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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

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 NOURBAKHSH
CAYATANO SANTOS

ALSO PRESENT:

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

A-G-E-N-D-A

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

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

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

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

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

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

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

He would let the pumps run. There was a high likelihood for that. There was a scenario that was dominant because of the cool down and repressurization. I daresay that there are good reasons today to reevaluate this decision of letting the pump running but I think it has to be dealt with.

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

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

Now, in the LOCAs you don't have

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

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

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

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

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

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

MR. ERICKSONKIRK: Yes.

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

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

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

MR. ERICKSONKIRK: Okay. Where we left

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

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

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

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

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

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

We did not perform sensitivity studies per se looking at doing changes to the flaw distribution. Not because we believe the flaw distribution to be certain but because there really isn't credible alternative information out there on which to base a sensitivity study.

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

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

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

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

MR. ERICKSONKIRK: It is close to the wall.

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

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

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

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

MR. DICKSON: Yes.

DR. WALLIS: Right.

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

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

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

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

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

DR. WALLIS: Yeah.

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

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

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

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

DR. WALLIS: I think you need a different rationale.

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

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

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

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

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

DR. WALLIS: It's much better than you had the first time.

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

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

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

MR. ERICKSONKIRK: That's right.

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

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

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

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

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

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

MR. ERICKSONKIRK: You have it already.

DR. WALLIS: We have it already.

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

Then the other thing is all of the

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

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

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

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

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

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

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

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

We've done, which I don't have here but can provide you, sensitivity studies where we increased the number of surface-breaking flaws from our baseline number dramatically and the through-wall cracking frequency doesn't go up much and that's expected because they are circumferential. Bruce.

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

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

MR. GAMBLE: My name is Ron Gamble and I work at Sartrex and I do a lot of work in this area for EPRI. I want to say one thing about flaws on the surface. This is a misconception you just keep hearing and hearing and hearing. All vessels that are manufactured and operating in the United States have had inspections on the surface so there is no vessel that is not in service that has not been inspected for flaws on the surface.

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

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

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

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

MR. ERICKSONKIRK: The cladding process

does create flaws in the cladding.

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

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

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

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

DR. FORD: Mark, that last statement may well be true but that is a calculated residual study profile. If you look at the data and make a comparison, for instance, double-V notched pipes or core shrouds, there's a considerable scatter of the actual data around that theoretical line. Now, if you put the upper bound of the observed residual stress profiles how would that statement --

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

MR. DICKSON: Yes.

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

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

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

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

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

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

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

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

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

Okay. Next one concerns the embrittlement shift model regarding which there's been a lot of discussion both within the NRC and within the ASTM community. FAVOR has adopted an embrittlement shift model. This is a model that calculates the shift in the Charpy 30-foot pound energy transition temperature as a function of copper, nickel, phosphorus, fluence and so on. FAVOR has adopted a model proposed by one of our contractors in the year 2000 that differs somewhat from the ASTM E900-02 standard that was adopted two years ago. It should be pointed out that the two models are similar but not identical.

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

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

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

MR. ERICKSONKIRK: It wasn't --

DR. WALLIS: Different amounts of cooper or something.

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

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

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

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

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

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

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

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

MR. ERICKSONKIRK: That's right. Absolutely.

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

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

DR. WALLIS: It must be.

MR. ERICKSONKIRK: Yeah, it is.

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

MR. ERICKSONKIRK: You mean like that?

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

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

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

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

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

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

MR. ERICKSONKIRK: And sometimes you do.

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

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

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

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

DR. WALLIS: He takes the black line?

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

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

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

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

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

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

CHAIRMAN SHACK: But then he adds a margin term.

MR. ERICKSONKIRK: No. In the current regulation he adds a margin term because that uncertainty wasn't accounted in the calculation.

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

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

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

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

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

DR. WALLIS: No, because it seems to me that you're -- the plant says its fluence is so and so. Therefore, my RT is something plus 100. It could well be 100 plus 170.

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

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

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

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

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

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

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

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

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

MR. ERICKSONKIRK: I haven't been tuned

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

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

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

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

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

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

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

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

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

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

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

MR. ERICKSONKIRK: Yes. But, anyway, the point of this graph is to say that FAVOR is faithfully reproducing the scatter in the original database in its simulation. It is simulating the amount of scatter that's in the database for all PWR surveillance materials.

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

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

Now, here's one I did just for Dr. Shack because he asked me yesterday. Where we got off not simulating the uncertainty in the Charpy shift to fracture tough and shift correlation. Here is another plot with scatter in it, not quite as bad as the last time, where we have on the horizontal access and, to be consistent, this should say delta T_{41} joules but it's the same metric, the shift and the Charpy 30-foot pound energy transition temperature versus the shift in the fracture toughness transition temperature.

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

DR. WALLIS: There's another thing. First of all, you start with a fluency you know and then you have to predict this delta T40.

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

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

MR. ERICKSONKIRK: I agree completely.

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

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

DR. WALLIS: You're solid with fluence which you know and delta T_{30} is subject to large uncertainty. Delta T_0 is what you really want and it's also subject to uncertainty when you get it from delta T_{30}.

MR. ERICKSONKIRK: That's right.

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

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

DR. WALLIS: Maybe I should be congratulating you.

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

The FAVOR simulation process to go back is to simulate the uncertainty in copper, nickel, phosphorus influence, use the main curve to calculate a value of delta T_{30} shift, and then we go to this relationship and simply convert it to delta T_0 shift by multiplying by these values, the slopes of these lines without simulating the uncertainty. Dr. Shack asked where I got off doing that.

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

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

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

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

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

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

MR. ERICKSONKIRK: No, no, no. You can't determine -- you need six tests to determine T0.

Actually, you need 12 tests to determine delta T_0.

DR. WALLIS: You get transition.

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

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

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

Whereas if there is truly an underlying physical basis consistent relationship going on that cuts across all these materials, you must expect that the materials that have the best defined shifts using the most data are going to lie closest to the line. Indeed, that is the case.

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

Whereas, if I went through and if I sampled the uncertainty in this relationship in simulating my delta T_0s, my delta T_0 uncertainties would be huge relative to what I measure in the laboratory. We would be overestimating the uncertainty in those values relative to anything that's been observed.

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

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

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

MR. ERICKSONKIRK: At this stage nobody has attempted to develop a delta T_0 embrittlement trend curve for a whole host of reasons. One is that the testing just hasn't been going on for that long. Virtually all of the delta T_0 points here come from test reactors, whereas the embrittlement trend curve comes from reactor pressure vessel covalents.

I don't have them with me. I can show you curves of delta T_0 versus fluence for individual data sets but not for an agglomeration of data sets. Obviously that would be the best thing to get rid of this artifice entirely and estimate delta T_0 directly from copper, nickel, and so on.

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

MR. ERICKSONKIRK: Yes, and we simulate that.

CHAIRMAN SHACK: And you simulate that.

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

CHAIRMAN SHACK: Obviously you don't want to double count. Before I was sort of wondering if I took this scatter in the copper and nickel whether I would reproduce the scatter that I see and you do. I mean, if you run through the plot, you can attribute most of that scatter in your uncertainty in your copper and nickel measurements. That doesn't seem unreasonable.

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

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

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

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

MR. DICKSON: Copper, nickel, phosphorus, and fluence. That gives me everything I need to calculate delta T_{30}. I have the unirradiated RT_{ndt} and then I've continued to go through the manipulations that he shows here, the .99 if it's weld, 1.1 if it's plate. Does that answer your question?

CHAIRMAN SHACK: So at the flaw level?

MR. DICKSON: Each flaw.

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

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

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

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

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

MR. ERICKSONKIRK: Certainly the copper

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

MR. ERICKSONKIRK: Thank you. Thank you again.

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

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

DR. WALLIS: Maybe Charpy should eventually disappear.

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

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

But what it did do, and this I view as

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

Now the more interesting things. Getting into the sensitivity studies that we did to look at factors that were sort of outside of our modeling. Clearly in order to get up to through-wall cracking frequencies around the $1X10^{-6}$ limit that we're trying to get to, we've had to really crank up the embrittlement beyond anything that has ever been observed.

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

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

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

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

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

The solid points, again green for Beaver and red for Palisades, we increase the reference temperature just by increasing the unirradiated value. All of the calculations you've seen are based on increasing time as a free variable which gives you more of a through-wall cracking frequency increase than increasing unirradiated RT_{ndt}. Again, these are both artificial.

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

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

MR. ERICKSONKIRK: Yeah, extremely long times.

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

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

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

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

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

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

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

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

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

Now, indeed, in the picture that we scaled there was a range of depths ranging from zero to two millimeters. However, the way we've coded the FAVOR program without fundamental restructuring --

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

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

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

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

DR. WALLIS: The two millimeters.

MR. ERICKSONKIRK: Two millimeters by generally longer.

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

MR. ERICKSONKIRK: No.

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

MR. ERICKSONKIRK: Yeah.

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

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

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

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

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

Even at very high levels of embrittlement that would normally be needed to get a plate vessel close to the through-wall cracking frequency criteria of $1X10^{-6}$, the forged vessels have a through-wall cracking frequency that's on average about three percent of a plate vessel.

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

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

MR. ERICKSONKIRK: That's right.

DR. FORD: You then put a weld overlay.

MR. ERICKSONKIRK: That's right.

DR. FORD: And that could, therefore, get some of those flaws to break the original surface.

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

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

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

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

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

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

MR. ERICKSONKIRK: And I think -- like I said, I absolutely refuse to argue stress corrosion cracking with anyone. Least of all you. I think that gets into is what you've just proposed does that rise to the test of being a credible model. If so, then the next thing I'm going to have to ask you is, okay, propose some numbers and we'll run it, and we certainly could. Certainly if those were surface-breaking flaws, I mean, you know the answer as well as I.

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

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

MR. SIEBER: Useful.

MR. ERICKSONKIRK: -- realistic.

DR. FORD: But that 80,000 was measured. Wasn't it?

MR. ERICKSONKIRK: It was measured.

DR. FORD: So it's realistic.

MR. ERICKSONKIRK: Well, it's realistic

for the 100X Micro it was taken off of.

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

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

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

DR. FORD: I don't know.

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

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

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

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

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

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

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

MR. ERICKSONKIRK: Sorry. Clarity in communication.

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

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

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

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

MR. ERICKSONKIRK: Yeah, as was pointed out.

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

MR. SIEBER: No, no, no.

DR. KRESS: You use nondestructive testing technique?

MR. SIEBER: It's UT.

MR. ERICKSONKIRK: I'm not qualified to

answer that question.

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

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

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

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

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

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

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

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

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

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

We argued out here that once you got to a big enough break diameter, it was the vessel properties that were controlling the through-wall cracking frequency and the details of the thermal hydraulic transient were relatively unimportant. Then we started looking at this graph and saying if that's true, then why out here at the very largest break size aren't all the points just landing smack dab on top of each other?

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

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

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

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

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

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

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

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

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

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

The Beaver Valley vessel, which is about half an inch thinner than the Palisades and Oconee vessels, was showing -- and this wasn't the only indication but it's the easiest illustration -- was showing a systematically lower through-wall cracking frequency.

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

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

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

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

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

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

That should be conditional probability of through-wall cracking ratioed to the conditional probability of through-wall cracking for that transient in the baseline Beaver Valley vessel. We find systematic and not marginal increases in through-wall cracking frequency as you go out in thickness for all the major transient classes.

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

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

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

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

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

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

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

MR. ERICKSONKIRK: I could not say for sure.

DR. FORD: There are not many. It's either Palisades or Palo Verde. My point is this is good from the analysis that you have done. If there was another embrittlement process with high nickel content welds, then what you're seeing is for those particular ones, to go back to your previous graph for the 11-inch area of wall thickness, you've got a very high sensitivity of that ratio to wall thickness.

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

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

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

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

questions?

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

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

If I continue talking, I'm likely to say something wrong so I'll refer you to the report and I can certainly get that for you. The statistical distributions that we sampled from were based on fits to our data derived from the inspections of the vessels, both destructive and nondestructive. The point I would make is that we are sampling from some pretty wide uncertainty bounds both for density and size.

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

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

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

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

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

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

MR. DICKSON: Generally speaking it's more sensitive to the bulk because what you tend to see is transients that have very low probabilities of fracture. The large flaws will kick in. They will be particularly significant for those very low probability transients.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

As I said yesterday, most of the -- one of the set of comments -- first set of comments was most parameters in the PIRT are system boundary conditions rather than physical models, and indeed that's true. There's a sensible reason why that is so. Are there any questions about the effect of thermal stratification and mixing and the cold leg and downcomer from ECCS injection and how well this can be modeled with RELAP?

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

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

So we did a number of sensitivity studies both from what I showed yesterday and what I didn't talk about. We ran I would say hundreds of RELAP calculations at the University of Maryland to investigate the sensitivities that results to different parameters.

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

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

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

As you go to a medium break, I think as we have cited a number of times, these variations are thought to become smaller so now we are down to 75K and maybe a few PSI within the medium-break LOCA bin. And then when you get a large-break LOCA these variations essentially are gone. For the three plants we end up representing the large-break LOCA class of events by a single transient.

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

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

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

In fact, this kind of effect is also present in CE and Westinghouse plants because when you add up the bypass area between the upper plenum and the downcomer, it is still substantial. It's about maybe, if I can recall correctly, it's about one-fifth of this area. It's equivalent to a hole of about one square foot.

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

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

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

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

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

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

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

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

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

MR. BESSETTE: Yes.

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

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

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

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

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

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

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

MR. BESSETTE: Yeah.

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

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

DR. WALLIS: But d is the wall thickness.

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

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

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

DR. WALLIS: We were told that for big breaks the wall governed? What you are suggesting is maybe this is not quite so simple as that?

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

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

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

DR. WALLIS: This is at a particular time?

MR. BESSETTE: Yes.

DR. WALLIS: I think if you wanted to demonstrate this, if you actually showed some temperature profiles for different h's, as I think has been done before, it showed how these temperature profiles evolved with time for different h's and you would probably show that after awhile h doesn't matter. H matters in the beginning.

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

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

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

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

MR. BESSETTE: Yes. In effect you don't start the PTS transient until you reach flow stagnation for a LOCA. But you can see on the second page you can change heat transfer coefficient by a factor of 10 and you are only changing heat flux by 20 percent. It's heat flux that really matters rather than the two parameters individually.

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

MR. BESSETTE: Yes.

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

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

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

MR. BESSETTE: Well, I showed you some

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

DR. WALLIS: So it's not insignificant.

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

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

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

MR. BESSETTE: Yes.

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

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

DR. WALLIS: This is for Dittus-Boelter?

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

DR. WALLIS: The factor of two is not true convection with the off fluid going up near the wall.

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

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

MR. BESSETTE: Excuse me?

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

MR. BESSETTE: Yes.

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

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

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

MR. BESSETTE: In fact, that's what motivated us to do this sensitivity study. There's been controversy going back to the early '80s about whether the heat transfer coefficient is important or not. If you just do a biot number analysis, you say, well, it's not important.

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

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

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

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

CHAIRMAN SHACK: One of the troubles is, of course, you stop here. You have to integrate this whole thing out to the end. It's very difficult at any point in here to know how this really affects the final result.

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

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

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

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

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

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

MR. BESSETTE: Yes. Yeah.

CHAIRMAN SHACK: It would be big.

MR. BESSETTE: It goes up.

CHAIRMAN SHACK: CPF does go up.

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

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

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

MR. BESSETTE: Well, they were --

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

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

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

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

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

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

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

CHAIRMAN SHACK: But that is the way you do the calculations so you've got five bins for small-break LOCAs and all the small-break LOCA sequences are dumped into one of those five bins.

MR. BESSETTE: Yes.

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

MR. BESSETTE: Three.

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

MR. BESSETTE: Yes.

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

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

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

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

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

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

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

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

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

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

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

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

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

MR. ERICKSONKIRK: That's our project mascot that was bought by John Kneeland at Palisades. He travels with the project so I have every hope that soon we will sign this set of reports out of research and I'm going to carry the whole stack of reports along with the penguin over to Matt Mitchell's office in NRR and we're just going to dump it. That was probably more than you wanted to know.

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

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

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

We talked about this. We made two model changes in response to Peer Reviewers' comments and major comments of clarification. I've got three reviewers here. Schultz and VanWalle were the PFM reviewers and then Murley also made some comments on PFM so we have addressed those as well.

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

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

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

Crack face pressure we've already talked about. There was a question and misunderstanding regarding the use of the Reg Guide 1.99, Rev. 2 attenuation function which we clarified and resolved.

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

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

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

Regarding the upper-shelf model he pointed out the inaccuracy of our then correlative approach, suggest a model change, and we changed it and I briefed you on that today. Questioned how we wired an interdependence of K_{Ic} and K_{Ia} and the through-wall cracking calculations.

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

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

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

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

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

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

Dr. VanWalle, again, some nice comments in general. However, some particular remaining issues that we've noted. He made several recommendations. The continuation of in-service inspection to substantiate the applicability of flaw distribution to all PWRs. In fact, he made a stronger comment that he felt that in-service inspection should be a prerequisite to using any rule that develops our of these calculations.

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

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

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

MR. ERICKSONKIRK: That's the final comment. Recommended continued/further validation of indeed both the crack arrest models and the upper shelf models by both further research to understand the physical mechanisms and further collection of data.

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

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

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

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

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

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

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

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

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

CHAIRMAN SHACK: Yes, but not with SAFT.

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

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

DR. KRESS: If you could do it.

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

DR. KRESS: You may have covered it already.

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

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

CHAIRMAN SHACK: The least important.

MR. SIEBER: But it's there.

MR. ERICKSONKIRK: I believe that's it.

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

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

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

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

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

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

CHAIRMAN SHACK: We are running out of time.

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

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

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

MR. ERICKSONKIRK: Okay. Back to that.

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

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

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

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

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

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

Basically what happens is taking all of these things together the Rancho Seco event would be substantially less important in today's world as we know it with the information that we have than it was at least initially with the calculation that we done at the time that the event occurred. That's not to say that this event is impossible.

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

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

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

Because we think that this event is at least as bad as what the Rancho Seco event would have been, we conclude that these types of events are just simply not particularly all that important. They can still happen but they are just not going to be important.

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

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

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

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

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

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

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

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

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

That's basically the comments that we had.

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

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

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

MR. WHITEHEAD: Okay.

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

MR. ERICKSONKIRK: Donnie.

MR. WHITEHEAD: Yes.

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

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

The curve here gives you an indication that initially we have a very steep rise in the CPI and CPF for valve reclosure. It maxes out somewhere in the vicinity of about 8,000 seconds or something like that. However, you have to remember that at very long time frames the operators would have been transitioning from their initial procedures into ones where they are actually beginning to shut the plant down.

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

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

MR. ERICKSONKIRK: May I add? I guess the things I would add to that is that Dr. Murley seemed to get focused on the fact that we hadn't picked the absolute peak of the curve. And to amplify on what Donnie said, the important thing is that we capture the whole curve and so, yes, sometimes perhaps the valve will reclose later in which case we would be underestimating. It's also equally probable that it could reclose really early in which case we are way over estimating the probabilities of failure.

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

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

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

MR. ERICKSONKIRK: Yes.

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

MR. ERICKSONKIRK: Yes. Okay. So what we're trying to do here is to use all this information, put it together and see if we can come up with a new screening criteria to suggest NRR replacement for the current screening criteria of 270 and 300 F on RT_{ndt}.

DR. WALLIS: "Criteria" is a plural?

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

DR. WALLIS: And "criterion" is singular?

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

CHAIRMAN SHACK: We noticed that.

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

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

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

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

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

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

I think the main thing to point out is that these formulas can be applied to calculate values of these various reference temperatures based only on the information that is currently in the RVID database that's maintained by the NRC and based on a diagram of the plant showing the locations of the welds and the plates.

DR. RANSOM: Are these values plant specific then?

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

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

So taking those -- I'm sorry. Taking the fits of those lines which is just a simple exponential fit through the available data, we then estimate the total through-wall cracking frequency is the sum of the three constituent parts with only the minor modification that we multiply the through-wall cracking frequency due to plate flaws by a factor of two.

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

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

Okay. So now we can use the equation to figure out what combination of these various RT values are either below or above any risk limit you want so we'll set the limit at $1X10^{-6}$ and we can do that. Take, for example, for an axial weld -- I'm sorry, plate plant with axial welds.

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

You know that the through-wall cracking frequency total is $1X10^{-6}$ so stick in $1X10^{-6}$ there and now set it up on a spread sheet to scroll through a whole bunch of values for RT axial weld and figure out what the RT plate needs to be to get you up to $1X10^{-6}$ or any value that you pick and you wind up carving out curves of constant through-wall cracking frequency. The interpretation of this is that -- and I highlighted the $1X10^{-6}$ limit in red because that's the value that's been proposed as consistent with Reg. Guide 1.174 guidance on LERF. The way a plant would use this would be to say, okay, I need to calculate -- if I have a plate welded plant I calculate RT axial weld and RT plate for my plant and I put a dot on this diagram. If I'm inside the red curve, lift is good. If I'm outside the red curve, I'm going to pay a lot of money to consultants to figure out how to move the point inside the red curve.

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

Again, the yellow box at the bottom there are certain provisos to this regarding forging at very high levels of embrittlement and applicability to thick vessels which we've noted.

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

That's where the points lie at end of license. You see none of the plants are very close at all to the failure of loci and, as we discussed yesterday, and I don't have this plot here but it's in the report, if I crank this up to end of license extension, the values move out by between 10 and 20 degrees but still not closely approaching the $1X10^{-6}$ limit. Of course, in general forgings are further from the limits than are the plate welded vessels.

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

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

CHAIRMAN SHACK: Indian Point 3.

MR. ERICKSONKIRK: Indian Point 3. Thank you.

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

MR. ERICKSONKIRK: What's that?

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

MR. ERICKSONKIRK: I've X'ed this out in response to Murley's comment and I've got a whole other slide looking at conservatisms and nonconservatisms. Just to look at the status of operating plants relative to this proposal, we find that all PWRs are in order of magnitude or more away from the $1X10^{-6}$ limit, that being controlled only by this plant over here.

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

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

Now, this is again in response to Dr. Murley's comment and I should point out we talked about all these. Everything on here -- well, maybe not everything. Many things on here are subject to judgement. I may have missed something. We tried to go through and list the conservative factors that have been left in the analysis and also the nonconservative factors that have been left in the analysis.

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

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

For circumferential flaws the fact that we assume them to propagate instantly all the way around the vessel is unquestionably conservative. Our models of material variability for copper, for RT_{ndt} we base the populations that we sample from samples taken from many, many materials that span the spectrum of RPV materials that are available. Unquestionably the uncertainty associated with any plant specific analysis is going to be less.

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

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

CHAIRMAN SHACK: Anymore questions? Tom, you had some items this morning that you felt were real important that you wanted to get in so before we go around the table since you had yours articulated.

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

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

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

On the air oxidation source term I tend to buy what you're saying that the conditional probability that you'll have an air oxidation event along with the conditional probability that it will lead to early containment failure are probably sufficiently low that the 1X10 $^{-6}$ offsets the effect you would have on LERF if you had an air oxidation event. But I kind of thought you approached it from the backside.

That is, you tend to deem that these

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

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

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

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

MR. ERICKSONKIRK: If I could ask a point of clarification getting to your reiteration of Dr. Bonaca's comment. I understood his comment and I think it was well taken that we needed a better explanation of why main steam line break was important before and no so much now.

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

DR. KRESS: You may have covered it.

MR. ERICKSONKIRK: Okay.

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

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

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

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

CHAIRMAN SHACK: Well, yeah.

DR. FORD: Are we proceeding around the table?

CHAIRMAN SHACK: I was just going to take them at random rather than go around the table.

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

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

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

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

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

MR. SIEBER: Yeah, put pictures in.

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

DR. FORD: I've got a comment. I still feel -- I find this list of the conservatism versus nonconservatism very useful so maybe my concern is hidden. When I look at the high sensitivity to the embrittlement shift, the function of fluence and material composition I get a wee bit worried as to whether, for instance, if you used the upper bound of that relationship that you put up on the screen, I think your words were that the situation would be untenable or words to that effect.

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

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

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

DR. FORD: No.

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

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

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

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

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

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

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

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

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

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

CHAIRMAN SHACK: You're up very high.

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

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

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

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

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

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

I think it's a plant specific application and is maybe where the concern should be. In terms of the simulations that they did, if the embrittlement that they calculate really does span the range of scatter that you see, then that's been taken into account.

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

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

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

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

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

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

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

But if you have plant specific data which suggest something different, you go to that data and you use it to supplement the information in your general model. We would hope, however, I think that given the amount of data that's being used in the new embrittlement models that there will be a very low likelihood that you will find a plant that has a statistically significant amount of data out there which shows a consistently high trend. I'm not going to say that can't happen or that it is not in existence but I think that will be by far the minority of the cases of all the operating facilities.

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

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

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

The standard deviations I didn't have a problem with but the average temperature difference where the pluses and the minuses are washing each other out between the different comparisons. That just is not a good way to characterize how well RELAP represented those data.

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

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

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

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

DR. RANSOM: What is?

MR. SIEBER: Flow. Downcomer flow. Since

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

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

MR. SIEBER: Well, that's --

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

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

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

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

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

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

DR. KRESS: It was a significant number.

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

MR. ERICKSONKIRK: Hopefully it will.

CHAIRMAN SHACK: Additional comments?

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

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

MR. SIEBER: Well done.

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

CHAIRMAN SHACK: Yes. Now, what are you

going to do with these final Peer Review comments?

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

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

MR. ERICKSONKIRK: Yes.

DR. KRESS: Good.

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

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