is some energy contribution from the metal, but in terms of blow down it doesn't, it's not a main contributor.

DR. BANERJEE: And the fuel?

MR. BESSETTE: The same thing with the fuel. The fuel is cold, by the way, fuel is the same temperature as the liquid. So in these vessel failure events, the fuel is passed about 300 F, with no stored energy.

We're not dealing with, there's not a difference. So when you have a large break, it occurs from here. And plus you have some additional, you have a significant energy input from the fuel from the stored energy.

These events, the fuel has, so to speak, no stored energy.

DR. BANERJEE: So zero time is vessel failure time?

MR. BESSETTE: Zero time here is the time of the initiating event. Now all these, these PTS events start with some sort of a LOCA. Let's say a four inch hot leg break or a safety valve sticking open.

Some time into the event is when the

vessel is predicted to break. So they say, some type, for a LOCA, the vessel is predicted to break at about 1,000 seconds. In these stuck open SRV cases it's dependent upon when we reclose the valve.

And typically we reclose it around 6,000 seconds. It takes another 1,000 seconds for the system to refuel and pressurize, so the failure occurs about 7.000 seconds.

In fact, these numbers are the calculated failure times here, by FAVOR.

DR. KRESS: The main point is that containments are designed to withstand LOCAs.

MR. BESSETTE: That's correct.

DR. KRESS: So if you have a LOCA is not going to fail the containment, unless you have other things going on.

MR. BESSETTE: That's correct. I'll show you the containment pressure plots for these two. Containment is designed to take this amount of energy, plus the, like core stored energy and instantaneously dump that into the containment.

And finally, this is the same plot for Palisades. Palisades is dominated by LOCAs, so we're dealing with vessel failures around here.

DR. KRESS: Yeah, we were concerned that

the blow down forces on the vessel might fail containment.

MR. BESSETTE: So we have, I have some indication on what kind of pressure differentials that they generate. We did calculations with three left. We used the Calvert Cliffs model, which was similar to, Calvert Cliffs is similar to Palisades.

We used Calvert Cliffs because we had an existing containment model for that plant. With two representative transients, the four-inch surge line break and a stuck open pressurizer safety valve that recloses at 6,000 seconds.

We looked at two vessel failure modes, an axial break at 12 square feet, that's a one foot by 12 foot break. And then a full 360 degree circumferential break on the vessel. With three break opening times, ten milliseconds, a tenth of a second and one second, this is, let's say, the fastest conceivable break time for the vessel.

And this perhaps, who knows exactly. This may be more representative. The, let's say the vessel break opening time is important because very fast breaks you can heave these subcooled pressurization waves going through the fluid.

DR. SIU: Excuse me, just for a second.

I forgot to point out, by the way, that the viewgraphs are this handout here. So this is a substitute for the packet, that segment in the package that you have.

DR. KRESS: So, containments are designed to stem double ended rupture of the largest pipe. And how does that 12 foot square compared to that.

MR. BESSETTE: A large cold led break is about six or seven square feet. So it's about half of the size.

DR. KRESS: So you're actually --

MR. BESSETTE: We're in the ball park.

DR. KRESS: You're in the ball park but you're subjecting the containment for a little more than normally it's designed for.

So it's a little bigger break occurring at lower system energy.

DR. KRESS: Oh, yeah, it's a lower energy, that's right.

MR. BESSETTE: This shows you where we located these breaks in the RELAP model. This is the circumferential break. This is the core region here, so its, we've located the break near the bottom of the core.

The break extended across six RELAP nodes, so you get junctions above and below, it says 12 junctions. This is the axial break. This extends 12 feet in the region, again, adjacent to the core.

So we got from the bottom in the downcomer, from the bottom of the core to the top of the core.

DR. KRESS: Now, you're using RELAP to calculate the blow down rate, is that what you're using RELAP for?

MR. BESSETTE: Yes, so we used RELAP for the blow down. We used RELAP for the entire transient starting time zero. We go through the initiating event which is four inch LOCA or the stuck open SRV.

And we initiate the vessel break at a, let's say at predetermined points in time. We put a flag, let's say, and RELAP opened the vessel break.

DR. KRESS: So it's still coming out at choke flow?

MR. BESSETTE: Yes, yes.

DR. RANSOM: You're doing this for a consequence analysis, is that right? I mean these are highly improbable events apparently.

MR. BESSETTE: Well, that's right. But we wanted to get some idea of the, let's say the pressure forces within the vessel and the containment pressurization.

DR. KRESS: But we asked them what the probability was of containment failure given this event.

DR. BANERJEE: And this doesn't take, the thing doesn't open up and throw missiles and things all over the place, nothing like that?

MR. BESSETTE: Well, that was one of the questions. How large are these, these blow down forces. And from what we can see so far, there's no, we're not filling the core barrel or we're not breaking up fuel assemblies, that sort of thing.

We're not generating ex-vessel missiles.

DR. BANERJEE: So this practice grows and stops. It doesn't sort of unravel the whole thing?

MR. BESSETTE: Well, that's the question too. We looked at both cases. We looked at cases where what possibility it is, it starts and it grows, let's say, the length of the weld, which is perhaps eight feet or so.

And it stops at the end of that particular plate weld. The other possibility is that it goes to that point and then it continues around a vessel, 360.

DR. BANERJEE: Is there sort of evidence of that. Because BSF, which is a company that did some vessel tests where they cracked open a vessel like this and it sort of just unwound and boom you had a really -- there's a lot of this documented.

Now I don't know if this is a muck thicker vessel or what it is, but these things sort of, there is evidence that they just come apart.

MR. BESSETTE: Well, yes, one of the candidate cases we looked at is there vessel was in two pieces circumferentially.

DR. SIU: The PNNL Study we talked about a little earlier in the presentation, certainly they did some analytical calculations to look at the progression of the crack. How far it would extend, whether it would turn.

They didn't calculate where the crack would arrest, but they also, in later parts of that report looked at missile generation. Talked about failure of vessels under pressure and what kind of missiles could be generated from that. And I'll talk to that a little bit later.

MR. BESSETTE: So these are the primary system conditions taken at the time that we failed the vessel. So for a four inch break-to-break, the vessel break time was 2,400 second.

The primary system pressure was 200 psi. The downcomer temperature was 250 degrees and that was at saturation and that's the corresponding FOP. Stuck open SRV case, we failed the vessel.

This was, let's say we imposed this time since we're not dealing with a FAVOR generated time in this case. Pressure was, we failed the vessel when we reached the safety valve set point at 2,400 psi.

Downcomer temperature was 355 in this case, F. This is somewhat higher because in Calvert Cliffs, even with the stuck open SRVs, we can't get cold enough in the downcomers we do, let's in Oconee where this transient shows up as being more significant.

And then for comparison, we did a large cold leg break LOCA. This initiates at time zero, initial system conditions.

MR. LEITCH: In the second case there, what are we assuming, the vessel, that their stuck open relief valve opens at time zero. And then at 82.30 seconds is when the vessel fails?

MR. BESSETTE: That's right. We opened at time zero, we closed at 6,000 seconds or 100 minutes.

MR. LEITCH: Okay.

MR. BESSETTE: And then --

MR. LEITCH: It recloses.

MR. BESSETTE: -- it took another 2,200

seconds in the primary system to completely refill. And once we lifted the safety valve casing, we broke the vessel.

DR. RANSOM: Now when you say you broke the vessel, do you mean you exceeded one of the these fractured criterias?

MR. BESSETTE: Well, this case, since this is a scoping study, which this is not, these calculations would not tie directly to FAVOR. We broke the vessel at this particular time. I can say this was tied, we tied this to the time when the primary system went water --

DR. RANSOM: So this kind of scenario would assume something more than the normal pressure, PTS type of transient that would rupture a vessel.

MR. BESSETTE: Yeah, but basically, these two, these two transients are quite representative of the risk dominant sequences. And we've got the, most, about two-thirds of our risk dominant sequences are the LOCAs. Most of the rest are these stuck open SRV cases.

DR. RANSOM: What's the probability of either one of those occurring?

MR. BESSETTE: Overall, yes.

DR. SIU: Again, what we were trying to do in this part of the study is talk about what ❻s acceptable as opposed to what we would achieve. And part of this discussion is to argue that there's margin between the occurrence of a PTS induced reactor pressure vessel failure and large early release.

So we're trying to get a sense of what are the forces involved, because if there are large forces involved, we might have to argue that the mitigating systems, such as containment spray or ECCFs in recirc mode are effected by the occurrence of the PTS event.

Therefore, there might not be much margin. If we can demonstrate that the forces are low, there's little dependence between the occurrence of the event and the failure of these systems and therefore there is probabilistic margin. And that's the essence of the argument that we're trying to present.

DR. BANERJEE: You're doing a consequence model here. Pure consequence. There's no risk, probability aspect.

DR. SIU: It's conditional, that's right. Exactly.

MR. BESSETTE: These are some of the results calculated for Calvert Cliffs by RELAP. We have, again, the three transients to be calculated to four inch surge line breaks and stuck open SRV.

And for reference the design basis accident LOCA. Here we're looking at two vessel break opening times, ten milliseconds and one second. We looked at axial and axial vessel breaks and circumferential vessel breaks.

And these are the peak differential pressures as calculated by RELAP. And one of the things, of course, is that these peak pressures are highly dependent on this vessel break opening time.

The slower the, you go from ten milliseconds down to one second. These peak pressures drop considerably in most cases. And the other thing about this is that I'm showing, these of course are peak pressures.

For these ten millisecond cases, these are, you know, you might say of sonic nature. So their durations, these peaks are very sharp. The durations are on the order of ten milliseconds. So that's kind of an impulse load.

And you can see these duration times, roughly speaking, are in this column. This basically gives the message that these pressures, these peak loads drop considerably with longer opening times.

And for these really fast break opening times, they are very short duration. But you can see, generally speaking, they are comparable to or much less than a design basis large break LOCA.

The vendors typically will analyze large break LOCAs for these conditions very quick, almost say instantaneous break openings. This is the calculated containment pressures from these events.

So on the bottom here, this is at the time of the vessel break. For comparison, this is the large, cold leg break design basis accident. This is the containment pressure. This is additional pressures, 15 psi and roughly atmospheric.

These four inch break LOCAs, since the LOCA has been in progress, you're starting from a slightly elevated containment pressure when the vessel breaks. And you can see the relative pressure rise.

You recall that there is very low system energy in these four inch break cases when the vessel fails, so you get only about a 3 psi, 4 psi pressurize.

DR. KRESS: Where did you get that initial pressure from?

MR. BESSETTE: This pressure here? We calculated this whole primary system containment.

DR. KRESS: Oh, you used RELAP as a containment model.

MR. BESSETTE: We used RELAP as a containment model.

DR. KRESS: Okay, thank you.

MR. BESSETTE: And these are the stuck open SRV cases. The pressurize is about 10 psi, compared with the cold leg break of about --

DR. KRESS: So this is RELAP as a containment model using one node in containment?

MR. BESSETTE: No, this is about containment, you can, you can --

MR. LOTT: They have about 15 nodes.

MR. BESSETTE: Yeah. You can nodalize, you can have some flexibility in terms of how you nodalize containment with RELAP. It's not like containment where you have a single node.

DR. KRESS: How do they compare the containment?

MR. BESSETTE: To contain?

DR. KRESS: Yes.

MR. BESSETTE: We don't have a comparison here for contain, but we've looked at RELAP with containment modeling versus other calculations. We did that for AP 600, and it's in the, it's in the right ball park.

DR. KRESS: The 36, how does that compare with the design pressure?

MR. BESSETTE: Design pressure is about, it's about 45 psi.

MR. LOTT: Tom, Norm Lott. It didn't you have all the containment features in it. All would have included fan coolers, but there were no fan coolers. But it does have a spray cooling unit and it has, dumping all the energy from these, both the transient and from the less than zero is the PTS transient dumps energy in as well. And then after the vessel break, you've got the vessel break energy. And I think that's the main thing that Dave is trying to show here.

That if you don't have a very energetic system, it doesn't pressurize and contain it very much.

DR. KRESS: Yeah, I think that we recognized that. Our concern was whether you've got a hole in the bottom of the side of those things and you've got a momentum forces tending to move the vessel and the penetration on the hot leg or the cold get going through the containment, would that, you know, contain it, I think was one of our concerns.

MR. BESSETTE: Yeah, we looked at this momentum flux aspects, you know, jet reaction force and that sort of thing. We don't have, we're still working on some of those things.

So it doesn't look like the, again, it doesn't look like the reaction forces you get from a vessel break or any worse certainly than a cold leg break.

DR. SIU: The other thing that's, again, worth pointing out, Dave had the right-hand column showing the duration of the pressure pulse. And it's very short. There's no time.

DR. KRESS: That's an impulse.

DR. SIU: Tens of milliseconds and this thing is over.

MR. HACKETT: Dave, this result, too, is large dry, right? This is showing Calvert Cliffs? It's specific to that type of containment?

MR. BESSETTE: Yes, Calvert Cliffs.

DR. KRESS: Would there be any special considerations for ice to the condenser containments. Would the steam go where it's suppose to go in those?

MR. BESSETTE: I mean off hand I can't think of any particular reason why things should be much different. Certainly the primary system energies are going to be the same. So the blow down potential is going to be the same.

DR. KRESS: The primary system is all the same, right.

MR. BESSETTE: So the enthalpy discharge when the vessel fails is going to be the same. And also, the rate at which this energy gets discharged in the containment is essentially so fast that whatever containment heat sinks are there --

DR. KRESS: Don't come into play much.

MR. BESSETTE: -- really don't come into play.

DR. SIU: So far you haven't seen any probabilities associated with these. What we were trying to do is establish a sense of the conditions that the containment would see and what the reactor pressure vessel would see.

And actually what you've seen is material that we've generated since, or finalized, I should say, since the writing of the report. So these arguments were not factored into the report, and so it's an additional conservatism, I think, on the results that we're going to talk about in a second.

This is a diagram here, again, it's in your hand out. It's not in the report, per se. It just is another slice at that 200 sequence event tree. APET, it shows the four scenarios that we identified in the report as being of potential interest.

I couldn't give you the numbers off hand, but it doesn't really matter. I'll walk you through some, just as an example. I passed around a hand out with some colors on it showing three different kinds of scenarios.

This is, again, basically that same picture blown up a little bit, but with some of the scenarios highlighted. The red scenarios, again, are those that lead to the unscrubbed large early release.

Blue scenarios that lead to a scrubbed release. And the pink scenario is something rather more benign. It could lead to the scrubbed release, but the probability should be significantly lower as I'll talk to you in a second.

So I'll try to talk about all three as I walk through the tree. Okay, so again, we enter with a PTS event. Crack orientation, as I indicated already, we think roughly a 90/10 split based on the plant-specific calculations to date.

Based on the PNNL work, NUREG/CR-4483, there are, there is a distribution of probability across the different crack extension possibilities. Remember the top branch associated with the crack arrest at the circ weld.

The next branch was crack progressions beyond the circumferential weld. And the bottom branch on the axial crack leads to a circumferential crack. This is the one where the crack turns the corner and continues.

And here on this tree you'll see that I've indicated both the arrest and the propagation possibilities for the case where the crack turns the corner. It's not that we're going to say that these numbers are hard and fast.

The PNNL report actually shows that there is significant variation across the three plants that they looked at when they did the calculations. It's just to indicate that there is some distribution and we didn't take any credit or significant credit for the fact that this particular branch might be, let's say, along the 45 percent line as opposed to 15 percent line.

We just didn't bother with that. But if one were to pursue this in more detail, obviously, that would be a potential place to look at. The hole sizes we looked at we associated deterministically with the different crack propagation possibilities.

So, again, the bottom,. let me focus on, I don't want to blind anybody. Okay. It's on already. Can you hear me? Okay. So, here we have the crack. This is an axial crack that initiates the circumferential crack.

And the crack progress and it's arrested. And then again we have the case also where the crack continues. And we didn't assign a split fraction associated with that. If the crack is arrested, there is a possibility of a moderate size hole, which turns out to have relatively low consequences. Or a larger hole.

This was the 100 to the 1,000 square inch hole opening. Following that, depending if the forces are roughly design basis or significantly greater design basis, that's the branch in here. And that's what Dave was just talking to you.

We did not, at the time of the report, we had a suspicion that the tree should go up in this direction, we didn't have a basis for that. Now I think we have a stronger basis for saying this branch seems to be rather low likelihood.

So again, the thermal hydraulic calculations to date would indicate we would probably head up the upper branch. But these two branches are branches that we've identified in the report as being potentially significant.

If the forces are a roughly designed basis, then the question of containment isolation, this question is really a question of independent failure at this point. If the forces are beyond design basis, then obviously there's a potential for dependence, that's the concern that you raised.

And so we allow for that. The containment spray, and this is probably the crux of the argument. If we had to boil it down to one slide, this would be it. We look for mechanisms by which we could fail containment spray due to this particular scenario.

We looked at the possibility of missiles and we looked at the energies associated with potential missiles and whether they could penetration the biological shield around the reactor pressure vessel and basically get to the containment spray lines which are running up the inside wall of the containment, and just did not see that that was happening.

There was just, the penetrating capability of these missiles, even if you assumed optimal shapes and assumed hardening, just the forces aren't there. So that tells us that the sprays are independent.

Now there is one potential fly in the ointment and that has to do with some blockage. We assumed that sump blockage, or programmatically you said sump blockage is an issue being addressed in the GSI-191. And we were very explicit about that in the report.

And presuming that that issue is addressed, then containment spray is indeed independent and the reliability is the reliability of a multi-trained system that should be ten to the minus two or even significantly less than that. Unreliability should be less than ten to the minus two.

DR. KRESS: Let's hope that that sump blockage is resolved before you actually get to a pressurized thermal shock effective full power year of 40 years.

DR. BANERJEE: But the issue of sump blockage would come from the insulation on breaking apart.

DR. SIU: That's right. Remember, we've entered this perhaps with a large LOCA. So you've got the same sump blockage issues, potential sump blockage issues. Recirculation generally we would predict to occur after the reactor vessel fails.

So any additional debris or stuff coming out might add to that problem. But there's already a problem independent of the PTS.

DR. BANERJEE: Right. So you either spray or you don't depending on sump blockage at that point.

DR. SIU: That's right, that's right. Okay, now would this be a road block in case the issue is not resolved? No, I think that, but then you'd have to purse the other lines of argument that Dave has already indicated.

The energy available and what that, whether, for example, it would lead to consequential failure of containment.

DR. KRESS: So with the sprays already you have a ten to the minus two.

DR. SIU: That's exactly the point, yes. We would, arguing independence based on the consideration of the causal mechanisms. Fuel location, I won't get into. Again with the low energies involved, you wouldn't expect.

In fact, we did a preliminary analysis looking at the core barrel distortion associated with some of the pressure differentials that Dave calculated. It showed relatively small strains and it's not a surprising result.

DR. KRESS: You know, for the large breaks, where you pretty much assume it goes to power oxidation event, because you dump the water out pretty fast. I recognize that the blow down will fail containment and then you've got a lot more energy coming out of air oxidation.

And maybe a lot of hydrogen. Does that worry you about the independence of the sprays?

DR. SIU: Well, this will get to an issue of timing which we clearly didn't address. The, thinking in terms of a large early release, when something has to occur within four hours or four hours or less.

DR. KRESS: You might have a, the early part of the large --

DR. SIU: Reactor pressure vessel failure, as we said, for the pressurized scenarios you're talking maybe 60, 120 minutes down the road from the initiating event. The LOCA events it does occur more quickly.

DR. KRESS: That kind of impacts on my issue that I think I've about got the Committee convinced is right, that we shouldn't just focus on large early release. There ought to be some considerations of late containment failure also.

DR. SIU: Yeah.

DR. KRESS: You know, pretty soon I'll get them on my side.

MR. BESSETTE: Of course you actually need steam oxidation to get a lot of hydrogen.

DR. KRESS: You need steam to get the hydrogen.

MR. ROSENTHAL: If it's an axial crack then we would, then for some of the cases they, even though the cracks there, you have like a wheel well effect, you're still pumping a lot of water.

And so you have water in the bottom of thing and so you'd melt the core in a steam environment. If you go down this ten percent probability path where the axial crack comes to the circumferential weld and then unzips around and the bottom head falls off, now you've got clearly an oxidizing environment.

And it's correspondingly lower probability and you still ask are sprays running to scrub. So we've tried to reason our way through it.

DR. SIU: Just to finish the tree off here, again, if the forces are roughly design basis then we wouldn't expect a knock on effect on to ECCS and, by virtue of pulling pipes. And so again you would get some high reliability out of that operation.

We did say well it's potentially dependent failure here, not knowing at the time of the report what the forces were. We also pointed out the possibility of cavity cooling. And for some of these plants you would expect, indeed, the water level to rise above the top of the fuel and to cool the fuel that way.

And so you shouldn't get core damage, let alone in a large early release there are other plans for which you can't count on that. You have some water in the cavity, but not enough to assure that the core remains intact.

Okay, so, as Dave pointed out, we believe that the accident energetics are more benign than many of the scenarios that we've already analyzed. We believe containment pressurization is likely to be less than what you would get from a design basis LOCA.

We, Dave showed you the delta ps associated with the cases that we analyzed. And so we think that it's likely, obviously this is not a full proof, we haven't looked at all the various possibilities, but it's likely that the blow down forces are likely to be on the same order of magnitude as the design basis LOCA or even less.

And again, point out that the time over which these forces are acting is very, very short. We actually think the containment spray failure probability might decrease for these events are compared to the risk significant events because you're not in a station black out situation.

So you're largely talking a hardware failure or possibly operator error. We talked about the likelihood of fuel cooling being dependent on reactor cavity design. And of course the point that GSI-191 is the issue addressing the sump blockage.

DR. WALLIS: I wasn't here for this, but if you unzip a reactor and it's got 2000 psi in it, would you apply 2000 psi to the whole --

DR. BANERJEE: It's down in there.

DR. WALLIS: If you split it in half, half goes up, half goes down?

DR. BANERJEE: It's down in pressure when it splits.

DR. WALLIS: I know the pressure goes down, but initially the pressure is very high. So the initial force is bigger than large break LOCA. It doesn't last very long.

MR. BESSETTE: Yes, well, if you look at the situation, you know, those events that have a stuck open SRV that closes, you are in need of a 2,400 psi. But that pressure is saying, it's not a thermal pressure, you've got a lot of cold water that's been pressurized by a pump.

So you're just dealing largely with the compressibility of water which is --

DR. WALLIS: Okay, so it goes away very quickly. It's not like steam.

MR. BESSETTE: That's right, it's not like hot 2400 psi water.

DR. SIU: Just on the separate hand out, viewgraphs 20 and 21, we had some of the calculational results. Okay, where are we in terms of conclusions.

The, we believe that the conditional probability of early fuel damage, and this is really the core damage question, would be extremely small for plants where you would get the flooding, but it's non-negligible for the plants, you could have fuel damage for plants where you're not going to get the flooding.

And this is absent any real, you know, phenomenological analysis. This is just based on rough consideration.

DR. KRESS: When you non-negligible, it still could be pretty small.

DR. SIU: It could be. Again, we did not do any calculations at this point. You'd have to look at --

DR. KRESS: I believe the reliability of sprays is at least less than .01.

DR. SIU: No, yeah, but I'm talking fuel damage in the first bullet.

DR. KRESS: Oh, oh.

DR. SIU: The second point is the sprays, right. That we believe regardless of the cavity design, the conditional probability of the early containment failure and a large early release would be very small, very small in that I've used that terminology saying less than .01.

However, should a large early release occur, we haven't done anything to show that large scale air oxidation will not occur also.

You'll see, if you were given the full event tree, which you weren't, you would see in that that most of the sequences involved large early release. Also we would say would involve large scale air oxidation. So they are, the conditions would lead to both.

DR. KRESS: And those sequences normally aren't the dominate PTS sequences, I thought I heard.

DR. SIU: Well, those sequences would, these are all, the APET is tied to the dominant PTS sequences. We don't think any of those sequences are likely. Conditional on the occurrence of the PTS induced reactor pressure vessel failure.

So, the implications for the reactor vessel failure frequency criterion, we think that the ten to the minus six value is consistent with the philosophy of the original PTS rule. It's consistent with the guidance you've given us in your July letter and with the safety goal policy statement.

We think it's consistent with the philosophy of the rule because basically we have this low conditional probability of large early release, given the occurrence of a PTS induced reactor vessel failure.

So that would ensure your low level of risk. I mean if you were just to take numbers literally, say, ten to the minus two times the ten to minus six, that gets you to ten to the minus eight. And that's extremely low.

And obviously for similar reasons, the relative contribution to total risk would be small because this would be a virtually negligible contributor. Ten to the minus six is indeed more limiting than what you might use otherwise in terms of core damage frequency.

And this was the point that you raised earlier and thought that we should look at something that was based on LERF considerations and not core damage frequency consideration

If you were just looking at core damage you would pick something like ten to the minus five.

We think that this is consistent or even conservative with respect to the quantitative health objectives, both in terms of prompt early fatalities and in terms of latent fatalities, because again if you equate the ten to the minus six with core damage, I think we would be right there.

So that's why we, in the report, we stated that we think that we can support a ten to the minus six per reactor year acceptance criterion. Again, as I indicated in the beginning, our expectation is that embrittlement limits would be set in a risk informed manner, so what we're talking about here is an important input to that process but it's not the only input.

And that's just basically the same thing I've just said. So, I think we're at the end of the hour.

DR. WALLIS: Now we were told this morning that the predicted frequency is actually much less than that.

DR. SIU: That's correct. These are acceptance criteria. This says what we are willing --

DR. WALLIS: So, I was thinking, what would be the effect then if you have a frequency which is far less than that, then would this lead the Licensees to say, now, we're no longer going to be limited by this, can we change something about how we operate our design.

Is that, is there something like that likely to happen.

MR. ROSENTHAL: Yes.

DR. WALLIS: And what sort of things would be likely.

MR. ROSENTHAL: Well, I think we told you earlier that we would expect that Licensees, these places were originally designed with flat core power distributions and high, and hence higher fluence in the vessel walls.

They'll want to regain some of that margin because it limits them with respect to the TCT and things like that. So they'll flat, and also fuel economy. So they'll go back to, to some degree, to flatter power distributions and higher fluences. But I think that we've addressed that.

DR. KRESS: And eventually it might even lead to a second license extension.

MR. HACKETT: It could be. So flatter power gives you more margin to LOCA and DNB.

DR. WALLIS: It is the immortal vessel.

DR. SIU: Well, recognizing of course that PTS is one class of scenarios and Mark talked earlier about some other considerations that would have to be thought about before we make these changes.

MR. ROSENTHAL: I also suspect I'm talking about less than factors of two on an issue with multiple orders of magnitude of certainty.

DR. SIU: Questions?

DR. KRESS: I think it's pretty clear what they did.

DR. SHACK: I mean you would come back to essentially your start up shut down would then be your limiting vessel operation and however you decide to change that, in all likelihood it would still end up being probably the controlling thing on the vessel.

DR. KIRK: Yeah, well the only reason why, at this stage, the start up shut down would be more limiting is having done this analysis where we've made our best effort to be realistic.

And when you consider that Appendix G

includes many of the same varied conservatisms or greater than we started with here, then they've not been done on a consistent basis.

But certainly you know, given the difficulty that a significant LOCA has in breaking the vessel, it's very difficult for me to envision that a controlled heat up and cool down, even done, you know, as aggressively as you would want to from an operational perspective, is going to be of any significant challenge whatsoever.

DR. KRESS: If I may ask you a strange question. When we talk about safety goals, prompt fatality safety goals, it was said because the way it was there were considerations that have at least 100 plants out there operating for about 40 years at that level of safety.

It kind of was that consideration. Now you've got one plant that you're talking about that's already used up all of its life and it's only honorable to set of sequences a short time. So the question is why isn't reasonable to think the safety goals is the right value to use here when, it's all right, I think you're all right with the safety goal, but was that even at in your thinking?

DR. SIU: No. Yeah, we actually, the

question came up in our recent public meeting from a somewhat different angle. That, whether the fact that the plants are only approaching this level or risk toward the very end of their life, whether that makes a difference.

Clearly it could. Safety goals, as I understand them, regardless of how they were derived, are stated in sort of an instantaneous frequency terms and that's kind of where we are.

DR. SHACK: And where we should stay.

DR. WALLIS: I wasn't around, sorry. Did you talk about the long term cooling or the long term situation at this station after it's had such an event?

DR. SIU: No, we were focused largely on the large early release issue.

DR. WALLIS: Yeah, I know that's the way that this Agency thinks. But I think the public might be concerned about something with was not clueable in the long run.

DR. KRESS: See, I have one convert already.

DR. SIU: But I guess again if you equate, even, and I think we've shown because of independence of various systems, that the occurrence of the PTS event does not equate, it's not equivalent to the occurrence of core damage. There is some margin. Certainly for some scenarios.

But even if you were to equate it to core damage, setting the limit at ten to the minus six per year for that should addressed that concern. And you're saying you just, I mean this is the point, I guess, Dr. Powers was making, there's very, very low likelihood this event is going to occur.

So low that it's in the thinking behind Reg Guide 1.174 and the definition of what small means. It's small, almost you can't measure it.

DR. SHACK: Well, I suggest we take a 15 minute break at this point and we can come back to discuss this proposed screening criteria.

DR. KRESS: Thank you, Nathan, that pretty well answered my questions on this.

DR. SIU: Thank you.

(Whereupon, the foregoing matter went off the record at 3:20 p.m. and went back on the record at 3:37 p.m.)

DR. SHACK: Back into session.

DR. KIRK: Okay. This is the discussion of the considerations regarding a new proposal on a materials based PTS screening limit. I've added a few slides here to try to make the points more clearly.

I'll start by reviewing some operational challenge considerations, discuss some materials considerations and then lay out the characteristics one would like to see in a physically motivated embrittlement metric, and then show you how the heck we got to RT_{NDT^*}.

And I should point out this is simply one possibility among many, but we think it has some desirable features. Operationally you've seen the graphs on this slide before in our discussion of the plant-specific results.

But the point I'd like to reiterate is what's shown in yellow that all materials factors held equal, the severity of PTS challenge is remarkably similar between the plant study. And the frequency of challenge is also fairly similar but with some greater plant dependencies.

The reason for pointing this out is this observation leads us to at least one metric of success on our embrittlement metric that we shouldn''t be really expecting to see much separation between the plants if we get the embrittlement metric right.

From a materials viewpoint, again, this is a repeat, but we'll reiterate the axial weld flaws and the material properties that can be associated with axial weld flaws are what's driving the through-wall cracking frequency.

So to set up, what do we want to see an embrittlement metric? Well, certainly, what we'd like to see is, again, shown in yellow. We'd like there to be a causal relationship between the embrittlement metric and through-wall cracking frequency.

Or, as my ten year old would say, you want to blame the right person for the failure. Don't go picking on me, it was my little brother that broke the vase. So given that principle, the axial weld and plate property should dominate the embrittlement metric because those are the properties that can be associated.

DR. KRESS: Is that because there are so many more axial welds than there are circumferential welds?

DR. KIRK: No, no. It's because the axial flaw orientation produces a higher crack driving force, than the circumferential flaw. And also --

DR. KRESS: Yeah it would with the thermal shock.

DR. KIRK: Right. And also of particular importance that higher driving force perpetuates much deeper into the vessel wall. The circumferential crack are much more likely to arrest. However, it is possible to get circumferentially oriented cracks that can fail the vessel.

So they do play a minor role. A third important point is of course that the relevant fluence has to be that where the flaws are. So the relevant fluence is that along the welds and that the large regions of plate and forging remote from the welds really don't count for much.

So these are some slides that I inserted, that since we have time, I thought we could step through.

DR. BANERJEE: Could we have copies of these?

DR. KIRK: Yes, absolutely. I thought we could step through these to go from an embrittlement metric of the type that we've got now to the one that we're proposing, so you can sort of see the thought process rather than just be confronted with a screen of algebra.

First off, there will be no margins here. So we're just not going to go there again. It was too painful the first time. So all RT_{NDTs} that you'll see plotted here would reflect an unirradiated RT_{NDT} plus an RT_{NDT} shift appropriate for the irradiation conditions of interest.

So right now the way we evaluate a vessel, setting aside the margin part, is we characterize the vessel as having the maximum RT_{NDT} , wherever it is in the vessel evaluated at the maximum fluence.

DR. WALLIS: So this RT_{NDT} here you're plotting is something that comes from the ASME formalism for evaluating and it doesn't come from anything you've corrected for your, the epistemic thing, it doesn't come from anything that gets you to the mean instead of the extreme. This is the traditional ASME RT_{NDT}?

DR. KIRK: Yes, yes. And the reason why we're using that is not because the traditional ASME RT_{NDT} has any desirable features except the one desirable feature it does have is that we've established and docketed a value for each and every material in each and every plant.

DR. WALLIS: And people know how to measure it.

DR. KIRK: And that's about the only thing it's got going for it.

DR. WALLIS: Isn't it also true that people know how to measure it, it's sort of traditional they know how to get it.

DR. KIRK: Yes, that's correct.

DR. SHACK: Wait, let me, let me, so you're not correcting for the 65 degree bias?

DR. KIRK: Yes and no.

DR. WALLIS: Oh, well, you can't have it both ways.

DR. KIRK: Yes, I can have it both ways. The correction for the 65 degree bias is inherent to these values. Because these have been calculated by FAVOR. However, there is no correction for, this is the straight ASME RT_{NDT} here. So we're just using that value, but these values have all the biases and aleatory and epistemic, that's all been accounted for.

DR. WALLIS: That went into the calculation.

DR. SHACK: The semi-regulatory RT_{NDT}.

(Laughter.)

DR. KIRK: Yes, that went into the TWCF.

DR. WALLIS: It didn't go into the RT_{NDT}.

DR. KIRK: Yes.

MR. ROSEN: Semi-log.

DR. KIRK: You can tell it's getting late in the day. Okay, so what's on the horizontal axis is the ASME RT_{NDT} plus the Sharpy shift, Sharpy shift evaluated from the Eason formula. No margin, no nothing. So all the information that was needed to calculate this is in the ASME RT_{NDT} method, Eason embrittlement formula and copper, nickel and phosphorous values in the Arvid database.

And what you come up with is a significant separation between the plants and in particular, you know this is, or one would expect that this wouldn't relate things terribly well because, for example, in Oconee the maximum RT_{NDT} is in the circ weld.

And we've already told you that the circ weld doesn't contribute much. So, in the context of my sons, I'm blaming the circ weld for breaking the vase, but actually it was axial weld that did it. So one.

DR. BANERJEE: Excuse me, what is the physical reason for the separation?

DR. KIRK: There is none. It's the wrong metric here. That's what we're trying to get to.

DR. BANERJEE: Oh, it's still the wrong metric? Oh, okay.

DR. KIRK: Yes. I'm working you to, remember I started here and said that a physical appropriate metric would have all the, there would be a causal relationship between the thing we're plotting on the X axis and the result on the Y axis.

The problem with the first iteration, which is very much akin to what we do now, is that causal relationship is broken because we just pick the maximum RT_{NDT} in the vessel and that might be a circ weld, and we know that circ welds aren't major contributors.

So at the first step, if we just take that out and say, okay, well, --

DR. WALLIS: Wait a minute. This RT_{NDT} is a function of, it's different for different welds on different parts.

DR. KIRK: Sure.

DR. WALLIS: I thought you got it from a Sharpy test. You do a Sharpy test of a weld?

MR. HACKETT: That's a way of getting it. There are a number of ways if you go through, as you're indicating ASME has methodology for getting at RT_{NDT}, and you can get it through measuring Sharpies, through drop weight NDT tests.

There are other forms of estimation, but yes it will work for different welds. It will vary upon conditions. The fundamental problem we're up against here, I just thought I'd mention it to see what it's worth.

Is we're trying to regulate to fracture toughness, which s the meaningful parameter here. The problem is these plants were all licensed before fracture mechanics, frankly, was all that much developed.

So we're sort of back fitting a science on something that wasn't ready for it and never will be. You know, in the case of the plants that are out there. So, as Mark is saying, you're trying to use a fairly imperfect estimator or index of the material toughness in RT_{NDT} to try to get to a fracture toughness or sort of more what is truth.

And it's got all the warts that you're seeing here and that's why it's so confusing.

DR. BANERJEE: These are measured RT_{NDT} at the inside of the vessel wall, I mean from specimens.

DR. KIRK: No, no. Let's be clear. What's going into all these, anything down here is the unirradiated, the RT_{NDT} measured before anything started, plus the Sharpy shift or the RT_{NDT} shift if you will, evaluated based on an embrittlement trend curve correlation evaluated using copper, nickel and phosphorous values that have been docketed by the plants as being representative of their materials.

DR. WALLIS: It's got the irradiation

shift which is in your Appendix as a shift NT_{zero}.

DR. KIRK: That's right. No, it's got the irradiation shift that's in the Appendix is a shift in Sharpy that's in the --

DR. WALLIS: Delta T-30, then.

DR. KIRK: Delta T-30, yes.

MR. HACKETT: And then a further clarification on the unirradiated RT_{NDT} as you're indicating in some cases their measured values. In other cases they're not. And that's as defined in our 10 CFR 50.60, 50.61, as to what you can and can't do there.

DR. KIRK: All the complexities and the different ways, and indeed I would agree with anybody that says that are current RT_{NDT} methodology is confusing. But all the complexities and the different ways of getting RT_{NDT} and Sharpy shifts and so on have been incorporated in the FAVOR methodology and so are reflected in the vertical axis values.

What we're simply trying to do is find a meaningful yet easy to evaluate based on available data parameter on the X axis to use.

DR. KRESS: That has a one-to-one correspondence for all plants for that side over there.

DR. KIRK: That would be the hope and the reason for putting --

DR. KRESS: Or as close to it as you can get.

DR. KIRK: The reason for putting up the previous slide was simply to suggest that the fracture mechanics tells us that if you had, if you had these three different vessels and held the embrittlement equal and put one flaw in them of the same size, the level of challenge of these various dominant transients is not grossly different from the different plants.

DR. KRESS: And that would be what would separate them.

DR. KIRK: That's right, that's right.

DR. WALLIS: Maybe your final report you'll have RT_{NDT} with some superscript or something which says ASME or regulatory or best estimate or whatever, so we know which one you're talking about.

DR. KRESS: When you get ready to make the rule, you won't even have that other stuff in there.

DR. KIRK: Yeah, certainly we could do a lot better on nomenclature. I'd be the first to agree with that.

DR. BANERJEE: So are those lines then a

function of fluence or did you just pick it at some point in time now.

DR. KIRK: Yes, they are a function of fluence. These are evaluated per example for --

DR. BANERJEE: At what fluence are they evaluated, those curves?

DR. KIRK: These are evaluated at the peak fluence in the particular material region --

DR. KRESS: For that plant.

DR. KIRK: -- for that plant. Well, no, because you've got different --

MR. HACKETT: At a particular time.

DR. KIRK: -- at a particular operating lifetime. So right now the formalism that you go through in 10 CFR 50.61, is you look at all the different plates, welds, forgings in your plant, you find the peak fluence within that geometric region and you evaluate the Sharpy shift based on your copper, nickel and phosphorus values at that peak fluence.

Then you find the highest value of all your different welds, plates and forgings, and that's what Mr. Mitchell will be forced to evaluate your plant based on. And so this is --

DR. WALLIS: That's your X axis.

DR. KIRK: That's the X axis. So this is

the parallel to the current regulation. But pointing out that at least in one case, for Oconee, we're plotting the results from a circ weld, and we know from doing the FAVOR analysis that the circ weld hardly contributed at all through all cracking frequencies.

So again we're posing a causal relationship where one doesn't exist.

DR. WALLIS: The most striking thing is the yellow, the Palisades is about two orders of magnitude above Beaver.

DR. KIRK: I would caution you not to interpret this, because that separation is not real.

DR. WALLIS: We have to interpret it if you show it to us.

(Laughter.)

DR. KIRK: Well, then I'll take it out. Really we know that in Oconee, as in all the plants, it was the properties associated with the axial cracks. So it's either the higher of the axial weld properties or the fake properties that are controlling the through-wall cracking frequency.

So when we take out the Oconee circumferential weld, which was there, and plot the Oconee axial weld, which is there, now Oconee and

Palisades, which are both axial welds, are correlated reasonably well.

The flyer down here is Beaver. Now when we came up with this result, Denny Weakland, who is the Chief Metallurgist at the Beaver Valley plant, was extremely happy because all of a sudden his plant, which is within fractional degrees of the PTS screening criteria was somehow far less embrittled than Oconee which is so far down that nobody at Oconee really cares much about this.

And that was all terribly surprising. But, as I pointed out earlier, the problem with this procedure is that the current procedure, you find the peak fluence anywhere in your material region and you combine that with the copper, nickel, phosphorus and evaluate your embrittlement shift.

The problem, the reason this didn't work so well for Beaver, is Beaver, with the help of Westinghouse, has intentionally placed their fluence peaks way out in the middle of the plate. Not at the weld, where the cracks are.

So that where the cracks are is actually in a fluence trough.

DR. WALLIS: It sounds like a good design.

DR. KIRK: It's a very good design. So if you evaluate now the Sharpy shift for Beaver Valley with the appropriate fluence, that being at the axial weld, you find that it now agrees fairly well with the other results.

However, it should also be noted that we've said and we keep driving home and you're probably sick of hearing it, like most things I say. That the axial flaws and the axial welds are important.

Well, in Beaver Valley and Oconee, there are two axial welds. In Palisades, there are three axial welds. So, again, all other things being equal, Palisades has half again more axial welds and half again as more axial flaws as Beaver Valley and Oconee.

So if you normalize out the weld length effect, you get a slightly better correlation.

DR. WALLIS: You seem to be struggling to get us back as close as possible to the 270 to 300 degree range.

DR. KIRK: But it's a different number.

MR. HACKETT: And he'll never be able to explain that.

(Laughter.)

DR. KIRK: Yes, I will.

DR. SHACK: It's amazing how well the Reg

Guide PTS does to correlate the data.

DR. WALLIS: Although it's the wrong one.

DR. KIRK: And that's got the margin in it and you know your colleague will never accept that.

DR. SHACK: I only look at the data.

DR. KIRK: Yeah, yeah. So what we came to, that was the thought process. But what we came up with as a weld length weighted embrittlement metric is illustrated on the screen here. And I'll, if you're interested, I'll try to step through this.

It includes to waiting factors and two weld length weighted reference temperatures. One weighting factor is for the plate and axial weld properties and it ranges anywhere from 90 to 97 percent contribution, which is consistent with our results.

And then you've got a reference temperature for plate and axial welds which depends upon the most embrittled of the two materials.

MR. HACKETT: I think you may have out done Nathan in powerpoint.

DR. KIRK: We're dueling, but he makes movies, so he beat me. The length of the weld and the max fluence along the weld. Then there's a weighting factor for plates, forgings and circ welds, which is good for anywhere between three and ten percent depending upon the number of circ welds, of course, the most embrittled material on either side and the max fluence along the weld.

Now, I have to admit that I was truly appalled that about five cells on a spreadsheet, which is really nothing more than a weighted average, turned into this much algebra when I laid it out, but that's how it turned.

DR. WALLIS: There was something I didn't understand in your report and that is that subscript U in parentheses.

DR. KIRK: Unirradiated.

DR. WALLIS: That's unirradiated? I thought it was something to do with uncertainty.

DR. KIRK: Certainly not, no, no.

(Laughter.)

DR. KIRK: No. Okay.

DR. WALLIS: It's not described, it's not defined, and I looked for it and I couldn't find it.

DR. SHACK: It's defined in the Appendix. Well, it's not, it appears.

DR. KIRK: This does my heart good that clearly people have read this report. And you've been sleeping well, I'm sure.

DR. WALLIS: Do you really want to know?

(Laughter.)

DR. KIRK: No, too much information. So when you put that, now if you use the RT_{NDT*} metric or the weld length weighted formula from the previous page, this is the relationship you get between the mean through-wall cracking frequency and RT_{NDT*}.

So taking the reactor vessel failure frequency criterion of one times ten to the minus six, one comes out with a 290 degree Fahrenheit RT_{NDT*} screening limit.

However, I should point out, as is probably obvious, that RT_{NDT*} is not the same as RT_{PTS}. First off, it doesn't have that blasted margin term which is good for at least 60 degrees. And when you do just a simple correlation, and it obviously various with fluence and a whole host of other things. But as an order of merit RT_{NDT*} is about 90 degrees Fahrenheit less than RT_{PTS}.

So at 290, RT_{NDT*} screening limit turns into approximately a 380 degree Fahrenheit RT_{PTS} screening limit. Or approximately an 80 to 110 degree Fahrenheit increase over the current screening limit is possible and still stay below one times ten to the minus six.

One other thing to point out is that as you saw in the earlier graphs when we were plotting versus effective full power years, in order to get results up in the E minus five, E minus six range, we had to go to what I think everybody would agree is absurdly long operating times.

And that all the results at reasonable operating lifetimes are considerably below the acceptance criterion limit. A couple of other points to make. One is that as we've discussed earlier, because these distributions are so skewed, the mean through-wall cracking frequency corresponds roughly to the 95th percentile through-wall cracking frequency.

And this next slide, I'm not sure if I see him, was motivated by a comment that Mark Cunningham made the other day about, you know, could we think of this in terms of a margin.

And he suggested plotting the, plotting where the median correlation would be drawn. So I, I didn't have time to go back to all the spreadsheets, but I sketched it on there that at the highest levels of embrittlement we looked at, there's approximately a one order, the median is about one order of magnitude down from the mean.

DR. KRESS: They only like to use a median if you don't really believe the tales. So here you're saying we believe the tales. So we don't, it's not a real margin.

DR. WALLIS: You wouldn't want to use the median anyway, would you?

DR. KIRK: No, I'm not suggesting to use the median. I'm just suggesting that there is a, if there is a significant different in either temperature or probability space.

DR. KRESS: Yeah, but it's not really a margin. So I would be careful about calling it that.

DR. WALLIS: Stay away from the word margin.

DR. KIRK: I, based on my experience today, I would agree.

DR. KRESS: And besides you don't need it.

DR. KIRK: And speaking of margins, and why we shouldn't use them, margin on RT_{NDT*} would be neither appropriate nor necessary and I came up with this slide far before I heard of Dr. Wallis' comments.

And this gets back to what I mentioned to Dr. Ford earlier. That buried in the guts of the FAVOR calculation we've reflected the maximum material uncertainties in FAVOR, because we've used generic data to derive these uncertainties.

And they've been explicitly accounted for. So any plant state of knowledge has to be better than we've simulated here. And also, if it hasn't already become clear, I would like to point out that this particular limit pertains only to one particular pathway of getting to this new proposed RT_{NDT} metric.

It's based on a measured unirradiated value and copper, nickel and phosphorus plugged into a particular embrittlement shift. There are certainly many other ways that at least in current practice the licensees will evaluate RT_{PTS} and --

DR. WALLIS: Tell me about that measured value. I'm not an expert on Sharpy and all this history of RT_{NDT} . But it looks from the data and I may refer to Chapter 1, I think it's Figure 1.3, it looks as if there are a lot of scatter on the curves looks not to be all the same shape and all that.

When you do these tests, are they repeatable.

DR. KIRK: I'm sorry, 1.3 is --

DR. WALLIS: Well, I mean, it's K versus RT_{NDT} for different steels. The EPRI data. How repeatable are these tests that give you this RT_{NDT} and what's the uncertainty in the test itself.

We seem to be treating this RT_{NDT} ASME as if it were something that was really known.

DR. KIRK: The unvarnished answer is it's not very repeatable at all. However, that uncertainty has been represented in the calculation.'

DR. WALLIS: Yeah, but when you plot something like RT_{NDT} on a graph, that is something which itself is very uncertain, isn't it?

DR. KIRK: That's correct. But the way RT_{NDT} has been designed, it's virtually impossible to underestimate it. Everything that you do in going through the, everything that you are forced to do by the ASME procedure, forces you to, if anything, overestimate the value.

DR. WALLIS: And that gets you to that Curve A in Appendix, way off to the side.

DR. KIRK: Yeah, yeah.

DR. KRESS: Are you going to sell this weighted thing to ASME and get them to change their ---

DR. KIRK: If we have enough time. Maybe.

DR. KRESS: It's not surprising that that weighted thing gives you a better correlation because it's based on your calculations, frequencies or contributions.

DR. KIRK: It's based on an understanding of what counts.

DR. WALLIS: So the Licensee has, this RT_{NDT} that the Licensee calculates, is that calculated by a formula giving all this chemical composition. Or is it calculated from tests on samples that are pulled out of the reactor.

DR. KIRK: Currently the answer is both. By the current regulation you are allowed to do both.

DR. WALLIS: And they have to be compatible or what? And how do you resolve, if you get different answers from each one.

MR. HACKETT: It all goes, it's all documented in 10 CFR. And also in the --

DR. WALLIS: All of the mystery there.

MR. HACKETT: Yeah, in the regulatory guide. But as Mark says you can come at a number of ways. The idea being that if you have data, you have hopefully somewhat greater certainty over what the actual property is.

But they also allow you to estimate if you don't have data, and they that's where you get into adding margins to hopefully address --

DR. WALLIS: That's what worried me is that, you know, everything is hung on this RT_{NDT}. You've done a great job of dealing with all these things, but I'm not quite sure how accurately the Licensee can estimate from these samples or whatever.

DR. KIRK: I think that we're not asking the Licensee to do anything really different than they've done before. And again the reason that it works in this case is that RT_{NDT} as measured, it has to be a bounding property. There is no way to do otherwise.

DR. KRESS: I think if you give 20 people the input that goes into calculating that from a given plant, which they just gather, they'd all calculate the same number.

DR. KIRK: Yeah, yeah, given the input.

DR. KRESS: Given the input, it's only, it's just the input that's the problem.

DR. WALLIS: The input, if it's a bound, because bounding means you have to have enough points to determine what's bounding. And it may be that some erratic point pushes the bound out.

DR. KRESS: Well, if they have to measure their copper and --

DR. WALLIS: But they don't have very many samples in the reactor. They are using experimental data.

DR. KRESS: Then they have to assume they got a certain amount in there and that's where the conservatism comes in.

DR. BANERJEE: Can they measure these things based on the surveillance samples in the reactor. I mean which are actually being exposed to fluence and all this stuff.

MR. HACKETT: Again, unfortunately the answer depends, depends on whether they have the limiting material in their surveillance program for that reactor. Or are they relying on, let's give an example.

In the case of the B&W plants, they have an integrated surveillance program where you may use Oconee's results to predict Three Mile Islands irradiation damage. But you have to argue some kind of equivalency of the irradiation environment.

So the answer there also is a mixed bag.

DR. BANERJEE: Presumably the fluence can be pretty accurately calculated.

MR. HACKETT: Presumably.

DR. BANERJEE: Presumably. There's another question I have. The mean TWCF, that you have there, that's a function of a whole lot of things. And it's sort of surprising that all these things collapse so well because that suggests that the sequences which are sort of risk dominate plus the transients plus all things are very similar between these plants. Essentially there is not too much difference between them.

DR. KIRK: And that was the slide that I tried to emphasize at the beginning of this development is, yes, it seems to be surprisingly so that the, between these plants the level of challenge if you will is indeed remarkably similar.

MR. ROSEN: And that's because it's dominated by LOCAs and LOCAs are primary system phenomenas that are relatively the same in BWRs. Even once-through steam generator PWRs and recirculating steam generators PWRs are not affected because the primary systems are pretty much the same even though the steam generators are different and behave differently.

You're looking at what happens when you punch a hole in the reactor system. And that's the same in a PWR. They both start out at 2,200 psi roughly and depressurize and there you are.

Operators go, oh, no, my gosh, keep your hands off, make sure the reactor scrammed and that's it.

DR. KIRK: Yup, that's correct.

DR. KRESS: And they use generic frequencies for their bin.

MR. ROSEN: So it's not a surprise.

DR. WALLIS: Now to get back on this, I'm sorry to keep on this. You did a beautiful analysis of epistemic RT_{NDT} and I thought what you were doing there was you were looking at taking this ASME RT_{NDT} how well does it correlate real toughness data.

And how well does the theory represent this real toughness data. That was what was your epistemic analysis. And that still assumes that one has a very good way of knowing what that ASME RT_{NDT} is.

DR. KIRK: No, actually it doesn't. Those ASME RT_{NDT} values, I mean the distribution that we showed before is that they are on average about 60 degrees too high.

DR. WALLIS: That's why you have this epistemic and --

DR. KIRK: That's right, that's right.

DR. WALLIS: That's if you want to get toughness results out of it.

DR. KIRK: That's right.

DR. WALLIS: But it may well be that some plants don't do a very good job of analyzing their samples. And that's not in there, is it?

DR. KIRK: Of analyzing the RT_{NDT} samples? The more careless somebody is doing an RT_{NDT} test, the more conservative it becomes.

DR. WALLIS: That doesn't make sense.

DR. KIRK: Because if you, okay, if I'm, when you do, when you test for RT_{NDT} you have to take these specimens that have a brittle weld bead on them and a notch and you have to go until you establish a break/no break condition.

DR. WALLIS: You either bust them or you stretch them.

DR. KIRK: Well, actually you have to just simply establish a no break condition.

So if I want to do that with a minimum of samples, I pick a high temperature, I slam the hammer down and I decide it hasn't broken.

That doesn't mean that the real temperature between break and no break might be 100 degrees Fahrenheit lower. I can always overestimate RT_{NDT} , I can't under estimate it by the way you go through the procedure.

So if I want to be, if I wanted to be very precise, I'd get a whole bunch of specimens and very carefully bracket the break/no break temperature. But all the ASME MB-2331 requires me to do is demonstrate no break performance.

DR. WALLIS: So long as it hasn't broken.

DR. KIRK: Yeah. So if I've only got two specimens to do that with, and I want to establish a code value, I'm going to guess high.

DR. WALLIS: Well, I guess I'm saying is that there's got to be quality control in the way it's tested and all that kind of stuff as well.

DR. KRESS: That's pretty standard.

DR. WALLIS: So standard that you have no doubts at all about that.

DR. KIRK: Yeah, the way the tests are conducted is indeed standardized and controlled by ASTM. The procedure you go through, if you will, to discern RT_{NDT} based on $ASTME208_{NDT}$ data and ASTME 23 Sharpy data is not very well specified. But, and this is the only good but, the way it's not well specified is that it forces you to overestimate the value.

MR. ROSEN: Now help me with my understanding of how to use this chart. If I'm in Oconee, Beaver or Palisades, I'm right on the 290 degree screening limit. Is that right?

DR. KIRK: Only if you operate your reactor until about the time that warp technology is invented.

DR. SHACK: When you replace it with your fusion plan.

MR. ROSEN: Why is that? I guess I must have missed that part of the discussion.

DR. WALLIS: Where are they now on this curve?

DR. KIRK: The now on the curve, everybody now is in the yellow oval.

DR. FORD: Even below it.

MR. ROSEN: Everybody.

DR. WALLIS: Well, they slide off the curve as they go on.

DR. KIRK: Yes, so time increases this way. And for Palisades that was a 500 year analysis. For Oconee, that was a 1,000 years. And for Beaver that was 100.

MR. ROSEN: Okay, because of the two orders magnitude. So you're saying that a clean plant now, low fluence, good materials is going to be off the bottom of that thing.

DR. KIRK: Yeah, because these were two of the --

DR. SHACK: The difference is really materials, not, I mean they're all going to have roughly the same fluence per years of operations, but the materials respond very differently.

MR. BESSETTE: Some plants have neutron belts, neutrons pads.

DR. SHACK: Yes, but I think that's small compared to the material difference.

DR. WALLIS: So for Oconee to get up to one to the minus six, it would be several thousand years?

DR. KIRK: Yes, a thousand.

DR. WALLIS: So it's not just 60 to 80, it's thousands of years.

MR. ROSEN: I don't its turbine will last that long.

(Everyone talking amongst themselves.)

DR. FORD: Mark, could I ask. Up until the time you showed us these graphs, I was absolutely with you.

(Laughter.)

DR. FORD: And I can understand why you're going the way you are. But you're making one big assumption. The assumption is that there is one unique curve, that one that you've shown there, which normalizes all plant.

And that's an assumption that I haven't

heard questioned physically. And then the second one you've gone into a bit of a g-ray pokery about a whole lot of different equations with ten percent and circumferential.

And I can understand where they came from, but I don't understand why they are on those specific algorithms that you've put down on this slide here. Now I don't doubt, the derivation of those long equations were being driven by the fact that you want there be one curve.

And I just feel uncomfortable because I don't understand some of those physics.

DR. KIRK: Actually the thought process here, I mean, honestly, the idea was what's shown on the screen now. Was simply to say, okay, let's lay the blame for through-wall cracking frequency on what's to blame.

So, let's not say that circ welds contribute a lot. Let's take account of differences in weld length. Let's get the fluence right. So all these things were done, and I shot myself in the foot by not presenting this in time sequence.

All these things were done and we got to, now I can't go fast through this damn thing.

DR. WALLIS: How did you pick 90 and 97?