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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.

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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.

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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.

17

18

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

19

MR. BESSETTE: Preliminary, yes.

20

DR. RANSOM: -- select the worst --

21

22

23

24

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.

25

DR. WALLIS: Well, I think, David,

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

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

10 DR. WALLIS: They don't determine  
11 anything.

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

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

17 MR. HACKETT: We do not right now.

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

22 And to establish the validity of  
23 uncertainty studies. Now there's no uncertainty study  
24 presented, so I don't know what that means. Because  
25 I don't, I don't know what's being established as

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1 valid because there is no uncertainty study in the  
2 report.

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

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

11 So, what I would propose at this point,  
12 since we don't want to waste the Committee's time in  
13 that regard, these are results that have been shared  
14 --

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

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

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

24 MR. BESSETTE: To some extent. It's not,  
25 I would say again, it's not, it can't be the final

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1 word because the final word is only obtained after you  
2 run these results through FAVOR.

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

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

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

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

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

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

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

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

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1 would expect in the parameters.

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

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

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

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

21 MR. BESSETTE: Yes.

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

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

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

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

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

18 DR. SHACK: That's fine.

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

22 MR. HACKETT: Absolutely.

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

24 MR. BESSETTE: It's difficult to tell you  
25 definitively about thermal hydraulic uncertainties in

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1 a stand alone basis. Because you can only tell if  
2 they're important, they are of relative important  
3 after you get the FAVOR output.

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

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

10 MR. BESSETTE: They are there.

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

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

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

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

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

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

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

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

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

24 DR. KIRK: Probabilistic fracture  
25 mechanics in six slides or so. Okay, all, we all know

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1 the PRA goes through TH and then comes into PFM. To  
2 expand PFM a little bit more in terms of what's inside  
3 the box, and again, of course in the report it goes  
4 into even greater detail.

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

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

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

16 DR. KIRK: In one slide.

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

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

25 All of that goes into the crack initiation

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

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

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

11 DR. KIRK: Quite a bit.

12 DR. WALLIS: Quite a bit.

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

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

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

23 DR. KIRK: Yes, yes.

24 DR. FORD: Mark, on that item, this was  
25 brought up at one of our earlier meetings. Do we have

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1 a good factual basis for the fluence attenuation  
2 through thickness of the wall that will impact on  
3 crack arrest?

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

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

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

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

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1 DR. KIRK: The other thing to point out is  
2 that, so we've adopted, quite independently of the  
3 EPRI report, but we adopted the Reg Guide 1.99, Rev.  
4 2, attenuation function. And I think if you ask me  
5 for a technical basis for choosing that, I'm going to  
6 reference the EPRI Report because it is indeed very  
7 good and I learned a lot.

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

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

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

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

24 DR. KIRK: Yes.

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

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

3 DR. KIRK: That's correct.

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

7 DR. KIRK: Yes.

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

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

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

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

24 DR. KIRK: Weld overlay, yeah.

25 DR. WALLIS: Isn't there a source of flaws

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1 in that weld overlay?

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

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

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

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

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1 five percent.

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

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

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

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

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

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

20 DR. KIRK: Yes.

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

24 DR. KIRK: Yes, yes indeed.

25 DR. FORD: And it happens to be from the

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1 austenitic.

2 DR. KIRK: Yes.

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

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

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

10 DR. KIRK: Yeah, yeah.

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

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

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

20 DR. KIRK: Right, right, yes.

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

23 DR. KIRK: No, but our thermal stresses  
24 don't come from those analyses. Our thermal stresses  
25 are calculated from the thermal hydraulic and the

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1 conduction equation, yeah.

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

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

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

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

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

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

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

25 In addition to that, the NRC Research

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

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

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

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

18 DR. KIRK: In all cases, yeah.

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

21 DR. KIRK: No, those were laboratory  
22 generated flaws. The characterization of flaws that  
23 are typical of what you would find in a reactor came  
24 out of the flaw distribution work that was conducted  
25 at the Pacific Northwest National Lab where they, both

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1 non-destructive and destructively evaluated primarily  
2 welds, but also have done works on plates, forgings  
3 and the stainless steel liner that we were just  
4 talking about.

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

10 I'll go through this and --

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

13 DR. KIRK: Yes.

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

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

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

7           DR. KIRK: Yes. Okay.

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

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

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

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

24          As we've already pointed, also, most, and  
25 by most I mean like 98 percent of the flaws are now

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1 embedded rather than surface flaws. And that's a  
2 major difference. However, there are many more flaws  
3 than there were before.

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

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

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

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

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

24 MR. DICKSON: Terry Dickson, Oak Ridge  
25 National Laboratory. The simple answer is no. We,

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1 when I did that sensitivity analysis the, paper that  
2 you are referencing, I just bundled it together and  
3 did the analysis.

4 DR. SHACK: Everything this is in there.

5 MR. DICKSON: Yeah, yeah.

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

8 MR. DICKSON: No.

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

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

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

17 MR. DICKSON: There you go.

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

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

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

25 DR. SHACK: So if you could show that the

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1 location of the flaw really drives this all, then it's  
2 a warm feeling.

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

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

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

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

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

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

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

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

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

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

23 DR. KIRK: Yes.

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

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1 another welder?

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

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

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

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

24 We do have a program that can predict  
25 those kinds of distributions. And we have run

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1 simulations with that code versus the data, and again,  
2 we get some pretty good comparisons. As Mark is  
3 indicating, the best data we have is from Shoreham.

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

7 MR. ROSEN: The BWR vessel.

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

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

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

19 MR. HACKETT: Yes, right.

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

22 DR. WALLIS: How big is that difference?

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

25 DR. WALLIS: Well, you're claiming one of

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1       them is typical of all and then you've got another one  
2       that's different. What should I conclude?

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

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

10              DR. KIRK: That's right.

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

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

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

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

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1 to the welding in general.

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

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

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

18 MR. HACKETT: That's right.

19 DR. KIRK: In areas other than flaws, in  
20 fluence we've used the calculational methodology  
21 expressed in our NUREG Guide. And the major change in  
22 our representation of fluence, relative to how we  
23 represented it before, is we recognized the spatial  
24 variation in fluence whereas previous analyses assumed  
25 that the maximum fluence existed throughout the vessel

1 which is an obvious over conservatism.

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

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

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

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

22 And has been widely recognized, we don't  
23 have a complete answer on that, but I would like to  
24 point out that some of the arrows are bigger than  
25 others. And the one related to removal of the

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1 systematic conservative bias in  $RT_{NDT}$ , is indeed a  
2 pretty big arrow on the, on the graph on the bottom of  
3 this slide.

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

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

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

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

23 DR. KIRK: Yeah, yeah. But in fact, and  
24 you're right and that can be, can certainly be better  
25 described. But the difference here is more than just

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1 the difference between a bounding curve and a best  
2 estimate curve.

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

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

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

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

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

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

22 DR. KIRK: That's the connection.

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

25 DR. KIRK: That's correct.

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

4 DR. KIRK: Okay.

5 DR. SHACK: Just a note on your  
6 presentation in Section 2314, you're very careful to  
7 put the epistemic air in the initial RD<sub>NDT</sub>, but then  
8 the irradiation model is presented deterministically.

9 DR. KIRK: That's correct.

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

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

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

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

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

24 DR. FORD: Mark, one of the questions that  
25 came out again in one of the earlier meetings was this

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1 question about the Eason correlation for the  
2 composition effects.

3 DR. KIRK: Yes.

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

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

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

14 DR. KIRK: yes.

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

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

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

25 DR. FORD: Correct.

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

3 DR. FORD: Okay.

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

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

17 DR. FORD: Okay.

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

25 DR. KIRK: Yeah. And to just be complete,

1 so that Stan doesn't jump out of his skin, it should  
2 also be pointed out that while I've now, based on, in  
3 response to your questions, pooh-poohed the importance  
4 of either getting the attenuation function very  
5 precisely right, or getting the embrittlement  
6 correlation very precisely right in PTS.

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

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

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

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1                   Those combine to make a very significant  
2 effect.

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

7                   DR. KIRK: Yes.

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

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

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

17                   DR. KIRK: The absence of metal?

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

20                   (Laughter.)

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

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

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

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

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

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

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

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

19 DR. KIRK: That's right.

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

22 DR. KIRK: The conversion between the data  
23 that was taken and it's mathematical representation  
24 has been to assume that everything is, as Ed said, a  
25 fatigue crack or anatomically sharp crack which is,

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1 you know, clearly everything is not that and so  
2 there's, you know, there is a buried conservatism or  
3 a buried margin.

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

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

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

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

24           But that's combined with a 1-D TH model  
25 and a 1-D fracture mechanics model. Another, again,

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1 unquantified, but I feel very comfortable in saying  
2 major change from the past is previously we modeled  
3 the whole vessel as being made out of the most  
4 embrittled material.

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

10 DR. WALLIS: That does make a big  
11 difference.

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

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

20 DR. SHACK: It's time for lunch.

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

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

25 DR. KIRK: That comes from the

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

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

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

14 But fluence will be higher on the wall.

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

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

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

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

5 DR. KIRK: Okay.

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

12 DR. KIRK: Okay.

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

16

17

18

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## AFTERNOON SESSION

1:30 p.m.

1  
2  
3 DR. SHACK: It's time to come back into  
4 session.

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

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

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

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

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

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

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

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

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

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

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

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

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

23            I'd just like to take a moment to point  
24 out, on the left-hand side we showed the bounds of the  
25 distribution that we draw the mean or the median

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1 estimates from. I'd just like to take a moment to  
2 point out that these distributions have some  
3 characteristics that's common to all of our results.

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

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

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

22 It's in fact a very natural consequence of  
23 the physics of cleavage fracture that results in  
24 absolute minima of  $K_{Ic}$  and  $K_{Ia}$ .. And so you've got,  
25 if you look at the distribution that's shown here in

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1 blue for Beaver Valley, 32 effective full power years.

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

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

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

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

23 DR. KIRK: What's that?

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

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1 --

2 DR. KIRK: Can we end the briefing now?

3 DR. KRESS: Just forget about it.

4 (Laughter.)

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

11 So that's how all the distributions --

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

14 DR. KIRK: Yeah.

15 (Laughter.)

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

22 However, in these distributions the mean  
23 in the 95th percentile approximately coincide. This  
24 slide speaks to what transients dominate through-wall  
25 cracking frequencies. And we've already sort of

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1 tipped our hand on this, in that for Westinghouse and  
2 CE Design Plants, LOCAs are the dominate contributor  
3 to risk.

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

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

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

23 And he said, well, that's great that the  
24 through-wall cracking frequencies are so low, but how  
25 is it made up. And of course, at least notionally,

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1 the through-wall cracking frequency is a product of  
2 how often things happen.

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

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

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

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

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

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

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

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

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

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

24 There is not much an operator can do in  
25 response to a LOCA. This graph, I'll apologize to the

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1 non-fracture mechanisms in the room because this is  
2 one of those inverse double normalized fracture geek  
3 plots.

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

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

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

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

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

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

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

24                   However, it was really an important  
25 contribution only in the B&W plant, and that occurred

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1 due to the greater tendency to decouple the reactor  
2 coolant system from the secondary due to the B&W steam  
3 generator design.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

24           And then, again, you've seen this type of  
25 presentation before. Just a comparison of the level

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1 of fracture driving force severity for the secondary  
2 side transients is relatively equal between the two  
3 plants.

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

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

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

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

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1 frequency.

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

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

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

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

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

24 DR. KIRK: We didn't do an analysis beyond  
25 100 years. At a, we stopped, obviously we had an

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1 inconsistent number of years. The consistent thing  
2 was we stopped running these analyses when we got  
3 total through-wall cracking in the E minus five, E  
4 minus six range.

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

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

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

24 From the materials side, the observation  
25 that nearly all of the weld flaws occur in the weld

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1 fusion line, the axial weld cracks therefore dominant  
2 the through-wall cracking frequency, so it's the  
3 properties that could be associated with axial weld  
4 cracks.

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

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

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

19 DR. KIRK: Yes.

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

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

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

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

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

25 If any, if a specific plant were to come

1 in, say Palisades, who spent a considerable amount of  
2 time measuring their material variability. They  
3 certainly have a greater state of knowledge regarding  
4 their material, their specific material, than was  
5 represented in these analyses because we use generic  
6 data and assume that the variability possible in any  
7 one weld was characteristic of the variability  
8 possible in all welds.

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

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

21 My guess is it's not going to influence  
22 them very much. Beaus I mean as materials people we  
23 look at distributions of copper and go, oh, my God.  
24 You know, that's really bad. And then Alan tells me,  
25 well, I've got a two order of magnitude certainty on

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1 how frequently this event occurs and all of a sudden  
2 I feel a lot better about what I know about copper.

3 DR. FORD: Okay. I have another  
4 question.

5 DR. KIRK: Okay.

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

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

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

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

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

25 But I think the insights that have come

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

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

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

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

24 DR. FORD: And then these other five  
25 plants, Fort Calhoun and the other four, they will be

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1 tackled in not quite maybe this rigor. I'm sorry I'm  
2 being --

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

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

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

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

25 So now we want to go out operationally and

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1 see do they both have the highest materials challenge  
2 and somehow have a greater level of operational  
3 challenge than we had seen in these other three  
4 plants.

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

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

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

24 To see if there's any operational  
25 challenge in any of these plants that is somehow more

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1 severe than something we haven't, than things that  
2 we've seen before.

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

7 DR. KIRK: Yes, it is.

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

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

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

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

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

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

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

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

14 Okay, I'm going to talk --

15 DR. SHACK: You could even cover the  
16 criterion.

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

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

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1 criterion using discussions of risk.

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

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

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

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

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

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

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

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

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

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

24           We also reviewed the results of the pilot  
25 plant calculations to look at the energy of the system

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1 at the time of reactor pressure vessel failure. So we  
2 were trying to use these calculations to inform the  
3 judgments that underlie qualitative analysis.

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

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

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

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

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1 sources.

2 We also, of course, wanted to bring in  
3 whatever thoughts had come about since the  
4 promulgation of the PTS rule in the 1980's, with  
5 whatever risk informing issues have occurred since  
6 then.

7 Principally the Reg Guide 1.174 and Option  
8 3 work. So we tried to make sure we were consistent  
9 with those, as we develop the options. These are the  
10 same options that we proposed to the Committee, so  
11 these were specifically in the SECY paper.

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

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

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

25 DR. KRESS: I see only two there.

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1 DR. SIU: I'm sorry?

2 DR. KRESS: I only see two there.

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

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

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

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

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

22 So, again, you'll see that we've done a  
23 scoping study and nothing more. And we're not  
24 pretending that this is a detailed analysis. We, of  
25 course, got the letter from ACRS indicating that we

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1 should base our considerations in terms of LERF.

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

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

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

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

25 We believe that we should be defining

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

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

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

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

22 We find both the same temperature  
23 dependency as well as the same distribution or similar  
24 distribution as is found in our laboratory  
25 experiments. So we've got a reasonable agreement, we

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1 feel, between specimen and structure data.

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

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

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

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

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

1 K<sub>1a</sub> data within an LEFM model in a similar way to the  
2 way that FAVOR does the probabilistic calculations, we  
3 get a reasonable prediction of these experimental  
4 results.

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

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

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

22 Our approach, we had identified a number  
23 of issues in SECY-02-0092. These were based on work  
24 done a little while ago by Idaho National Engineering  
25 Laboratory. We took, this was largely on the in-house

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1 reinvestigation of those issues, where we asked what  
2 do we know about the progression of events past the  
3 reactor vessel failure. And refine that list.

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

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

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

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

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

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

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

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

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

22 This, and then we'll have a similar  
23 animation for an event tree that shows the key  
24 sequences. A couple of things I want to point out  
25 with this event tree. First of all, the top events

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1 largely correspond to the issues that we, the  
2 technical issues that we'd identified.

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

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

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

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

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

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

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

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

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

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

25 And as Mark indicated, again, there is

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

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

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

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

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

24 So, again, these just identify the  
25 possibilities. We're not, in general, talking about

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1       likelihoods, yet. I'll talk to likelihoods a little  
2       bit later. There were certain hole sizes associated  
3       with these crack extensions and, again, we were  
4       relying on the old study to give us an indication  
5       there.

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

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

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

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

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1 basis LOCA loads.

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

8 We ask if sprays are working. If the, if  
9 there's a large hole in the vessel, does the fuel get  
10 relocated outside of the vessel or does it stay  
11 within the reactor pressure vessel, so that was a  
12 possibility that we asked about.

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

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

22 To answer your question, Dr. Kress, we  
23 looked at each of those scenarios and we decided,  
24 depending on whether ECCS was working and whether we  
25 had cooling, obviously, if you don't have cooling it

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1 would lead to core damage.

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

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

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

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

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

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

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

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

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

23           And again, this is an analysis that  
24 assumes that the through-wall crack has occurred. And  
25 that's just, we're focusing on the conditional aspects

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1 of the scenario. First of all, power is available.  
2 We're not in a station blackout.

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

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

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

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

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

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1           And this has an effect on the thermal  
2 hydraulic state when we challenge the vessel. For the  
3 stuck open and safety relief valves, the system is at  
4 pressure, perhaps 2,400 PSI or thereabouts. But the  
5 pressure vessel failure is predicted to occur between  
6 60 and 120 minutes after the trip.

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

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

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

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

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

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

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

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

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

20                   DR. KRESS: This is enthalpy.

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

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

25                   MR. BESSETTE: It does not include the

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

5 DR. BANERJEE: And the fuel?

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

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

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

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

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

25 Some time into the event is when the

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1 vessel is predicted to break. So they say, some type,  
2 for a LOCA, the vessel is predicted to break at about  
3 1,000 seconds. In these stuck open SRV cases it's  
4 dependent upon when we reclose the valve.

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

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

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

13 MR. BESSETTE: That's correct.

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

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

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

25 DR. KRESS: Yeah, we were concerned that

1 the blow down forces on the vessel might fail  
2 containment.

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

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

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

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

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

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

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

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

10 DR. KRESS: So you're actually --

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

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

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

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

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

24 The break extended across six RELAP nodes,  
25 so you get junctions above and below, it says 12

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1 junctions. This is the axial break. This extends 12  
2 feet in the region, again, adjacent to the core.

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

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

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

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

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

18 MR. BESSETTE: Yes, yes.

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

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

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

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

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

12 We're not generating ex-vessel missiles.

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

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

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

23 DR. BANERJEE: Is there sort of evidence  
24 of that. Because BSF, which is a company that did  
25 some vessel tests where they cracked open a vessel

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1 like this and it sort of just unwound and boom you had  
2 a really -- there's a lot of this documented.

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

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

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

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

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

24 The primary system pressure was 200 psi.  
25 The downcomer temperature was 250 degrees and that was

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1 at saturation and that's the corresponding FOP. Stuck  
2 open SRV case, we failed the vessel.

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

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

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

16 MR. LEITCH: In the second case there,  
17 what are we assuming, the vessel, that their stuck  
18 open relief valve opens at time zero. And then at  
19 82.30 seconds is when the vessel fails?

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

22 MR. LEITCH: Okay.

23 MR. BESSETTE: And then --

24 MR. LEITCH: It recloses.

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

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

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

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

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

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

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

24 MR. BESSETTE: Overall, yes.

25 DR. SIU: Again, what we were trying to do

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1 in this part of the study is talk about what <sup>is</sup>  
2 acceptable as opposed to what we would achieve. And  
3 part of this discussion is to argue that there's  
4 margin between the occurrence of a PTS induced reactor  
5 pressure vessel failure and large early release.

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

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

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

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

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

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

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

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

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

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

24                  And for these really fast break opening  
25 times, they are very short duration. But you can see,

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1 generally speaking, they are comparable to or much  
2 less than a design basis large break LOCA.

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

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

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

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

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

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

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

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

3 DR. KRESS: Okay, thank you.

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

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

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

11 MR. LOTT: They have about 15 nodes.

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

16 DR. KRESS: How do they compare the  
17 containment?

18 MR. BESSETTE: To contain?

19 DR. KRESS: Yes.

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

25 DR. KRESS: The 36, how does that compare

1 with the design pressure?

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

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

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

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

24 MR. BESSETTE: Yeah, we looked at this  
25 momentum flux aspects, you know, jet reaction force

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1 and that sort of thing. We don't have, we're still  
2 working on some of those things.

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

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

11 DR. KRESS: That's an impulse.

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

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

17 MR. BESSETTE: Yes, Calvert Cliffs.

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

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

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

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

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

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

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

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

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

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1 in the report as being of potential interest.

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

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

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

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

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

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

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

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

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

24           So, again, the bottom,. let me focus on,  
25 I don't want to blind anybody. Okay. It's on

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1 already. Can you hear me? Okay. So, here we have  
2 the crack. This is an axial crack that initiates the  
3 circumferential crack.

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

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

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

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

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

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

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

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

24                   Now there is one potential fly in the  
25 ointment and that has to do with some blockage. We

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1 assumed that sump blockage, or programmatically you  
2 said sump blockage is an issue being addressed in the  
3 GSI-191. And we were very explicit about that in the  
4 report.

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

12 DR. KRESS: Let's hope that that sump  
13 blockage is resolved before you actually get to a  
14 pressurized thermal shock effective full power year of  
15 40 years.

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

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

24 So any additional debris or stuff coming  
25 out might add to that problem. But there's already a

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1 problem independent of the PTS.

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

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

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

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

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

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

24 DR. KRESS: You know, for the large  
25 breaks, where you pretty much assume it goes to power

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1 oxidation event, because you dump the water out pretty  
2 fast. I recognize that the blow down will fail  
3 containment and then you've got a lot more energy  
4 coming out of air oxidation.

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

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

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

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

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

24 DR. SIU: Yeah.

25 DR. KRESS: You know, pretty soon I'll get

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1       them on my side.

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

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

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

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

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

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

25                   We did say well it's potentially dependent

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1 failure here, not knowing at the time of the report  
2 what the forces were. We also pointed out the  
3 possibility of cavity cooling. And for some of these  
4 plants you would expect, indeed, the water level to  
5 rise above the top of the fuel and to cool the fuel  
6 that way.

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

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

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

24 And again, point out that the time over  
25 which these forces are acting is very, very short. We

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1 actually think the containment spray failure  
2 probability might decrease for these events are  
3 compared to the risk significant events because you're  
4 not in a station black out situation.

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

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

13 DR. BANERJEE: It's down in there.

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

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

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

22 MR. BESSETTE: Yes, well, if you look at  
23 the situation, you know, those events that have a  
24 stuck open SRV that closes, you are in need of a 2,400  
25 psi. But that pressure is saying, it's not a thermal

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1 pressure, you've got a lot of cold water that's been  
2 pressurized by a pump.

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

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

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

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

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

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

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

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

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1 at --

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

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

6 DR. KRESS: Oh, oh.

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

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

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

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

24 DR. SIU: Well, those sequences would,  
25 these are all, the APET is tied to the dominant PTS

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1 sequences. We don't think any of those sequences are  
2 likely. Conditional on the occurrence of the PTS  
3 induced reactor pressure vessel failure.

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

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

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

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

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

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

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

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

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

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

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1 than that.

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

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

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

12 MR. ROSENTHAL: Yes.

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

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

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

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

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

5 DR. WALLIS: It is the immortal vessel.

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

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

13 DR. SIU: Questions?

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

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

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

25 And when you consider that Appendix G

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

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

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

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

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

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

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

10 DR. SHACK: And where we should stay.

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

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

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

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

23 DR. SIU: But I guess again if you equate,  
24 even, and I think we've shown because of independence  
25 of various systems, that the occurrence of the PTS

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1 event does not equate, it's not equivalent to the  
2 occurrence of core damage. There is some margin.  
3 Certainly for some scenarios.

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

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

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

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

18 DR. SIU: Thank you.

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

22 DR. SHACK: Back into session.

23 DR. KIRK: Okay. This is the discussion  
24 of the considerations regarding a new proposal on a  
25 materials based PTS screening limit. I've added a few

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1 slides here to try to make the points more clearly.

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

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

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

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

24 From a materials viewpoint, again, this is  
25 a repeat, but we'll reiterate the axial weld flaws and

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1 the material properties that can be associated with  
2 axial weld flaws are what's driving the through-wall  
3 cracking frequency.

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

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

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

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

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

24 DR. KIRK: Right. And also of particular  
25 importance that higher driving force perpetuates much

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1 deeper into the vessel wall. The circumferential  
2 crack are much more likely to arrest. However, it is  
3 possible to get circumferentially oriented cracks that  
4 can fail the vessel.

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

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

14 DR. BANERJEE: Could we have copies of  
15 these?

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

22 First off, there will be no margins here.  
23 So we're just not going to go there again. It was too  
24 painful the first time. So all  $RT_{NDTs}$  that you'll see  
25 plotted here would reflect an unirradiated  $RT_{NDT}$  plus

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1 an  $RT_{NDT}$  shift appropriate for the irradiation  
2 conditions of interest.

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

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

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

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

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

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

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1 traditional they know how to get it.

2 DR. KIRK: Yes, that's correct.

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

5 DR. KIRK: Yes and no.

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

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

15 DR. WALLIS: That went into the  
16 calculation.

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

18 (Laughter.)

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

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

21 DR. KIRK: Yes.

22 MR. ROSEN: Semi-log.

23 DR. KIRK: You can tell it's getting late  
24 in the day. Okay, so what's on the horizontal axis is  
25 the ASME  $RT_{NDT}$  plus the Sharpy shift, Sharpy shift

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1 evaluated from the Eason formula. No margin, no  
2 nothing. So all the information that was needed to  
3 calculate this is in the ASME  $RT_{NDT}$  method, Eason  
4 embrittlement formula and copper, nickel and  
5 phosphorous values in the Arvid database.

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

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

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

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

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

22 DR. KIRK: Yes. I'm working you to,  
23 remember I started here and said that a physical  
24 appropriate metric would have all the, there would be  
25 a causal relationship between the thing we're plotting

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1 on the X axis and the result on the Y axis.

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

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

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

13 DR. KIRK: Sure.

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

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

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

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

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

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

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

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

25                   DR. WALLIS: It's got the irradiation

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

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

5 DR. WALLIS: Delta T-30, then.

6 DR. KIRK: Delta T-30, yes.

7 MR. HACKETT: And then a further  
8 clarification on the unirradiated  $RT_{NDT}$  as you're  
9 indicating in some cases their measured values. In  
10 other cases they're not. And that's as defined in our  
11 10 CFR 50.60, 50.61, as to what you can and can't do  
12 there.

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

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

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

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

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

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

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

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

16 DR. WALLIS: Maybe your final report  
17 you'll have RT<sub>NDT</sub> with some superscript or something  
18 which says ASME or regulatory or best estimate or  
19 whatever, so we know which one you're talking about.

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

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

25 DR. BANERJEE: So are those lines then a

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

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

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

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

9 DR. KRESS: For that plant.

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

12 MR. HACKETT: At a particular time.

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

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

24 DR. WALLIS: That's your X axis.

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

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

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

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

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

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

16 (Laughter.)

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

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

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

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

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

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

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

24 DR. WALLIS: It sounds like a good design.

25 DR. KIRK: It's a very good design. So if

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1 you evaluate now the Sharpy shift for Beaver Valley  
2 with the appropriate fluence, that being at the axial  
3 weld, you find that it now agrees fairly well with the  
4 other results.

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

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

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

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

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

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

23           (Laughter.)

24           DR. KIRK: Yes, I will.

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

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1 Guide PTS does to correlate the data.

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

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

5 DR. SHACK: I only look at the data.

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

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

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

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

22 DR. KIRK: We're dueling, but he makes  
23 movies, so he beat me. The length of the weld and the  
24 max fluence along the weld. Then there's a weighting  
25 factor for plates, forgings and circ welds, which is

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1 good for anywhere between three and ten percent  
2 depending upon the number of circ welds, of course,  
3 the most embrittled material on either side and the  
4 max fluence along the weld.

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

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

13 DR. KIRK: Unirradiated.

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

16 DR. KIRK: Certainly not, no, no.

17 (Laughter.)

18 DR. KIRK: No. Okay.

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

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

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

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1 DR. WALLIS: Do you really want to know?

2 (Laughter.)

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

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

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

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

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

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

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

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

25           DR. KRESS: They only like to use a median

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1 if you don't really believe the tales. So here you're  
2 saying we believe the tales. So we don't, it's not a  
3 real margin.

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

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

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

12 DR. WALLIS: Stay away from the word  
13 margin.

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

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

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

21 And this gets back to what I mentioned to  
22 Dr. Ford earlier. That buried in the guts of the  
23 FAVOR calculation we've reflected the maximum material  
24 uncertainties in FAVOR, because we've used generic  
25 data to derive these uncertainties.

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1                   And they've been explicitly accounted for.  
2                   So any plant state of knowledge has to be better than  
3                   we've simulated here. And also, if it hasn't already  
4                   become clear, I would like to point out that this  
5                   particular limit pertains only to one particular  
6                   pathway of getting to this new proposed  $RT_{NDT}$  metric.

7                   It's based on a measured unirradiated  
8                   value and copper, nickel and phosphorus plugged into  
9                   a particular embrittlement shift. There are certainly  
10                  many other ways that at least in current practice the  
11                  licensees will evaluate  $RT_{PTS}$  and --

12                  DR. WALLIS: Tell me about that measured  
13                  value. I'm not an expert on Sharpy and all this  
14                  history of  $RT_{NDT}$ . But it looks from the data and I  
15                  may refer to Chapter 1, I think it's Figure 1.3, it  
16                  looks as if there are a lot of scatter on the curves  
17                  looks not to be all the same shape and all that.

18                  When you do these tests, are they  
19                  repeatable.

20                  DR. KIRK: I'm sorry, 1.3 is --

21                  DR. WALLIS: Well, I mean, it's K versus  
22                   $RT_{NDT}$  for different steels. The EPRI data. How  
23                  repeatable are these tests that give you this  $RT_{NDT}$   
24                  and what's the uncertainty in the test itself.

25                  We seem to be treating this  $RT_{NDT}$  ASME as

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1 if it were something that was really known.

2 DR. KIRK: The unvarnished answer is it's  
3 not very repeatable at all. However, that uncertainty  
4 has been represented in the calculation.'

5 DR. WALLIS: Yeah, but when you plot  
6 something like  $RT_{NDT}$  on a graph, that is something  
7 which itself is very uncertain, isn't it?

8 DR. KIRK: That's correct. But the way  
9  $RT_{NDT}$  has been designed, it's virtually impossible to  
10 underestimate it. Everything that you do in going  
11 through the, everything that you are forced to do by  
12 the ASME procedure, forces you to, if anything,  
13 overestimate the value.

14 DR. WALLIS: And that gets you to that  
15 Curve A in Appendix, way off to the side.

16 DR. KIRK: Yeah, yeah.

17 DR. KRESS: Are you going to sell this  
18 weighted thing to ASME and get them to change their  
19 ---

20 DR. KIRK: If we have enough time. Maybe.

21 DR. KRESS: It's not surprising that that  
22 weighted thing gives you a better correlation because  
23 it's based on your calculations, frequencies or  
24 contributions.

25 DR. KIRK: It's based on an understanding

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1 of what counts.

2 DR. WALLIS: So the Licensee has, this  
3  $RT_{\text{NDT}}$  that the Licensee calculates, is that calculated  
4 by a formula giving all this chemical composition. Or  
5 is it calculated from tests on samples that are pulled  
6 out of the reactor.

7 DR. KIRK: Currently the answer is both.  
8 By the current regulation you are allowed to do both.

9 DR. WALLIS: And they have to be  
10 compatible or what? And how do you resolve, if you  
11 get different answers from each one.

12 MR. HACKETT: It all goes, it's all  
13 documented in 10 CFR. And also in the --

14 DR. WALLIS: All of the mystery there.

15 MR. HACKETT: Yeah, in the regulatory  
16 guide. But as Mark says you can come at a number of  
17 ways. The idea being that if you have data, you have  
18 hopefully somewhat greater certainty over what the  
19 actual property is.

20 But they also allow you to estimate if you  
21 don't have data, and they that's where you get into  
22 adding margins to hopefully address --

23 DR. WALLIS: That's what worried me is  
24 that, you know, everything is hung on this  $RT_{\text{NDT}}$ .  
25 You've done a great job of dealing with all these

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1 things, but I'm not quite sure how accurately the  
2 Licensee can estimate from these samples or whatever.

3 DR. KIRK: I think that we're not asking  
4 the Licensee to do anything really different than  
5 they've done before. And again the reason that it  
6 works in this case is that  $RT_{NDT}$  as measured, it has to  
7 be a bounding property. There is no way to do  
8 otherwise.

9 DR. KRESS: I think if you give 20 people  
10 the input that goes into calculating that from a given  
11 plant, which they just gather, they'd all calculate  
12 the same number.

13 DR. KIRK: Yeah, yeah, given the input.

14 DR. KRESS: Given the input, it's only,  
15 it's just the input that's the problem.

16 DR. WALLIS: The input, if it's a bound,  
17 because bounding means you have to have enough points  
18 to determine what's bounding. And it may be that some  
19 erratic point pushes the bound out.

20 DR. KRESS: Well, if they have to measure  
21 their copper and --

22 DR. WALLIS: But they don't have very many  
23 samples in the reactor. They are using experimental  
24 data.

25 DR. KRESS: Then they have to assume they

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1 got a certain amount in there and that's where the  
2 conservatism comes in.

3 DR. BANERJEE: Can they measure these  
4 things based on the surveillance samples in the  
5 reactor. I mean which are actually being exposed to  
6 fluence and all this stuff.

7 MR. HACKETT: Again, unfortunately the  
8 answer depends, depends on whether they have the  
9 limiting material in their surveillance program for  
10 that reactor. Or are they relying on, let's give an  
11 example.

12 In the case of the B&W plants, they have  
13 an integrated surveillance program where you may use  
14 Oconee's results to predict Three Mile Islands  
15 irradiation damage. But you have to argue some kind  
16 of equivalency of the irradiation environment.

17 So the answer there also is a mixed bag.

18 DR. BANERJEE: Presumably the fluence can  
19 be pretty accurately calculated.

20 MR. HACKETT: Presumably.

21 DR. BANERJEE: Presumably. There's  
22 another question I have. The mean TWCF, that you have  
23 there, that's a function of a whole lot of things.  
24 And it's sort of surprising that all these things  
25 collapse so well because that suggests that the

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1 sequences which are sort of risk dominate plus the  
2 transients plus all things are very similar between  
3 these plants. Essentially there is not too much  
4 difference between them.

5 DR. KIRK: And that was the slide that I  
6 tried to emphasize at the beginning of this  
7 development is, yes, it seems to be surprisingly so  
8 that the, between these plants the level of challenge  
9 if you will is indeed remarkably similar.

10 MR. ROSEN: And that's because it's  
11 dominated by LOCAs and LOCAs are primary system  
12 phenomenas that are relatively the same in BWRs. Even  
13 once-through steam generator PWRs and recirculating  
14 steam generators PWRs are not affected because the  
15 primary systems are pretty much the same even though  
16 the steam generators are different and behave  
17 differently.

18 You're looking at what happens when you  
19 punch a hole in the reactor system. And that's the  
20 same in a PWR. They both start out at 2,200 psi  
21 roughly and depressurize and there you are.

22 Operators go, oh, no, my gosh, keep your  
23 hands off, make sure the reactor scrambled and that's  
24 it.

25 DR. KIRK: Yup, that's correct.

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1 DR. KRESS: And they use generic  
2 frequencies for their bin.

3 MR. ROSEN: So it's not a surprise.

4 DR. WALLIS: Now to get back on this, I'm  
5 sorry to keep on this. You did a beautiful analysis  
6 of epistemic  $RT_{NDT}$  and I thought what you were doing  
7 there was you were looking at taking this ASME  $RT_{NDT}$   
8 how well does it correlate real toughness data.

9 And how well does the theory represent  
10 this real toughness data. That was what was your  
11 epistemic analysis. And that still assumes that one  
12 has a very good way of knowing what that ASME  $RT_{NDT}$   
13 is.

14 DR. KIRK: No, actually it doesn't. Those  
15 ASME  $RT_{NDT}$  values, I mean the distribution that we  
16 showed before is that they are on average about 60  
17 degrees too high.

18 DR. WALLIS: That's why you have this  
19 epistemic and --

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

21 DR. WALLIS: That's if you want to get  
22 toughness results out of it.

23 DR. KIRK: That's right.

24 DR. WALLIS: But it may well be that some  
25 plants don't do a very good job of analyzing their

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1 samples. And that's not in there, is it?

2 DR. KIRK: Of analyzing the RT<sub>NDT</sub> samples?  
3 The more careless somebody is doing an RT<sub>NDT</sub> test, the  
4 more conservative it becomes.

5 DR. WALLIS: That doesn't make sense.

6 DR. KIRK: Because if you, okay, if I'm,  
7 when you do, when you test for RT<sub>NDT</sub> you have to take  
8 these specimens that have a brittle weld bead on them  
9 and a notch and you have to go until you establish a  
10 break/no break condition.

11 DR. WALLIS: You either bust them or you  
12 stretch them.

13 DR. KIRK: Well, actually you have to just  
14 simply establish a no break condition.  
15 So if I want to do that with a minimum of samples, I  
16 pick a high temperature, I slam the hammer down and I  
17 decide it hasn't broken.

18 That doesn't mean that the real  
19 temperature between break and no break might be 100  
20 degrees Fahrenheit lower. I can always overestimate  
21 RT<sub>NDT</sub>, I can't under estimate it by the way you go  
22 through the procedure.

23 So if I want to be, if I wanted to be very  
24 precise, I'd get a whole bunch of specimens and very  
25 carefully bracket the break/no break temperature. But

1 all the ASME MB-2331 requires me to do is demonstrate  
2 no break performance.

3 DR. WALLIS: So long as it hasn't broken.

4 DR. KIRK: Yeah. So if I've only got two  
5 specimens to do that with, and I want to establish a  
6 code value, I'm going to guess high.

7 DR. WALLIS: Well, I guess I'm saying is  
8 that there's got to be quality control in the way it's  
9 tested and all that kind of stuff as well.

10 DR. KRESS: That's pretty standard.

11 DR. WALLIS: So standard that you have no  
12 doubts at all about that.

13 DR. KIRK: Yeah, the way the tests are  
14 conducted is indeed standardized and controlled by  
15 ASTM. The procedure you go through, if you will, to  
16 discern  $RT_{NDT}$  based on ASTM E208<sub>NDT</sub> data and ASTM E 23  
17 Sharpy data is not very well specified. But, and this  
18 is the only good but, the way it's not well specified  
19 is that it forces you to overestimate the value.

20 MR. ROSEN: Now help me with my  
21 understanding of how to use this chart. If I'm in  
22 Ocone, Beaver or Palisades, I'm right on the 290  
23 degree screening limit. Is that right?

24 DR. KIRK: Only if you operate your  
25 reactor until about the time that warp technology is

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1 invented.

2 DR. SHACK: When you replace it with your  
3 fusion plan.

4 MR. ROSEN: Why is that? I guess I must  
5 have missed that part of the discussion.

6 DR. WALLIS: Where are they now on this  
7 curve?

8 DR. KIRK: The now on the curve, everybody  
9 now is in the yellow oval.

10 DR. FORD: Even below it.

11 MR. ROSEN: Everybody.

12 DR. WALLIS: Well, they slide off the  
13 curve as they go on.

14 DR. KIRK: Yes, so time increases this  
15 way. And for Palisades that was a 500 year analysis.  
16 For Oconee, that was a 1,000 years. And for Beaver  
17 that was 100.

18 MR. ROSEN: Okay, because of the two  
19 orders magnitude. So you're saying that a clean plant  
20 now, low fluence, good materials is going to be off  
21 the bottom of that thing.

22 DR. KIRK: Yeah, because these were two of  
23 the --

24 DR. SHACK: The difference is really  
25 materials, not, I mean they're all going to have

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1 roughly the same fluence per years of operations, but  
2 the materials respond very differently.

3 MR. BESSETTE: Some plants have neutron  
4 belts, neutrons pads.

5 DR. SHACK: Yes, but I think that's small  
6 compared to the material difference.

7 DR. WALLIS: So for Oconee to get up to  
8 one to the minus six, it would be several thousand  
9 years?

10 DR. KIRK: Yes, a thousand.

11 DR. WALLIS: So it's not just 60 to 80,  
12 it's thousands of years.

13 MR. ROSEN: I don't its turbine will last  
14 that long.

15 (Everyone talking amongst themselves.)

16 DR. FORD: Mark, could I ask. Up until  
17 the time you showed us these graphs, I was absolutely  
18 with you.

19 (Laughter.)

20 DR. FORD: And I can understand why you're  
21 going the way you are. But you're making one big  
22 assumption. The assumption is that there is one  
23 unique curve, that one that you've shown there, which  
24 normalizes all plant.

25 And that's an assumption that I haven't

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1 heard questioned physically. And then the second one  
2 you've gone into a bit of a g-ray pokery about a whole  
3 lot of different equations with ten percent and  
4 circumferential.

5 And I can understand where they came from,  
6 but I don't understand why they are on those specific  
7 algorithms that you've put down on this slide here.  
8 Now I don't doubt, the derivation of those long  
9 equations were being driven by the fact that you want  
10 there be one curve.

11 And I just feel uncomfortable because I  
12 don't understand some of those physics.

13 DR. KIRK: Actually the thought process  
14 here, I mean, honestly, the idea was what's shown on  
15 the screen now. Was simply to say, okay, let's lay  
16 the blame for through-wall cracking frequency on  
17 what's to blame.

18 So, let's not say that circ welds  
19 contribute a lot. Let's take account of differences  
20 in weld length. Let's get the fluence right. So all  
21 these things were done, and I shot myself in the foot  
22 by not presenting this in time sequence.

23 All these things were done and we got to,  
24 now I can't go fast through this damn thing.

25 DR. WALLIS: How did you pick 90 and 97?

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