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Thermal Hydraulic Phenomena  
Reliability and Probabilistic Risk Assessment

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

December 1, 2004

The contents of this transcript of the proceeding of the United States Nuclear Regulatory Commission Advisory Committee on Reactor Safeguards, taken on December 1, 2004, as reported herein, is a record of the discussions recorded at the meeting held on the above date.

This transcript has not been reviewed, corrected and edited and it may contain inaccuracies.

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UNITED STATES OF AMERICA  
NUCLEAR REGULATORY COMMISSION  
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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS  
(ACRS)  
JOINT MEETING OF THE SUBCOMMITTEES ON  
MATERIALS AND METALLURGY,  
THERMAL-HYDRAULIC PHENOMENA,  
RELIABILITY AND PROBABILISTIC RISK ASSESSMENT

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WEDNESDAY,

DECEMBER 1, 2004

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS:

- WILLIAM J. SHACK, Chairman
- RICHARD S. DENNING, Member
- MARIO V. BONACA, Member
- F. PETER FORD, Member

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COMMITTEE MEMBERS: (cont.)

- THOMAS S. KRESS, Member
- VICTOR H. RANSOM, Member
- STEPHEN L. ROSEN, Member
- JOHN D. SIEBER, Member
- GRAHAM B. WALLIS, Member

ACRS STAFF PRESENT:

- HOSSEIN NOURBAKSH
- CAYATANO SANTOS

ALSO PRESENT:

- DAVID E. BESSETTE, RES
- MARK ERICKSONKIRK, RES
- ALLEN HISER, RES
- DONNIE WHITEHEAD, Sandia

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

8:30 a.m.

CHAIRMAN SHACK: The meeting will now come to order. This is the second day of a two-day meeting of the ACRS Joint Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena, and Reliability and Probabilistic Risk Assessment.

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

The purpose of this meeting is to discuss the technical basis for potential revision of the PTS screening criteria and the PTS rule 10 CFR 50.61. The joint subcommittees will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee. Dr. Hossein Nourbakhsh is the designated federal official for this meeting. Also Mr. Tanny Santos, ACRS staff, is in attendance to provide technical support.

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

1 meeting is being kept and will be made available as  
2 stated in the Federal Register Notice. It is  
3 requested that speakers first identify themselves and  
4 speak with sufficient clarity and volume so they can  
5 be readily heard.

6 We have received no written comments or  
7 request for time to make oral statements for members  
8 of the public regarding today's meeting. We will now  
9 proceed with the meeting and Mario Bonaca would like  
10 to make a couple of comments before he has to leave  
11 today.

12 DR. BONACA: The reason that I ask is that  
13 I'm going to leave before 10:00. Yesterday I raised  
14 those issues about the differences between different  
15 PWRs, etc. You already heard those. It's more a  
16 question of inter-run documentation to address some if  
17 there are, and I believe there are.

18 The other issue was, and my memory came  
19 back so I have to bring it up now, in your slide where  
20 you talk about the main stream line break difference  
21 on previous analysis and the current technical basis  
22 for Ocone and Robinson you said main stream line  
23 break was most important because LOCAs were not  
24 modeled. Well, I mean, they were not stupid. The  
25 people that did not model the LOCA was because they

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1 did not lead to repressurization. That was the issue.

2 The issue of major concern that I remember  
3 clearly now was, and I think is important for the  
4 record in the documents so that there is a historical  
5 understanding of why it was raised and why it was what  
6 it is. The concern was for the B&W plant you have  
7 very fast cool down.

8 You have a very high set point for the  
9 high pressure injection. Typically they are  
10 set at 1700 psi. I think Oconee is there. And they  
11 are high-capacity pumps. They pump in a lot of cold  
12 water and you have an extremely rapid cool down and  
13 then you have repressurization. Remember that we're  
14 using curves where you repressurize the 2500 psi which  
15 was safest.

16 Now, why were they allowed to do that?  
17 They gave no credit for operator action because this  
18 was 1980. TMI had just happened and there were no  
19 symptom-oriented procedures. The instruction to the  
20 operator was you have a locker. Use safety injection.  
21 There was a sense that maybe the operator could not  
22 understand if he was in a steam line break scenario at  
23 the beginning.

24 He would let the pumps run. There was a  
25 high likelihood for that. There was a scenario that

1 was dominant because of the cool down and  
2 repressurization. I daresay that there are good  
3 reasons today to reevaluate this decision of letting  
4 the pump running but I think it has to be dealt with.

5 In the discussion that we had last year  
6 with Alan from SAIC I remember we talked about some  
7 operator action and he, in fact, defended them very  
8 intensely. He reviewed the Oconee procedures, spoke  
9 with the operators, interviewed the operators during  
10 steam line break simulations. He built a case for  
11 saying that the scenario is still there but is not a  
12 significant contributor anymore because, you know, the  
13 operators will take care of it.

14 They will prevent this going solid. And  
15 so my point is simply that in the preparation of the  
16 report it's important that this historical perspective  
17 be given because there was a reason. I looked at the  
18 comments from Tom Murley and he's asking the same  
19 question. "How come the transient is not there  
20 anymore?" There has to be a reason. T h e  
21 reason is not that they forgot to include the LOCAs.  
22 The reason is that they were concerned about  
23 pressurized thermal shock so the thermal cool down and  
24 then the repressurization.

25 Now, in the LOCAs you don't have

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1 repressurization but I guess you don't have to be  
2 concerned about that. I don't know why it's going  
3 away, the concern with repressurization. It's still  
4 something that has to be said because the definition  
5 of transients has really changed. That's pretty much  
6 it.

7 MR. ERICKSONKIRK: I certainly agree and  
8 appreciate the comment that we can do a better job of  
9 documenting than what has been done before and your  
10 comments are very helpful in that regard. I don't  
11 think it's correct to say that we're no longer  
12 concerned about repressurization. Certainly we find  
13 that repressurization transients on the primary side  
14 are, if you'll forgive me for the use of a judgmental  
15 word, bad.

16 It's just that on the secondary side it  
17 doesn't get cold enough to drop the material toughness  
18 enough for the repressurization to matter that much.  
19 Certainly your first comments to do a better job about  
20 documentation we need to follow up on.

21 DR. BONACA: The point I wanted to make is  
22 that they didn't just forget about LOCAs existing  
23 there. It was simply that they did not see it as a  
24 severe combination of factors. You just go down on  
25 the pressurization. In the LOCA you do not have any

1       pressurization taking place.

2                   MR. ERICKSONKIRK:       Yes, certainly.  
3       Judgements were made at the time regarding what was  
4       believed to be important based on the knowledge that  
5       they had and based on that knowledge they excluded  
6       certain things that they didn't think would be large  
7       contributors just as we've done today.

8                   DR. BONACA:   But I'm saying, again, there  
9       is a logic for justification of the elimination of the  
10      sequence.

11                   MR. ERICKSONKIRK:   Yes.

12                   DR. BONACA:   Logic is symptom-oriented  
13      procedures.   The credibility of those actions of  
14      operators following those procedures as in operator  
15      training into the simulators and all those things. Of  
16      course, now we've got core cooling that did not exist  
17      at that time when they had those panels.

18                   We had no help to the operator to do that.  
19      These are elements that have to be described so that  
20      one can say this transient may still exist possibly  
21      but it's so likely that there's no treatment. Thank  
22      you.

23                   CHAIRMAN SHACK:   I'll turn it back to you,  
24      Mark.

25                   MR. ERICKSONKIRK:   Okay.   Where we left

1 off in our somewhat modified agenda yesterday was in  
2 the middle of going through Chapter 9 which generally  
3 talks to our ability to generalize our results from  
4 three plant specific analyses to PWRs in general.

5 Yesterday before we left we heard from  
6 Donnie Whitehead of Sandia National Laboratories about  
7 plant to plant differences and design and operator  
8 action and things of that nature that matter to PTS  
9 sequences. And we also heard from Dave Bessette on  
10 sensitivity studies regarding thermal hydraulic  
11 analysis.

12 There are two portions of our  
13 generalization work remaining that we'll talk about  
14 this morning. The first one I'll talk about which is  
15 sensitivity studies in PFM. Then Donnie Whitehead  
16 will come up and talk about why we feel it's  
17 appropriate to essentially ignore the contribution of  
18 external events as initiators. This presentation  
19 concerns sensitivity studies on the PFM model.

20 We performed those sensitivity studies  
21 with two objectives in mind. One is to provide  
22 confidence in the robustness of the PFM model so we  
23 performed sensitivity studies on credible alternative  
24 models and credible input perturbations to see if they  
25 change the results enough to justify some change in

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1 the baseline model.

2 And we've also provided -- I'm sorry,  
3 performed sensitivity studies to provide confidence  
4 that the through-wall cracking frequency results that  
5 were generated for the three study plants can in fact  
6 be generalized to apply to all PWRs. The focus there  
7 is to perform sensitivity studies to assess the  
8 influence of factors that have not been fully  
9 considered in our analysis of the three study plants  
10 but exist in the PWR fleet in general.

11 It was noted on the title slide there's a  
12 NUREG that goes into all this information in detail.  
13 That's NUREG-1808 which you should have electronic  
14 copies of now. This information is also summarized in  
15 Section 9.2 of NUREG-1806. Now this details the  
16 sensitivity studies that we performed in each of these  
17 categories and I'm going to have a slide or two on  
18 each of these so we'll start with the ones to provide  
19 competence and the robustness of the PRM model and  
20 then we'll go on to the generalization sensitivity  
21 studies.

22 We did not perform sensitivity studies per  
23 se looking at doing changes to the flaw distribution.  
24 Not because we believe the flaw distribution to be  
25 certain but because there really isn't credible

1 alternative information out there on which to base a  
2 sensitivity study.

3 I mean, we could certainly increase the  
4 density of the flaws by two, increase the size of the  
5 flaws by two, and a simple examination of the PFM  
6 equation show that those things would increase  
7 through-wall cracking frequency. Instead of doing  
8 that or, I maybe say, in lieu of doing that, we did  
9 want to provide some information here and in the  
10 report on the characteristics of flaws that contribute  
11 the most to the through-wall cracking frequency.

12 Certainly we discussed yesterday that the  
13 dominant contributor is oriented axially and that's  
14 just a natural consequence of the driving force in the  
15 vessel. The flaws are also very close to the inner  
16 diameter. I think the most important thing on this  
17 slide is the realization that the flaws that  
18 contribute the most to the through-wall cracking  
19 frequency are, in fact, small in dimension.

20 If you have very large flaws, the cracked  
21 tips of those flaws are located too deep into the  
22 vessel to feel the effect of the thermal shock so they  
23 are essentially at a low-stress condition and they  
24 don't contribute very much at all to the through-wall  
25 cracking frequency.

1 DR. WALLIS: I don't understand that. Why  
2 can't part of the flaw be close to the wall?

3 MR. ERICKSONKIRK: It is close to the  
4 wall.

5 DR. WALLIS: It might pop the wall. If it  
6 pops into the wall, it breaks through the wall if it's  
7 close enough to the wall when you pull on it.

8 MR. ERICKSONKIRK: I think I might have to  
9 defer to Terry on this. Perhaps you can help me why  
10 we check for crack initiation at both crack tips.

11 MR. DICKSON: For embedded flaws we check  
12 for the initiation at the inner crack tip, the one  
13 that's closest to the clad based interface for two  
14 reasons. It's the worse case for two reasons. It's  
15 worse case because the stress is higher there as you  
16 go out through --

17 DR. WALLIS: It's closer to the surface.

18 MR. DICKSON: Yes.

19 DR. WALLIS: Right.

20 MR. DICKSON: You have a higher stress and  
21 you also have a higher embrittlement.

22 DR. WALLIS: So why is that necessarily  
23 further in? Maybe the center of the flaw is further  
24 in but its tip isn't. It could be right next to the  
25 surface.

1 MR. DICKSON: Well, it could be. This pot  
2 that Mark has on the left, that's what that is talking  
3 about. That is actually the inner crack tip location.

4 DR. WALLIS: But that doesn't explain why  
5 the big flaws are less effective. He said they were  
6 less effective because they were further in. That's  
7 what I'm questioning. I don't think the tip is  
8 necessarily further in. Certainly the middle is  
9 further in if they are bigger.

10 CHAIRMAN SHACK: I think what he was  
11 referring to if you had a one-inch deep crack so that  
12 the tip was an inch from the inner wall it would be a  
13 huge crack.

14 DR. WALLIS: Yeah.

15 CHAIRMAN SHACK: But, in fact, because the  
16 tip is an inch in --

17 DR. WALLIS: It's an inch in but it could  
18 be an inch-long crack which is a thousandth of an inch  
19 from the inner wall as far as its tip goes.

20 DR. RANSOM: It seems like it has to start  
21 and end somewhere. Wherever it starts and end is at  
22 the surface.

23 MR. ERICKSONKIRK: I apologize. I think  
24 I didn't express where I was trying to go.

25 DR. WALLIS: I think you need a different

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1 rationale.

2 MR. ERICKSONKIRK: Yes. What we do find  
3 on the graph on the lower right-hand side is that the  
4 flaws that are driving the through-wall cracking  
5 frequency fully 90 percent of them are fairly small  
6 flaws and that's the observation.

7 DR. WALLIS: Because there aren't very  
8 many big ones? Is that what it is? It's more  
9 probable that you would have a small flaw under the  
10 surface?

11 MR. ERICKSONKIRK: Absolutely. There's a  
12 very low probability of having big flaws and even if  
13 you increase the big flaw probability by credible, or  
14 even incredible factors, it wouldn't matter much. I  
15 apologize for that. You are absolutely correct. The  
16 first rationale was erroneous.

17 DR. WALLIS: This flaw distribution is  
18 based on rather skimpy evidence. This is one of the  
19 areas where -- I mean, heat transfer Dittus-Boelter if  
20 you believe that. It's based on data points. But the  
21 floor distribution in these walls is based on a few  
22 examinations. Isn't it?

23 MR. ERICKSONKIRK: A few examinations but  
24 infinitely more than we had the first time.

25 DR. WALLIS: It's much better than you had

1 the first time.

2 MR. ERICKSONKIRK: Much better than we had  
3 the first time. I think as a laboratory geek at heart  
4 I have to admit I would really like to have more data  
5 on this and I don't think there's anybody in the  
6 technical community that would disagree with this.

7 But I think it's also important to  
8 recognize that the flaw distribution doesn't rest on  
9 experimental evidence alone. Certainly we started  
10 with -- excuse me. We start with experimental  
11 evidence both from destructive and nondestructive  
12 evaluations but that's then also bolstered by --

13 DR. WALLIS: But those were of individual  
14 reactor vessels.

15 MR. ERICKSONKIRK: That's right.

16 DR. WALLIS: But there are a hundred  
17 reactor vessels. I don't know how convincing it is  
18 that the flaw distribution that you measured in a  
19 couple of vessels which were taken apart is typical of  
20 all other vessels.

21 MR. ERICKSONKIRK: No. I think it would  
22 be unfair to say that a single experimental  
23 distribution derived from two vessels could be just  
24 looked at and thought to be representative of the  
25 other vessels.

1                   However, the expert group that we got  
2 together to help us construct the flaw distribution  
3 used physical models, used expert judgment in the  
4 process of constructing the distribution. As I  
5 indicated yesterday, in the process of constructing  
6 the distribution every time they came to something  
7 where they felt they had to make a judgment, that  
8 judgment was made in a systematically conservative  
9 direction.

10                   DR. WALLIS: This is all documented in  
11 some --

12                   MR. ERICKSONKIRK: This is all documented  
13 and I don't have this NUREG --

14                   DR. WALLIS: Hopefully we are going to  
15 get --

16                   MR. ERICKSONKIRK: You have it already.

17                   DR. WALLIS: We have it already.

18                   MR. ERICKSONKIRK: That was the first  
19 document you got. Bruce, I'll get you in just one  
20 second. Just to give a couple of examples, we  
21 simulate surface walls to exist in the vessels despite  
22 the surface-breaking flaws despite the fact that no  
23 surface-breaking flaw has ever been observed so that's  
24 clearly conservatism.

25                   Then the other thing is all of the

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1 inspections, destructive or indestructive, any  
2 indication that was found was taken to be a planar  
3 crack. In other words, something that could  
4 initiate clear retractor. Whereas unquestionably if  
5 you talk to any NDE person they will tell you the  
6 easiest thing to find in an inspection is not a planar  
7 crack but a volumetric crack. The huge -- that's  
8 perhaps an overstatement.

9 A lot of the indications that we  
10 characterize as planar cracks and, therefore, believe  
11 or treat in our calculation as contributing to the  
12 probability of failure are, in fact, more akin to my  
13 Magic Eight Ball and aspect ratio and, therefore, are  
14 very unlikely to initiate a crack.  
15 That's but a few of the examples of the conservatisms  
16 that we are taking.

17 CHAIRMAN SHACK: Is that truly a  
18 conservatism? I mean, do you have statistically --  
19 have you put in statistically such a high number of  
20 surface-breaking flaws that you would be surprised  
21 that you hadn't found one in the inspections you did?  
22 Or is the number just small enough that if I inspect  
23 25 meters of weld I wouldn't expect to find them but  
24 if I expected a thousand meters of weld --

25 MR. ERICKSONKIRK: Indeed, the motivation

1 for including surface-breaking flaws when none have  
2 been found was based on the fact that in the end while  
3 we inspected much, much more material than we ever had  
4 inspected before, it was still a small amount.

5 CHAIRMAN SHACK: No. But are you so  
6 conservative that you should have found -- you know,  
7 does your distribution say that you should have found  
8 surface-breaking flaws in which case I would agree  
9 that your inclusion is conservative or you've just in  
10 a statistically realistic number of surface-breaking  
11 flaws.

12 MR. ERICKSONKIRK: I would agree more with  
13 your second opinion and I would say a statistically  
14 realistic yes.

15 CHAIRMAN SHACK: You can't take credit for  
16 conservatism.

17 MR. ERICKSONKIRK: Well, however, the  
18 other thing to recognize is the only physical  
19 mechanism that is capable of producing the surface-  
20 breaking flaw is lack of inter-run fusion in the  
21 austenitic stainless steel cladding and those are all  
22 circumferentially oriented.

23 We've done, which I don't have here but  
24 can provide you, sensitivity studies where we  
25 increased the number of surface-breaking flaws from

1 our baseline number dramatically and the through-wall  
2 cracking frequency doesn't go up much and that's  
3 expected because they are circumferential. Bruce.

4 MR. BISHOP: I'm Bruce Bishop from  
5 Westinghouse. I was involved as part of the industry  
6 V&V of the distributions for the flaw. Both the  
7 density, the depth, direction, and the aspect ratio  
8 for the surface-breaking flaws, the embedded flaws,  
9 and the plate flaws.

10 One point to keep in mind is for the  
11 embedded flaws there is not one distribution. We  
12 recognize that there is uncertainty on the limited  
13 amount of data. In those three parameters I mentioned  
14 there are significant uncertainties and instead of  
15 generating one distribution we actually generate 1,000  
16 distributions and use those in the FAVOR code so there  
17 is a fair amount of uncertainty. There is not  
18 just one flaw distribution. There is a family of  
19 distributions with fairly big uncertainties to allow  
20 for the lack of a significant amount of data.

21 MR. GAMBLE: My name is Ron Gamble and I  
22 work at Sartrex and I do a lot of work in this area  
23 for EPRI. I want to say one thing about flaws on the  
24 surface. This is a misconception you just keep  
25 hearing and hearing and hearing. All vessels that are

1 manufactured and operating in the United States have  
2 had inspections on the surface so there is no vessel  
3 that is not in service that has not been inspected for  
4 flaws on the surface.

5 Part of the fabrication process is to do  
6 a dye penetrant examination after welding to look for  
7 defects and if they're found they're repaired. The  
8 dye penetrant is done on all welds. It's a mag  
9 particle which means that it has the capability to  
10 detect flaws that are on the surface and slightly  
11 below the surface maybe five to 10 thousandths of an  
12 inch.

13 So I think you have to remember that all  
14 vessels are inspected on the surface of all welds. We  
15 seem to get the impression that we've never had these  
16 inspections or that we have some small sample from two  
17 plants of a couple of meters. It's not true. Every  
18 vessel is inspected on the weld on the surface in  
19 every plant.

20 MR. ERICKSONKIRK: Okay. I'll just note  
21 that we'll get back to this topic when we go over the  
22 Peer Reviewers' comments.

23 DR. WALLIS: The cladding process doesn't  
24 create new flaws?

25 MR. ERICKSONKIRK: The cladding process

1 does create flaws in the cladding.

2 DR. WALLIS: But not on the vessel base  
3 metal?

4 MR. ERICKSONKIRK: In fact, that's a nice  
5 lead-in to my discussion of subclad cracking which  
6 we'll get to in about 10 slides. Okay, weld residual  
7 stresses. In the FAVOR code we conservatively assume  
8 that the residual stresses produced by welding are not  
9 relieved by through-wall crack propagation which, of  
10 course, has to be true to meet the boundary  
11 conditions.

12 In lieu of doing a detailed analysis,  
13 which would have undoubtedly taken a lot more time and  
14 money, what we tried to somehow systematically relieve  
15 the stresses as the crack propagated through the  
16 vessel wall.

17 We just took them away as soon as the  
18 crack initiated but it turned out that the removal of  
19 that conservatism didn't alter the through-wall  
20 cracking frequency hardly at all, which is perhaps not  
21 surprising because the residual stress contribution to  
22 driving force is small compared to the pressure and  
23 temperature components.

24 DR. FORD: Mark, that last statement may  
25 well be true but that is a calculated residual study

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1 profile. If you look at the data and make a  
2 comparison, for instance, double-V notched pipes or  
3 core shrouds, there's a considerable scatter of the  
4 actual data around that theoretical line. My  
5 if you put the upper bound of the observed residual  
6 stress profiles how would that statement --

7 MR. ERICKSONKIRK: I think I need to go  
8 back and I'll ask Terry to tell me if I've got this  
9 wrong or not. This profile was determined by  
10 experimental measurements made on a thick wall vessel.  
11 Was it not?

12 MR. DICKSON: Yes.

13 DR. FORD: Did that experiment -- how many  
14 data points were there to confirm that?

15 MR. ERICKSONKIRK: Terry, do you remember?  
16 I just don't have those details.

17 MR. DICKSON: I don't remember all the  
18 details. That's been seven, eight, or 10 years ago we  
19 did that study and wrote the paper, but it's a  
20 combination of measured data and analysis from which  
21 this weld residual stress distribution was derived.  
22 But this is also consistent with other people in the  
23 literature that had done the same type of work, the  
24 same shape and the same magnitude. No doubt there is  
25 probably some scatter about it but that's not

1 considered in the analysis.

2 DR. FORD: No. I have no problem at all  
3 with the shape of that curve. As you see, many people  
4 have seen similar shapes of the double-V notched  
5 welds. My question is if you look at the data, what  
6 is the upper bound of that data compared with that  
7 curve that you put into the FAVOR code? If it's, you  
8 know, 10 ksi more positive than that, would that  
9 impact on your end conclusions?

10 MR. ERICKSONKIRK: I simply don't have  
11 that information although we can certainly recover it.

12 DR. FORD: It seems to me the whole point  
13 of these presentations is sensitivity.

14 MR. ERICKSONKIRK: Yes. That's a good  
15 point.

16 DR. FORD: Could you have a situation  
17 where the 10 ksi in the real case of the specific  
18 pressure vessel you're trying to analyze, could those  
19 curves be 10 ksi more positive?

20 MR. ERICKSONKIRK: We'll look into that.  
21 I don't have that knowledge stored away but it's  
22 certainly available.

23 Okay. Next one concerns the embrittlement  
24 shift model regarding which there's been a lot of  
25 discussion both within the NRC and within the ASTM

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1 community. FAVOR has adopted an embrittlement shift  
2 model. This is a model that calculates the shift in  
3 the Charpy 30-foot pound energy transition temperature  
4 as a function of copper, nickel, phosphorus, fluence  
5 and so on. FAVOR has adopted a model  
6 proposed by one of our contractors in the year 2000  
7 that differs somewhat from the ASTM E900-02 standard  
8 that was adopted two years ago. It should be pointed  
9 out that the two models are similar but not identical.

10 DR. WALLIS: Now, in the figure that you  
11 showed us a year or nine months ago or something,  
12 there was fluence on one access and then there was  
13 this shift on the other and the data seemed to be all  
14 over the place.

15 MR. ERICKSONKIRK: I'll show you that in  
16 just a second. You liked that plot.

17 DR. WALLIS: Well, maybe it was all over  
18 the place --

19 MR. ERICKSONKIRK: It wasn't --

20 DR. WALLIS: Different amounts of cooper  
21 or something.

22 MR. ERICKSONKIRK: It wasn't a FAVOR plot  
23 but, anyway, we'll get to that. I recalled your  
24 hankering for plots with lot of scatter so I've got  
25 one.

1 DR. WALLIS: No, I didn't hanker. I just  
2 noticed.

3 MR. ERICKSONKIRK: Anyway, the models are  
4 similar in form but certainly not identical. The  
5 regulatory model includes some terms that were  
6 intentionally conservative relative to the E900 model.

7 We did a sensitivity study which is  
8 reported in our documentation and the use of ASTM  
9 E900-02 model reduces the through-wall cracking  
10 frequency relative to our baseline model in FAVOR of  
11 about a factor of three.

12 We should also point out that the work by  
13 our contractor has been ongoing since the year 2000  
14 incorporating advancing physical understandings and  
15 also incorporating new surveillance data that have  
16 become available. While that model is still being  
17 worked on and hasn't been adopted by either ASTM or in  
18 the FAVOR code, it should be pointed out that the  
19 model we're currently working on is closer to the ASTM  
20 E900-02 standard than the model we are currently  
21 using.

22 DR. WALLIS: This is important because  
23 what the plant knows is what its fluence is.

24 MR. ERICKSONKIRK: And its copper and its  
25 nickel and its phosphorus.

1 DR. WALLIS: Right. It has to deduce this  
2 shift in this key thing.

3 MR. ERICKSONKIRK: That's right.  
4 Absolutely.

5 DR. WALLIS: And if there is a little  
6 uncertainty in that, that seems to me pretty  
7 significant. If you know your fluence but you can't  
8 know your RT very well, then the whole basis of your  
9 analysis is this --

10 MR. ERICKSONKIRK: And, indeed, that  
11 uncertainty is incorporated into our analysis.

12 DR. WALLIS: It must be.

13 MR. ERICKSONKIRK: Yeah, it is.

14 DR. FORD: I was having exactly the same  
15 question. The correlation factor on the Eason model,  
16 for instance, is remarkably low between the model and  
17 the data so it comes down to this question.

18 MR. ERICKSONKIRK: You mean like that?

19 DR. WALLIS: Yes, that's the one.

20 DR. FORD: I don't know what the  
21 correlation factor is but it's got to be less than .1  
22 I would imagine.

23 MR. ERICKSONKIRK: It should be pointed  
24 out there are other ways to judge a model with a  
25 correlation factor.

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1 DR. WALLIS: The spread is huge. If I  
2 know my fluence, then I don't know my delta  $T_{30}$  within  
3 maybe 50.

4 MR. ERICKSONKIRK: Before we go too far on  
5 this, I want to assure everyone that that level of  
6 uncertainty is incorporated in all of the calculations  
7 that you've seen. We're not trying to hide or sugar  
8 coat anything. It's in there.

9 DR. FORD: I know but, again, looking at  
10 your hypothetical weld you're trying to analyze,  
11 assume that you put it at the upper bound.

12 MR. ERICKSONKIRK: And sometimes you do.

13 DR. FORD: Okay. Does that affect your CF  
14 value that much? That's the bottom line.

15 MR. ERICKSONKIRK: If it was always up,  
16 yes. If 100 percent of the time it was always at the  
17 upper bound, certainly --

18 DR. WALLIS: This is your screening  
19 criteria, or used to be. You had 270 degrees or  
20 something. The guys says, "Okay, my fluence is 2E to  
21 the 19." He looks there and says, "Now I've got to  
22 calculate what my --

23 MR. ERICKSONKIRK: But remember we are  
24 asking him to calculate --

25 DR. WALLIS: He takes the black line?

1 MR. ERICKSONKIRK: Remember, we are asking  
2 him to calculate that value using maximal values of  
3 fluence, of copper.

4 DR. WALLIS: He takes the black line but  
5 what he reports is --

6 MR. ERICKSONKIRK: He's plugging those  
7 maximum values of copper, nickel, phosphorus, and  
8 fluence into the black line calculation so he's using  
9 upper-bound input values. It would be inappropriate  
10 to ask them to use both upper-bound input values and  
11 an upper-bound correlation.

12 DR. WALLIS: I don't know because I don't  
13 know what that does to the conclusion he reaches.

14 MR. HISER: This is Allen Hiser from  
15 Research Materials Engineering Branch. I don't  
16 believe that upper-bound copper and nickel are used.  
17 It's best estimate values.

18 MR. ERICKSONKIRK: You're right. We're  
19 using the regulatory values that have been agreed to  
20 between the licensee and NRR. You're right. I  
21 apologize.

22 CHAIRMAN SHACK: But then he adds a margin  
23 term.

24 MR. ERICKSONKIRK: No. In the current  
25 regulation he adds a margin term because that

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1 uncertainty wasn't accounted in the calculation.

2 CHAIRMAN SHACK: You can address your  
3 uncertainty directly or you can add a margin.

4 DR. WALLIS: Don't talk about the current  
5 regulation because we know that the margin compensates  
6 for a very strange way of accounting for uncertainty  
7 in this previously. You add something when you should  
8 have subtracted it and then you add it again somewhere  
9 else. We don't want to go into that ever again.

10 MR. ERICKSONKIRK: Certainly not. That we  
11 all certainly agree. But, yes, in the old  
12 relationship we accounted for -- in the current way of  
13 doing things we account for this uncertainty after the  
14 fact with a margin term. In this case we have  
15 incorporated it into the calculation.

16 DR. WALLIS: I understand that. I  
17 understand statistically you can do that. It just  
18 sort of makes me a little suspicious of whether this  
19 is the right way to do it when I see that sort of  
20 scatter.

21 MR. ERICKSONKIRK: Is there a better way  
22 to do it?

23 DR. WALLIS: No, because it seems to me  
24 that you're -- the plant says its fluence is so and  
25 so. Therefore, my RT is something plus 100. It could

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1 well be 100 plus 170.

2 MR. ERICKSONKIRK: But how much of that  
3 scatter is experimental error in resolving the shift?  
4 A large part of it because, remember, you're trying to  
5 nail down and the regulation includes some funny  
6 statements. It says, "The licensee shall perform  
7 Charpy testing to define the 30-foot pound shift  
8 without error."

9 DR. WALLIS: If you look at the history of  
10 Charpy testing, you again get all sorts of causes of  
11 error. Here you see the key variable is this RT and  
12 this delta T is used to calculate that  $RT_{ndt}$  or  
13 whatever. That's why I've always been -- I'm sure  
14 you're doing very consistent stuff but it seems to me  
15 you're hanging your hat on something which is somewhat  
16 difficult to define.

17 MR. ERICKSONKIRK: Bruce, do you have a  
18 comment?

19 MR. BISHOP: This is Bruce Bishop from  
20 Westinghouse. The only comment I had to make is  
21 you're right, there is a chance that the  $RT_{ndt}$  instead  
22 of being 100 could be 170 but, again, it's not always  
23 -- again, what you have to look at is what's the  
24 probability that it's going to be 170 versus 100.  
25 That's the distribution that's built into the

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1 evaluations that we do.

2 DR. WALLIS: You're saying it's all  
3 aleatory. There may be some plant which is always at  
4 the top of the curve.

5 MR. ERICKSONKIRK: I don't think you find  
6 that. When these correlations have been developed  
7 what you find is that if you look at individual data  
8 sets relative to the mean line, they are scattered  
9 about the mean line. You don't see systematic biases  
10 where Palisades is always --

11 DR. WALLIS: So I shouldn't say that  
12 Palisades might be all at the top.

13 MR. HISER: That's not universal. There  
14 are some plants, some materials that have a  
15 sensitivity that skews upwards. There are some plants  
16 that because of their surveillance data are not  
17 allowed to use the correlations in the current reg  
18 guide. They are required to use a higher chemistry  
19 factor to compensate for that.

20 DR. FORD: Mark, can you put a nagging  
21 problem in my mind? When we were discussing the  
22 research project last year, the question came up about  
23 anomalies in high-nickel, low-copper alloys in Santa  
24 Barbara. Is that no longer an issue?

25 MR. ERICKSONKIRK: I haven't been tuned

1 into that. Allen, do you have any comments on that?

2 MR. HISER: No. I don't have any  
3 information right at hand but we can dig into it and  
4 get it to you.

5 DR. FORD: It's just that looking at that  
6 it seems that the welds have the highest scatter and  
7 I was wondering if there is any correlation at all  
8 between this question of the high-nickel, low-copper  
9 that don't fall into any known correlations so far.

10 MR. ERICKSONKIRK: That, indeed, is a  
11 topic of current research.

12 DR. WALLIS: I think if you're honest in  
13 showing this figure, which I don't think is in the  
14 handout --

15 MR. ERICKSONKIRK: No, it's not. I added  
16 it last night. I'm not asleep at the switch up here.  
17 Since we're talking about uncertainties and what I  
18 would agree is a ghastly looking plot, in constructing  
19 FAVOR we had to decide it was appropriate to simulate  
20 those uncertainties.

21 What we did was we start with, as Allen  
22 properly corrected me, the licensing values of copper,  
23 nickel, phosphorus which are taken to be best  
24 estimates based on available data and then we sample  
25 from copper, nickel, and phosphorus distributions.

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1                   Where those copper, nickel, and phosphorus  
2 distributions are drawn from extremely large data sets  
3 drawn from many, many materials, and so I would argue  
4 have to be upper bound to the copper, nickel, and  
5 phosphorus uncertainty that you get in any particular  
6 material. We have mean values of copper, nickel, phos  
7 and, indeed, fluence.

8                   Then we sample from distributions, put it  
9 into the model and do that zillions and zillions of  
10 times. What we find out is that -- that's what's  
11 shown over here where the blue line with the Xs is a  
12 plot of the standard deviation of simulated  
13 embrittlement shift values that's coming out of FAVOR.

14                   And what you're finding is that down here  
15 the green line, and that's simulated -- I'm sorry, for  
16 a weld. The green line is the standard deviation of  
17 all this mess of scatter from the model. The reason  
18 why the standard deviation dips down here as you go to  
19 zero fluence is we don't allow FAVOR to simulate  
20 negative shift so it's truncated from below so you  
21 would expect a smaller standard deviation.

22                   DR. WALLIS: Almost by definition it's got  
23 to go through the origin

24                   MR. ERICKSONKIRK: Yes. But, anyway, the  
25 point of this graph is to say that FAVOR is faithfully

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1 reproducing the scatter in the original database in  
2 its simulation. It is simulating the amount of  
3 scatter that's in the database for all PWR  
4 surveillance materials.

5 Again, I would argue if I did one of the  
6 PowerPoint animation things and scrubbed away all the  
7 points except those for one particular weld, you would  
8 see much less scatter. FAVOR is simulating this much  
9 scatter, or this much scatter if you like, but  
10 unquestionably the amount of scatter in any one weld  
11 would be less or plate.

12 And also the other thing to point out here  
13 is that it wouldn't be appropriate to simulate the  
14 uncertainties in copper, nickel, phosphorus, and  
15 fluence and then simulate a relationship uncertainty  
16 on top of that because then we would be not  
17 approaching the scatter in the original experimental  
18 data base but approaching a value that's approximately  
19 twice that.

20 Now, here's one I did just for Dr. Shack  
21 because he asked me yesterday. Where we got off not  
22 simulating the uncertainty in the Charpy shift to  
23 fracture tough and shift correlation. Here is another  
24 plot with scatter in it, not quite as bad as the last  
25 time, where we have on the horizontal axis and, to

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1 be consistent, this should say delta  $T_{41}$  joules but  
2 it's the same metric, the shift and the Charpy 30-foot  
3 pound energy transition temperature versus the shift  
4 in the fracture toughness transition temperature.

5 Both of these are experimentally measured  
6 values made in the laboratory on RPV welds, plates,  
7 and forgings. Both -- well, obviously, before and  
8 after radiation. You have to have an unirradiated  
9 curve and then irradiated to various levels.

10 DR. WALLIS: There's another thing. First  
11 of all, you start with a fluency you know and then you  
12 have to predict this delta  $T_{40}$ .

13 MR. ERICKSONKIRK: No, this is not a  
14 prediction.

15 DR. WALLIS: The delta  $T_0$  is a much more  
16 reasonable useful physically based thing than delta  
17  $T_{30}$ .

18 MR. ERICKSONKIRK: I agree completely.

19 DR. WALLIS: Charpy is an antique and  
20 delta  $T_0$  is more related to what you are trying to  
21 predict.

22 MR. ERICKSONKIRK: And that's exactly why  
23 we have to go through this relationship.

24 DR. WALLIS: You're solid with fluence  
25 which you know and delta  $T_{30}$  is subject to large

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1 uncertainty. Delta  $T_0$  is what you really want and  
2 it's also subject to uncertainty when you get it from  
3 delta  $T_{30}$ .

4 MR. ERICKSONKIRK: That's right.

5 DR. WALLIS: It's amazing that with all  
6 this you can come up with something which makes sense.

7 MR. ERICKSONKIRK: I'm tempted to say  
8 something but it goes on the record so I won't.

9 DR. WALLIS: Maybe I should be  
10 congratulating you.

11 MR. ERICKSONKIRK: Thank you. I'll just  
12 say thank you like in the commercials. The one thing  
13 I do want to clarify is that in this plot all the  
14 values are measured. Delta  $T_{30}$  is not arrived at by  
15 the previous correlation. It's measured based on  
16 Charpy test just as delta  $T_0$  is measured based on  
17 fractured toughness test. Anyway, there's obviously  
18 considerable uncertainty apparent in the empirical  
19 relationship and it's these curves that we use in  
20 FAVOR.

21 The FAVOR simulation process to go back is  
22 to simulate the uncertainty in copper, nickel,  
23 phosphorus influence, use the main curve to calculate  
24 a value of delta  $T_{30}$  shift, and then we go to this  
25 relationship and simply convert it to delta  $T_0$  shift

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1 by multiplying by these values, the slopes of these  
2 lines without simulating the uncertainty. Dr. Shack  
3 asked where I got off doing that.

4 DR. WALLIS: Is there a trend here with  
5 individual samples or something?

6 MR. ERICKSONKIRK: No, and that's what I'm  
7 about to show you. We should choreograph this better.  
8 However, what we see, now, remember, these curves --  
9 I mean, certainly, as you pointed out, Dr. Wallis, the  
10 protocols for determining delta  $T_0$  are much more  
11 consistently lined out. In fact, there's an ASTM  
12 standard for determining delta  $T_{30}$ .

13 However, having said that, some of these  
14 delta  $T_0$  points can be derived using only six samples.  
15 That's the minimum that's allowed. Some of them have  
16 upwards of 100 or even more samples from the detailed  
17 laboratory test performed at Oak Ridge and others.

18 DR. WALLIS: Each one of these points is  
19 an average?

20 MR. ERICKSONKIRK: Each one of these is a  
21 best estimate.

22 DR. WALLIS: So if we plotted the six  
23 different tests, we would get even more --

24 MR. ERICKSONKIRK: No, no, no. You can't  
25 determine -- you need six tests to determine  $T_0$ .

1 Actually, you need 12 tests to determine delta  $T_0$ .

2 DR. WALLIS: You get transition.

3 MR. ERICKSONKIRK: That's right. Anyway,  
4 the origin of this scatter I argue is not uncertainty  
5 in the relationship which is to say that for some  
6 materials the delta  $T_0$  is much smaller than the delta  
7  $T_{30}$  and for other materials the delta  $T_0$  is much larger  
8 than delta  $T_{30}$ .

9 But its measurement error because when we  
10 wipe away the points that have been determined with  
11 the small data sets and look only at the points that  
12 have been determined by the large data sets, you see  
13 them clustering much more closely to the line.

14 This is a cartoon. Well, it's real data  
15 but it's a cartoonist attempt to do a residuals  
16 analysis which is actually presented in the document.  
17 For example, if there was a true material-to-material  
18 dependency in this relationship, then it would be  
19 equally likely that the large data set points were  
20 these flyers out here as the ones populating close to  
21 the line.

22 Whereas if there is truly an underlying  
23 physical basis consistent relationship going on that  
24 cuts across all these materials, you must expect that  
25 the materials that have the best defined shifts using

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1 the most data are going to lie closest to the line.  
2 Indeed, that is the case.

3 That's our justification for not sampling  
4 the uncertainty here. Another justification,  
5 perhaps more practical one, is that, I mean, we can  
6 measure in the laboratory the uncertainty on delta  $T_{30}$   
7 and we can measure the delta  $T_0$ . And because delta  $T_0$   
8 is more rigorously defined, the uncertainty tends to  
9 be smaller.

10 Whereas, if I went through and if I  
11 sampled the uncertainty in this relationship in  
12 simulating my delta  $T_0$ s, my delta  $T_0$  uncertainties  
13 would be huge relative to what I measure in the  
14 laboratory. We would be overestimating the  
15 uncertainty in those values relative to anything  
16 that's been observed.

17 DR. WALLIS: Do you have the other plot  
18 which is delta  $T_0$  versus the fluence or has that not  
19 been done in terms of experiment?

20 MR. ERICKSONKIRK: I have that. I don't  
21 have that with me.

22 DR. WALLIS: Is it better or is it just  
23 as --

24 MR. ERICKSONKIRK: At this stage nobody  
25 has attempted to develop a delta  $T_0$  embrittlement

1 trend curve for a whole host of reasons. One is that  
2 the testing just hasn't been going on for that long.  
3 Virtually all of the delta T<sub>0</sub> points here come from  
4 test reactors, whereas the embrittlement trend curve  
5 comes from reactor pressure vessel covalents.

6 I don't have them with me. I can show you  
7 curves of delta T<sub>0</sub> versus fluence for individual data  
8 sets but not for an agglomeration of data sets.  
9 Obviously that would be the best thing to get rid of  
10 this artifice entirely and estimate delta T<sub>0</sub> directly  
11 from copper, nickel, and so on.

12 CHAIRMAN SHACK: But, again, if you have  
13 uncertainties in your measurement of copper and nickel  
14 you would expect to see a reasonable amount of  
15 scatter.

16 MR. ERICKSONKIRK: Yes, and we simulate  
17 that.

18 CHAIRMAN SHACK: And you simulate that.

19 MR. ERICKSONKIRK: What we try not to do  
20 is to compound the scatter.

21 CHAIRMAN SHACK: Obviously you don't want  
22 to double count. Before I was sort of wondering if I  
23 took this scatter in the copper and nickel whether I  
24 would reproduce the scatter that I see and you do. I  
25 mean, if you run through the plot, you can attribute

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1 most of that scatter in your uncertainty in your  
2 copper and nickel measurements. That doesn't seem  
3 unreasonable.

4 In FAVOR how is that sampling done? I  
5 mean, for a vessel do you pick one? When is the  
6 sampling done on the copper and nickel in Monte Carlo?

7 MR. ERICKSONKIRK: I think Terry can  
8 provide a more direct answer of that. Before I let  
9 Terry talk, I'm going to keep talking so he can't say  
10 anything. You've got different starting or mean  
11 copper, nickel, phosphorus values for each region, for  
12 each weld plate forging. Now onto Terry.

13 MR. DICKSON: Well, remember we're in a  
14 Monte Carlo loop here so let's take vessel No. 1, flaw  
15 No. 1. Flaw No. 1 is going to be located in some  
16 subregion that has a chemistry and a fluence. Those  
17 are going to be treated as the best estimate or mean  
18 values and then you're going to sample. You have the  
19 mean and there's some defined standard deviation  
20 that's input data.

21 CHAIRMAN SHACK: Okay. So I've got the  
22 mean. I've got the flaw and now I'm going to sample  
23 over copper and nickel.

24 MR. DICKSON: Copper, nickel, phosphorus,  
25 and fluence. That gives me everything I need to

1 calculate delta  $T_{30}$ . I have the unirradiated  $RT_{ndt}$  and  
2 then I've continued to go through the manipulations  
3 that he shows here, the .99 if it's weld, 1.1 if it's  
4 plate. Does that answer your question?

5 CHAIRMAN SHACK: So at the flaw level?

6 MR. DICKSON: Each flaw.

7 MR. ERICKSONKIRK: And the only further  
8 modification to that that should perhaps be pointed  
9 out is if in a particular vessel two flaws are  
10 simulated to occur in the same subregion so close to  
11 each other, the FAVOR code then remembers that it's  
12 already simulated what the copper and nickel and  
13 phosphorus is in that subregion and it doesn't then  
14 sample again with as big an uncertainty level.

15 DR. WALLIS: Is the copper and nickel and  
16 stuff diffused?

17 MR. ERICKSONKIRK: Not once it's a solid.

18 DR. WALLIS: Is it uniform? From the  
19 process of welding is it homogeneous in the weld? We  
20 are getting into too much --

21 CHAIRMAN SHACK: The composition is in --  
22 I mean, that was part of the problem they had in  
23 characterizing these things. A weld sample is not a  
24 weld sample.

25 MR. ERICKSONKIRK: Certainly the copper

1 isn't uniform. We know it's not uniform through the  
2 thickness because the copper comes from copper coating  
3 on the weld spools and you can't fill up -- the RPV  
4 welds are so big that you can't fill up an entire  
5 axial or circumferential weld with a single weld  
6 spool.

7           Through thickness samplings of chemistry  
8 can show systematic, even step function variations of  
9 copper through the thickness. In fact, that's  
10 something that we've attempted to simulate in FAVOR by  
11 the procedure where every time we get to a quarter of  
12 the way through the vessel and a through-wall cracking  
13 calculation when we get to the quarter point, the half  
14 point, and the three-quarter point. We go and we  
15 reassimilate the copper value knowing that it could be  
16 -- that you could be experiencing a step function.

17           One of the questions that Dr. VanWalle and  
18 the Peer Review Committee asked is, "Well, that's all  
19 very well and good but I would just be curious to know  
20 what would happen if you didn't reassimilate the  
21 copper?" We did that and removing that resampling  
22 increases the through-wall cracking frequency by a  
23 factor of 2.5 on average. The reason for that is  
24 every time you resample the chemistry, two things can  
25 happen.

1                   You could get a worse material or a better  
2 material. If you get a worse material, the crack was  
3 already going through and it's going to keep going  
4 through. If you get a better material, you stand a  
5 chance of stopping it. Every time you resample, it's  
6 like giving the material another chance so the  
7 direction of the trend is expected.

8                   DR. WALLIS: By the time you get these  
9 factors of two from this and the factor of two from  
10 that and a factor of two somewhere else, soon you have  
11 a factor of 10.

12                   MR. ERICKSONKIRK: But I've got many more  
13 factors going the other way and I've got a slide on  
14 that. You want to take this show on the road  
15 sometime?

16                   DR. WALLIS: It just seems to me that by  
17 manipulation of these factors and choosing which one  
18 you actually want to represent on your figure, you  
19 could make this 10 to the -7 become 10 to the -9 or 10  
20 to the -5.

21                   MR. ERICKSONKIRK: I think I could make it  
22 10 to the -9. I don't think I could make it 10 to the  
23 -5. Honestly, I don't think I could drive it down  
24 considerably. I feel like I could drive it down more  
25 than I could drive it up.

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1                   The one slide I don't have but in a spread  
2 sheet I've been tracking from the time that we first  
3 presented to you and after we wrote the December 2002  
4 report, I've been keeping a spread sheet of every time  
5 we do a new full analysis. The through-wall cracking  
6 frequency values haven't really changed that much over  
7 all.

8                   There are some changes that have affected  
9 more plants than others but in bulk all the changes  
10 that we've made have not shifted things around too  
11 much. I have the feeling we've gotten to the point  
12 where certainly when you focus on any one of these  
13 things, you can say, "That's absolutely wrong," or  
14 "That's a horrendously big factor."

15                   But when they are taken all in bulk, the  
16 law of averages is actually helping us and the values  
17 just aren't changing that much. And the general  
18 direction is down rather than up.

19                   DR. WALLIS: Like an expert elicitation  
20 where you get sort of tremendous scatter between the  
21 experts, but when you take the average it gets close.

22                   MR. ERICKSONKIRK: Yes. Okay. Another  
23 change motivated by our reviewers is that Dr. Schultz  
24 pointed out that FAVOR has ignored the effect of  
25 pressure on the crack face in calculating the crack

1 driving force.

2 Originally I thought no, we couldn't have  
3 ignored something that simple. Later I came  
4 to learn that indeed we had ignored something that  
5 simple so we put it in. For the transients where  
6 pressure is a significant contributor, namely the  
7 stuck-open-valve transients including the crack face  
8 pressure increases the conditional probability  
9 through-wall cracking by less than a factor of two, by  
10 between 25 percent and 75 percent.

11 For all the other transients where  
12 pressure has played a major role has virtually no  
13 effect. Then when you wade in the frequency with  
14 which these events occur, the rolled-up effect on the  
15 overall plant through-wall cracking frequencies is at  
16 most 6 percent and more typically like 1 percent.  
17 We've included it in there because it certainly should  
18 be there. It's obvious that it's there but it doesn't  
19 make a big change.

20 DR. WALLIS: I should say at this point  
21 I'm very happy to see that you seem to be much more  
22 professionally responsive to questions than sometimes  
23 happens in these meetings. This is a complement.

24 MR. ERICKSONKIRK: Thank you. Thank you  
25 again.

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1 I think this is the last one in this  
2 series. Yes. Then another thing Dr. VanWalle from  
3 the Peer Review group brought up is, as I said, an  
4 interim model between the time we last briefed you and  
5 now is we included an upper shelf toughness model but  
6 instead of indexing the level of upper shelf toughness  
7 to a fractured toughness property, we indexed it to  
8 Charpy.

9 Dr. VanWalle looked at those correlations  
10 more aghast than the one you were looking at. I have  
11 elected even not to show that. I think he said  
12 something like, "It's not professionally responsible  
13 to do it that way."

14 DR. WALLIS: Maybe Charpy should  
15 eventually disappear.

16 MR. ERICKSONKIRK: Perhaps it should, but  
17 we recently had the centenary conference and everybody  
18 still liked it, a 100 years of bad testing.

19 Anyway, we talked about this yesterday.  
20 A new model was available where we could estimate the  
21 level of upper shelf toughness directly from a  
22 toughness measurement. We included that in the model.  
23 The overall effect in the through-wall cracking  
24 frequency was small, about a five percent change.

25 But what it did do, and this I view as

1 being a very good thing, is it eliminated any  
2 predictions of failures in the vessel in regions  
3 having low  $RT_{ndt}$ . That makes sense because now all of  
4 the toughness measures, cleavage initiation toughness,  
5 cleavage arrest toughness, and upper shelf toughness  
6 are linked to the  $RT_{ndt}$  measure.

7 Now the more interesting things. Getting  
8 into the sensitivity studies that we did to look at  
9 factors that were sort of outside of our modeling.  
10 Clearly in order to get up to through-wall cracking  
11 frequencies around the  $1 \times 10^{-6}$  limit that we're trying  
12 to get to, we've had to really crank up the  
13 embrittlement beyond anything that has ever been  
14 observed.

15 Now not only are we basing the irradiation  
16 shift on a correlation with lots of scatter, but we're  
17 extrapolating beyond the empirical database. It's a  
18 good thing that correlation has a physical basis  
19 because that gives us at least some confidence that we  
20 are extrapolating right, but obviously data's better.

21 Anyway, there were two ways that we  
22 considered artificially increasing the level of  
23 embrittlement. The method that we have been using is  
24 just to increase time as a free variable. Increase  
25 effective full-power years.

1                   Alternatively we could have taken the view  
2                   that we wanted to keep time in what would be  
3                   considered a logical operating range and instead  
4                   increase the unirradiated value of  $RT_{ndt}$  and then begin  
5                   embrittlement from there.

6                   Both of those changes are artificial  
7                   because we have neither materials that are that crumbly  
8                   before you start irradiation in the database, nor do  
9                   we have irradiation values out to those extended  
10                  periods of time. Both of them are clearly artificial  
11                  attempts to increase the level of embrittlement.

12                  What this plot shows is that we've used  
13                  the approach that is more conservative in our baseline  
14                  calculations. The points where we've got the crosses  
15                  and the pluses show how much the through-wall cracking  
16                  frequency increases using the 32 EFPY analysis as a  
17                  baseline, plotted versus the increase in reference  
18                  temperature where the crosses and the pluses we  
19                  increase the reference temperature from the 32-year  
20                  base line by turning up the time meter.

21                  The solid points, again green for Beaver  
22                  and red for Palisades, we increase the reference  
23                  temperature just by increasing the unirradiated value.  
24                  All of the calculations you've seen are based on  
25                  increasing time as a free variable which gives you

1 more of a through-wall cracking frequency increase  
2 than increasing unirradiated  $RT_{ndt}$ . Again, these are  
3 both artificial.

4 The only thing I would say in favor of  
5 increasing time over increasing unirradiated  $RT_{ndt}$  is  
6 unirradiated  $RT_{ndt}$  isn't even a factor in the  
7 correlation whereas time and temporal variable and  
8 time dependent variables like fluence are so there is  
9 at least some belief that the correlation accounts for  
10 those whereas it doesn't account for unirradiated  
11  $RT_{ndt}$ .

12 DR. WALLIS: You can find a way of getting  
13 your numbers to be at the point where you would be  
14 concerned with through-wall cracking, a factor of 300.

15 MR. ERICKSONKIRK: Yeah, extremely long  
16 times.

17 Okay. Now, to your question about  
18 forgings. People have probably noticed by now that  
19 all the vessels we've analyzed are rolled plates that  
20 are welded, whereas there are a good number of vessels  
21 that we license that are forgings and forgings don't  
22 have the same flaw populations as axial welded  
23 vessels. Certainly they don't have flaws associated  
24 with axial welds which are the flaws that are driving  
25 this analysis.

1           So, on that basis alone, just removing the  
2 axial weld flaw population you should believe that the  
3 through-wall cracking frequency at equal levels of  
4 embrittlement as indexed by  $RT_{ndt}$  should be much less.  
5 However, forgings do have their own unique flaw  
6 populations that need to be accounted for.

7           They've got both flaws that have formed as  
8 part of the forging process and they've also got those  
9 nasty little things called subclad flaws which form  
10 perpendicular to the direction of the cladding so they  
11 are axial and that's bad. We performed some  
12 sensitivity studies to try to assess this.

13           The first thing we had to do was to  
14 construct new flaw distributions to simulate forging  
15 flaws and subclad flaws. For this we relied on the  
16 help of Dr. Fred Simonen at the PNNL laboratory who  
17 has done the rest of our flaw distribution work on the  
18 forging flaws and this is, again, all documented in  
19 our reports.

20           The forging flaw distribution was based on  
21 destructive evaluation of forgings that were performed  
22 at PNNL under our contract. They have a similar  
23 morphology and similar sizes to plate flaws but a  
24 somewhat greater density. Not very much.

25           The subclad flaws are the bigger concern

1 because they form preferentially -- oh, and I should  
2 say the forging flaws, like plate flaws, they don't  
3 have a preferential orientation. Subclad flaws are  
4 the bigger concern.

5 They form preferentially in certain  
6 forging chemistries at high cladding heat inputs.  
7 They form as dense arrays of shallow cracks that are  
8 oriented perpendicular to the clad welding direction  
9 so now they are axial rather than circumferential and  
10 that, of course, makes them bad.

11 The density and depth of the flaws that we  
12 put in our subclad flaw distribution were estimated by  
13 Dr. Simonen based on a review article that was  
14 published in 1978 which was the time when forging  
15 flaws were the fashion, or subclad flaws were the  
16 fashion.

17 The density of these things is amazing.  
18 You are reading that right, 80,000 flaws per square  
19 meter. Now, this is an extrapolation and I believe a  
20 conservative one because that was based on one scaling  
21 of one picture that was in the Dhooge report. All of  
22 the simulated flaws have a depth of two millimeters so  
23 we've got 80,000 flaws per square meter all with a  
24 depth of two millimeters.

25 Now, indeed, in the picture that we scaled

1 there was a range of depths ranging from zero to two  
2 millimeters. However, the way we've coded the FAVOR  
3 program without fundamental  
4 restructuring --

5 DR. WALLIS: If they have a depth, what  
6 kind of dimension do they have in the other dimension?

7 MR. ERICKSONKIRK: What did we simulate,  
8 Terry? We simulated a range of events?

9 MR. DICKSON: Greg gave us some data on  
10 that, I believe.

11 MR. ERICKSONKIRK: They were simulated  
12 with the same length aspect ratios as the --

13 DR. WALLIS: The two millimeters.

14 MR. ERICKSONKIRK: Two millimeters by  
15 generally longer.

16 DR. WALLIS: It's not quite the raised  
17 surface and they don't show up.

18 MR. ERICKSONKIRK: No.

19 DR. WALLIS: If they were over a  
20 centimeter long, they would actually join up at this  
21 20K --- 80K.

22 MR. ERICKSONKIRK: Yeah.

23 MR. DICKSON: But by definition they are  
24 inner cracked, too, at the plant base interface. In  
25 other words, it's as close as an embedded flaw can be

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1 to the inner surface.

2 MR. ERICKSONKIRK: And I think the point  
3 should be made here this is not like the external  
4 event study that Donnie is about to talk about. Our  
5 attempt here has not been to do something best  
6 estimate but to do something in a bounding sense  
7 because we believe that the results will still be  
8 sufficiently good that we can still use our plate-  
9 based screening criteria. Indeed, that is the case.  
10 Certainly you can refine this an awful lot.

11 So the way we made up forged vessels is we  
12 used our existing models for Palisades and Beaver  
13 Valley but we simply assigned both the plates and the  
14 axial welds to have material properties that were  
15 characteristic of forgings. The forging properties  
16 that we used we took out of the RVID database and we  
17 used the most radiation sensitive forgings that are in  
18 the fleet, those being Sequoyah and Watts Bar.

19 MR. SIEBER: How many total forged vessels  
20 are there in service?

21 MR. ERICKSONKIRK: I've got that written  
22 down. It's like a dozen. I think more. I think 12  
23 to 16. It's not the majority of the PWR population  
24 but it's not just one or two either. So the results  
25 of the sensitivity studies looking at the flaws that

1 formed as part of the forging process.

2 Even at very high levels of embrittlement  
3 that would normally be needed to get a plate vessel  
4 close to the through-wall cracking frequency criteria  
5 of  $1 \times 10^{-6}$ , the forged vessels have a through-wall  
6 cracking frequency that's on average about three  
7 percent of a plate vessel.

8 That factor is roughly consistent with  
9 just removing the contribution of axial flaws which  
10 is, indeed, what has happened. That makes a lot of  
11 sense. We've removed the axial flaws which, if you  
12 remember my graph from yesterday, contributed a 100  
13 times more to the through-wall cracking frequency than  
14 the plate flaws. All that's left is forging flaws  
15 which had a slightly higher density than the plate  
16 flaws so instead of a factor of 100 we've got a factor  
17 of like 20.

18 DR. FORD: Mark, take a scenario. You've  
19 got 80,000 flaws per square meter. Near the surface  
20 there are nonsurface-breaking flaws but just below the  
21 surface.

22 MR. ERICKSONKIRK: That's right.

23 DR. FORD: You then put a weld overlay.

24 MR. ERICKSONKIRK: That's right.

25 DR. FORD: And that could, therefore, get

1 some of those flaws to break the original surface.

2 MR. ERICKSONKIRK: No, but the flaws form  
3 as a consequence of putting the weld overlay on.

4 DR. FORD: So what happens if the weld  
5 overlay stress grows cracks?

6 MR. ERICKSONKIRK: I have been informed  
7 not to answer questions like that. I don't know. I  
8 don't know.

9 DR. FORD: Now you've got a surface crack,  
10 stress erosion crack.

11 MR. ERICKSONKIRK: Is there a mechanism to  
12 support that, stress corrosion cracking of the  
13 cladding?

14 DR. FORD: In a pressurized water reactor  
15 probably unlikely but not completely unlikely. I  
16 mean, if you've got copper in the system, you can get  
17 cracking at PWR with the steam generator but that  
18 doesn't matter. If you've got copper into the primary  
19 system, you could crack. It could. It's a long shot  
20 but you need one of these things to go or we don't  
21 have a business. I'm looking at the long shot.

22 MR. ERICKSONKIRK: And I think -- like I  
23 said, I absolutely refuse to argue stress corrosion  
24 cracking with anyone. Least of all you. I think that  
25 gets into is what you've just proposed does that rise

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1 to the test of being a credible model. If so, then  
2 the next thing I'm going to have to ask you is, okay,  
3 propose some numbers and we'll run it, and we  
4 certainly could. Certainly if those were surface-  
5 breaking flaws, I mean, you know the answer as well as  
6 I.

7 DR. FORD: I'm really going off of  
8 Graham's earlier question. If you put a weld overlay  
9 onto a severely defected but not surface-breaking  
10 forging, would you expect some of those subsurface  
11 cracks to coalesce to form one fairly large connected  
12 crack?

13 MR. ERICKSONKIRK: Possibly, but I think  
14 before going there you also have to -- if we wish to,  
15 if we feel it's appropriate, to refine that part of  
16 the analysis, we should also go back and refine this  
17 estimate of 80,000 flaws per square meter to, again,  
18 do something that is --

19 MR. SIEBER: Useful.

20 MR. ERICKSONKIRK: -- realistic.

21 DR. FORD: But that 80,000 was measured.

22 Wasn't it?

23 MR. ERICKSONKIRK: It was measured.

24 DR. FORD: So it's realistic.

25 MR. ERICKSONKIRK: Well, it's realistic

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1 for the 100X Micro it was taken off of.

2 CHAIRMAN SHACK: But I thought you were  
3 asking a different one. You wanted to pop the axial  
4 weld flaw through from the stresses of the clad weld,  
5 not the underclad cracking that he's seeing here.

6 DR. FORD: Well, I was wondering if you  
7 get some of those subsurface flaws, that these  
8 small --

9 CHAIRMAN SHACK: Those presumably would  
10 have been sampled in the vessels that he looked at.

11 DR. FORD: I don't know.

12 CHAIRMAN SHACK: That population should be  
13 part of the population they're looking at because  
14 they've got welded vessel with --

15 DR. FORD: I'm trying to look at a  
16 realistic worse case scenario if that's not an  
17 oxymoron. You've seen 80,000 of these flaws.

18 DR. WALLIS: That's caused by the welding  
19 process.

20 MR. ERICKSONKIRK: That's caused by the  
21 weld. Those aren't preexisting. They occur as a  
22 consequence of the welding itself.

23 DR. FORD: But then you put the butter on.

24 MR. ERICKSONKIRK: No, no. They occur as  
25 a consequence of the austenitic cladding process.

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1 DR. FORD: Okay. Never mind. I take it  
2 back.

3 MR. ERICKSONKIRK: Sorry. Clarity in  
4 communication.

5 DR. WALLIS: But you are doing this  
6 welding over the whole vessel so if there is something  
7 waiting to happen, you would find it.

8 MR. ERICKSONKIRK: Are you proposing this  
9 is nondestructive testing technique?

10 DR. WALLIS: No, but it seems to me that  
11 your numbers are so small for this that one has to pay  
12 attention --

13 MR. SIEBER: There is surface exams as  
14 part of the code.

15 MR. ERICKSONKIRK: Yeah, as was pointed  
16 out.

17 DR. KRESS: How do you find these flaws?  
18 They are put in there after you put the weld surface  
19 on. Do you have to take it back off and then find the  
20 flaws?

21 MR. SIEBER: No, no, no.

22 DR. KRESS: You use nondestructive  
23 testing technique?

24 MR. SIEBER: It's UT.

25 MR. ERICKSONKIRK: I'm not qualified to

1 answer that question.

2 MR. SIEBER: It's a volumetric test done  
3 every 10 years.

4 MR. ERICKSONKIRK: Are they close enough  
5 to find with mag particle?

6 MR. SIEBER: The problem is putting all  
7 that stuff into a vessel that you would like to use  
8 again.

9 MR. ERICKSONKIRK: Okay. I'll try to go  
10 on.

11 MR. SIEBER: This is vertical so mag  
12 particle is not real good.

13 MR. ERICKSONKIRK: So for the subclad flaw  
14 sensitivity study, over likely operational lifetimes  
15 meaning up to 60 EFPY which is obviously beyond what  
16 we are licensing today, the through-wall cracking  
17 frequency of the forged vessel with the subclad flaws  
18 was between 1 percent and 20 percent of that and a  
19 comparable plate vessel.

20 However, as you crank of the level of  
21 embrittlement over the longer operational lifetimes,  
22 you got to the point where the through-wall cracking  
23 frequency for the simulated subclad vessel based on  
24 all these assumptions was much, much higher than it  
25 was for the plate vessel.

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1                   That leads to the first proviso that we  
2 would put on our suggested screening criteria which  
3 is, again, in the report that if somebody were  
4 assessing the through-wall cracking frequency of a  
5 forged vessel that was believed to be sensitive to  
6 subclad cracking and there are papers that tell you  
7 based on the forging chemistry and the welding process  
8 if you've got a susceptible vessel or not.

9                   If somebody was intending to operate the  
10 lifetimes that are much, much beyond what we are  
11 considering today or to embrittlement levels that are  
12 much, much beyond what we are considering today,  
13 certainly a more detailed analysis of this phenomena  
14 would be warranted.

15                   Okay. Now the one that I like, thickness  
16 effects on through-wall cracking frequency. You saw  
17 this graph yesterday of the variation of through-wall  
18 cracking -- I'm sorry, conditional probability of  
19 through-wall cracking with break diameter for the  
20 primary site pipe break.                   These are all pipe  
21 breaks, no repressurization.

22                   We argued out here that once you got to a  
23 big enough break diameter, it was the vessel  
24 properties that were controlling the through-wall  
25 cracking frequency and the details of the thermal

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1 hydraulic transient were relatively unimportant. Then  
2 we started looking at this graph and saying if that's  
3 true, then why out here at the very largest break size  
4 aren't all the points just landing smack dab on top of  
5 each other?

6 Because we argued that assuming that  
7 you're going down to the same temperature, which you  
8 always are in this case, we argued that the things  
9 that were controlling the severity of the thermal  
10 stress was the thermal conductivity of the steel which  
11 is consistent from material to material and the  
12 thickness of the vessel. The thermal conductivity  
13 hasn't changed in these analyses but the thickness is.

14 DR. WALLIS: I'm surprised that the  
15 thermal transient goes in far enough to make a  
16 difference and that you can't always treat it as an  
17 infinitely thick vessel. It's just a surface effect.  
18 Apparently thermal stresses are not just a surface  
19 effect.

20 MR. ERICKSONKIRK: No. They depend on the  
21 thickness of the vessel.

22 DR. WALLIS: That's because double  
23 transient has penetrated it enough. If it doesn't  
24 penetrate it at all, just the surface, then it doesn't  
25 really matter how thick it is.

1 MR. ERICKSONKIRK: It's a restraint  
2 imposed on the structure. I mean, if I take a Coke  
3 can, I can't support it.

4 DR. WALLIS: If you are only heating up a  
5 very thin layer, then the restraining effect of six  
6 inches and 10 inches is the same.d

7 MR. ERICKSONKIRK: Yeah. You're  
8 propagating through the vessel.

9 DR. WALLIS: I'm surprised it propagates  
10 in that far. The vessel propagated in quite a bit  
11 then, the thermal effect.

12 MR. ERICKSONKIRK: Yeah. What you see is  
13 that as the transients develop with time, pick any  
14 crack and the applied K -- here we go -- the applied  
15 K of the driving force fracture goes up peaks and then  
16 it drops off as the thermal wave passes through.

17 Getting back to where this started, we  
18 looked at this and said, okay, if our rationale is  
19 correct, then all these things -- all the through-wall  
20 cracking frequencies out here should be lined up at  
21 roughly equivalent levels of embrittlement but they  
22 weren't.

23 The Beaver Valley vessel, which is about  
24 half an inch thinner than the Palisades and Oconee  
25 vessels, was showing -- and this wasn't the only

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1 indication but it's the easiest illustration -- was  
2 showing a systematically lower through-wall cracking  
3 frequency.

4 Terry ran a number of individual crack  
5 analyses where we took a crack, we put it in the  
6 vessel, and then we traced the K applied for different  
7 transients.

8 We did this for all the major transient  
9 classes, main steam line break, stuck-open valves, and  
10 so on, and I'm just showing you this, for the 16-inch  
11 LOCA but the trend is consistent that as you increase  
12 the vessel wall thickness, here we've gone from an  
13 eight-inch vessel out to an 11-inch vessel and you  
14 systematically are increasing the peak applied to  
15 stress intensity factor and it's that peak that is  
16 controlling the through-wall cracking frequency by not  
17 insignificant amounts.

18 And, again, I haven't showed all of the  
19 plots here. We have them and I can provide them to  
20 you. We looked at this for a dominant transient from  
21 each of the major classes and it was apparent in all  
22 of them. We find that -- I guess I bypassed this  
23 here. You can look at different reasons why as  
24 thickness goes up should through-wall cracking  
25 frequency go up or should it go down.

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1                   It should go up because the thermal  
2 stresses increase which increase the applied K which  
3 is certainly true. It should also go up because as  
4 you get a thicker vessel you've got more weld fusion  
5 line area and you've got more flaws so you've got more  
6 possibility to initiate.

7                   However, it might also go down because the  
8 thicker vessel has thickness, more opportunity to  
9 arrest a crack. However, what we found out we then  
10 did a probabilistic analysis where we looked at the  
11 16-inch hot leg break in Beaver Valley, the four-inch  
12 surge line break, the main steam line break, and  
13 stuck-open SRV that recloses later.

14                   We started off with the base line Beaver  
15 Valley thickness and then we just increased the -- the  
16 only thing we changed is we just increased the  
17 thickness of the Beaver Valley vessel and the vertical  
18 axis shows the ratio of the through-wall -- I'm sorry.  
19 That shouldn't be through-wall cracking frequency  
20 because these are individual transients.

21                   That should be conditional probability of  
22 through-wall cracking ratioed to the conditional  
23 probability of through-wall cracking for that  
24 transient in the baseline Beaver Valley vessel. We  
25 find systematic and not marginal increases in through-

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1 wall cracking frequency as you go out in thickness for  
2 all the major transient classes.

3 Then we said, okay, well, this is a plot  
4 I should have on my wall and now I do. What wall  
5 thickness is there in service? Well, certainly the  
6 great majority of the PWRs are in the eight to nine-  
7 and-a-half inch range which is the range that we  
8 covered in our baseline analyses.

9 There are a few vessels. My favorite, the  
10 Kewaunee vessel, down here that are thinner. There  
11 are also a few cases out here that are thicker. Based  
12 on this analysis we say in general our through-wall  
13 cracking frequency results can be applied without any  
14 modifications to vessels in this thickness range.

15 The conservative for the thinner vessels  
16 and, oh my gosh, we've got nonconservative results for  
17 the thicker vessels. Fortunately, the three thicker  
18 vessels are the three CE vessels at Palo Verde that  
19 all have extremely low levels of embrittlement.

20 So summary of the sensitivity studies. We  
21 believe that the sensitivity studies have shown that  
22 the through-wall cracking frequency predictions of the  
23 PFM model is implemented in favor of 04.1 or robust  
24 with regards to credible changes in either the  
25 submodels or in our inputs.

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1           We also believe that these results are  
2 applicable to PWRs in general from a PFM perspective  
3 with two minor propisios. That being if somebody had  
4 a forged vessel that was known to be susceptible to  
5 subclad cracking and they wanted to operate it to very  
6 high levels of embrittlement, they should be advised  
7 that they need to do a more detailed analysis of the  
8 through-wall cracking frequency of subclad cracks,  
9 more detailed than we've done here.

10           Also that if you want to apply these  
11 results to the very thick walled vessels at Palo  
12 Verde, they don't directly apply. However, as I would  
13 point out, Palo Verde because it's a more recently  
14 constructed plant is a very low embrittlement plant so  
15 I wouldn't anticipate any particular problems, just to  
16 say that you can't just use the results straight.

17           DR. FORD: On Palo Verde isn't Palo Verde  
18 one of those ones with the high nickel content?

19           MR. ERICKSONKIRK: I could not say for  
20 sure.

21           DR. FORD: There are not many. It's  
22 either Palisades or Palo Verde. My point is this is  
23 good from the analysis that you have done. If there  
24 was another embrittlement process with high nickel  
25 content welds, then what you're seeing is for those

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1 particular ones, to go back to your previous graph for  
2 the 11-inch area of wall thickness, you've got a very  
3 high sensitivity of that ratio to wall thickness.

4 MR. ERICKSONKIRK: Yeah. I think -- I  
5 mean, I'll make my plug for continued research since  
6 you set me up. I mean, clearly it would be silly to  
7 say that we know everything there is to know about  
8 irradiation embrittlement so we should continue to  
9 study that from a physical basis.

10 But also this is exactly the reason why,  
11 at least in my opinion, and I'll label it as such,  
12 nobody should be talking about discontinuing  
13 surveillance sampling programs because while I -- and,  
14 again, a personal view.

15 While I think in performing plant  
16 assessments it's generally better to just use the  
17 copper, nickel, and phosphorus and plug it into the  
18 embrittlement equation and go and use that,  
19 surveillance performs an important role of just doing  
20 a consistency check on that because if you do continue  
21 surveillance and you start to see values that are  
22 deviating from this correlation, that's a clear  
23 indication that something is going on that you didn't  
24 anticipate.

25 I think that's it. That's it. Any other

1 questions?

2 MR. DENNING: Yeah. Let me ask a question  
3 about the flaw distribution. Could you remind me  
4 again how did you account for the uncertainties in  
5 what that flaw distribution is since they are large?

6 MR. ERICKSONKIRK: Okay. The  
7 uncertainties are accounted for by essentially putting  
8 statistical bounds and they turn out to be very wide  
9 statistical bounds on the available data. We sample  
10 from a range of crack sizes and also a range of  
11 densities.

12 If I continue talking, I'm likely to say  
13 something wrong so I'll refer you to the report and I  
14 can certainly get that for you. T h e  
15 statistical distributions that we sampled from were  
16 based on fits to our data derived from the inspections  
17 of the vessels, both destructive and nondestructive.

18 The point I would make is that we are sampling from  
19 some pretty wide uncertainty bounds both for density  
20 and size.

21 CHAIRMAN SHACK: Again, how does that work  
22 in FAVOR?

23 MR. DICKSON: Okay. Going back and  
24 remembering that we are in a Monte Carlo loop here,  
25 vessel No. 1, flaw No. 1. Okay. For vessel No. 1

1 there is a statistical distribution that describes the  
2 possibilities for flaws. The number of flaws, the  
3 depth of the flaws, the width of the flaws, and the  
4 location of the flaw in the wall of the vessel.

5 There's a statistical distribution that  
6 you are going to use for that first vessel, all of the  
7 flaws in vessel No. 1. Then as Bruce Bishop said a  
8 minute ago, there's a thousand such distributions that  
9 are input to FAVOR through the input data.

10 For each flaw you are sampling from a  
11 distribution that has uncertainty and then in the  
12 global picture you have a thousand such statistics so  
13 you have a thousand distributions to sample from.  
14 Vessel No. 1 you use statistic No. 1. Vessel No. 2,  
15 statistic No. 2. After you get to a thousand you go  
16 back and repeat.

17 DR. WALLIS: Does the through-wall  
18 cracking frequency prediction depend on the tails of  
19 these distributions? Is it very sensitive to the  
20 extreme tails because this is always a problem with  
21 extrapolating statistics and estimating what happens  
22 way out at the end of the tail. Or is it more  
23 sensitive to the sort of bulk of the --

24 MR. DICKSON: Generally speaking it's more  
25 sensitive to the bulk because what you tend to see is

1 transients that have very low probabilities of  
2 fracture. The large flaws will kick in. They will be  
3 particularly significant for those very low  
4 probability transients.

5 In other words, you may have to have a  
6 one-inch flaw right at the clad base interface to get  
7 it to go. But those transients aren't going to matter  
8 too much anyway since they are low probabilities.  
9 Which brings me back to Dr. Wallis' question, which  
10 ones matter. It's not necessarily the deeper flaws.  
11 It's just kind of the quarter-inch to half-inch flaws  
12 that sort of dictate.

13 Then that concludes the presentation on  
14 PFM sensitivity studies. The next presentation  
15 concerns external events the write-up on which can be  
16 found in Section 9.4 of NUREG 1806 and the presenter  
17 will be Donnie Whitehead of Sandia National  
18 Laboratory.

19 MR. WHITEHEAD: The approach that we took  
20 for looking at external events, which basically  
21 determined whether or not -- the approach that we took  
22 was to determine whether or not the contribution of  
23 external events to our through-wall cracking frequency  
24 is greater than that which would be calculated from  
25 internal events.

1                   What we used for this particular analysis  
2 was the CPFs that had been calculated for the 60  
3 effective full power years. This analysis was  
4 purposely conservative and we were doing that to see  
5 whether or not we could bound the through-wall  
6 cracking frequencies from such external events.

7                   The specific effects are obviously plant  
8 specific. If we were to actually do detailed analyses  
9 and calculations for each of the plants it would be  
10 considerable resources and cost associated with this.  
11 What we wanted to do was to see if we were to do a  
12 conservative analysis would that affect the bottom  
13 line answer that we were trying to develop and that  
14 being is there enough justification to allow a  
15 modification to the existing rule.

16                   The conclusion that we came to from this  
17 particular analysis was that the results show that the  
18 conservative approach that we took basically would  
19 yield through-wall cracking frequencies that would be  
20 approximately designed as we have from the internal  
21 events at 60 EFPY. That's the conservative answer  
22 that we would arrive at.

23                   CHAIRMAN SHACK: We're running short here  
24 so I would like to finish up in a relatively few  
25 minutes so we have time for the Peer Review stuff.

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1 MR. WHITEHEAD: I can do that. Basically  
2 what we did was we reviewed to begin with information  
3 that was available from Calvert Cliffs PRA and  
4 sampling information from IPEEs. We examined  
5 information from licensee event reports.

6 What we found was there was a suggestion  
7 that external events would be a small contributor but  
8 there wasn't anything definitive so what we did was,  
9 again, we performed out detailed -- rather, our  
10 conservative analysis looking at various types of  
11 scenarios that might occur from various external  
12 events. LOCAs, secondary faults, things put together  
13 with both LOCA and secondary faults.

14 An example of the type of analysis that we  
15 did, let's look at a small-break LOCA and we looked at  
16 two cases, HCLPF value of .3 which is the review level  
17 earthquake for most of the plants, and a HCLPF value  
18 of .5g which is typical for the west coast plants.

19 If you look at the hazard curves for those  
20 two, you'll find that the first one gives you a value  
21 of about 1.6E-4 per year. The other one gives you a  
22 value of 5E-4 per year. Being conservative we chose  
23 the 5E-4. We combined that with the highest value  
24 that we had for LOCAs at 60 EFPY and we found that --  
25 well, let's see.

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1                   What we did was by doing that we have  
2 assumed that the LOCA event itself would actually  
3 occur at the point 5g HCLPF value. We have assumed  
4 that there's no credit taken for possible operator  
5 actions to mitigate the response to the event. We've  
6 also assumed that there's no credit for the possible  
7 failures of injection systems.

8                   What we found at least follows on the next  
9 page. This is basically the information that's in the  
10 report. It gives you the bounding values that we  
11 found by going through the same type of process for  
12 the small. Obviously we would look at the medium-  
13 break LOCA and the large-break LOCA, so forth and so  
14 on, both for the full-power case and the hot zero  
15 power case.

16                   Information from the other analyses that  
17 we did, this is all in the report. Here is one where  
18 we essentially go through the same process except now  
19 we combine both primary and secondary faults. The  
20 only case that we could come up with was a seismic  
21 event, again using the values with the high seismic  
22 frequency using a .1 probability for concurrent  
23 significant secondary fault using the worse case CPFs.  
24 You end up with various through-wall cracking  
25 frequency estimates.

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1           The overall results from this is that our  
2 best estimate through-wall cracking for internal  
3 events that we talked about previously is something a  
4 little less than 2E-8 per year. The total bounding  
5 through-wall cracking frequency for external events is  
6 about 2E-8. If you sum up all of the bounding  
7 analyses that we did, the answer is about 2E-8.

8           So what we conclude is that the external  
9 event contribution to through-wall cracking frequency  
10 is not any worse than what we have already calculated  
11 for the internal events.

12           In reality considering all of the  
13 conservatisms that we've done by always taking the  
14 highest for seismic events, always taking the highest  
15 HCLPF value, taking no credit for operator actions in  
16 any of the analyses that we've done, things of that  
17 nature, if you will, the true answer would be, and we  
18 would expect to be possibly significantly less than  
19 the 2E-8 that we calculated here.

20           So we see no reason why external events  
21 should pose any problem to the determination that we  
22 can move forward with rulemaking if we so choose to do  
23 so.

24           CHAIRMAN SHACK: We'll take a break now  
25 for 15 minutes.

1 (Whereupon, at 10:05 a.m. off the record  
2 until 10:28 a.m.)

3 CHAIRMAN SHACK: I'd like to come back  
4 into session.

5 MR. BESSETTE: I'm one of these people  
6 that predates technology or something. I'm going to  
7 go over the main comments we got from the Peer Review  
8 people. One of the comments that doesn't appear here  
9 we had six people. There were two fracture people and  
10 two thermal hydraulics people and one PRA guy and then  
11 Tom Murley.

12 Basically the way these things work out is  
13 that the two thermal hydraulics guys say, "Well, I  
14 don't know about this other stuff so I'm just going to  
15 focus on thermal hydraulics." Their comments are  
16 along those lines.

17 I think actually you need to keep all  
18 three disciplines considered as much in an integrated  
19 fashion as possible so you keep these relative  
20 uncertainties and whatnot in context. For example,  
21 from what Mark just showed with some of the standard  
22 deviations and whatnot.

23 As I said yesterday, most of the -- one of  
24 the set of comments -- first set of comments was most  
25 parameters in the PIRT are system boundary conditions

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1 rather than physical models, and indeed that's true.  
2 There's a sensible reason why that is so. Are there  
3 any questions about the effect of thermal  
4 stratification and mixing and the cold leg and  
5 downcomer from ECCS injection and how well this can be  
6 modeled with RELAP?

7 I think I showed -- we looked at a lot of  
8 experimental data which indicates we are getting the  
9 downcomer temperature pretty accurately. Although it  
10 is always large thermal stratifications in the cold  
11 leg during ECC injection, these don't translate into  
12 nonuniform temperatures in the downcomer.

13 There was a focus on the effective wall  
14 heat transfer coefficient, convective heat transfer  
15 coefficient, if you can separate this effect out and  
16 how important is that to the predictions of failure  
17 and questions about the use of 1D versus 2D downcomer  
18 nodalization which I talked about yesterday. They  
19 also took note of the fact that if you're not careful  
20 you can get these numerically induced flows in 2x4  
21 plants.

22 So we did a number of sensitivity studies  
23 both from what I showed yesterday and what I didn't  
24 talk about. We ran I would say hundreds of RELAP  
25 calculations at the University of Maryland to

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1 investigate the sensitivities that results to  
2 different parameters.

3           These, indeed, I'll show that the boundary  
4 conditions dominate the determination of downcomer  
5 temperature. And so as a consequence to that, we  
6 tried as diligently as possible to define specific  
7 transients that would capture the important variations  
8 through the boundary conditions.

9           And Mark showed some of these kind of  
10 plots yesterday. This is a spectrum of results that  
11 you get, in this case for Palisades, looking at the  
12 center of transients that constitute small-break LOCA.  
13 On the left is temperature and on the right is  
14 pressure.

15           You can see that for this category of  
16 events we are getting variations of at least 100  
17 degrees K, 180 degrees F. It's the difference in  
18 break size basically. We are getting variations for  
19 these kind of pressure variations from about 100 psi  
20 to 1,000 psi. There is quite a range of variations.

21           As you go to a medium break, I think as we  
22 have cited a number of times, these variations are  
23 thought to become smaller so now we are down to 75K  
24 and maybe a few PSI within the medium-break LOCA bin.  
25 And then when you get a large-break LOCA these

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1 variations essentially are gone. For the three  
2 plants we end up representing the large-break LOCA  
3 class of events by a single transient.

4 These results are from some of the  
5 University of Maryland sensitivity studies. This was  
6 done for 2.8 inch break LOCA which calls into the  
7 category of a small-break LOCA. They investigated 17  
8 different parameters and they effect they had. In  
9 this case they have chose as a figure of merit an  
10 average downcomer temperature over the duration of the  
11 transient.

12 You can see that if you fail all of HPI,  
13 of course, you get a big benefit. You see the most  
14 important benefits are from failing HPI which you  
15 might expect. Put it in cold water and it doesn't get  
16 so cold.

17 This is for Oconee. We did a sensitivity  
18 on hold the reactor vessel vent valves open. Remember  
19 that B&W plants have the potential for very large  
20 opening between the upper plenum and the downcomer to  
21 the vent valves.

22 In fact, this kind of effect is also  
23 present in CE and Westinghouse plants because when you  
24 add up the bypass area between the upper plenum and  
25 the downcomer, it is still substantial. It's about

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1 maybe, if I can recall correctly, it's about one-fifth  
2 of this area. It's equivalent to a hole of about one  
3 square foot.

4 What this does is it allows in-vessel  
5 circulation which has an effect on downcomer  
6 temperature and so you can see that the magnitude is  
7 effected. It's about 25 degrees K. Of course, things  
8 like varying the break between the hot leg and cold  
9 leg. We deal with set pump curves for, let's say, HPI  
10 flow and these have some uncertainties. We varied  
11 that.

12 We varied heat transfer coefficient  
13 throughout the total reactor cooling system and  
14 conditions of summer and winter. This affects the  
15 injection temperature to ECC and things like this is  
16 the pressure of the accumulators or core flood tanks.  
17 So these are various things we varied. You can see  
18 the effect in terms of a downcomer temperature.

19 When you go to medium-break LOCA you see  
20 these effects substantially decrease. For large-break  
21 LOCA they are almost nonexistence. That fits in with  
22 these kind of plots here. Many things can affect  
23 small-break LOCAs but, on the other hand, the CPFs for  
24 small breaks kind of fall off the bottom of the map.

25 That's it. There's one other thing I

1 probably should have pointed out yesterday. I just  
2 handed out two viewgraphs. I didn't keep a copy for  
3 myself. Basically when we varied -- when we put  
4 factors on the heat transfer coefficient we did that  
5 in a conservative sense in a way in that we simply  
6 took the RELAP output and put multipliers in the  
7 output so we didn't do it as integral calculation.

8 When you calculate heat flux, we submit  
9 three parameters to fracture people which is pressure,  
10 temperature, and heat transfer coefficient, but really  
11 there's only two real parameters. There's heat flux  
12 and pressure.

13 DR. WALLIS: You model the wall as well.  
14 You must model the wall.

15 MR. BESSETTE: We model the wall so we  
16 model all the -- we model the total conduction in the  
17 metal structures. It's interesting to note that we  
18 are solving this combined convection and conduction  
19 equation in RELAP. When you do that the wall  
20 temperature, which is the wall surface temperature,  
21 and the convective heat transfer coefficient are not  
22 independent parameters. If you look --

23 DR. RANSOM: You feed in the fluid  
24 temperature and the heat transfer coefficient to  
25 FAVOR. Right?

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1 MR. BESSETTE: Yes.

2 DR. RANSOM: And it does this calculation  
3 to get the  $(t)/dx$  at the surface.

4 MR. BESSETTE: Yes. So what I was  
5 pointing out is that when we put multipliers on heat  
6 transfer coefficient and add that to FAVOR, it's kind  
7 of a conservative way of doing it as opposed to  
8 actually making a change in the physical model.

9 DR. RANSOM: So from this relationship, of  
10 course, the  $(t)/dx$  is increased by whatever you  
11 increase the heat transfer coefficient by, the initial  
12 state.

13 MR. BESSETTE: Yes. You know, this  
14 interplay between these three factors, fluid  
15 temperature, wall temperature, and kind of backing out  
16 in the sense of the heat transfer coefficient.

17 DR. RANSOM: Incidentally, do you know  
18 what the biot number is for the heat transfer  
19 situation that you're in there?

20 MR. BESSETTE: Yes, it's for close  
21 diagnation conditions. It's about 10. On the order  
22 of 10.

23 DR. RANSOM: So it is convention dominated  
24 then, I guess. The biot number is  $HD$  over  $K$ .

25 MR. BESSETTE: Yeah.

1 DR. RANSOM: If it's 10, that means --

2 MR. BESSETTE: Convection dominated.  
3 You're right.

4 DR. WALLIS: But  $d$  is the wall thickness.

5 CHAIRMAN SHACK: But what are you using  
6 for the characteristic length?

7 MR. BESSETTE: Typically you use the total  
8 wall thickness. As you have seen from these plots the  
9 flaws that cause the vessel to fail are within the  
10 first half inch to inch. There are really sort of two  
11 characteristic lengths at play here. There's the  
12 characteristic length with the whole vessel wall  
13 thickness which determines the overall temperature  
14 profile, the one with the forier number.

15 It determines that temperature profile of  
16 the whole vessel from which you get the thermal  
17 stress, but then there's the more localized effect.  
18 What are the critical flaw sensing? Those were within  
19 the first half inch to an inch of depth. When you  
20 look at that, the biot number changes by a factor of  
21 10 so instead now it's about one. The process is not  
22 conducted and controlled anymore. Both convection and  
23 conduction come into play.

24 DR. WALLIS: We were told that for big  
25 breaks the wall governed? What you are suggesting is

1 maybe this is not quite so simple as that?

2 MR. BESSETTE: There is still the  
3 overwhelming effect. If you look at fluid temperature  
4 and heat transfer coefficient if you have an infinite  
5 heat transfer coefficient, then the wall surface is  
6 the same as the fluid temperature. The more you  
7 reduce the heat transfer coefficient, more of a delta  
8 T you have between the fluid and the wall.

9 If you look at the second viewgraph, you  
10 can see that kind of the delta T that you get as a  
11 function of heat transfer coefficient. Let's say at  
12 the low end of the range for 850 watt square meter you  
13 have a delta T between the fluid and the wall of about  
14 23 degrees C.

15 You say how far off could I be? Let's  
16 assume an infinite heat transfer coefficient you would  
17 be lowering your wall surface temperature by about 23  
18 degrees C. That in a sense is how much can heat  
19 transfer effect the answer is basically kind of  
20 equivalent to 23 degrees C change in fluid  
21 temperature.

22 DR. WALLIS: This is at a particular time?

23 MR. BESSETTE: Yes.

24 DR. WALLIS: I think if you wanted to  
25 demonstrate this, if you actually showed some

1 temperature profiles for different h's, as I think has  
2 been done before, it showed how these temperature  
3 profiles evolved with time for different h's and you  
4 would probably show that after awhile h doesn't  
5 matter. H matters in the beginning.

6 MR. BESSETTE: Yeah, I could do that. I  
7 think I've got it some place upstairs. I just don't  
8 have it here.

9 DR. WALLIS: It would be interesting to  
10 know what the Tf-T wall initial. You know, how taunt  
11 was the wall to start out with so that you see the  
12 maximum delta T and how relative this difference is  
13 compared to what it was at the initial beginning of  
14 the transfer.

15 MR. BESSETTE: Well, your initial wall  
16 temperature is the same as your cold leg temperature.  
17 It's about 545 degrees F. You don't start cooling  
18 down the wall until you get the flow stagnation  
19 conditions.

20 DR. RANSOM: So you get some fluid there,  
21 I guess.

22 MR. BESSETTE: Yes. In effect you don't  
23 start the PTS transient until you reach flow  
24 stagnation for a LOCA. But you can see on the second  
25 page you can change heat transfer coefficient by a

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1 factor of 10 and you are only changing heat flux by 20  
2 percent. It's heat flux that really matters rather  
3 than the two parameters individually.

4 DR. RANSOM: Is this  $dT(t)/dx$  is governed  
5 by?

6 MR. BESSETTE: Yes.

7 DR. WALLIS: It all has an interplay with  
8 the flaw distribution and as you are talking about  
9 that small dimension. If all the action is in a very  
10 thin layer near the surface, then  $h$  is very  
11 important. If the action is an inch from the surface,  
12 then  $k$  is much more important. Actually, I think as  
13 we saw earlier, that is the region where all of the  
14 flaws are sort of important but they are most  
15 important near the surface.

16 MR. BESSETTE: I think the reason the  
17 surface -- one of the reasons that surface flaws show  
18 up this way is because of this near-surface metal  
19 experiences that temperature more quickly than the  
20 deeper so it's going to have the lowest -- at any  
21 given time it's going to have the lowest fracture  
22 toughness.

23 DR. WALLIS: So what's the sensitivity of  
24 the overall result to this  $h$ ?

25 MR. BESSETTE: Well, I showed you some

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1 numbers yesterday. It turns out to be factors of two  
2 or three.

3 DR. WALLIS: So it's not insignificant.

4 MR. BESSETTE: Not insignificant but I  
5 think Mark was showing other examples of change this  
6 as factor of three, change that as factor of three.

7 MR. ERICKSONKIRK: We'll have a summary at  
8 the end.

9 DR. WALLIS: But if this  $h$  were bigger  
10 because of the stirring we talked about yesterday,  
11 bigger than Dittus-Boelter because of this mixing,  $h$   
12 would be bigger and that would be a bigger challenge  
13 to the wall.

14 MR. BESSETTE: Yes.

15 DR. WALLIS: The factor of two or three  
16 would be up.

17 MR. BESSETTE: But, you know, we expect  $h$   
18 -- I mean, we calculate  $h$  to be around 1,700.

19 DR. WALLIS: This is for Dittus-Boelter?

20 MR. BESSETTE: This is actually from  
21 Churchill-Chu or Ivan. When you get down to that  
22 range, Churchill-Chu gives you a higher coefficient  
23 than Dittus-Boelter and RELAP will look at both and  
24 choose the --

25 DR. WALLIS: The factor of two is not true

1 convection with the off fluid going up near the wall.

2 MR. BESSETTE: It chooses whatever gives  
3 the higher number. As you can see, of course, how the  
4 T wall minus T fluid drops as you go off at h. Where  
5 you get to 10,000 it's down to about 2.5 degrees C.

6 DR. WALLIS: Churchill-Chu TWISTF actually  
7 feeds back to h.

8 MR. BESSETTE: Excuse me?

9 DR. WALLIS: TWISTF is in the Grashoff  
10 number which affects h.

11 MR. BESSETTE: Yes.

12 CHAIRMAN SHACK: We're going to have to  
13 move on.

14 MR. BESSETTE: That's basically it for me.

15 MR. NOURBAKHSH: Just one comment. There  
16 was a CSNI report that the US participated, too.  
17 There was comprised wall heat transfer coefficient for  
18 different participating countries for a medium-break  
19 LOCA. Have you looked at that and see what is the  
20 range of heat transfer there? There was Frenchman  
21 doing CFD calculation to get that heat transfer  
22 coefficient. I don't know if they were successful or  
23 not.

24 MR. BESSETTE: In fact, that's what  
25 motivated us to do this sensitivity study. There's

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1       been controversy going back to the early '80s about  
2       whether the heat transfer coefficient is important or  
3       not. If you just do a biot number analysis, you say,  
4       well, it's not important.

5               But then you start looking at actual FAVOR  
6       calculations and you say sometimes it's not  
7       insignificant. What's the true story? The true story  
8       is that you are really dealing with two characteristic  
9       lengths and two characteristic times when you look at  
10      this.

11              One which is the whole vessel thickness  
12      and one which is where the flaws are that cause your  
13      problems. That's really the source for this people  
14      talking across purposes, is it important or not.

15              MR. NOURBAKSH: But they offer different  
16      heat transfer coefficient. There were some that they  
17      were using upper plenum test facilities to come up  
18      with this transfer coefficient.

19              MR. BESSETTE: Yes. Some people in that  
20      study suggested that you don't become -- you have to  
21      consider heat transfer coefficients up to 10,000 and  
22      so that's how we picked this range is from that  
23      exercise.

24              CHAIRMAN SHACK: One of the troubles is,  
25      of course, you stop here. You have to integrate this

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1 whole thing out to the end. It's very difficult at  
2 any point in here to know how this really affects the  
3 final result.

4 MR. BESSETTE: That's what I was saying.  
5 Of course, that's why you have to have a deterministic  
6 thermal hydraulics input into FAVOR is because the  
7 whole time/temperature history is what matters. Is  
8 that what you were saying?

9 CHAIRMAN SHACK: Well, all I want to know  
10 is you tell me if it's important or not important.  
11 It's not important or important until I see what it  
12 does to the vessel failure frequency.

13 MR. BESSETTE: Well, that's true. That's  
14 why I said yesterday when you look at thermal  
15 hydraulics results you can't look at them by  
16 themselves. You only know what is important or not  
17 important after you run them through FAVOR.

18 MR. NOURBAKSH: Now, if you put 10,000  
19 heat transfer coefficient, how much do you think the  
20 frequency of through-wall crack is going to change?  
21 How important is this?

22 MR. BESSETTE: I don't know. I know  
23 there's a -- I'm going to try to find the study that  
24 Terry did about seven years ago along these lines and  
25 get you that answer.

1 CHAIRMAN SHACK: That's like putting a  
2 factor of five on your current heat transfer  
3 coefficient roughly. Right?

4 MR. BESSETTE: Yes. Yeah.

5 CHAIRMAN SHACK: It would be big.

6 MR. BESSETTE: It goes up.

7 CHAIRMAN SHACK: CPF does go up.

8 MR. BESSETTE: Two gave them an order of  
9 magnitude from the '97 study. Is that what I recall  
10 from yesterday?

11 CHAIRMAN SHACK: I think we're going to  
12 have to move on.

13 DR. RANSOM: Just one comment, though.  
14 You went through these sensitivity studies and I guess  
15 these were not carried out over into FAVOR. Is that  
16 right?

17 MR. BESSETTE: Well, they were --

18 DR. RANSOM: To find out what the through-  
19 wall cracking frequency and how it is affected by  
20 these changes?

21 MR. BESSETTE: They were, in fact. Well,  
22 the way they were carried forth in the FAVOR was we  
23 did sensitivity studies to characterize a range of  
24 behavior that constituted small-break LOCAs. And then  
25 from that we picked five individual RELAP runs that

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1 attempted to reasonably cover this range of behavior.

2 We fed those five runs into FAVOR and said  
3 we have to give you a deterministic input but in order  
4 to characterize a range of uncertainty as to what is  
5 a small-break LOCA, we give you five runs with  
6 associated probabilities.

7 DR. RANSOM: I'm just wondering why you  
8 didn't plot through-wall cracking frequency instead of  
9 just the temperature difference for these sensitivity  
10 studies. That would have answered a lot of these  
11 questions.

12 MR. BESSETTE: We do have that kind of  
13 information. It's in the report from the University  
14 of Maryland.

15 DR. WALLIS: I think the message should be  
16 the next time you present that. Next time you should  
17 present that. The same way that Mark presents the  
18 effect of everything on through-wall cracking, you  
19 should do it, too. Or you should get together and do  
20 it or something.

21 MR. BESSETTE: There's a chapter in the  
22 report from the University of Maryland which --

23 CHAIRMAN SHACK: But that is the way you  
24 do the calculations so you've got five bins for small-  
25 break LOCAs and all the small-break LOCA sequences are

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1 dumped into one of those five bins.

2 MR. BESSETTE: Yes.

3 CHAIRMAN SHACK: You've got how many bins  
4 for medium-break LOCAs?

5 MR. BESSETTE: Three.

6 CHAIRMAN SHACK: Three. And one bin for  
7 large-break LOCAs.

8 MR. BESSETTE: Yes.

9 CHAIRMAN SHACK: And so you hand them one  
10 of those transients depending on which bin he happens  
11 to be sampling from.

12 MR. BESSETTE: Yeah, so we hand them for  
13 small-break LOCAs. We say here's five transients. We  
14 subdivide the small-break LOCA probability five ways.  
15 Each of these five is run through FAVOR and in the end  
16 we sum all these up and get a total failure  
17 probability.

18 CHAIRMAN SHACK: But within the bin  
19 there's only transient history that you hand them?

20 MR. BESSETTE: Yeah. It's necessary to  
21 hand them results from individual RELAP calculations  
22 and to represent things by multiple RELAP calculations  
23 instead of some sort of -- the multiple RELAP  
24 calculations represent a probability of distribution  
25 or probability of consequence distribution.

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1 CHAIRMAN SHACK: I think we're going to  
2 have to move on.

3 MR. ERICKSONKIRK: What do you want to do  
4 next, PFM or PRA comments?

5 CHAIRMAN SHACK: PRA. Well, let's do PFM.

6 MR. BESSETTE: I should just maybe say one  
7 more thing. The final comments we got from the Peer  
8 Review group were essentially the same as the initial  
9 comments which we always find. I think we have  
10 adequate answers to all the comments.

11 CHAIRMAN SHACK: Does that mean they asked  
12 them again because your answers weren't satisfactory  
13 or was there a miscommunication?

14 MR. BESSETTE: Well, they certainly were  
15 happy the last time we met with them but it was just  
16 basically kind of a rehash of what their initial  
17 concerns were.

18 DR. WALLIS: Can you tell if we're happy  
19 or not?

20 MR. BESSETTE: I'm never sure of that.

21 MR. ROSEN: What is the significance of  
22 the penguin?

23 MR. ERICKSONKIRK: That's our project  
24 mascot that was bought by John Kneeland at Palisades.  
25 He travels with the project so I have every hope that

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1 soon we will sign this set of reports out of research  
2 and I'm going to carry the whole stack of reports  
3 along with the penguin over to Matt Mitchell's office  
4 in NRR and we're just going to dump it. That was  
5 probably more than you wanted to know.

6 CHAIRMAN SHACK: I thought you were doing  
7 it all in Linux? Linux uses the Penguin.

8 MR. ERICKSONKIRK: Oh, no. Okay.  
9 Obviously there were lots and lots of individual  
10 comments. I have divided them up into several  
11 categories. Comments that led to model changes,  
12 changes in the FAVOR model we discussed yesterday,  
13 those being the addition of crack face pressure and  
14 the upper shelf model. I think I've got a slide on  
15 that that I'm just going to pass because we have  
16 beaten that one.

17 Major comments of clarification bring up.  
18 I'll skip on the minor comments and clarification.  
19 Many of the reviewers made comments pertinent to  
20 rulemaking which are generally not discussed here  
21 unless they got into the new comments, in which case  
22 I will bring them up.

23 We talked about this. We made two model  
24 changes in response to Peer Reviewers' comments and  
25 major comments of clarification. I've got three

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1 reviewers here. Schultz and VanWalle were the PFM  
2 reviewers and then Murley also made some comments on  
3 PFM so we have addressed those as well.

4 Questioned the applicability of the flaw  
5 distribution to PWRs in general and suggest that  
6 operators should be required to demonstrate that the  
7 flaw distribution is appropriate perhaps by linking  
8 use of the rule to ISI.

9 Said something about a potential  
10 correlation between flaw and chemistry variables.  
11 Right now the flaw distribution and chemistry are  
12 independently sampled. However, admitted that he had  
13 no credible basis for such a correlation so we didn't  
14 do anything with that.

15 Questioned our ability to accurately  
16 predict multiple run-arrest events. That led to a  
17 long interchange of e-mails between Dr. Schultz and  
18 Richard Bass and Claud Pugh at Oak Ridge National  
19 Laboratory that finally resulted in we felt that our  
20 basis was adequately demonstrated in Appendix E which  
21 referenced the Oak Ridge pressurized thermal shock and  
22 thermal shock experiments. However, this reappeared  
23 in Dr. Schultz' final comments and I don't think we  
24 have convinced him yet.

25 Crack face pressure we've already talked

1 about. There was a question and misunderstanding  
2 regarding the use of the Reg Guide 1.99, Rev. 2  
3 attenuation function which we clarified and resolved.

4 For Dr. VanWalle question the  
5 applicability of the results to PWRs in general which  
6 was addressed in Chapter 9, I believe, to his  
7 satisfaction. Questioned the mathematical treatment  
8 of mixed uncertainties and basically the need to treat  
9 something as either fully aleatory or fully epistemic.  
10 Expressed dissatisfaction that there wasn't a  
11 mathematical procedure to treat that but accepted that  
12 was the current state of practice.

13 Questioned in our crack initiation and arrest model  
14 the point where we -- I'm sorry. I'm losing it --  
15 where we don't sample the apparent uncertainty on the  
16 embrittlement trend and Charpy toughness shift models.  
17 We shared with Dr. VanWalle the same information we  
18 shared with you today.

19 I think generally we were less successful  
20 in convincing him than convincing you if I judged the  
21 head nods appropriately so that's a residual point of  
22 disagreement. I think he still believes that we  
23 should be sampling on both chemistry uncertainty and  
24 on model uncertainty.

25 Regarding the upper-shelf model he pointed

1 out the inaccuracy of our then correlative approach,  
2 suggest a model change, and we changed it and I  
3 briefed you on that today. Questioned how we wired an  
4 interdependence of  $K_{Ic}$  and  $K_{Ia}$  and the through-wall  
5 cracking calculations.

6 When we perform a through-wall cracking  
7 calculation, once the crack initiates, we then, of  
8 course, have to simulate a value of  $K_{Ia}$ . We don't  
9 allow FAVOR to simulate a value of  $K_{Ia}$  that would  
10 immediately stop the crack. We allow the crack to  
11 keep propagating.

12 He questioned and we provided in our  
13 write-up of what we believe is a physical rationale  
14 supporting that model. He questioned whether that was  
15 appropriate. However, we haven't really done anything  
16 with that because relative to the alternative of  
17 having no correlation between the two, our model is  
18 conservative whether you believe our physical  
19 reasoning or not. He brought up the question of the  
20 composition grading and copper for welds and we showed  
21 you the results of the sensitivity study on that.

22 For Dr. Murley, he asked that we perform  
23 deterministic calculations of through-wall cracking to  
24 illustrate the various parts of the model which we  
25 have included in Appendix F of the report. That was

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1 it for him.

2 Okay. So summary. I'm sorry I animated  
3 all this. It makes it hard to go through. So in  
4 general -- I'm not going to read this to you. In  
5 general each of these three reviewers had, I think,  
6 positive things to say about the project overall and  
7 the PFM model in particular.

8 They all had some remaining issues with  
9 regard to Dr. Schultz. It had to do with the comment  
10 that he wasn't fully satisfied. That the flaw  
11 distribution applied to all plants and thought that  
12 there should be some obligation on the part of the  
13 licensee to demonstrate that the flaw distribution was  
14 appropriate.

15 He also has a remaining issue about the  
16 appropriateness of our crack initiation arrest, run  
17 arrest model. Again, finish with the recommendation  
18 that licensees should be required to demonstrate the  
19 appropriateness of the assumed flaw distribution to  
20 their vessels.

21 Dr. VanWalle, again, some nice comments in  
22 general. However, some particular remaining issues  
23 that we've noted. He made several recommendations.  
24 The continuation of in-service inspection to  
25 substantiate the applicability of flaw distribution to

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1 all PWRs. In fact, he made a stronger comment that he  
2 felt that in-service inspection should be a  
3 prerequisite to using any rule that develops our of  
4 these calculations.

5 Over time he recommended the evolution  
6 towards the direct use of fracture toughness  
7 measurements made on surveillance specimens instead of  
8 our current correlative approach using Charpys and  
9  $RT_{ndt}$ .

10 DR. WALLIS: On this mixed uncertainty  
11 thing that was one of my comments before. You seemed  
12 to have removed some epistemic uncertainty but it  
13 didn't seem to remove the uncertainties.  
14 Uncertainties still seem to be the same. Then you  
15 have aleatory uncertainty.

16 It looked to me as if you were somehow  
17 double counting. I haven't looked at it since then.  
18 This was over a year ago. I had some problems with  
19 the way you treated these mixed uncertainties. I  
20 wondered if you weren't actually doing some double  
21 counting along the way. That's just my memory of it.

22 MR. ERICKSONKIRK: That's the final  
23 comment. Recommended continued/further validation of  
24 indeed both the crack arrest models and the upper  
25 shelf models by both further research to understand

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1 the physical mechanisms and further collection of  
2 data.

3 Again, Murley -- finally, Murley,  
4 generally positive comments. Said he had some  
5 concerns that wouldn't rise to the level to seriously  
6 challenge the logic of the overall approach.

7 Okay. I'm sorry. Final comments  
8 continued. And he had some -- in the case of Murley  
9 he had some what I would consider new things that he  
10 raised in his final letter. Here I have only  
11 attempted to summarize the ones that pertain to PFM.

12 There were some things where clearly we  
13 had some errors in understanding which we have taken  
14 as indications that our documentation isn't well  
15 enough explained and we will be endeavoring to explain  
16 it better so those understandings won't re-arise and  
17 we will be communicating with him on that.

18 He also pointed out that we needed a more  
19 thorough discussion of what he called the residual  
20 uncertainties both conservative and nonconservative  
21 that underlie our proposed screening limits. He  
22 further commented that the discussion would serve as  
23 a basis for decision makers in terms of whether our  
24 existing screening limits could be used without the  
25 need of an additional margin term if a margin term

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1 would be needed.

2 We have tried to as best we can in the  
3 two-day's time since we've had the comment to make a  
4 more balanced approach on that. I showed that in the  
5 intro and I'll show it again if I get a chance to do  
6 the screening criteria presentation. We thought that  
7 comment was appropriate.

8 I think -- also, commented on the  
9 applicability for the flaw distribution of all PWRs.  
10 That's the one comment that cuts across all the  
11 reviewers and I think the general comment was, "Yes,  
12 we understand you have more information than before.

13 Yes, we understand that you don't have a  
14 really credible basis to modify this but you need to  
15 do something in terms of continual monitoring to make  
16 sure you are in future applications of this rule and  
17 you are validating that your assumptions continue to  
18 be correct where you're applying it.

19 DR. KRESS: Is that possible? I mean, you  
20 have to take each of these vessels and determine some  
21 sort of flaw distribution.

22 MR. ERICKSONKIRK: Well, in-service  
23 inspection is now a requirement.

24 CHAIRMAN SHACK: Yes, but not with SAFT.

25 MR. ERICKSONKIRK: No. No. And I'll

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1 happily leave that to rulemaking but I think the  
2 general comment is there that just like continued  
3 surveillance is a good idea, monitoring or somehow  
4 further understanding documentation, demonstration of  
5 the flaw populations that we are trying to apply this  
6 to, again, a general comment across reviewers, and I  
7 think it's fair to say a general comment around this  
8 table, that is something that is a prudent step.

9 DR. KRESS: If you could do it.

10 MR. SIEBER: Well, with 80,000 flaws per  
11 cubic meter, I don't think ISI is going to show all  
12 this.

13 DR. KRESS: You may have covered it  
14 already.

15 CHAIRMAN SHACK: Well, that's just under  
16 clad cracking.

17 MR. SIEBER: That's right. That's just  
18 one kind of flaw.

19 CHAIRMAN SHACK: The least important.

20 MR. SIEBER: But it's there.

21 MR. ERICKSONKIRK: I believe that's it.

22 DR. WALLIS: But you're inspecting the  
23 clad, too, so that's easier to do.

24 MR. HISER: I think I would differ with  
25 that. There really is no clad inspection in the ISI.

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1 DR. WALLIS: So if there is a cracking of  
2 that you wouldn't know it?

3 MR. DENNING: Do we want to pass on that  
4 and move on? If we're running out of time, I don't  
5 think this is a high value area.

6 CHAIRMAN SHACK: Oh. You want to just  
7 pass on the --

8 MR. DENNING: Yeah. I mean, if we're  
9 running out of time.

10 CHAIRMAN SHACK: We are running out of  
11 time.

12 MR. ROSEN: Why do you want to pass on  
13 this?

14 MR. DENNING: It seems to me it's  
15 comparatively clean.

16 MR. ROSEN: Murley makes a couple of  
17 interesting comments. I would like to hear what the  
18 staff things about it.

19 MR. ERICKSONKIRK: Okay. Back to that.

20 CHAIRMAN SHACK: Let's go through it  
21 quickly then.

22 MR. ERICKSONKIRK: Okay. I've got to get  
23 Donnie. Maybe just do the really pretentious ones.

24 MR. ROSEN: There's only two, I think, in  
25 the PRA area with Murley.

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1 MR. WHITEHEAD: Okay. What I'll try to do  
2 is I'll just try to hit the high points on the  
3 comments. Probably the one that's caused some  
4 discussion is the discussion about the Rancho Seco  
5 event. We had a comment from Dr. Murley about that  
6 and he wanted to know basically what's changed and why  
7 does this one not show up to be particularly important  
8 in the current analysis.

9 There's a short description of what the  
10 event is. I'm sure that you are familiar with that so  
11 what I'll do is I'll go to the reply here. Basically  
12 what's happened is that we've had substantial  
13 improvements in the equipment that failed in the  
14 initial event.

15 We've had redesign of control room  
16 indications. We have better operator training and  
17 procedures. We have more emphasis on overcooling  
18 events. The fracture mechanics calculations that we  
19 currently have basically show that this type of event  
20 is not particularly all that important.

21 Basically what happens is taking all of  
22 these things together the Rancho Seco event would be  
23 substantially less important in today's world as we  
24 know it with the information that we have than it was  
25 at least initially with the calculation that we done

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1 at the time that the event occurred. That's not to  
2 say that this event is impossible.

3 It's just that all of the things that have  
4 been changed, the modifications to the control rooms,  
5 the operator training, so forth and so on, have tended  
6 to reduce the frequency of such an occurrence and also  
7 the fracture mechanics calculations that we would do  
8 in today's world would show this would be less  
9 important.

10 We demonstrated that by taking a look at  
11 sequences that were similar in the analyses that we  
12 did for Oconee. We identified reactor turbine trip.  
13 One or two stuck open relief valves on the steam  
14 generators which we believe to be a little bit worse  
15 than the Rancho Seco event.

16 We had continued flow to the steam  
17 generators and we had high-pressure injection until we  
18 reached the shutoff point from the pressurizer safety  
19 relief valves. With the current fracture mechanics  
20 calculation even using an extremely artificially high  
21 EFPY for Oconee of 1,000 years, we basically find that  
22 there is zero estimate for this particular event that  
23 we did for Oconee.

24 Because we think that this event is at  
25 least as bad as what the Rancho Seco event would have

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1       been, we conclude that these types of events are just  
2       simply not particularly all that important. They can  
3       still happen but they are just not going to be  
4       important.

5                       We had a comment from Dr. Murley on  
6       external events and we went through an external event  
7       presentation here. We believe that the bounding  
8       analyses that we did is acceptable from the point of  
9       view that even if you use the results from the  
10      bounding analyses, which we believe to be very high,  
11      it would still not alter our ability to relax the PTS  
12      rule.

13                      We had a question about external flooding  
14      of the reactor pressure vessel. The analyses that we  
15      looked at have dealt with internal cooling of the  
16      reactor vessel. Basically what happens is that an  
17      external cooling of the reactor vessel wouldn't really  
18      be any worse than what we have for our main steam line  
19      break. Main steam line breaks are not particularly  
20      all that important so we think that we would be okay.

21                      There are several comments that deal with  
22      the use of LERF. I believe that was adequately  
23      addressed yesterday by Nathan's presentation. What  
24      would happen with our oxidation and so forth and so  
25      on. I'll skip that one.

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1                   We had a question about the use of the  
2 value that we use for adjusting the frequencies for  
3 hot zero power. Basically what I presented yesterday  
4 was that the real value that we used -- the value that  
5 we used in our calculation was two percent of the time  
6 per year the information from the plant suggest that  
7 it's really between one to 1.5 percent so rounding it  
8 up to two percent allows us to bound any of the near  
9 hot zero power transition states as well.

10                   We had several comments that dealt with  
11 the issue of what kinds of regulatory guidelines that  
12 would be needed to ensure that utility calculations,  
13 if they were needed, what kind of standards that  
14 should be met. It's really not a researcher's role to  
15 address those. That would obviously be part of any  
16 rulemaking that took place so we'll just leave that  
17 one.

18                   There were comments on the use of Reg.  
19 Guide 174 for formulating -- let's see. Okay. There  
20 were several comments that dealt with the issue of  
21 what the utilities would be required to submit in any  
22 type of analyses that they had to do. Again, that's  
23 a rulemaking issue and we decided that it was more  
24 appropriate for NRR to deal with those issues since  
25 that is their purview.

1           There was a question as to what kind of  
2           standard the actual analyses done by the NRC what they  
3           were done to. While our analyses started before the  
4           issuance of the ASME standard for full power PRA  
5           calculations, the people involved in actually doing  
6           the PRA were aware of the things being discussed as  
7           part of the development of the standard.

8           While we never have gone back and actually  
9           done a one-to-one check to make sure that all of the  
10          requirements in the standard have been met, we have  
11          done a review of those and we believe that we have met  
12          the intent of what the standard was trying to get at  
13          as to how to actually do a PRA calculation for power  
14          analyses.

15          And we had a comment on the justification  
16          for the 3,000 and 6,000 second timing of the reclosure  
17          of the valve and that has been discussed yesterday and  
18          actually today and so we think that we have bounded  
19          the results that you would get from such a  
20          calculation.

21                           That's basically the comments that we had.

22                           MR. ROSEN: Have you read the new comments  
23                           by Murley?

24                           MR. WHITEHEAD: I got those yesterday and  
25                           I just skimmed over them.

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1 MR. ROSEN: Okay. Well, I don't know if  
2 it's fair to ask you about his second comment about  
3 the possible valve reclosure times greater than 6,000  
4 seconds.

5 MR. WHITEHEAD: Okay.

6 MR. ROSEN: And whether or not your  
7 analysis would -- if you used it and did it beyond  
8 6,000 second, whether that would yield even more.

9 MR. ERICKSONKIRK: Donnie.

10 MR. WHITEHEAD: Yes.

11 MR. ERICKSONKIRK: I would like a cut at  
12 it after you get done.

13 MR. WHITEHEAD: This issue has been  
14 presented -- I mean, Mark made a presentation on this  
15 where we did actually go back and do a sensitivity  
16 calculation varying the time at which the valve  
17 reclosed from 3,000 seconds to somewhere around 14,000  
18 seconds.

19 The curve here gives you an indication  
20 that initially we have a very steep rise in the CPI  
21 and CPF for valve reclosure. It maxes out somewhere  
22 in the vicinity of about 8,000 seconds or something  
23 like that. However, you have to remember that at very  
24 long time frames the operators would have been  
25 transitioning from their initial procedures into ones

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1 where they are actually beginning to shut the plant  
2 down.

3 I believe a slide that we had yesterday  
4 indicated that if you look at it, what we've captured  
5 is the very steep part of the curve as it's going up  
6 so I believe that we are pretty confident that, you  
7 know, we have captured the vast majority of what the  
8 response would be. You also have to remember that we  
9 are choosing two single points in time to represent  
10 the fact that in reality a valve could close at any  
11 point in time. It could close very early in the  
12 event.

13 If it does, then the CPIs are going to be  
14 very small. If it closes really late in the event,  
15 as we indicated here, the values are also going to be  
16 going back down. We believe that we realistically  
17 captured the worse that the thing could be.

18 MR. ERICKSONKIRK: May I add? I guess the  
19 things I would add to that is that Dr. Murley seemed  
20 to get focused on the fact that we hadn't picked the  
21 absolute peak of the curve. And to amplify on what  
22 Donnie said, the important thing is that we capture  
23 the whole curve and so, yes, sometimes perhaps the  
24 valve will reclose later in which case we would be  
25 underestimating. It's also equally probable that it

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1 could reclose really early in which case we are way  
2 over estimating the probabilities of failure.

3 The graph I prepared yesterday attempted  
4 to illustrate that what we are trying to pick is not  
5 really a maximum or any single point on this curve but  
6 we are attempting to essentially get the area under  
7 the curve right. We feel like we've done it.

8 The other point I would like to make is in  
9 reading the final draft comments from the reviewers.  
10 If you read Dr. Johnson's comments, his view on this  
11 is that we have a gross over-conservatism in our model  
12 because it's his viewpoint that if the valve is going  
13 to reclose, it's likely to reclose very early so he  
14 questions where we get off with these very long  
15 reclosure times c I think in looking at the Peer  
16 Review group comments, you know, we need to look at  
17 all of them and clearly there is a point of  
18 disagreement in what we've done here.

19 MR. ROSEN: I think I understand your  
20 points from the analytical point of view but I think  
21 from an operational point of view this chart has  
22 important ramifications and needs to be communicated  
23 well to those people who are trying to write new  
24 procedures under a new rule.

25 MR. ERICKSONKIRK: Yes.

1 CHAIRMAN SHACK: Are you going to go onto  
2 screening criteria now, Mark?

3 MR. ERICKSONKIRK: Yes. Okay. So what  
4 we're trying to do here is to use all this  
5 information, put it together and see if we can come up  
6 with a new screening criteria to suggest NRR  
7 replacement for the current screening criteria of 270  
8 and 300 F on RT<sub>ndt</sub>.

9 DR. WALLIS: "Criteria" is a plural?

10 MR. ERICKSONKIRK: I've never been clear  
11 on that.

12 DR. WALLIS: And "criterion" is singular?

13 MR. ERICKSONKIRK: Yes, you're right.  
14 Grammar isn't my thing.

15 CHAIRMAN SHACK: We noticed that.

16 MR. ERICKSONKIRK: Yes, I'm sure you have.

17 MR. SIEBER: It's equal to your spelling.

18 MR. ERICKSONKIRK: That's right. Okay.  
19 So by way of introduction and summary of where we've  
20 been subject to limited equivocation, the TWCF values  
21 we have from the detailed study plans we believe do  
22 apply to PWRs in general and that general  
23 applicability would then support the development of a  
24 materials based screening criteria on the basis of our  
25 analysis.

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1                   We've used reference temperature  
2 definitions to characterize the through-wall cracking  
3 frequency of different flaw populations. The reason  
4 we do that is we get better correlations of through-  
5 wall cracking frequency when we use the reference  
6 temperature that characterizes toughness at the  
7 locations where the flaws are.

8                   So we have developed through-wall cracking  
9 frequency correlations versus RT correlations for the  
10 study plants who then use those correlations to  
11 estimate through-wall cracking frequency and use that  
12 relationship to figure out what the screening limit  
13 would be associated with a 1E-6 LERF limit. Then we  
14 also compare those proposed limits to calculated  
15 values of the screening limits for all of the  
16 operating PWRs.

17                   So we've been through this before. This  
18 is just to say that we need to pick the reference  
19 temperature to characterize material toughness where  
20 the flaws are. We know where the flaws are in the  
21 vessels so we can calculate the locations and these  
22 are the formulas that we would use.

23                   I think the main thing to point out is  
24 that these formulas can be applied to calculate values  
25 of these various reference temperatures based only on

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1 the information that is currently in the RVID database  
2 that's maintained by the NRC and based on a diagram of  
3 the plant showing the locations of the welds and the  
4 plates.

5 DR. RANSOM: Are these values plant  
6 specific then?

7 MR. ERICKSONKIRK: Yes, they are plant  
8 specific as they are currently. Every plant has its  
9 own  $RT_{ndt}$  value so then every plant would have its own  
10 RT axial weld plate and circ weld. This is a graph  
11 that you've seen several times which shows the  
12 relationship between the reference temperatures at the  
13 various flaw location with the through-wall cracking  
14 frequency arising from flaws at those locations.

15 Again, axial weld flaws are the things  
16 that drive the through-wall cracking frequency. Plate  
17 flaws make some contribution at higher embrittlement  
18 levels. Circ weld flaws while they make a  
19 contribution if you look at the relative effects, it's  
20 very, very minor.

21 So taking those -- I'm sorry. Taking the  
22 fits of those lines which is just a simple exponential  
23 fit through the available data, we then estimate the  
24 total through-wall cracking frequency is the sum of  
25 the three constituent parts with only the minor

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1 modification that we multiply the through-wall  
2 cracking frequency due to plate flaws by a factor of  
3 two.

4 The reason that we did that is that just  
5 because of the way the fit was the Beaver Valley  
6 plant, where the plate flaws make a contribution, and  
7 because we had a lower contribution in Palisades,  
8 Beaver Valley was systematically being under predicted  
9 so we put in the factor of two.

10 Then this graph just shows the  
11 relationship between the FAVOR predicted values of  
12 through-wall cracking frequency and the fit values of  
13 through-wall cracking frequency by this equation.

14 Okay. So now we can use the equation to  
15 figure out what combination of these various RT values  
16 are either below or above any risk limit you want so  
17 we'll set the limit at  $1 \times 10^{-6}$  and we can do that.  
18 Take, for example, for an axial weld -- I'm sorry,  
19 plate plant with axial welds.

20 There's a circ weld contribution but, as  
21 we said before, it's small so just set this to a value  
22 that's above any value that you expect to reach. Say  
23 300. It doesn't factor in enough to matter. I'm  
24 sorry, reference temperature circ weld to 300 and that  
25 gives you a very small number out here.

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1           You know that the through-wall cracking  
2 frequency total is  $1 \times 10^{-6}$  so stick in  $1 \times 10^{-6}$  there and  
3 now set it up on a spread sheet to scroll through a  
4 whole bunch of values for RT axial weld and figure out  
5 what the RT plate needs to be to get you up to  $1 \times 10^{-6}$   
6 or any value that you pick and you wind up carving out  
7 curves of constant through-wall cracking frequency.  
8 The interpretation of this is that -- and I  
9 highlighted the  $1 \times 10^{-6}$  limit in red because that's the  
10 value that's been proposed as consistent with Reg.  
11 Guide 1.174 guidance on LERF.           The way a  
12 plant would use this would be to say, okay, I need to  
13 calculate -- if I have a plate welded plant I  
14 calculate RT axial weld and RT plate for my plant and  
15 I put a dot on this diagram. If I'm inside the red  
16 curve, lift is good. If I'm outside the red curve,  
17 I'm going to pay a lot of money to consultants to  
18 figure out how to move the point inside the red curve.

19           Similarly, with forging plants except they  
20 don't need to bother with RTAW. They just calculate  
21 RT circ weld which given that the asymptotic limit is  
22 over 450 degrees nobody is every going to hit so they  
23 just need to worry about the RT plate value.

24           Again, the yellow box at the bottom there  
25 are certain provisos to this regarding forging at very

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1 high levels of embrittlement and applicability to  
2 thick vessels which we've noted.

3 So what we did is we took the information  
4 in RVID. We didn't have the resources to go get the  
5 diagrams of how the welds are oriented so we took a  
6 conservative approach estimating the RT axial weld  
7 values and just calculated the value of RTAW for each  
8 of the axial weld fusion lines and forgot any length  
9 averaging and just used the maximum value.

10 That's where the points lie at end of  
11 license. You see none of the plants are very close at  
12 all to the failure of loci and, as we discussed  
13 yesterday, and I don't have this plot here but it's in  
14 the report, if I crank this up to end of license  
15 extension, the values move out by between 10 and 20  
16 degrees but still not closely approaching the  $1 \times 10^{-6}$   
17 limit. Of course, in general forgings are further  
18 from the limits than are the plate welded vessels.

19 MR. SIEBER: Who's the plant that is  
20 furthest out?

21 MR. ERICKSONKIRK: You know, I knew that  
22 and I --

23 CHAIRMAN SHACK: Indian Point 3.

24 MR. ERICKSONKIRK: Indian Point 3. Thank  
25 you.

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1 CHAIRMAN SHACK: River Keeper will be glad  
2 to know that.

3 MR. ERICKSONKIRK: What's that?

4 CHAIRMAN SHACK: River keeper will be glad  
5 to know that.

6 MR. ERICKSONKIRK: I've X'ed this out in  
7 response to Murley's comment and I've got a whole  
8 other slide looking at conservatisms and  
9 nonconservatisms. Just to look at the status of  
10 operating plants relative to this proposal, we find  
11 that all PWRs are in order of magnitude or more away  
12 from the  $1 \times 10^{-6}$  limit, that being controlled only by  
13 this plant over here.

14 By in large they are several orders of  
15 magnitude away from the limit. There is at least 60  
16 degrees fahrenheit and usually much much more that  
17 separate any PWR from the limit.

18 You can compare that to the situation  
19 we're in today where the limiting plants are within  
20 fractional degree fahrenheit from the 270 and 300 F  
21 limit. As I noted before, if we extend this  
22 evaluation out to EOLE, all the plants move between 10  
23 and 20 degrees fahrenheit closer to the limits.

24 Now, this is again in response to Dr.  
25 Murley's comment and I should point out we talked

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1 about all these. Everything on here -- well, maybe  
2 not everything. Many things on here are subject to  
3 judgement. I may have missed something. We tried to  
4 go through and list the conservative factors that have  
5 been left in the analysis and also the nonconservative  
6 factors that have been left in the analysis.

7 There are some things that I think there  
8 wouldn't be too much debate about. For example, for  
9 main steam line break our modeling of main steam line  
10 break is unquestionably conservative because the most  
11 severe transients that control the main steam line  
12 break contribution are those that occur in  
13 containment.

14 When you break a line in containment you  
15 pressurize it so you can't boil the water at 212.  
16 You're boiling it at a much higher temperature. We  
17 haven't accounted for that in our model. That's an  
18 unquestionable conservatism.

19 For circumferential flaws the fact that we  
20 assume them to propagate instantly all the way around  
21 the vessel is unquestionably conservative. Our models  
22 of material variability for copper, for  $RT_{ndt}$  we base  
23 the populations that we sample from samples taken from  
24 many, many materials that span the spectrum of RPV  
25 materials that are available. Unquestionably

1 the uncertainty associated with any plant specific  
2 analysis is going to be less.

3 I'm not going to go into all these except  
4 to point out that, again, some of these people may  
5 argue one side of the line or the other but I think  
6 taking it away this is an appropriate way to think  
7 about whether you would recommend to the regulator to  
8 take these limits without margin, whether the  
9 regulator should take these limits without margin.

10 It was the point that Dave made that it's  
11 really easy to get buried and say, oh, you've got a  
12 factor of potential two nonconservatism here or you've  
13 got a factor of three conservatism here. You really  
14 need to try to get on one page everything that you  
15 view and people's views are sometimes going to be  
16 different as being conservative or nonconservative and  
17 then think about what's that telling you with regards  
18 to whether if you could spend the money, spend the  
19 time to get the right failure loci whether that would  
20 in general be moving out that way or in that way.  
21 I've got my own personal opinion but obviously  
22 everybody is entitled to theirs. That's it.

23 CHAIRMAN SHACK: Anymore questions? Tom,  
24 you had some items this morning that you felt were  
25 real important that you wanted to get in so before we

1 go around the table since you had yours articulated.

2 DR. KRESS: Yes. Considering the last day  
3 and a half that we've had it seems to me like the  
4 things that we need as far as the heat transfer  
5 coefficient is concerned and its sensitivity, I think  
6 you need to show a better technical basis for that.

7 Perhaps it's the Catton paper that  
8 determines that particular heat transfer coefficient  
9 based on experimental data actually taken at a  
10 downcomer but I haven't seen that. Maybe that's the  
11 technical basis we need.

12 I agree with Mario Bonaca's statement that  
13 there is a need to better discuss why LOCAs were not  
14 originally considered but now they tend to be  
15 dominant. It's just a discussion of why that is.

16 On the air oxidation source term I tend to  
17 buy what you're saying that the conditional  
18 probability that you'll have an air oxidation event  
19 along with the conditional probability that it will  
20 lead to early containment failure are probably  
21 sufficiently low that the  $1 \times 10^{-6}$  offsets the effect  
22 you would have on LERF if you had an air oxidation  
23 event. But I kind of thought you approached it from  
24 the backside.

25 That is, you tend to deem that these

1 conditionals were small enough that you didn't have to  
2 look at air oxidation events. I would prefer you  
3 approach it from the front end and say, "Give me a  
4 technical basis for what these conditional  
5 probabilities are, the condition that you'll have --  
6 that a break will lead to air oxidation. And also the  
7 condition that will not lead to an early containment  
8 failure.

9 Then based on those values, actual values,  
10 tell me what effect it would have on the acceptance  
11 LERF that would be for probability. I thought you  
12 approached it from the backside and I would prefer to  
13 see that from the front side. Along with the  
14 thermal hydraulics choice of the heat transfer model  
15 and its technical basis, I thought it was your choice  
16 of 30 percent for sensitivity had a weak technical  
17 basis. Thirty percent probably was an epistemic --

18 CHAIRMAN SHACK: No, that's a reasonable  
19 number for a well-controlled experiment.

20 DR. KRESS: Yes, for a well-controlled  
21 experiment. I just think there's more model  
22 uncertainty involved than that and I think you need to  
23 think about that a little more. Those were basically  
24 my thoughts about it.

25 MR. ERICKSONKIRK: If I could ask a point

1 of clarification getting to your reiteration of Dr.  
2 Bonaca's comment. I understood his comment and I  
3 think it was well taken that we needed a better  
4 explanation of why main steam line break was important  
5 before and no so much now.

6 Just with regards to why LOCAs were  
7 ignored before but included now, I would ask if folks  
8 have read what is said in Section 8.5.2.5 of the  
9 report because that's where we go into that.

10 DR. KRESS: You may have covered it.

11 MR. ERICKSONKIRK: Okay.

12 DR. KRESS: We just didn't cover it very  
13 well.

14 MR. ERICKSONKIRK: No, no. I agree with  
15 that. Okay.

16 CHAIRMAN SHACK: Anybody else have any  
17 further comments they would like to make as to things  
18 they might see that are needed before we feel we are  
19 comfortable writing a letter on this?

20 MR. SIEBER: I'd like to have the  
21 documents.

22 CHAIRMAN SHACK: Well, yeah.

23 DR. FORD: Are we proceeding around the  
24 table?

25 CHAIRMAN SHACK: I was just going to take

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1 them at random rather than go around the table.

2 MR. SIEBER: Even the ones you've sent us  
3 before.

4 MR. ERICKSONKIRK: I think just to make  
5 things very clear, and I spoke with Tanny about this,  
6 is we're going to get a disk to all of you that has  
7 all of the presentations that were made in the past  
8 day and a half in their final form and all of the  
9 documents.

10 MR. SIEBER: Okay. That's going to be  
11 more than one disk?

12 MR. ERICKSONKIRK: I don't think in PDF  
13 form it will be more than one disk, no.

14 DR. WALLIS: The whole documentation so  
15 far is only 80 megabytes. We can give you more.

16 MR. SIEBER: Yeah, put pictures in.

17 MR. ERICKSONKIRK: But just to make sure  
18 that everybody's got everything, a complete new disk.

19 DR. FORD: I've got a comment. I still  
20 feel -- I find this list of the conservatism versus  
21 nonconservatism very useful so maybe my concern is  
22 hidden. When I look at the high sensitivity to the  
23 embrittlement shift, the function of fluence and  
24 material composition I get a wee bit worried as to  
25 whether, for instance, if you used the upper bound of

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1 that relationship that you put up on the screen, I  
2 think your words were that the situation would be  
3 untenable or words to that effect.

4 Yet, that is a very realistic situation.  
5 You do have materials which are offered. Maybe it's  
6 because aleatory uncertainties, etc., as to why  
7 they're there, but there are data points at the top  
8 end of that distribution code. There is the ones that  
9 are going to bite us in a practical sense.

10 Now, Bill assures me that all of this is  
11 covered quite adequately by doing the Monte Carlo on  
12 the mean value of that relationship. I've still got  
13 a feeling of unease. One will bite you and that's  
14 what worried me.

15 DR. KRESS: I thought we heard that if  
16 they took a single vessel and made that plot, you  
17 would still get that kind of --

18 DR. FORD: No.

19 DR. KRESS: Rather than it being clean  
20 they might be able to have some location that you get  
21 an uncertainty.

22 MR. ERICKSONKIRK: You'd get uncertainty  
23 but you would get less. You would get less.

24 DR. KRESS: But it would tend to still  
25 cluster around the mean.

1 MR. ERICKSONKIRK: I think it's fair to  
2 say that in most cases it would cluster around the  
3 mean. However, as Allen pointed out, there are some  
4 cases that would either be systematically high or  
5 systematically low. Again, I think that's also one of  
6 the reasons we do surveillance.

7 MR. DENNING: I do think that the way the  
8 Monte Carlo analysis is done that effectively they are  
9 taking that upper bound in a sense in that the mean  
10 value is close to the 95th percentile value.

11 My interpretation is that effectively in  
12 the way they are treating their mean which, again, is  
13 95th percentile, and the way that the sampling is  
14 done, I think they really are accounting for the upper  
15 parts of that distribution rather than the mean curve  
16 that went through it. That's my feeling. I may be  
17 overstating that.

18 MR. ERICKSONKIRK: No. I think I would  
19 agree in that the upper part -- I mean, just like the  
20 tails of any of the distributions that you've seen,  
21 the tails are weighted in at their relative frequency  
22 if you've got the 95th to 100th percentile upper tail  
23 impacts calculation five percent of the time. And the  
24 other thing to note is that it's like any failure  
25 analysis you've ever done. It's when multiple things

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1 go wrong that we get the high failure frequency.

2 It's when you've got a larger-than-average  
3 flaw and a higher-than-average copper and all those  
4 things have gone wrong so if you look at the -- if you  
5 go in and you compare the populations of flaws or  
6 chemistries or whatever that are contributing  
7 dominantly to the failure frequencies and you overlay  
8 those distributions on the distributions that you  
9 originally sampled, you find out you've got to buy a  
10 sample and you're sampling depending upon what's worse  
11 the upper bound or the lower bounds of the  
12 distributions.

13 CHAIRMAN SHACK: But that's sort of like  
14 Rich's argument. Everything is sort of governed by  
15 the guy at the top because all the other guys don't do  
16 much.

17 MR. ERICKSONKIRK: Not the guy at the very  
18 -- the one at the 8th sigma level that never occurs  
19 doesn't matter.

20 CHAIRMAN SHACK: You're up very high.

21 MR. ERICKSONKIRK: It's weighed in there  
22 and I guess the thing that I would ask is what's the  
23 credible basis to do a biased sampling on that  
24 distribution? How would you construct that  
25 sensitivity study?

1 CHAIRMAN SHACK: Oh, if you think there  
2 really is a plant that goes along the upper bound. A  
3 plant that sits along the upper bound.

4 MR. ERICKSONKIRK: And, again, I think  
5 that's something that is covered in surveillance.

6 MR. HISER: But, Mark, wouldn't the effect  
7 of that be that you're moving up and down the curves  
8 that have the results of through wall crack frequency  
9 versus  $RT_{ndt}$ . I mean if you are skewed high, you just  
10 move higher up on the curve.

11 I don't know that the curve itself would  
12 change at all. In terms of doing a calculation where  
13 you are going to compare any plant relative to any  
14 proposed screening limits, you would need to add in a  
15 biased term to account for that. But the limits that  
16 you're comparing to would be unchanged.

17 I mean, you have the same relationship  
18 between  $RT_{ndt}$  and through wall crack frequency. It's  
19 just where you go in to Mark's curve at. You might go  
20 in at a higher value by some degree to account for  
21 that increased sensitivity.

22 I think it's a plant specific application  
23 and is maybe where the concern should be. In terms of  
24 the simulations that they did, if the embrittlement  
25 that they calculate really does span the range of

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1 scatter that you see, then that's been taken into  
2 account.

3 CHAIRMAN SHACK: Oh, yeah. I would agree  
4 on the population. This is a plant specific  
5 application that we're talking about here I think.

6 DR. FORD: So what you're saying is the  
7 fact that those ones at the upper end of your scatter  
8 band let's assume that they were copper plants. That  
9 would come into the plant specific analysis?

10 CHAIRMAN SHACK: His  $RT_{aw}$  in that sense  
11 takes care of that.

12 MR. HISER: But you would still calculate.  
13 Based on the model you would still calculate an  $RT_{aw}$   
14 or  $RT_{p1}$  that's low because that's the mean value.  
15 It's not the high value that is reality for that  
16 plant.

17 MR. MITCHELL: Matthew Mitchell, Materials  
18 and Chemical Engineering Branch, NRR. I would just  
19 like to echo Dr. Hiser's comments that I think the  
20 concern that you're expressing over the potential for  
21 an individual plant to have a material which is acting  
22 atypical with respect to what the general model  
23 predicts is a concern that is appropriately addressed  
24 when that plant calculates its reference temperature  
25 for the material which is acting atypically.

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1           You need to look at the amount of data  
2 that is available for that material so that plant's  
3 plant specific surveillance program. And to determine  
4 whether the amount of information that is available is  
5 statistically significant relative to the amount of  
6 data that is supporting the general model and whether  
7 it appears to indicate that there needs to be an  
8 additional factor added to that plant's determination  
9 of its reference temperature for comparison to the  
10 screening limits that are being specified in the more  
11 general analysis.

12           I mean, that is, in effect, in large part  
13 what we do today in Reg. Guide 199, Rev. 2 when we  
14 have provisions available to use plant specific data  
15 to override the tables which we in the Reg. Guide  
16 which is, in effect, the same thing as having a model  
17 like the Eason model. It's sort of the default.

18           But if you have plant specific data which  
19 suggest something different, you go to that data and  
20 you use it to supplement the information in your  
21 general model. We would hope, however, I think that  
22 given the amount of data that's being used in the new  
23 embrittlement models that there will be a very low  
24 likelihood that you will find a plant that has a  
25 statistically significant amount of data out there

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1 which shows a consistently high trend. I'm not going  
2 to say that can't happen or that it is not in  
3 existence but I think that will be by far the minority  
4 of the cases of all the operating facilities.

5 MR. DENNING: Yes. If I can I would like  
6 to make just a couple of quick comments. I echo what  
7 time said about the heat transfer coefficient. That  
8 hasn't been put together adequately but it's doable.

9 I think that it's definitely doable with  
10 available resources to do that. One of the things we  
11 do have to recognize is that 2D RELAP annulus model is  
12 no better than the 1D. Both of them are wrong. The  
13 flow regime is a critical element of this.

14 I think they can use better -- make better  
15 use of comparisons with experiments that exist out  
16 there to make their case. I do have one thing that I  
17 really don't like about the comparison with the  
18 experiments, though, and that is the use of an average  
19 temperature difference and in averaging those between  
20 plants is not a good characterization of how well  
21 RELAP is able to model those particular things.

22 The standard deviations I didn't have a  
23 problem with but the average temperature difference  
24 where the pluses and the minuses are washing each  
25 other out between the different comparisons. That

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1 just is not a good way to characterize how well RELAP  
2 represented those data.

3 DR. RANSOM: Let me make a comment along  
4 those lines. From everything that I've heard it seems  
5 that the thermal stress in the wall is governed more  
6 by how the fluid temperature in the downcomer changes  
7 with time as opposed to what the heat transfer  
8 coefficient is at any given time.

9 It would seem like a better approach to  
10 this would be to simply make that heat transfer  
11 coefficient very large. It can be bounded more or  
12 less along the lines of what Murley is talking about  
13 and eliminate that as a parameter.

14 I suspect the results will not change much  
15 because the thermal stress is really governed by how  
16 the temperature changes with time due to the ECC water  
17 or wherever the cold water is coming from. For those  
18 kinds of transients which are just inventory type  
19 transients RELAP5 is fairly adequate. I mean, it's  
20 just how much do you over time flush out the hot water  
21 from the wall.

22 MR. SIEBER: It seems to me, though, that  
23 flow is a key characteristic also.

24 DR. RANSOM: What is?

25 MR. SIEBER: Flow. Downcomer flow. Since

1 you don't conserve momentum, you almost have to fudge  
2 the flow in order to get it to go in the right  
3 direction at the speeds, velocities that the  
4 experimental evidence would provide. To me that is  
5 sort of a shaky kind of a --

6 DR. RANSOM: Well, they are two-  
7 dimensional. They are shaky that way. But the 1D  
8 downcomer is --

9 MR. SIEBER: Well, that's --

10 DR. RANSOM: -- a little bit different.  
11 That's an inventory problem.

12 MR. SIEBER: Right. It's one bucket to  
13 the next.

14 DR. RANSOM: That would be a simple way of  
15 eliminating some of the concern about non-  
16 conservatisms.

17 CHAIRMAN SHACK: Well, I guess I would  
18 echo that. It seems to me that the biggest remaining  
19 issue is to show the relevance of the heat transfer  
20 relation whether it's -- I think you need to compare  
21 it with some relevant data, downcomer data. If worse  
22 comes to worse CFD calculations for the right job.

23 Somehow that just has to be -- I agree  
24 with Tom. The .3 just doesn't do it without more  
25 justification. I'm not sure I'm convinced by Vic.

1 The 1997 calculation there when he doubled that  
2 sucker, it really pushed it up there.

3 DR. KRESS: It was a significant number.

4 CHAIRMAN SHACK: It was a significant  
5 number. It looks to me like the bounding number we  
6 would need to use is like a factor of 5 and I don't  
7 think they really want to give that up at this point.  
8 I think you are going to have to come back and make  
9 that case that it's a lot better than that. Hopefully  
10 that will be included in the final documentation.

11 MR. ERICKSONKIRK: Hopefully it will.

12 CHAIRMAN SHACK: Additional comments?

13 MR. DENNING: Somebody had a comment what  
14 a nice -- how nice the presentations were. I thought  
15 they were very well put together and I thought the  
16 whole package looks good. It's just some weaknesses  
17 that still have to be cleaned up.

18 DR. KRESS: I think I second that. Very  
19 nice piece of work.

20 MR. SIEBER: Well done.

21 MR. ERICKSONKIRK: So we owe you a disk of  
22 all the presentations and all the reports to date and  
23 then we owe you the final thermal hydraulics reports.  
24 Is that correct?

25 CHAIRMAN SHACK: Yes. Now, what are you

1 going to do with these final Peer Review comments?

2 MR. ERICKSONKIRK: They are going to be --  
3 I'll keep talking until Allen tells me to shut up.  
4 They are going to be put into, I think, Appendix B and  
5 to the extent that we can, the staff will respond to  
6 them. Like I said, certainly there were some things  
7 in there that, you know, clearly we said A and the  
8 reviewers heard B which means that we didn't explain  
9 it right so we are going to change the documentation  
10 to try to keep that from happening in the future.

11 DR. KRESS: But you're going to hold open  
12 the option to continue to disagree?

13 MR. ERICKSONKIRK: Yes.

14 DR. KRESS: Good.

15 CHAIRMAN SHACK: If there are no more  
16 comments, I think we can adjourn.

17 (Whereupon, at 11:56 a.m. the meeting was  
18 adjourned.)

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CERTIFICATE

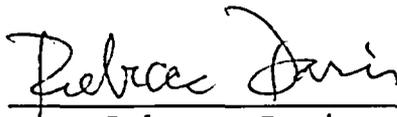
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Rebecca Davis  
Official Reporter  
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## *Heat Flux*

$$\underline{h(t) [T_{\underline{w}}(t) - T_{\underline{f}}(t)] = k(t) \, dT(t)/dx}$$

- $T_{\underline{w}}$  and  $h$  are not independent parameters.

Variations in  $h$  Are *Damped* With Respect to  $q''$   
Heat Flux From Wall to Fluid for Several  $h$  Values

| $\tau = 1800\text{s}$ , time = 1500s |                       |                        |                  |
|--------------------------------------|-----------------------|------------------------|------------------|
| $h$ , $\text{W/m}^2\text{-C}$        | $(T_w - T_f)\text{C}$ | $q_w''$ $\text{W/m}^2$ | $q_w$ , % change |
| 850                                  | 23.5                  | 20,000                 | 22               |
| 1700                                 | 13.4                  | 22,800                 | 10               |
| 3400                                 | 7.2                   | 24,500                 | 4                |
| 5100                                 | 4.9                   | 25,024                 | 2                |
| 10214                                | 2.5                   | 25,535                 | 0                |