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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON MATERIALS AND METALLURGY

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TUESDAY

JANUARY 15, 2002

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, T2B3, 11545 Rockville Pike, at 8:30 a.m., F. Peter Ford, Chairman, presiding.

COMMITTEE MEMBERS:

F. PETER FORD, Chairman, ACRS, Member

MARIO V. BONACA, ACRS, Member

WILLIAM J. SHACK, Member, ACRS

STAFF PRESENT:

NOEL F. DUDLEY, ACRS Staff Engineer

ALSO PRESENT:

EDWIN HACKETT

MICHAEL MAYFIELD

MARK KIRK

ROY WOODS

DAVID BESSETTE

ALAN KOLACZKOWSKI

ALI MOSLEH

FRED SIMONEN (on the phone)

TERRY DICKSON

BRUCE BISHOP

SHAH MALIK

A-G-E-N-D-A

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

(8:37 a.m.)

CHAIRMAN FORD: The meeting will now come to order. This is a meeting of the ACRS Subcommittee on Materials and Metallurgy. I am Peter Ford, Chairman of the Materials and Metallurgy Subcommittee. The other ACRS Members in attendance are: Mario Bonaca, William Shack, and Graham Wallis will be here at lunch time.

The purpose of this meeting is for the Subcommittee to review the status of the Pressurized Thermal Shock Technical Basis Reevaluation Project. In particular, the staff will present the initial results of the reactor vessel failure frequency of Oconee Unit 1 as calculated by the FAVOR (Fatigue Assessment of Vessels - Oak Ridge) probabilistic fracture mechanics code.

The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions, as appropriate, for deliberation by the full Committee at the next meeting in February.

Mr. Noel Dudley is the Cognizant ACRS Staff Engineer for this meeting. 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 December 19, 2001.

A transcript of this meeting is being kept, and will be made available as stated in the Federal Register Notice. In addition, a telephone bridge has been set up to allow individuals outside the meeting room to listen to the proceedings.

It is requested that the speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. We have received no written comments or requests for time to make oral statements from members of the Public.

The staff briefed ACRS Subcommittees on the PTS Reevaluation Project in March, April, and September 2000, and in January and July 2001. The ACRS commented on the PTS Reevaluation Project in a letter dated October 12, 2000.

In this letter, the ACRS stated that the Project was well thought out and recommended that the staff examine the implications of using a large early release frequency (LERF) acceptance guideline based on an air-oxidation source term on the acceptance value for reactor pressure vessel failure frequency.

The staff has issued SECY papers

concerning the following:

- development of the current PTS screening criterion and the motivation for reevaluating the criterion;
- identification of PTS scenarios and estimates of their frequency, thermal hydraulic boundary conditions for the fracture mechanics analysis of the reactor vessel, and the probability of thru-wall cracks; and
- identification of key inputs to the probabilistic fracture mechanics analyses, such as, generalized flaw distribution, neutron fluence, fracture toughness models and embrittlement correlations, FAVOR code development and the associated verification and validation process, and calculation of PTS thru-wall crack frequency.

We will now proceed with the meeting and I call upon Mr. Michael Mayfield, of the Office of Nuclear Reactor Regulations, to being.

MR. MAYFIELD: Good morning. Staff has come today, as Dr. Ford has suggested, to present a briefing, a fairly detailed briefing as you can tell by the size of the package, on the work on pressurized thermal shock and reevaluation of the technical basis for the PTS rule.

We have appreciated the time and effort that the ACRS as a full committee and this subcommittee in particular has invested over the last year or two in supporting this activity. We have taken this on as a major effort within the Office of Research and we think it is providing some direction for future risk informed activities for rules like PTS.

The uncertainty analysis has been a big piece of what we've been trying to do. Professor Apostolakis had asked us to present an example where we walked all the way through the work and through the uncertainty analysis.

One of the things the staff is prepared to present to you at this meeting is an example of how it all goes together. We have some preliminary results from the Oconee plant and then we'll use those to provide an example of how the whole analysis goes together and hopefully clarify what we've been promising to come tell you about for some time now.

With that I would like to turn it over to, I guess, Mark Kirk or Ed Hackett to open the briefing. We hope that we'll satisfy your interest, at least for today.

MR. HACKETT: Thanks, Mike. We have quite a team arrayed here to do this briefing. To my right is Alan Kolaczkowski. My name is Ed Hackett. I'm Assistant Chief of the Materials Branch in the Office of Research. Mark Kirk, Shah Malik, and Dave Bessette are over here to my left and there will be others coming.

I guess a couple of items of administrative business. On your schedule if you have that in front of you, we are proposing at this point swapping Roman numerals III and IV. After this status briefing, which will be largely a combination of myself, Mark Kirk, and Alan Kolaczkowski, we will go into the modeling process before we get into Oconee specific results.

Also, I know, as the Chairman mentioned at the opening of the meeting, there are a number of people joining us on the phone line. To allow opportunity for more to join in, we are not going to identify the folks on the phone line until the break. At the break point we will take some time to ask you to identify yourselves.

At this point, also I guess I should mention the purpose of this brief is -- I guess we could go to the first slide, Mark -- is a status briefing. At this point we are not looking at requesting a letter from the committee. Obviously if you feel a letter is necessary for some reason, that's a different story but we are not coming requesting a letter in any specific fashion on the project.

I guess it is unfortunate, as Mike mentioned, a lot of this particular meeting was driven by Professor Apostolakis. We will miss him today but I'm sure he will make sure that whatever is presented today we will be taken to task for -- well, we will be compounding with Professor Apostolakis at some point. He was a key driving force behind wanting to see us step through the uncertainty process. We will miss him today.

That's going down the list of meeting objectives that you see here. That's a key part of the meeting today to describe the modeling process and the treatment of uncertainty. Major departure from what we've done in the past for those of you who are familiar with this rule, 10 CFR 50.61.

The PTS rule is a rule that is basically keyed towards the best estimate approach and there are margins added to account for uncertainties in rather a crude fashion compared to what we think we can do today. One of the key goals in this project, which is at least two to three years into the tech evaluation at this point, was to try and treat those as we went along.

I think I can say it is the most comprehensive treatment we've ever done in this technical area. However, I think it would be fair up front to point out that what you will see is the uncertainty treatment has also been done on I guess I would say not an equal basis.

There will be some elements of the project where you will see uncertainty treated about as well as we know how to do. Then there are other areas where we weren't able to do as well for any variety of reasons. In a lot of cases lack of data or lack of appropriate data. You'll see that as we go through the day and a half here.

Also, at the end of the status briefing I'll try and leave you with our bottom line so you don't make everyone here wait a day and a half to see where we're going.

The Chairman mentioned the other big part of the brief is to discuss current results and insights from the analysis of the Oconee plant which was one of four that we're doing in the project for the tech bases and provide one detailed example of the modeling in the uncertainty process.

With that, I guess we'll move on to the basic overview slide of project status. The graphic gives you really the three pieces that have gone into this in terms of the technical or scientific areas. PRA events, sequence analysis is basically the beginning part.

The thermal hydraulic analyses to describe in detail the transients and then the integrating piece which is the probabilistic fracture mechanics analysis, No. 3 up there and ultimately going down to yielding a yearly frequency thru-wall cracking which is the way the previous rule was also set up.

This is the way we set up the project for the technical evaluation so we have developed an approach to do that. That is described in the graphic. I think I basically said everything under the second bullet.

Most recent accomplishments include the finalization -- maybe I shouldn't use finalization. The FAVOR code is now a working code that includes what we think are all the updates that were necessary to start the runs.

That was completed largely thanks to some pretty heroic efforts on the part of the Oak Ridge

National Laboratory, most notably Terry Dickson and Richard Bass and then Paul Williams. That was released to the public as version 1.0 in October. There will be, no doubt, refinements and enhancements as we go along. That was a major piece to enable the rest of this.

We are estimating the risk of vessel failure for four plants and attempting to generalize from there. We have two combustion engineering plants, one Westinghouse plant, and one BNW plant that you can see listed there.

Right now the Oconee analysis is pretty much in a draft form. It has been completed to a draft state in all the technical areas complete with an estimation of the thru-wall cracking frequency that is a draft of preliminary effort at this point that we'll describe.

The other three plants you can see their status there. We have in a lot of cases, I guess I should mention, the licensees in the industry have been a very key part of this effort. A lot of the inputs on the PRAs and the thermal hydraulic inputs were from the licensees in the industry on a volunteer basis.

This has been -- the project couldn't have been done without that type of assistance. A lot of credit is due to the industry participants. Bob Hardy this year had three reactor vessel integrity groups within the Materials Reliability project. That has been extremely beneficial cooperation along the way for us. That is the overall status of the project right now.

We are on schedule to complete the technical basis. We are looking at a schedule to complete the technical basis in 2002. Hopefully we would be looking at embarking on rulemaking somewhere after that.

Work remaining, on the next slide. We have discussed some of this with the committee previously. A fairly major effort underway on Q/A that is pervasive to the project. That has been underway simultaneous with the technical effort most of the way. A particularly large effort they are devoted to Q/A of the FAVOR code, validation and verification of the FAVOR code.

Finishing the internal events analyses for the four plants, that will hopefully complete in the first half of this year and then we'll have a fair bit of integration to do to go from there.

Another element we could spend a few

minutes discussing I will come to in a minute and show you that the results for the Oconee plant are going the way we had hoped that the project would go. The risk is looking like it is less than expected, or less than we had looked at in the past.

One of the things that was a factor in the project -- Nathan Siu couldn't join us today unfortunately but Nathan was one to flag up early to the working staff on the project and also to the ACRS. The notion of external event risk contribution has not really been addressed previously in PTS evaluations largely because the internal events dominate so dramatically.

In this case it's looking like that risk number is coming down low enough that I think we will have to be considering external events. We have initiated work in that area but that is something we have not embarked upon before so that is a bit of a departure for us in terms of that effort.

Under that we're talking about things like fires, for instance, natural hazards and how they may contribute either in tandem or singly with some of the PTS initiators. That is something we will spend at least a little bit of time discussing today, too.

Then there is the overall integration of the results and the risk criteria. Those of you who have followed this project know that the regulatory guide on the plant specific analysis which is regulatory guide 1.154 is really key to a risk criterion of 5E-6.

We are not coming here to discuss that today. That will be the subject of a separate meeting. We are in the process led by the PRA branch working on a SECY paper that will outline the staff's approach to what that risk criteria should be.

I guess in short right now it's not clear that will stay at 5E-6 or move to some other number in that range. That remains to be determined and will be the subject of the SECY paper and some potential discussion debate with the commission itself. There is some work that remains.

CHAIRMAN FORD: If I could ask a couple of questions. In some of the earlier correspondence on this whole reevaluation project. Mention was made of the continuant and that it would not be covered in the current work scope, the current paid for budgeted work scope.

Since you are coming to the end of the project by the end of this year, do you have any thoughts as to where you stand on this or are we going to lure the frequency down to E-6?

MR. HACKETT: These are really good questions, Peter. As you know, they have been the subject of a lot of debate. I'm probably not the -- Nathan Siu would probably be the most qualified person on the staff to get into that, Nathan or Mark Cunningham.

Since they weren't able to be here with us today, the staff's current thinking on that is to stay nominally with the criteria that exist at the moment which is a CDF based approach. Also based on, as we'll get into extensively today, RT_{NDT} as RT_{NDT} is an index.

At least the staff's thinking going into this is we will retain those features. As you mentioned, we are pretty late in the game to be looking at changing horses significantly, but that will remain an effort to outline in the SECY paper.

When Nathan put together an outline, this is vintage about November of 2001, we were looking at pursuing several options. One of them would have had a departure that would have included addressing LERF and containment integrity.

We at this point would be -- I think I can speak for maybe myself and a few others here -- not looking at pursuing that path right now as a recommendation, but that remains to be debated at higher levels. Obviously it's not my decision as to how we go forward with that.

Or it may be that we go forward with a version that is RT_{NDT} CDF base now and continue work looking at the LERF element and see if it's warranted that we make a change to the rule in the future along those lines. I think that is the way I would answer it. I don't know if anybody else has anything they would like to add to that.

CHAIRMAN FORD: Actually, you're an ideal feed man because the other question I had was related to you mentioned what's going to happen in the future on the regulation point of view.

Given the fact that two of your reactors that you've got on your study, Palisades and Beaver Creek, if you are using the current screen criteria, you've only got a couple of degrees, 1 degree, 2 degrees difference between RTPTS and current screening criteria. Those problems for those particular reactors are very current.

Ford Calhoun coming up for a license renewal is also very close and that's this year. What is the timing? You say that this is going to end.

The technical part is going to end this year and then you are going to some sort of regulatory discussions.

MR. HACKETT: Right.

CHAIRMAN FORD: Is there a lack of urgency here?

MR. HACKETT: There is in all fairness less than there was. There was a time when we started this project where it was to a large degree being driven by a number of events, a number of things involving the technical accomplishments that have made it possible is one thing.

But also being driven by a lot of the events at the Palisades plant where at one time -- I think that maybe representatives from Palisades are here on the line -- at one time their PTS screening criteria date looked like it was going to be in the range of 2003/2004.

They have since made submittals to NRR that have taken them, my understanding is, to 2011 and those were largely arguments based on fluence and dosimetry is my understanding. That urgency went away to that degree.

However, you raise a key point that there is still a fair bit of urgency in this project because, as you mentioned, people are making decisions for their plant's futures based on, in large part, whether or not they are going to have a viable reactor vessel for that renewal period.

Those decisions are very here and now so we take that very seriously from a schedule perspective. But to go further, the technical basis we are anticipating completing in 2002 the idea is we would start a rulemaking process in parallel with that. That would be Cindy Carpenter's branch in NRR and they have prepared resources for 2002/2003 time frame to undertake rulemaking in this area.

Again, schedule impact can be critical because we are probably talking a rulemaking that is two years in the doing. That would be from, say, this fall. We would be into 2004/2005 time frame before we would have a rule that would be on the books in 10 CFR.

In the meantime, hopefully the technical basis would be there so that people could feel encouraged that it's going a certain way. It's a little premature now to say just based on the results from one plant that everything is going to come up nice and rosy. At the moment that's the way it's heading.

MR. MAYFIELD: This is Mike Mayfield. I

think it's worth pointing out that while indeed there are some fact-of-life changes that have happened with some of the plants that maybe take some pressure off, there still remains significant interest from, at least, at the office level.

Ashok Thadani is keenly interested in this rule or this activity and making sure that it stays on track. That part of the pressure has not gone away. He is focused on it. Some key members of the project team did get distracted from some of the staff's efforts dealing with the September 11 events. We are working to try and recover from that.

There was an impact that we are trying to deal with now. Mr. Thadani and Mr. Zimmerman remain focused on this rule and keeping it on the schedule, at least as much as we possibly can.

MR. HACKETT: I guess with that we'll flip to the bottom line. This is after a day-and-a-half worth of presentation tomorrow what we will come to. We just thought we would hit you with that up front to let you know what's coming. We ended up in a lot of debate over how to present this so it's a work in progress. It does say preliminary on it and I would encourage people to take that seriously.

The current risk criteria from the

regulatory guide set at 5E-6. You also see two curves on there that describe the results of the project for Oconee that are basically looking at the RTPTS versus the thru-wall cracking frequency where you have two key junctures.

You're looking at it after 40 years of operation and then you're looking at it if you were to project ahead to the actual screening limits where they are set now, 270 and 300.

What you can see over on the left-hand side there is that some pretty exciting results here. Approximately 4 orders of magnitude lower than the nominal risk criterion after 40 years of operation and 2 orders of magnitude lower at the current screening limits.

This is a pretty exciting result for us. Again, we are hoping this holds as we go through some more advanced and refined Q/A of the codes and the data that produce this result. You will hear a lot about that and the propagation and uncertainty as it goes through this analysis.

I wanted to get this to you up front because this is what we went into the project hoping would be the case. Philosophically the idea was that there were a lot of conservatisms, or at least significant conservatisms embedded in the current rule and that with the aid of more accurate analyses and evaluations we would hopefully be able to remove some of that.

I guess what I would leave you just for this opening is it does look like it's going in that direction right now so we are pretty happy with that result.

PARTICIPANT: Where is 60-year life?

MR. KIRK: It's the next dot up.

CHAIRMAN FORD: Bill, he's off the computer -- the microphone. What was the answer to that?

MR. KIRK: The answer is it's the next dot above 40.

MEMBER BONACA: Okay. So just above one.

MR. KIRK: Not much further up.

CHAIRMAN FORD: I see. And you mentioned that obviously this is because of excessive conservatisms in the current code. Does it turn out to be one significant conservatism?

MR. HACKETT: You're a good straight man. The next slide is our attempt to go into that.

MR. KIRK: Should I give him the $20 now?

MR. HACKETT: Wait. We debated this one

a lot, too. That question has occurred to a lot of folks as we've been through this. What is the key driver for this? In the past it has been for the PFM inputs, the flaw density, and distribution. That has been a key driver.

But I think at this point we are not able to say exactly in terms of quantitative assessment X percentage came from this area, X percentage came from another area. We will hopefully get to that point.

What we tried to do on this slide is just show you the trends and sort of how they hit here with the green arrows showing items that would tend to reduce the conservatism or decrease the risk, and the red arrows showing areas that might be or would be tending to increase the risk.

The only three upward arrows on here are acts under PRA. Acts of commission that were considered were operators did the wrong thing at the wrong time, external events, when they are considered, would obviously increase the risk over all. We have not gotten into that to any significant degree yet.

Some nonconservatisms removed in the arrest and embrittlement models also have a slight upward trend there, too. But many more down arrows, down green arrows here than there are the upward red arrows. The overall effect on the project has been to reduce the risk and the conservatism.

Let's see where I wanted to focus here. PRA I think, and Alan will get into this, a lot of the PRA effort goes into much more refined binning. Alan will talk about that in detail. The previous binning of some of the event sequences was much greater than has been done in this project. That has been a major improvement.

Under PFM there was a significant conservative bias in the toughness model that has been removed or mitigated. Most of the flaws now we find, and this goes with expectation from some of our experimental work, the flaws are embedded. They are not surface breaking. In fact, we have not seen surface breaking flaws in the actual experimental evaluations we've done from real vessel materials. That's been a big factor.

Also, and you'll see details of this, we are now looking at spacial variations of the fluence where as before we assumed that all the materials were at the maximum fluence and the vessel was made of the most brittle material. Now we are considering the spacial map in the vessel belt line region. That is a significant aid.

I guess with that --

MEMBER SHACK: On the thermal hydraulic sequences, is that just basically the binning again? You are not assuming that all the horrendous thermal hydraulic events?

MR. HACKETT: Right. I think largely, Bill, it's mainly binning that has been the improvement there. Dave?

MR. BESSETTE: I think that's right. There hasn't been that much change and the way we predict these transients is the fact that we can do a lot more sequences than we could 15 years ago.

MEMBER BONACA: What are you talking about? Operator action credited in the sequences, you mean?

MR. BESSETTE: Yes. We ran sequences in which the operator action is credited and that makes a big difference a great deal of the time.

MR. HACKETT: That's one I should have highlighted, too. Dr. Bonaca raises a good point, especially for Oconee which is a BNW plant. With the one steam generators there were the previous sequences that, I don't want to say dominated, but were very significant which included things like main steam line break and steam generator two rupture.

By virtue largely of crediting operator actions much more significantly, you'll see when Alan goes through his presentation that those have come way down in terms of contributors to PTS. That has been another major improvement.

MEMBER BONACA: And you will discuss the uncertainties later on.

MR. HACKETT: Yes.

MEMBER BONACA: We'll have a feeling for what dominates uncertainty here if you have some specific --

MR. HACKETT: Specific transients and sequences.

MEMBER BONACA: Specific elements of this. You are showing us improvements and, one that is in my mind, what is driving uncertainty more than other things. I mention, for example, further action would drive uncertainty.

MEMBER SHACK: They only gave us point estimates before. Presumably we are going to see uncertainty bands.

MEMBER BONACA: I understand that.

CHAIRMAN FORD: Ed, could you go back to the previous graft? It's more for my edification. Thanks.

There's another way of looking at it that if you want to decrease the cracking frequency down to 10 to the -6 taking into account uncertainties on the containment. Is another way of looking at it you could just go to extreme criterion way up at 300? I'm just eyeballing this thing. 300 or 350 or 375?

MR. KIRK: Where you -- well, how we take this information and turn it into a screening criteria will be an interesting piece of technical work. At least in looking at it this way, let's be clear that reflects an assumption that we would intend to look at it this way.

Looking at some materials based value on the bottom and RTPRS type value versus predicted through all cracking frequency, there are two things that you can change that hope out on this plot. You can change the position of the vertical line -- the horizontal line, I'm sorry, as you pointed out, but you can also, as Bill was saying, he wanted to see some distributions.

I'm thinking, "Oh, God. I've got to go find some distributions." All the lines you'll see plotted today are 95th percentile not reflecting any policy decision, just based on normal scientific practice. What it does suggest is that there are distributions behind all of these lines. Of course, that means that 95 percent of the failures are below the line.

Where the screening criteria comes up is really based on a combined decision of where the horizontal line goes and what percentile you put in. As a materials person it would be my hope that we could make those two decisions and then do the screening based on best estimate values of the horizontal access variable.

You could keep the risk criteria where it is and pick a 99.999 percentile. I'm not suggesting that is a good thing to do, but achieve the same result as a very low risk criterion in a 25th percentile.

I think those are the decisions and if we think about them in that way, we can think about them in a rational scientific way as opposed to letting ourselves be led around by what the numbers actually are.

CHAIRMAN FORD: I guess this is a topic that will undoubtedly come up further than today. If you're talking about one sigma type of methodology, should you not be -- if you go for a 95 percentile --

MR. KIRK: Oh, yes. I see.

CHAIRMAN FORD: -- the 95 percentile and given the urgency of this -- not the urgency of this problem, the potential severity of this problem, shouldn't you work into a six sigma or 99. whatever six sigma?

MR. KIRK: That's a very good point. By looking at it in this way, it enables us to think in that manner and make a decision consistent with the severity of the accident if it did, indeed, occur. Terry Dickson knows better what the tails look like on these distributions than anyone. Certainly the percentile that's picked should be appropriate to the severity of the accident.

MEMBER SHACK: Since we truncated .1 and 99.9, we'll have a hard time probably going to it.

CHAIRMAN FORD: Sorry. We have talked enough at this point.

MR. KIRK: Yes.

MR. HACKETT: I guess a couple of comments I'll add just in closing of this sort of opening session here. This has been a really focusing event for the team. I've got to say the team has worked really hard on preparing for this presentation.

I think it was very good to have Professor Apostolakis point us in this direction because it pointed out some things both in a good and bad sense where we needed to spend some more attention. It has involved all three disciplines in the Office of Research and a lot of cooperation with the industry and with NRR.

That has come off remarkably well. It's fair to say it's not something that we're used to doing. We're getting better at it but I think it's about as good as it's been on this project.

Maybe just as a last anecdotal comment, a lot of credit to especially Mark Kirk for bringing this together including his facility with PowerPoint. You'll come to appreciate that he made all this possible today because the team was here fairly late last night getting things together.

MEMBER SHACK: You just need a printer that comes up with his skills.

MR. HACKETT: And you need to keep the files smaller. Also slides curtesy of Kinkos because the NRC reproduction facilities weren't able to accommodate our tight schedule here either. A lot of credit to the team and hopefully we can launch now into more detailed discussion.

I'll turn it over to Mark.

MR. KIRK: Okay. Thank you. Actually, I

only have a few slides here and then we go quickly to Alan. We are in the part of the agenda here, I think, this was previously item 4 on your agenda. Actually, items 4, 5, and 6 on your agenda. They've gotten moved up and we'll do the Oconee results after this.

Just to seal yourself, this is probably going to take us about three hours to walk through. As always, questions invited along the way so that we can get our voices back.

The purpose of this phase of the discussion is to work through the overall modeling and uncertainty process that we have undertaken in this project. In terms of what you're going to see here, the first two bullets are like one slide each and then the last bullet is approximately 30 slides per discipline so there is an uneven weighting here.

I would like to discuss the guidelines that we establish for doing uncertainty models in this project and talk about our intentions regarding the material screening criteria; discuss the interaction and integration of the three technical disciplines; and then, of course as everyone is aware, concept for model development on uncertainty treatment was established in 1999 by Nathan Siu.

You've been briefed on that already.

Today what we're going to focus on is what actually happened. As Ed has already pointed out, you'll see that sometimes theory meets practice.

Other times practice falls a bit short of theory but the aim of this presentation is to present this with a good degree of candor so you get a good picture of what happened and, indeed, what did not happen.

The guiding principles that we started with were sort of those laid out by Nathan. The methodology that we've adopted in the PTS reevaluation project requires an explicit treatment of uncertainties across all of the technical disciplines.

As Ed pointed out, relative to where we are now in 10 CFR 5061, that is a bit of a departure where uncertainties tend to be -- it's like relatives you don't like. You tend to bury them and hide them in parameters. Well, they are all out here in the open now and we're going to talk about them.

We classify uncertainties as being either aleatory or epistemic. Those are my two new words for last year. Now that we have identified them and given them a name, we can put a number on them and then put them in the FAVOR code and Terry swims laps while the FAVOR code is running.

The second point on this slide is that our intent in where we are going with this is to, of course, reset, or hope to reset, the material screening criteria for vessel embrittlement which is right now called RTPTS as expressed in 10 CFR 5061.

Our hope has been that in going through this we won't be requiring the licensees to make any new measurements. It will be able to use the advanced state of knowledge of computation of whatever is developed over the last two decades to be smarter about the screening criteria, but still express it in the same way relative to NDT, chemistry, and Charpy data. Everything is looking good with regards to that intent right now, I should say.

CHAIRMAN FORD: That last sentence, no new material measurements, when you're doing this project, you are obviously.

MR. KIRK: Certainly, yes. As associated with the project, the new measurements have been predominately focused in the flaw area. We have, indeed, collected together a lot of toughness data but that's not necessary new measurements. I should say no new surveillance measurements by the licensee.

MR. HACKETT: I guess including a couple of comments there, too, including things like the inspections. We've had those discussions. At least the idea going into the project was that it wasn't going to result in a new level of inspection technology that would be required on vessel wells.

CHAIRMAN FORD: The way it's written right now hits a raw spot with me in that you didn't do anymore experiments in this project and that's not what you meant.

MR. HACKETT: No, not at all.

MR. KIRK: Actually, we softened that statement from some that we had previous. Another example could be, for instance, the work of Ernie Eason and Bob Odette and others on the embrittlement correlation flagged up that phosphorous looks to be a contributor again as it was at the very beginning.

That then raises a question how does the industry address that. Is that a new measurement they need to make or are there default values or other ways you can address that.

And it looks like the answer is there will be -- there are alternative ways of addressing that. That is the kind of example. That did flag up out of the technical assessment and it will have to be dealt with.

MR. KIRK: Okay. Actually, we've used

this graphic before and you'll probably get sick of it by the end of the day. This is the very highest level view of what's going on in this project. Like I said, we will be referring back to it just so you can see where you are.

We, of course, start with the PRA event sequence analysis. That gives us two things, both sequence definitions and sequence frequencies. The sequence definitions go into the thermal hydraulic analysis. We use the RELAP code to commute pressures, temperatures versus time from those. Those go into the PFM analysis which magically pops out a conditional probability of vessel failure. That is then combined in a deceivingly simple matrix multiply with sequence frequencies to get the yearly frequency through all vessel cracking.

As we were dry running this yesterday my colleagues admonished me to point out that this is shown as a deceptively simple and linear process here and it's not either of those. Which is to say, we just don't pass through this once and call it a day. If we did, we would have met our schedule a lot better.

There's an awful lot of -- I shouldn't shall awful. Awful is a bad word. There's a lot of feedback that goes on at many points in this process as we get our results and it's just the normal engineering process of saying, "Well, that doesn't quite look right. What did we do there?" We feedback and we do it again.

As Ed pointed out on the status, and you've seen some of the results, and you'll see some more of those results today. We've got what we feel like is a good draft on Oconee. Having said that and going through that draft, we found certain things that they clearly need to be redone and will be redone but this is the overall process.

Now what we would like to do is to go through each of these three main elements, the three blue boxes, PRA, thermal hydraulics, and PFM and for each element present you with a presentation that in its total describes how we implemented our model development and uncertainty treatment procedures.

We are going to try to stick to this format as much as we possibly can to make it clearer, but in each of these three blue boxes we're going to start by talking about whatever constraints were imposed on the element or fundamental assumptions we have to make at the start. Those types of things would tend to constrain what we did.

We will then break down those three deceiving simple blue boxes into many more boxes or lines or what have you. There is an awful lot hidden in there. It's kind of like Pandora's Box. We will then discuss the process used for model building, if indeed models were built, talk about uncertainty treatment, and we'll try to wrap up each presentation by focusing on significant changes since the 1980's evaluation providing a bit more meat to Ed's down green arrows and up red arrows that you saw earlier.

With that, unless there are any questions --

CHAIRMAN FORD: I've got a very, very general question.

MR. KIRK: Sure.

CHAIRMAN FORD: It's more for best practices for the future. Along with the thermal hydraulic code research is developing. This is an extremely multi-dimensional multi-disciplinary exercise. We are presuming it's going to work. We are positive.

Do you have any lessons learned as to how you make such a thing work when you're talking to people on the west coast, the south presumably, in the east, the west?

MR. HACKETT: We have enough challenges just within 2 White Flint or just at the NRC headquarters complex. I guess it's kind of a philosophical question.

CHAIRMAN FORD: It's more a management question which will increasingly become if not philosophical, real management.

MR. HACKETT: It's interesting. As I mentioned in the overview when we opened things up, we have been in the past, I think, guilty of being more compartmented here at the headquarters operation and with the contractors that we do our probabilistic fracture mechanics in our branch and David, Roy, and Alan, and others are doing PRA and thermal hydraulics.

There wasn't in the past, quite a while back now, as much cross-talk as there needed to be. This project has made that necessary. Mike Mayfield could give the best history of this you could possibly get.

Mike has made a number of stabs at trying to do this over the last decade. We have met with some significant challenges in the past because of failure in the interactions between the technical disciplines.

I think this is the first time it has

really come together to work as well as it has, I think, frankly, it's out of necessity. We were just not able to make -- we can't run FAVOR without pressure temperature traces and event sequences and combine these into meaningful numbers for the rule without the kind of cooperation we've had.

I think it was those features plus an office director, Ashok Thadani, who got behind us in a very forceful way to have his three divisions taken on as a priority. It is one of the top priority projects within the Office of Research.

We've also had NRR supporting us all along the way. And then the industry, too. I think that was an element that was missing when we tried these things previously. We did not really work it cooperatively with the industry. We did this time from the very beginning and I think that's been another big factor. At least from my perspective those have been some major influences on the success we've had so far.

MR. MAYFIELD: This is Mike Mayfield. I think there are some things we have done differently from a management point of view. As Ed mentioned, this was taken on as a team activity involving inputs from all three of the divisions in the office. We had support from all three division directors.

The division directors meet basically bi-monthly briefing from the team on where they stand. The division directors meet separately on a regular basis and some of these top-tier programs that have high visibility are discussed.

We budget as a team activity rather than stovepiping the way we used to. The annual budget input is put together on an issue basis. There have been a number of changes like that that have contributed to making this go forward. I think they are lessons learned from some failures in the past that Ed mentioned and what is looking like a successful project right now.

We have adjusted. We've made some budget adjustments as we go along. By looking at it from an overall project standpoint rather than individual pieces I think we have been able to keep it moving.

MR. HACKETT: I think Roy had a comment.

MR. WOODS: This is Roy Woods. I'm involved with the PRA part of this. I heard Mike, and all that's true and very vital. But I also have to point out to you that the three of us at the lower level, PRA and myself, thermal hydraulics, Dave, Fracture Mechanics, partly, at least, Mark here, we meet sort of impromptu almost every morning now.

I wonder down to where their offices are and we've had some of the more meaningful and important exchanges of technical information or the way something is going or something that the other group needs to know about.

My point really here is in addition to what Mike was talking about where you made it a high level, director level, the workers also have to talk about the details of what's going on. I think that's been very important.

CHAIRMAN FORD: Thank you.

MR. KIRK: Coffee mess discussions have gotten us very far in this project.

MR. HACKETT: And it's Alan's turn.

MR. KIRK: With that, yes. We'll go to the detailed discussion of PRA models and uncertainty and I'll turn the presentation over at this point largely to Alan Kolaczkowski of SAIC and Roy Woods of the staff.

MR. KOLACZKOWSKI: Alan Kolaczkowski. First of all, thank you very much for the opportunity to, again, present the status to the committee members, etc. What I'm going to go over now is primarily address the key modeling aspects and the treatment of uncertainty in that first box that we see here on this diagram which is where we first define the sequence definitions that we're worried about.

Obviously a major product of that also is coming up with an estimate of the frequencies of those accident sequences that could represent a serious PTS challenge that is later combined with the conditional probabilities of vessel failure towards the tail end of the process to actually come up with estimates of the yearly frequency of thru-wall cracking.

This is, if you will, just another representation really of the same thing in the previous slide just shown perhaps in a little bit more detail. Unless you have specific questions, I'm not going to go through this in any detail but, again, this is meant to represent really what is going on throughout the entire project.

The PRA aspect of this, which is sort of the beginning part of the analysis through a primarily event tree modeling, we define what the potential PTS challenge accident scenarios could be and come up with the frequencies of those. Of course, those frequencies have uncertainties associated with them and we will be addressing that aspect of it later.

Those are binned and where sequences are likely to represent, if you will, very similar plant responses in terms of the way the plant will respond thermal hydraulically to those sequences. Those sequences are binned into what we call thermal hydraulic bins and then RELAP runs, etc., are run on those sequences to actually come up with the pressure temperature profiles for those sequences.

Now, again, while I'm presenting this in a very serial fashion. As has already been mentioned by Mark, this is quite an iterative process. You do some binning and you find out you've been too course or whatever and maybe, in fact, you've got to break those bins down into others and so you go back and you rebin those sequences and then more hydraulic runs are made so that we're not binning things quite so grossly where it looks as those something really does make a difference.

So, again, while we are presenting this in a very serial fashion, in fact, it's quite a iterative process and you go through this over and over and over again to keep refining the work to try to, if you will, remove many of the conservatisms that were certainly part of the original 1980's work.

MEMBER BONACA: Now here you include the operator actions in the binning process.

MR. KOLACZKOWSKI: The operator actions, of course, are actually some of the events that are in the event scenario so the operator actions get defined as part of the event scenario. Hopefully that will become clearer as we move on to some of the modeling process. Surely by the time we get to the example sometime either very late today or tomorrow, hopefully we will demonstrate for you very clearly exactly how the operator actions --

MEMBER BONACA: The reason why I asked, you know, we're talking about the managing of this effort. I'm sure you had interactions with the plant or whatever.

MR. KOLACZKOWSKI: Yes, we did. I don't know of the Oconee staff are listening but I think they will verify that I probably asked them too many questions too many times. Again, they responded to all our needs and that was quite an interactive process going on with Oconee, for instance.

MEMBER BONACA: Good.

MR. KOLACZKOWSKI: As is going on with the other plants, Palisades, Beaver Valley, and so on.

MEMBER SHACK: Were there any physical changes in the plants that effected the event sequences that you looked at?

MR. KOLACZKOWSKI: You mean from the '80s?

MEMBER SHACK: From the '80s, yeah.

MR. KOLACZKOWSKI: Well, I mean, to some extent certainly, yes. Oconee being a BNW plant has, of course, an integrated control system feature. Oconee has gone through an effort of updating that ICS system from the system the way it used to look back in the '80s time period. That is reflective ultimately on what the likelihood is of the integrated control system inducing faults that could cause PTS challenges, etc. We had to look at that. That is just an example. Probably if I thought about it a little harder I could think of some more.

Clearly one of the things that we had to do and did do in order to properly represent the potential PTS challenge scenarios for Oconee was get the latest information on what the plant looked like, how it's operated, what are the procedures they are using, what's the operator training, to what extent are they sensitive to PTS challenge, etc., etc. All of that needed to be, and was, done.

That's why we had to, in fact, interact with the licensee considerably to make sure that our model was indeed reflective of the way the plant is designed now, the way it's operated now, the training the operators go through, etc. There was a lot of work done in that area. That's just an example. If I thought about it, I could probably think of others.

So I'm going to be talking about, again, what's happening sort of in that first box in terms of, at least, some of the major modeling features and what we did in terms of handling, at least, the key uncertainties that we need to worry about, that portion of the analysis.

Okay. As the outline suggest, one of the first things we want to address is limitations or, if you will, constraints that we sort of impose on ourselves in terms of some limits that we put in terms of what we are going to analyze.

Also I want to point out a few keys things that were considered that are important to ultimately assessing what the PTS risk is from the PRA portion of the analysis.

Under limitations or, if you will, constraint side, I think we have to recognize that we are using an event tree PRA type of modeling structure to represent what those accident scenarios are. Along with that comes all the typical underlying PRA assumptions. Things are binary, although even there I know like a TBV, turbine bypass valve, is either going to stick open completely or it's going to reclose like it's supposed to.

Generally the model does not address it sticks open 30 percent or something like that. On the other hand, I should say that towards the end of this process, if we found out that something like that was very important and, in fact, it is in a few sequences.

The example that we'll get to at the end of this entire presentation will point that out, as a matter of fact, where we did have to go back and treat the fact, well, is the valve only 30 percent open or is it 80 percent open, etc., as part of the uncertainty.

Where it was important to do so, we did digress from the typical PRA binary look at the world. But in general it is a binary look at the world. Another example, the assumption that random events occurring follow some poison distribution process.

That's an underlying assumption that we've always used in PRA so you've just got to recognize that if you're going to model these sequences using a PRA event tree structure that there are some underlying assumptions that have just come along with the PRA process. Those are still there for the most part.

The next thing I should point out is that there was a screening step performed before really the collective PRA information about the sequence as well as the T-H information about the sequence. Before that information was passed on to the fracture mechanics modeling there were some screening done.

For instance, the PFM folks did not analyze every sequence that comes out of the PRA model. There is something like 160,000 sequences, I think, in this model. We could not do 160,000 PFM runs or whatever. Even if you include the fact that we binned a lot of them, etc., there would still be many more than what PFM analyzed.

Largely that screening was done on what we think are very conservative either low frequency scenarios. In other words, the frequency was so low that even if you assumed the conditional probability of thru-wall crack frequency would be -- excuse me -- that the conditional probability of vessel failure would be something approaching one and you just knew it was no way going to dominate the results. Those really, really low frequency scenarios were screened and weren't even analyzed in the PFM part of the analysis.

Similarly, on a T-H basis some of the

accidents are some of the scenarios that are analyzed in the PRA model. Many of them, especially for taking credit for operator action, for instance, would lead to a pretty benign cooling event.

If it was pretty clear that either the rate of cooling that we were getting or, in fact, that the ultimate temperature that we would reach to were such that that rate was just so slow, say, well under the typical 100 degrees per hour cool down rate, or if, in fact, that the ultimate temperature that we would reach was something approaching 400 degrees in 10,000 seconds into the scenario, clearly those kinds of cooling events are not going to represent significant challenges in terms of this phenomena occurring.

Where we saw that a scenario, even though it may have a high frequency, for instance, from the PRA aspect, was very benign from a cooling standpoint, then that scenario was screened and wasn't analyzed in the fracture mechanics portion of the model.

Having said that, I should point out that actually there wasn't a lot of screening done. I would say a lot of the scenarios passed through and were analyzed anyway just to make sure that they were not PTS challenges.

The point is if it was pretty clear that the frequency was just so low, or the amount of cooling was just so benign that it was not going to be a dominate contributor to this challenge, the PFM folks never even saw that scenario so there was a certain amount of screening done.

As has already been pointed out, at this point in time external event types of scenarios, fires, floods, seismic, that could somehow cause an overcooling event and how would the operator respond to this event given that there is also a fire going on in the plant, for instance. That has not been analyzed yet. It will be.

We have an approach, a couple of approaches actually, outlined. Those are being reviewed and we should be planning to proceed on doing something in the external event area very shortly. Realize the results you're going to see now and the things I'm going to talk about don't address external events at all.

On the considerate side, I think a couple of key things that we need to keep in the back of our mind as we look at the results of Oconee and subsequent studies that are going on in terms of Beaver Valley and the other plants.

We are looking at both full power and hot zero power, initial conditions. We are looking at different decay heat levels when this "overcooling event" occurs.

Obviously if you're at a hot zero powered condition and all of a sudden you have a severe overcooling event, the plant response is going to be somewhat different because it doesn't have that high decay heat to kind of slow the thermal response down in the plant. We are looking at both full power and hot zero power types of scenarios.

Secondly, the timing of the events. Again, a PRA event tree model in large part does not see time. It doesn't know when things happen. It just knows that they happen. Either a valve sticks open or it doesn't but it doesn't say when does that valve stick open.

For the large part, the model does not see time. However, having said that again, where time is important and where we did need to think about late occurrences of events or whatever, those have been included and, again, I think that will become clearer as we proceed through the presentation and particularly in the example when we get to it.

Finally, a point I want to make about

operator actions is that typically in PRA we have always tended the model what we hope for the more important errors of omission. I think a serious attempt has been made to look at acts of commission.

In fact, there are a lot of -- I'm calling them acts on purpose because they are really not errors given the situation in most part. But there are conditions under which the operator will induce, if you will, a cooling to the primary system. Just to give an example, in a loss of heat sink type of accident where, let's say, we've tripped a plant and we've lost all feed to the steam generators.

One of the things the procedures and the training will direct the operators to do is to depressurize the secondary side of the plant to try to get feed into the steam generators by virtue of the condensate booster pumps.

That act of depressurization causes a cooling in the primary system because he's going to open up by-pass valves to depressurize the plant which is going to cause cooling but that's a proceduralized directed act of commission to, if you will, add cooling to the plant. Obviously the operator is going to try to do it in a controlled fashion and then not cause a serious overcooling event.

Nevertheless, those types of acts are included in the model and we actually did look for other types of acts of commission where it would be a mistake on the operator's part, if you will. Quite frankly, we didn't find too much in the way of contacts that would cause a high probability of that occurring.

Nevertheless, there are procedurally directed acts which do add cooling to the event when it's necessary to do so to, for instance, avoid a core damage event and those acts are included in the model here.

Finally, there are four functions of interest really that we are looking at and I'm going to point those out here in the next slide if there are no questions on this one.

This is sort of a complex diagram and I certainly don't plan on going through each and every line of this unless there are questions from the committee. I think the main thing that I want you to walk away from in terms of looking at this diagram, this is a functional representation of the types of scenarios that are actually in this 160,000 sequence model that we have super simplified down to one page. As you can see, what we are really looking at is some sort of an initiating even that comes along either while the plant is operating at full power or as a hot zero power condition.

Then we're looking at the status of four functions that are listed there across the top. What is the status of the primary integrity. For instance, do we have a loss of cooling accident going on or is the primary basically still intact.

What is the status of secondary pressure. That's where you're covering things like do we have a turbine by-pass valve stuck open that is depressurizing the plant and causing a cooling of the primary system.

What is the status of secondary feed. Is that being properly controlled or are we indeed overfeeding the steam generators which also, again, could induce cooling in the primary system and, therefore, have an effect on the downcomer wall.

Then, finally, what is the status of the primary flow and pressure conditions. This is where you address such things as high pressure injection, potential repressurization events, are the reactor coolant pumps on or off because, again, that's going to have something to do with the amount of mixing that's going on in the primary versus the potential for stagnation conditions.

Those are the four functions of interest. Let me just say that the model addresses not only each one of those functions individually but also all the interactions between them, as well as the fact that you can have combinations of those occurring at once. Maybe you are overfeeding the steam generator and at the same time there's a stuck open TBV so two of the functions are, in fact, inducing cooling in the situation.

All of those interactions, the multiples of those interactions, etc., are all handled as part of this 180,000 or 160,000 sequences. Unless there are questions on that, that was really all I was planning to go through there.

This and the next slide serve to illustrate sort of the process that went on in building the model. Again, I don't plan on going through this in a lot of detail unless there are questions. There are a few things that I want to point out about the process.

As with any modeling process, the first thing you've got to do is go out and get a bunch of information on what it is you are going to model and how you're going to model it.

You can see there on the left sort of a long list of things that were collected in order to make sure that the model was going to be an accurate representation of the Oconee plant. You can see the list there. I won't go through it but a couple of things I do want to point out, especially the last two bullets there.

We had the fortunate luxury to be able to visit the Oconee plant and actually spent, I think, like two or three days there the first time that we went. During that process Oconee staff and some of their regular crews were able to perform, I think in their case, three or four transient scenarios, different scenarios that represented overcooling situation.

We got to observe the interactions of the crew, how they operate, roughly how long did it take to get through various steps in the procedures, etc. That helped us an awful lot on the human reliability aspects of this portion of the analysis. I want to thank the Oconee staff for providing that simulator time to us and helping us through that aspect of the analysis.

Then lastly the interactions. Again, this went on continuously. I don't know if Steve Nader is listening but if he is, he'll tell you that we bugged him way more times than they were hoping. If we had questions about anything, "How does this ICS system work again?" etc., etc., "Where would the operator be in this step of the procedure at this point in time?" Whatever questions we had, we would e-mail those questions to them and the licensee was very prompt in providing responses to us. We would get on the telephone and we would discuss those responses. That was a continuous step throughout much of the analysis.

Again, a point I want to make was it isn't like we visited the plant on the first day, spent three days, and then never talked to them again. It was an ongoing effort through much of the analysis and almost up to the present time so there's a couple of things I want to point out.

From the information you collect, you begin to identify things that you need to make sure that the model includes and you can see a representative list of some of those things. I'm going to talk about some of the arrows in such a minute.

Then we started building the actual model. I just want to point out it's a large event tree, small fault tree type of modeling process. As I said, it's something like 150,000 or 160,000 sequences represented in the model.

At this step you put a model together. You include in it all the major equipment items that you need to model like the reactor coolant pump status, HPI injection, etc., etc. At this point I just what to point out this is where we did an initial cut of what the human error probabilities should be.

I should point out relevant to PTS challenges, especially for secondary events where we have a problem on the secondary, the operator plays a very key role in arresting that event. The human aspects of this are vitally important and need to be addressed. I think that's one of the major improvements that we made over some of the 1980's work.

Nevertheless, we have an initial model, an initial cut of accident sequences including human error probabilities and so on. There's a preliminary quantification of that.

At that point we went back to Oconee and while there was an internal review going on of the model, the preliminary results, the human error probabilities we put in, I should say at that point we already had not only mean estimates for those human error probabilities, but also had uncertainty bounds on those human error probabilities.

That was all looked at again by the licensee. We actually went back to Oconee and visited them, I think for a day, presented the preliminary results, left them with information on CDs and whatever, and then they provided comments back to us where they felt that we were either overly conservative or, even in some cases, I can remember one in particular, where the Duke staff pointed out that they thought we were a little too optimistic with our human error probability.

Nevertheless, it was give and take there where they commented on the preliminary results. So this analysis also took advantage of an intermediate step where the Oconee staff looked at our preliminary results, commented on that in terms of the PRA model structure, some of the data that we had, the human error probabilities, etc., provided comments back to us, as well as we, of course, were performing an internal review.

MEMBER SHACK: Where were your human error probabilities coming from? What models were you using?

MR. KOLACZKOWSKI: The human error

probabilities, actually the way they were done on Oconee it turns out it's going to be slightly different from the way we may do it on some of the other plants. We had a number of, I guess for lack of a better phrase I'll call them, human reliability experts pull together among the NRC contractors.

Recognize that we had the procedures. We had observed these simulations, etc., and so forth. We had many discussions with the licensee. The first cut was that we went through essentially an expert elicitation process.

We looked at the various scenarios and the NRC contractors came up with an estimate of the mean probabilities as well as a cut at uncertainty bounds on the probabilities of human error failures and various conditions. As I pointed out, these covered again both errors of omission as well as what we thought were key acts of commission.

Then that was presented to the licensee when we went back to Duke. In fact, I sat down in an all-day meeting with a number of their operators and trainers, as well as some of their engineering staff, and we went through almost number by number what the human error probabilities were in the model and presented why we thought the means ought to be what they are, why we thought the uncertainty bounds ought to be what they are.

The uncertainty bounds did try to consider, if you will, the varying context of the scenario such as what if that key instrument were failed. How much higher would the human error probability get without that key instrument or whatever. Of course, now you've got to also factor in what is the likelihood of that key instrument failing.

Quite frankly this was done in, I'll call it, not subjective but I'll say a simplified manner. The point is the uncertainty bounds did try to account for variations on, if you will, the ideal state. That was all gone over with the Duke staff. Then they provided us some comments on the spot on some of those as well as then provided some written comments later. We incorporated those comments where we thought it was appropriate.

To that extent it certainly reflects not only the contract or expert's opinions on what those probabilities of failure should be, but also the licensee's feeling on what those probabilities of failure should be through the comment process.

Let me just mention on some of the other plants we are actually like in Palisades I'll just point out that we actually sat down with the licensee and came up with the human error probabilities together as a group through an expert elicitation process.

To that extent we have even done, I think, a little better job on Palisades as opposed to Oconee where the contractors did a first cut and then we had the licensees review and comment and then we changed accordingly where we thought appropriate.

In the case of Palisades we actually sat down together. Licensees and contractors were among the experts and we came up with what we thought the human error probabilities ought to be.

CHAIRMAN FORD: Alan, this whole question of human performance is not my area but you did mention that it was a large input to your overall event tree scenario. When you look at Ed's view graph 7 and 6, are these curves in view graph 6 going to change much if Oconee is a good plant. They are good operators. Presumably there are some bad operators. Will the bad operators markedly move those curves?

MR. KOLACZKOWSKI: I don't think so. You have to recognize that you are asking a question where I'm trying to now predict what are all the other plant analyses going to look like.

CHAIRMAN FORD: Right.

MR. KOLACZKOWSKI: I guess I'll just say this. I'll try to answer it this way and we'll see if that is satisfactory for you. I think certainly today in the year 2000 we are much more sensitive to worrying about PTS than perhaps we were back when these analyses were originally done which is representative more of late '70s, early '80s kind of event in terms of procedures, training, etc.

I think the industry at large has made vast improvements in terms of dealing with potential overcooling events and that is reflected in the way the procedures are written, whether it's a BNW plant or a Westinghouse plant or a CE plant. A lot of improvements have been made in the procedures and the training to be more sensitive to overcooling, to control overcooling events, etc.

Having said that, as a result, I have a feeling that a lot of the secondary kinds of scenarios that can cause overcooling, be they overfeeds to the steam generator, be they secondary depressurizations, etc., I think the procedures today and the training today in large part, no matter which plant we look at -- again this is a presumption on my part but you asked me the question and I'll try to answer as best

I can -- I think are such that we are going to find that the operators are going to take actions fairly promptly to deal with that situation.

Therefore, almost because of through their acts we can make a lot of the secondary kinds of overcooling events go away to the point that the training and the procedures we are going to see throughout the 3 NSSS vendors a consistency in terms of that there is a concern about overcooling, that the operators are trained to address those, the procedures are written to address them promptly.

I would hope that we are going to find across all the plants that one day you will be able to give a considerable amount of operator credit for arresting those events before they become serious overcooling events.

That leaves the primary side. On the primary side, depending on the nature of the event, let's say, for instance, we take small loss of coolant accidents which are going to lead to overcooling situations and we need to inject obviously to deal with the loss of inventory, in that situation there's not much the operator can do, quite frankly.

I mean, the LOCAs happen, the cooling is going at some rate and they have to inject water into the plant. Obviously we do need to worry about throttling that water when we do meet those throttling criteria. That aside, at least during the initial 1,000 seconds in the event, the plant is going to respond the way it's going to respond and there's not much that can be done.

There the operator does not, in fact, at least during the early phases of that accident, play all that significant a role. I guess I'm giving a long answer to your question but I think to the extent that we find that the procedures and the training are reasonably consistent and there is a sensitivity to PTS challenges, hopefully we will continue to show that the operator can arrest a lot of the secondary kinds of problems.

The primary, there are some that you just can't do much about, at least during the initial phase of the accident. However much of cooling we are going to get, that's how much we're going to get and there's really not much the operator can do about it.

The point is that remains consistent regardless of whether we are looking at Calvert Cliffs or Beaver Valley or Fort Calhoun or whoever. The point that that continues to exist, I think we'll see these general conclusions kind of holding. Do I expect vast changes in that curve that we saw in slide 7? My hope is that we won't.

CHAIRMAN FORD: Presumably jumping forward to maybe the very last graph, when the licensee in the future uses this new procedure, not the regular tried 1.153 or whatever the number was, he's going to have to measure his HEP for his plant. No?

MR. KOLACZKOWSKI: I don't believe so. I think we're going to try to by being conservative about where we ultimately set the risk criteria. Hopefully we can make an argument that we are covering all the various plant designs that are out there as well as all the variance that there might be operator actions recognizing, as you said, that there are some different levels in terms of the amount of perhaps attention to this as an event.

There are different training programs, etc. Nevertheless, I think they all have some degree of homogeneity to it in terms of addressing PTF challenge events. I think we will try to take credit for that. Maybe Mark can provide a better response.

MR. KIRK: No. I don't think so.

MR. KOLACZKOWSKI: Well, we certainly aren't going to ask all the plants to do a human reliability test.

MR. KIRK: No. Maybe I would just like to say two words and then expound on them; screening criteria. We are trying to develop a screening criteria in the same sense that we've got one now and accepting the fact that right now it's widely regarded that the screening criteria is the limit and you just give up.

The fact is it is a screening criteria and you can do other things like Reg Guide 1.154. I am reminded of getting letters in the mail from my financial adviser with the big warning that says, "Past performance does not indicate future trends."

Right now the trends are pretty positive that if we continue to go the way of the Oconee plant, that we could be looking at raising the screening criteria by anywhere from 30 to 80 degrees, just to pick numbers out of the air.

Perhaps more importantly, that screen criteria might be based on mean values rather than adding margin terms that we do now. That provides a substantial relief relative to -- you cited Beaver Valley and Palisades that are right now sort of on the brink to provide that much of an increase in the materials screening criteria which, for all intents and purposes certainly for a 40-year license life, take PTS off the map.

I don't have the numbers stored in my brain for 60 years but probably there, too. Again, that is assuming things continue to go the way they are. If somebody crossed that line, then yeah, they would have to do a more detailed analysis and would have to quantify some of these things.

MR. HACKETT: That's what 1.154 would be about. As Mark said, that is not an option that has been a popular option. It may not be in the future.

One other thing I'll just make a comment on. Mark said you just want to be careful since this is a transcribed meeting, Beaver Valley and Palisades being on the brink. That would be at EOL, obviously not right now.

CHAIRMAN FORD: Yes, yes.

MR. HACKETT: And those are significantly in the future for both of those plants.

MEMBER SHACK: When you are comparing with the Oconee PRA, they would presumably screen a lot of these sequences out of their PRA since you don't have an embrittled vessel they don't lead to core damage. Do they include these and then screen them at some point? When do they get cut out of the PRA?

MR. KOLACZKOWSKI: In terms of the type of accidents that can happen or types of challenging scenarios that can happen?

MEMBER SHACK: Yes.

MR. KOLACZKOWSKI: As you see in the diagram right down here in the lower left it says, "No frequency screening," etc. We did not screen in the model at this point on the basis of --

MEMBER SHACK: I was thinking back to the Oconee PRA where presumably they don't consider PTS as a failure.

MR. KOLACZKOWSKI: Oh, in terms of their core damage scenarios.

MEMBER SHACK: They screen these scenarios out somewhere along the way and I was just wondering if they have them somewhere and you've gone back and compared with them or they have screened them out at such an early stage there's nothing to compare with.

MR. KOLACZKOWSKI: I wish in a way -- is anybody here from Oconee? Okay. I'm trying to remember. I don't think that their core damage model -- let me call it that -- probably has any significant PTS scenarios remaining in it. I hope I've said that correctly.

On the other hand, having said that, the PTS scenario work that was done in the '80s is probably in large part what they still represent as, if you will, their PTS risk. Okay? Now, that is essentially going to get updated, I'm sure, by this work.

Now, to what extent that this will get folded into their core damage model, I guess I really can't speak to that. You will have to ask Duke that question. Yeah, clearly their core damage model now certainly does not have a significant portion of it dedicated to worrying about PTS challenges.

This work, I'm sure, especially if they feel like -- I do think at this point they feel like it's a reasonable representation of their plant and their PTS risk. You would hope this will be reflected in their analyses in the future or whatever. Again, I can't speak to that.

MEMBER SHACK: You tried to make this sort of a best estimate, right?

MR. KOLACZKOWSKI: Best estimate with uncertainty.

MEMBER SHACK: More conservative than you typically are in a PRA. On an unlimited budget you would have done it even better.

MR. KOLACZKOWSKI: That is correct. But you're right. The purpose of this was to try to be best estimate. However, reflecting the uncertainties in all of the inputs that go in, and I'll be talking more about the uncertainties shortly. But, yes, it is meant to be a best estimate and not to be needlessly conservative where we don't have to be. That is correct.

MEMBER BONACA: Just a question I have. Regarding the primary system injection they were talking about some sequences where he doesn't have anything he can do.

On the other hand, I mean, if you have a large-break LOCA, clearly they will have a pressure challenge so you will have excessive cool down but you don't have a high-pressure challenge. If you have small-break LOCA, you would be hanging up there in pressure but you don't put in much water. Even feed and bleed sequences, it seems to me, you would have the same situation. This is pretty much self-controlling.

MR. KOLACZKOWSKI: Yeah, to some degree.

MEMBER BONACA: Only found some sequences which are, in fact, a challenge. I was trying to understand it.

MR. KOLACZKOWSKI: That is true. They are to some degree self-controlling. Again, I didn't mean to imply that if we have a loss of cooling action in the primary, well, that's it. We have a major PTS challenge and there is nothing the operator can do about it. If it came across that way, I apologize.

MEMBER BONACA: No, no, no. I was trying to understand, in fact, if the procedures would have to have some warning to the operator, for example, for bleed and feed in which you are intentionally open your PRVs and feed into the system. Even in their condition it's --

MR. KOLACZKOWSKI: It's only going to feed in so fast because obviously the pressure is at a certain pressure so the pump can only pump so much flow, etc. You are absolutely right. I mean, the smaller the LOCA, while the pressure may tend to stay higher, obviously the amount of cooling you get is going to be not as severe as if it was a much larger LOCA.

Then again, on that side, you also have the pressure staying relatively low so that is helping you to some degree. So you are right, there is some self-limiting features as to the way the physics works. What I was trying to imply is that in the first maybe 1,000 seconds of that event, really the operator is not influencing the event very much.

It's going to do what it's going to do and then at some point it begins to throttle back injections so that we don't get into a repressurization, let's say, scenario if the LOCA is small enough. Then the operator is also again influencing what the response is going to be thereafter. But in the first 1,000 seconds of the event, pretty much the plant is going to do what it's going to do.

MEMBER SHACK: Let me rephrase Mario's question in perhaps a different way. Is there some small set of the 108,000 sequences that really dominate the PTS risk?

MR. KOLACZKOWSKI: We believe so. Yes.

MEMBER SHACK: Can we describe -- I mean, are we talking about LOCAs?

MR. KOLACZKOWSKI: Maybe a dozen.

MEMBER SHACK: A dozen?

MR. KOLACZKOWSKI: A dozen of that order.

MEMBER SHACK: They are slow-break LOCAs?

MR. HACKETT: I guess we are preempting. Alan does get to this.

MEMBER SHACK: Okay. Maybe we'll just wait then.

MR. KOLACZKOWSKI: But the short answer is it's primarily LOCAs varying in sizes from very small to what I would typically characterize as maybe a medium break, not the double guillotine 34 or 30-inch line break.

MEMBER SHACK: LOCAs that could actually happen.

MR. KOLACZKOWSKI: Well, I don't know. That depends on your opinion of what could happen.

MR. KIRK: You've quantified the probability of that.

MR. KOLACZKOWSKI: We have quantified the probability of that. Small and medium LOCAs of varying types, relief valves sticking open, that kind of thing on the pressurizer.

CHAIRMAN FORD: Ed, Alan --

MR. KOLACZKOWSKI: Were are going to describe later in more detail what the scenarios are that are dominated.

CHAIRMAN FORD: Okay. We are scheduled to have a break at 10:15. I don't want to destroy your crescendo here. I'll leave it up to you two to decide when we are going to have a break.

MR. KOLACZKOWSKI: I'll tell you what. If you'll let me just go through the next three slides and then I'll be ready to address the uncertainty, maybe that's a good place to do a break first. Is that okay?

CHAIRMAN FORD: Good.

MR. KOLACZKOWSKI: This is just a continuation of the process. I pointed out that we went through an interim step of sort of getting some preliminary results, going to Duke, getting some comments, etc., incorporating those comments where appropriate. Obviously there was a revision of the model, that step to incorporate those and requantify.

I should also point out that the binning that went on that I mentioned before, it wasn't like we took sequences, bin them once and then forever they were in those bins. As we learned more about what was dominating, about what was important to the plant response we may decide that we have to make the binning a little bit more refined than what we have currently done.

We would go back and redefine a new bin, rebin the accident sequences, do a thermal hydraulic run on that bin so we had a representation of what that bin represented, etc. There was an iterative process going on here as binning became more refined such that the bins really were a reflection of what the scenario really was as best as we could fit.

We weren't doing these gross bins like back in the '80s work where they also had 180,000 sequences and they put them into 10 bins. Whether it was a small steam line break or a large steam line break, thermal hydraulically looked like a large steam line break.

We were able to make those distinctions because we could run many more bins computer power being what it is today, etc., and so forth. I just want to point out that the binning kept getting refined, refined through this process.

Finally we get to a final quantification and binning step and out comes eventually for each T-H bin what the frequency is of the scenarios that fit into that particular T-H bin. That is really the primary product, a description of the scenario and what the frequency of that scenario is.

The only other thing I want to point out on both this and the previous slide is that you see a lot of arrows going to sequence definitions from various steps and then T-H input being provided back to the PRA process.

Again, that is just meant to be a representation. It isn't like we took the sequences and in one step provided them to the T-H folks and they ran a bunch of T-H analyses and then we went off and provided that stuff to PFM. This was highly interactive, kept occurring over multiple steps in the process to refine the interactions.

We talked about the fact that this project had to interact a lot between the PRA, or representation of this model, the T-H representation of these scenarios, and then the PFM folks.

This largely is just meant to show you the amount of interaction that was going on between the T-H folks and the PRA folks who had to talk to each other sometimes on a daily basis.

This is just meant to be a very quick summary of some of the major features of the model. On the left-hand side you see the initiating events that we included in the model. Let me just point out that they in large part are the same as what was in the '80s work but, in fact, we did include, I think, one or two things that perhaps the early work did not.

But for the large part there was not a significant difference here in terms of the initiating events that are modeled in this work as opposed to what was done in the early '80s work.

Again, you notice these were looked at both from a full-power and hot zero power condition.

Then over on the right-hand side you just see a quick summary of, again, the four major functions that we are concerned with and the equipment that is represented in the PRA model.

What is the status of high pressure injection charging? What is the status of emergency feed? Is it overfeeding the steam generator? Is it underfeeding? Is it being properly controlled by the operator?, etc. You get a feel for the equipment that is manifested in the PRA model somewhere in terms of the status of that equipment.

Of course, another important part, as I pointed out, is the operator actions. This is, again, probably an over-simplification but, nevertheless, does represent the types of operator actions that are considered in the model some place.

I just want to point out again that if you look at the list you will see both examples of errors of omission but you will also see acts of commission such as under secondary pressure control operator creates an excess steam demand.

As I pointed out, in a loss of heat sink accident where we've lost all feed, they are going to purposely depressurize the secondary side of the plant to try to get condensate feed into the steam generator. Of course, they will try to do that in a controlled manner.

Nevertheless, the operator is inducing to some degree an excess steam demand event by procedure. That act is included as a potential mechanism for how we can get an overcooling situation in the plant. Again, this is a list of the types of operator actions that are considered in the model.

MEMBER BONACA: In the thermal hydraulic analysis did you assume, for example, complete mixing and just bulk temperatures, or did you have also an assessment of azir muthal in the vessel?

MR. KOLACZKOWSKI: I'll let Dave Bessette maybe answer that question.

MR. BESSETTE: Of course, the RELAP code doesn't only treat large volumes with a single fluid temperature so we had to address those questions differently. We did that for combination of the experimental program at Oregon State University in the apex facility.

Also looking back on the various mixing experiments we did back in the '80s in the aftermath of the first PTS study. In addition, we did some CFD analysis associated with the Oregon State work to compare where you do get three-dimensional fluid flow.

So the combination of the CFD analysis and experiments we were able to conclude that there are no substantial azir muthal or circumferential temperature variations in the downcomer adjacent to the core region.

MEMBER BONACA: So you didn't apply any multiplier factor or anything of that kind? I mean, you didn't have to do that?

MR. BESSETTE: That's right. We found a simple temperature boundary condition to pass on to the PFM people to sufficient and adequate.

MEMBER BONACA: Okay.

MR. HACKETT: As we had suggested, this is a good point to take a break.

MR. DUDLEY: And during the break I would like to check the people who are on the bridge line so if they would stay there until we chat. Thank you.

CHAIRMAN FORD: Okay. I hereby recess until 10:30.

(Whereupon, at 10:14 a.m. off the record until 10:34 a.m.)

CHAIRMAN FORD: Okay. I'd like to call us back into session. Since Graham Wallis will not be here until lunch time, I think Ed and I have decided to swap the EFM and the thermal hydraulic sessions to get a good reviewing.

MR. KIRK: It works for me. I'm not sure Dave would like it.

CHAIRMAN FORD: Okay.

MR. KOLACZKOWSKI: Shall I go ahead?

CHAIRMAN FORD: Yes.

MR. KOLACZKOWSKI: Okay. You've heard at least the major aspects of the modeling process and, if you will, at least some of the main features of what is included in the model in the way of the functions of the equipment that, therefore, was relevant equipment that we need to worry about, and at least a quick overview of the operator actions that we tried to consider.

I would like to now change focus a little bit and talk a little bit about the uncertainty treatment in this aspect of the entire analysis. This first slide here, No. 21, is meant again just to keep in mind in terms of what is the PRA portion of the analysis trying to produce. What is its product and ultimately working towards estimating a thru-wall crack frequency on the vessel.

I have a statement of that. Hopefully a somewhat succinct statement of what the PRA product is trying to produce and, therefore, what uncertainties we need to worry about.

Let me go ahead and -- I apologize. I'm going to go ahead and read the statement and try to focus on the key words. We are trying to come up with the frequencies of a wide range of representative plant responses to plant upsets. I'll call those plant responses the plant upset scenarios.

We are trying to get the frequencies of a wide range of scenarios that are each described by some set of thermal hydraulic curves in terms of pressures and temperatures and so on which occur as a result of mitigating equipment successes and failures as well as operator actions, that result in various degrees of overcooling of the internal reactor vessel downcomer wall.

Keep in mind here that we really have as an output unlike the typical core damage PRA type models where we sort of define a state of core damage and then we say, "Okay, does this scenario lead to core damage or not."

In this case, we have a much more complex situation in which you cannot define a single state that says this is overcooling or this is a PTS challenge and this is not. We actually have degrees of overcooling and, therefore, degrees of PTS challenges. In reality we don't have this binary output of either it is core damage or it's not.

What we really have is an output that says this represents some amount of overcooling which may or may not be a serious challenge from a PTS standpoint. This scenario represents yet a different degree of overcooling, maybe worse, that represents perhaps a greater challenge.

That is why you have to go through all this binning and separate those, etc. It isn't that all the scenarios are being binned to a single output that says core damage. These are degrees of PTS challenge that makes the process much more complex as a result.

Nevertheless, we are trying to come up with frequencies of scenarios that represent potential PTS challenges and they are various degrees of overcooling in the plant that could occur.

Now, in trying to come up with that product, of course, we build a model to do that. As a result, the model represents, to some degree of course, sources of uncertainty in that how accurate is the model really representative of the real world. We'll talk about that in just a moment.

Then even given the model which

represents, if you will, these scenarios that are going to go on to the rest of the analysis and will be analyzed both thermal hydraulically and then where it is a serious challenge also model from the fraction mechanics point of view.

Even given the model then, of course, the other important product of this portion of the analysis is to come up with a frequency of scenarios each of which is an overcooling situation. Obviously there is uncertainty in what those frequencies are and we'll talk about that.

CHAIRMAN FORD: Alan, is it a given in all those scenarios that the pressure remains constant?

MR. KOLACZKOWSKI: No.

CHAIRMAN FORD: So is that not also the criterion?

MR. HACKETT: I was going to say you could probably have added to Alan's definition overcooling and potential overpressurization.

CHAIRMAN FORD: Maintenance of pressure.

MR. KOLACZKOWSKI: That is probably true.

CHAIRMAN FORD: Okay.

MR. KOLACZKOWSKI: I did not intend to mean that we did some sort of screening or didn't represent certain pressure situations or not. I did not mean to imply that.

Next slide. From the modeling perspective, again, what the PRA and the way PRA models the world, if you will, is that each scenario represents a collection, if you will, an interaction of events.

The plant is sitting there at some stable state, be it either at full power conditions or hot zero power conditions, and then along comes some sort of an upset, an initiating event, as we call it, which causes a transient and a subsequent response to be required in terms of the plant response.

The plant response depends on what the status is of various equipment, whether it be the status of emergency feed, whether it be overfeeding or being controlled, what is the status of the term by-pass valves, have they properly throttled or is one stuck open which causes an overcooling situation, etc., depending on the nature and the status of the various equipment that is relevant to a potential overcooling scenario or repressurization scenario.

Depending on the status of that equipment and the subsequent operator actions, you could view each scenario, if you will, as an initiating event, some status of equipment, and some status of operator actions which combine together either does lead to a overcooling type of scenario and it leads to a potential PTS challenge, or a very controlled non PTS kind of an event. That's the way the model essentially is representing the real world.

I want to point out, and I mentioned earlier, that the model is for the most part time independent. It doesn't necessarily know when these things are happening. It's just modeling the world in a way that says this initiating event has occurred. Either the valve has stuck open or has not, but we don't necessarily, at least in the first cut of the model, say anything about when the valve stick open.

Now, it does turn out that where that is important in describing the scenario and the potential amount of overcooling, etc., we do go back later and add timing into the situation. Again, I think the example later on will demonstrate that hopefully very well.

I do want to point out that there was a place where timing was introduced into the model right from the start, and that is when the operator action occurs. Recognize, as an example, we do have a secondary depressurizaton event going on, some sort of excess of steam demand.

By procedure one of the things that the operator will be embarking on once they detect that situation is to isolate that faulted steam generator. How quickly the operator performs that in large part will dictate how much overcooling we get. The more delayed the actions, the more overcooling we will have. Whereas if he takes those actions very promptly, we'll hardly have any kind of overcooling event at all.

What we did do and, again, the example will demonstrate this, is that we did in the model from the start take this time continuum and break it into very discreet time points in which we said let's address and quantify sequences such that we are quantifying.

What's the likelihood that we have this initiating event, some status of equipment states, and the likelihood of the operator takes whatever the action is supposed to be within, let's say for the sake of argument, 10 minutes into the event.

If the operator does successfully perform those actions, maybe the event is for all practical purposes over. Maybe not even a very serious challenging event and would go into, if you will, a very benign overcooling bin.

On the other hand, in the model we also say what if he didn't take that action in 10 minutes? What's the likelihood that he would have taken it by 20 minutes into the event and we reassess a probability for that likelihood.

Now, obviously if the operator does not perform the action within 10 minutes time but does perform it within 20, he may have moderated the overcooling situation ultimately, but obviously as conditions got much worse before that arrest took place so that would perhaps go into a different T-H bin.

We would actually perform a thermal hydraulic analysis of the same scenario with the operator taking the action in 10 minutes and then the same scenario with the operator action occurring in 20 minutes and we put those in two different bins.

MEMBER BONACA: Just for clarification, you're talking about time was of an element but if the cooling occurs at the rate which is less than the normal cool down rate, you will not consider it other ways.

MR. KOLACZKOWSKI: That is true. Generally if a scenario was cooling down at less than 100 degrees per hour, that was screened from --

MEMBER BONACA: You just take it out.

MR. KOLACZKOWSKI: We still went ahead and kept it in the model and calculated a frequency for it. Ultimately none of that, primarily because of the thermal hydraulic response, as you say, would be a slow cooling situation was ever passed on to the fracture mechanics analyst who analyzed it.

MEMBER BONACA: You are using the word overcooling.

MR. KOLACZKOWSKI: That is correct.

MEMBER BONACA: To some degree, I mean, you still -- the main concern is temperature gradient across the wall or the vessel.

MR. KOLACZKOWSKI: That is correct.

MEMBER BONACA: That is a time dependent function. Even for those overcooling situations, which are benign, you are saying, at what point do you introduce the dependency so that you can eliminate some of those, or do you introduce the dependence at all?

MR. KOLACZKOWSKI: Well, in this -- I'm not sure I follow your question. In this portion of the analysis we did not, for instance, eliminate the sequence from being quantified.

MEMBER BONACA: Okay.

MR. KOLACZKOWSKI: Even if we saw that it would be a fairly benign or slow cooling ramp, we didn't at that point say we're not even going to figure out what its frequency is. We kept it in the model and so many of those 180,000 sequences are, in fact, I'll call them relatively benign cooling scenarios but we went ahead and calculated their frequencies anyway so they are in the model.

MEMBER BONACA: So this benign definition you're using is more qualified in the sense that it is a slower cool down rate. There is still a cooling respect to the cool down rate but it's not such a challenge. I mean, you haven't attached any quantitative definition of what it means.

MR. KIRK: This is Mark Kirk. If I could just interject something that came up in our discussions yesterday that is perhaps relevant to point out here. Yeah, we made some -- well, I was going to say a priori assumptions but assumptions based on our previous understanding of PTS and we've already talked about them. Sequences where the mean temperature didn't fall below 400 sequences and it didn't cool at a rate in excess of 100 degrees fahrenheit per hour were not passed onto PFM.

Having said that, there were some

sequences that were just over that line that were and we have quantitative information on them and those invariably came up with zero conditional probability failure. We haven't directly tested our screening but we no that those that just made it over the bar didn't matter anyway. I think that makes me feel good at least.

MR. KOLACZKOWSKI: Yes, it somewhat validates the screening that we did do.

Okay. So I talked about the fact that there is one very important aspect of timing that is introduced in the model from the start, and that is the timing of the operator actions. At some point we had to say if they do it by X minutes, we will make the assumption that the crew never does it. That would be the worst of conditions, if you will, that the operator does not arrest the situation at all.

Quite frankly, we use judgement as to when to pick those times depending on how fast the ramps were going down, when did we think it would matter. Again, a lot of that came from our prior knowledge of PTS conditions as to what were some reasonable times to pick that we would probably put the sequence into different bins and then pick those accordingly and came up with operator failure probabilities for the various times, had to account for dependencies, of course.

But at some point we said we are going to assume if they don't do it by this time that they will never do it and, therefore, that would be the worst of conditions and came up with a probability of the scenario actually going that far.

The other point I wanted to make -- the last two points is, again, for the most part modeling uncertainties were really not quantified per se. In other words, we built a model of the plant response in terms of the various statuses of plant equipment, what initiators could occur, whether operator actions occurred or not, etc., and we said that's our representation of the world.

It's a binary model for the most part as PRA normally is. However, having said that, what this third bullet is meant to imply here is that if we did have to worry about something additional that is not normally treated in a typical PRA model of the world such as when does the SRV reclose and, therefore, it could potentially repressurize the entire system.

We did go and address that as, if you will, a final step in the process. The model was initially built without worrying about the timing of the SRV reclosure. We just calculated what is the probability it will reclose. Then when we recognized that was a high enough frequency to worry about, we went back and addressed what would be the timing of that reclosure.

What is the probability it would reclose early versus what is the probability it will reclose late because that represents a very different thermal hydraulic response and we have to put those two events into two different bins.

We did make modeling changes to treat uncertainty of timing, for instance, and other factors but we only did it where it looked like it was going to be important, which leads to the final bullet.

Therefore, all other modeling uncertainties were not explicitly quantified. When we talk about the uncertainties ultimately of the frequencies that are passed on to the PFM portion of the analysis, much of the modeling uncertainties are not really quantified unless we deemed that it was important to do so and then we adjusted the model and came up with probabilities to try to address that aspect of the modeling uncertainty.

MEMBER BONACA: So you really leave it to the PFM analysis to make a decision whether or not a sequence should be eliminated? What I mean is that the same input that -- here you are using the frequency for the sequence and then the sequence is input to the thermal hydraulics that comes up with a certain profile of pressure and temperature versus time.

MR. KOLACZKOWSKI: Correct.

MEMBER BONACA: And that may say for this transient the probability is zero or very low so, therefore, you eliminate the sequence. That is really how you go through the logic process.

MR. KOLACZKOWSKI: That is true.

Okay. Now, the other aspect I mentioned was even given the model we often now calculate the sequence frequencies and there are uncertainties associated with that. I want to address a little bit about how we look at that from an uncertainty standpoint.

Again, just a reminder that each scenario is an interaction of essentially an initiating event along with the status of various equipment and along with the status of various operator actions which may or may not occur, or may occur at some time.

Therefore, you can think of each scenario -- oh, by the way, the model, therefore assumes that those events are, in fact, random events. We don't know what initiating event is going to happen, when it's going to happen, although we do try to calculate a rate of its occurrence on a per year basis, for instance.

Again, we model the various states of the mitigating equipment. But in terms of when that initiating event occurs, how the mitigating equipment is going to act whether it's going to fail that particular time or whether it's going to succeed, that is treated as a random event with some failure probability, some failure rate associated with the occurrence of that failure.

The same thing with the operator actions. Think of it as another component of the system at some failure rate and the operator may or may not perform the action quickly or in a delayed fashion or whatever. That is all treated as random events.

Therefore, the occurrence of each scenario is a random event. There are many ways to challenge the vessel from a PTS viewpoint. We talked about 180,000 sequences from benign to various serious challenges. Which challenge is actually going to be the next one we don't know.

The occurrence of each scenario is really a random event and think of it as nothing more than the probability -- excuse me, than the multiplication of the frequency of the initiating event which is a random event times the probability of the equipment response times the probability of the operator actions all of which are random events so the scenario ends up being a random event.

I want to jump to the next bullet first. Therefore, the various scenarios and their frequencies are really characterizing, if you will, the aleatory uncertainties, the randomness of how we may get a PTS challenge.

You will see later that what Terry and the Oak Ridge folks do is that they sample from each one of those sequences to ultimately come up with this combination of sequence frequencies combined with the conditional vessel failure probability to come up with a distribution for what is the thru-wall crack frequency. They are sampling from each of those possible scenarios in terms of the way a PTS can challenge.

When they are doing that, what they are really doing is they are quantifying the aleatory aspects, the randomness of how PTS challenge can occur. Those scenario definitions and each one of those frequencies really represents the aleatory aspect of the uncertainty.

Now, along with the fact that each scenario is this multiplication of the frequency of initiating event and so on as you see in the equation there, obviously what we're trying to do is predict what we think is the true failure probability of the equipment, what is the true failure probability of the operator, etc.

Therefore, what is the uncertainty on that "failure rate" or that failure probability. There we are really addressing the epistemic uncertainties. We don't really have the knowledge to know what the true failure probability is for a turbine by-pass valve to stick open.

Nevertheless, we make an estimate as to what that is and we try to assess an uncertainty bound as to what that probability is. When we are doing that, we are addressing the epistemic uncertainties.

Those epistemic uncertainties, coming to the last bullet, are indeed propagated through the analysis using a Latin hypercube sampling approach.

Essentially when we go to calculate the frequency of a scenario, we are putting in distributions for each one of those inputs in that equation where that distribution represents the epistemic uncertainty on that probability or on that initiating event frequency.

Then we sample from those distributions to get a distribution out as to what the frequency of the scenario is. At that point what we're doing is propagating the epistemic uncertainties through.

Unless there are questions on this, I'll move on to the next slide.

Okay. That's a quick overview of how the uncertainty is being handled. Now, I do want to address just briefly what is different between this work and the previous 1980s work. What are sort of the dominate things that are different.

What you see, anything here in the red -- and I apologize for those that have a black and white copy of this, but what you see in red are those things that tend to -- would tend in general to increase the PTS risk from what was done in the earlier work.

What you see in green are those things that we have changed from the earlier work that would tend to decrease the risk from the early assessment. You can see here, and I mentioned before that pretty much the initiating events and, for the most part, a lot of the scenarios that we have modeled were in large part covered by the earlier work.

However, we did do what is characterized here as a slight extension of some of the possible scenarios. In fact, you will find out that the example one we're going to go through, one of the more dominate scenarios, is a scenario that was not included in the original analysis.

There has been a slight expansion of the scenarios that we analyzed relative to the '80s work and also some of the treatment of the support systems like instrument air and component cooling and that kind of stuff.

Those in general tend to increase the risk. The more scenarios you add, the risk is going to go up. If you fail to analyze a particular type of scenario before and we're analyzing it now, that's going to generally add to the risk.

On the other hand, by using the latest equipment failure probabilities, initiating event frequencies, I know the staff is well aware of the fact that if we look at the number of plant trips that we're having these days on a per year basis versus what we were having back in the '70s, we used to have six A transients a year that could potentially lead to overcooling events.

Now most plants are operating at half a trip per year kind of rate. Obviously that kind of information being reflected in the current analyses is tending to decrease the overall PTS risk.

The detail HRA. I pointed out the fact that in the early work for the most part there was either very simplified treatment of the human or, in fact, in many scenarios, little to no treatment at all or credit was given to operator actions. We are doing much more now to credit the operator actions where we think it is appropriate to do so.

I think we went through a very detail expert elicitation licensee involvement process to try to come up with best estimates and error bounds on those human failure probabilities for the scenarios. You can see some features of the HRA process here which I won't go through unless you have questions.

Again, the last sub-bullet there, consideration of acts of commission that could exacerbate cooling, again, those things would tend to increase the risk where you include that.

Very important point, much more binning than we ever did before. I think that is something that should not be overlooked. Never mind the values and the numbers that went in and so on and so forth.

Just the fact that rather than having to put everything into 10 bins, we could put things into 100 bins has removed a lot of the conservatism in the original analysis because we could do a much more finer scenario definition to bin process such that the bin really represents a few scenarios that are really quite representative of that bin and didn't put in, for instance, small main steam line breaks into a bin that has large steam line breaks and then treat it as large steam line break. We are able to avoid those kinds of issues.

Obviously the uncertainty analysis itself, just doing it probably in general does tend to increase the risk -- I've got to be a little careful here -- in that from a best estimate point of view when you do the uncertainty analysis you are accounting for those tails out there that do tend to bring the mean up than where they would be if you were just doing a best estimate point estimate analysis.

You do have to keep in mind that the original '80s work, however, was conservatively done so from that aspect, I guess you would make the argument the uncertainty analysis probably isn't increasing the risk.

If they had added too much conservatism

into the original analysis. But I'm just saying from a best estimate point of view if you were just doing a point estimate analysis, you do have to keep in mind if you do a full-blown uncertainty analysis, you are going to get some tails out there which are going to tend to increase your best estimate of the mean a little bit than if you were just doing a point estimate mean kind of evaluation. I've got to be a little bit careful about how I characterize that last bullet.

Unless there are questions there, those are some of the major differences, I think, between the work that we've done on the PRA aspect of this versus what was done in the early work.

Is that my last slide or is there --

MR. KIRK: You have one more.

MR. KOLACZKOWSKI: This is just meant again to be a cartoon as to what the major products of the PRA process are that then goes into the rest of the analysis. There are really two things that are coming out, although one of them is really contributed by the T-H folks, and that is what is coming out of this whole process is a scenario that is being described by a set of thermal hydraulic curves.

Then there is a frequency associated with that bin or that scenario, if you will. That frequency is being described by distribution, by a histogram, which is representative of the epistemic uncertainty in the frequency of each one of those scenarios.

You see there we basically describe that distribution using 19 quintal levels. There was also a 95 percent confidence interval put on each quintal value. That information along with the T-H curves that represent that scenario is what was passed onto the fracture mechanics people for them to do their thing.

Last slide.

CHAIRMAN FORD: Sorry. I'm being a bit slow here.

MR. KOLACZKOWSKI: That's okay.

CHAIRMAN FORD: You see the density, that --

MR. KOLACZKOWSKI: That's a probability of distribution function of the -- that's our belief on what the true frequency of the scenario is.

CHAIRMAN FORD: So that distribution code is for a specific --

MR. KOLACZKOWSKI: T-H bin.

CHAIRMAN FORD: Okay.

MR. KOLACZKOWSKI: T-H bin. Remember, it may have some multiple scenarios in them. Nevertheless, we think that they are all so similar that they can be represented by one set of T-H curves. Then essentially what you have to do, if you will, is add up the frequencies of all the sequences that are in that bin to get the frequency of that bin.

The frequency of that bin is ultimately represented by a distribution which we then broke into 19 quantiles, etc., for descriptive purposes so that the fracture mechanics people could then go and sample from this distribution at the same time that they're sampling from the output of the PFM code to put together to get a thru-wall crack frequency in its distribution. I don't know if I cleared that up or not.

That is the uncertainty on what the true frequency of the bin is. You can describe that histogram by a mean 95 percentile, a 5, etc., so forth, and that's our description of the frequency of that.

CHAIRMAN FORD: Okay.

MR. KOLACZKOWSKI: I guess a couple of things I would like the community to walk away from the PRA aspect of this in terms of the modeling we did and the uncertainty treatment, I guess these are the major points.

We feel that we have modeled all the relevant initiators functions and equipment of concern that through the status of those could represent some sort of a potential PTS challenge. I keep saying this, I know, but operator actions plays a very key role in the arresting of many overcooling events.

To model the situation without operator action would obviously not do any justice at all and we would be back to the '80s work that gave very little credit to operator actions. I think we have tried to reflect that credit appropriately based on current day standards of procedures, training, and so on and so forth.

CHAIRMAN FORD: Just to make sure I understand, if you go back to VG23, I can understand what you have done in VG23.

MR. KOLACZKOWSKI: Okay.

CHAIRMAN FORD: I can understand the epistemic actions. I can understand the physical reality of what is happening there. The aleatory uncertainties don't mention that at all. How much is that going to affect your conclusions? I'm going to assume these are things that are completely random so how do you take that into account? Was it just --

MR. KOLACZKOWSKI: If I can go back to the overall diagram which would be like No. 7 and 13. When the [CPF] * [fr] takes place, remember we are providing to that circle sequences and their frequencies.

CHAIRMAN FORD: Right.

MR. KOLACZKOWSKI: Each frequency has a distribution which is representative mostly of the epistemic uncertainty and what the true frequency is. Although, quite frankly there is some aleatory probably buried in that that is not separated but is largely represented in the epistemic uncertainties. Each sequence, and there's a lot of sequences, we don't know which sequence is going to happen that will represent a PTS challenge.

A collective set of sequences is representative of the randomness of how the next pTS challenge is going to occur. The PFM folks when they do this combination in the circle, they are picking a frequency value from every one of the sequences that could represent a PTS challenge when they do this combination of [CPF] * [fr].

That is the place where the aleatory aspect of this; that is, the randomness of how the sequence is going to proceed gets factored into an estimate of the yearly frequency of thru-wall crack. Does that answer your question?

So that's where the aleatory is really -- the aleatory really doesn't get treated until essentially the last step in the process but we are propagating the epistemic all the way through all the time. Does that help?

CHAIRMAN FORD: Yeah.

MR. KOLACZKOWSKI: I think I was on that last slide if there are no other questions. Again, I talked about we think the model is a pretty good representation. Again, modeling uncertainties for the large part, though, are not quantified except for where it's important.

We did at the end of the process have to go in and adjust the model to treat those, but the frequency uncertainty, the epistemic uncertainties are propagated all the way through. We determine or put a distribution on each one of the inputs, get a distribution on the output, propagate that all the way through to get a distribution on the frequency output, thus described ultimately as a histogram.

That is sort of a summary, if you will, of how the uncertainty is treated. The aleatory ultimately gets captured in that very last step. I think that hopefully will also become clearer as we get to that portion of the presentation. Does that help? I think that ends my --

MR. KIRK: That's your last slide.

MR. HACKETT: Any other questions for Alan?

CHAIRMAN FORD: When we go through Oconee we'll go through this again step by step.

MR. HACKETT: Right. Exactly.

MR. KOLACZKOWSKI: This is sort of an overview of the generalization. As we get through the Oconee and the example, hopefully some of this will even crystalize some more.

MR. HACKETT: As Peter mentioned earlier, we are going to look at another departure from our plan here. To accommodate Dr. Wallis' arrival we are going to now go into the probabalistic fracture mechanic aspects of the program in more detail. Mark and I talked and hopefully we can try to get at least most of that in before the lunch break. That will be our goal anyway.

MR. KIRK: It depends on how many questions Dr. Shack has.

If you go to view graph 60 in your

handout, that is the beginning of the PFM part of the talk. So again, you see the most frequently used slide in the pack. We are now going to focus on expanding the probabalistic fracture mechanics process.

As I indicated, behind each of these blue boxes is sometimes a frightening level of complexity. Now what I am showing you is what is inside the PFM box. This is something like those Russian dolls. You keep taking them apart and you get more and more and more. You can either lose yourself or make a career in it, however you want to think about it.

This slide illustrates the first doll inside the box marked PFM. It's got both a material resistance side which is shown down at the bottom of the slide, and applied driving force side which is shown off to the left. We just worked through some of the steps in this. I'll be discussing the irradiation shift model, index temperature model, and fracture mechanics model in more detail.

We start off with data concerning chemistry, fluence, and, of course, temperature estimates from thermal hydraulics. That feeds into our radiation shift model. From that we estimate what the affect of irradiation at a particular temperature for a given length of time is on a material having a particular chemistry.

That tells us how much irradiation damaged our material. We add that to an un-irradiated index temperature which tells us where the toughness data was before. Irradiation started and that gives us an irradiated index temperature which we combine with a reference fracture toughness curve to get our fracture toughness resistance. Now we know in very general terms what the material can take.

To define the degree of challenge, we go over to the stress intensity factor model which combines thermal hydraulic inputs, design variables, physical properties like modulus, Poison's Ratio, things like that.

And, of course, flaw data to get the applied stress intensity factor. A comparison of those two which is done in the code -- we'll show you the mathematics of that probably tomorrow -- allows us to estimate our probability of crack initiation and probability of thru-wall cracking.

Going through this process, we focused our model development and uncertainty efforts in the areas that I've highlighted now, for those of you looking on the screen, in yellow and in orange. It is relevant to point out here, because I don't have another opportunity to point it out, that certain things in this model have been taken as deterministic.

For example, design variables physical properties. This is not to say that the elastic modulus of the material is not variable. It is to say that when compared with other variabilities like fracture toughness and embrittlement, it's about as constant as the elevation in Champagne, Illinois, if you've ever been there.

Design variables like the vessel diameter are also treated as deterministic so we have made some judgements at the beginning of this model and decided to treat some things deterministically.

What I would like to do there are now two parts to this presentation. There's a discussion of the model development and uncertainty in the fracture toughness and embrittlement model that is highlighted in yellow. I'll go through that first. Then I'll discuss the uncertainty treatment in the flaw data which is highlighted in orange.

Just to benchmark ourselves for the fracture toughness and embrittlement models in terms of where we are and what we need to do, we start off with our existing toughness model, our existing embrittlement model, what we use today in 10 CFR 50.61 to estimate the material state of the plant.

In all of this nontoughness data meaning data from Charpy specimens, NDT specimens is assumed to represent toughness data. When we get into uncertainty space, that presents us with some very interesting challenges. That assumption has led to uncertainties being treated implicitly and uncertainty types between aleatory and epistemic being mixed into a spaghetti bowl.

We needed to take that all apart in order to get this into a PRA framework because of the constraint that we stated at the beginning, or the intended constraint that we would like to come out of this with a revised PTS screening criteria that still relies on NDT and Charpy data. That was the hope. We believe we have achieved that but in order to deal with uncertainties, we need to take this apart yet further.

This slide discusses the constraints and fundamental assumptions that we have gone into, the fracture toughness model with one, which I stated before, is that we would like to come out of this still allowing the licensees to assess the state of their plant based on the nontoughness data that they now have.

We need to somehow express that nontoughness based model in the PRA context. Also we retained the linear elastic fracture mechanics basis of FAVOR. It would, of course, be possible to construct a version of FAVOR based on elastic plastic fracture mechanics. However, the nature of the PTS challenge rather says that a linear elastic model is appropriate so we've stuck with that.

Looking now at the process we've used in the toughness embrittlement models for building the models and characterizing uncertainty, I would like to just mention at this point, or I should say acknowledge the significant impact that the industry provided here through the EPRI MRP and their funding of their contractor, Marjory Detician, who was largely responsible for developing this whole process that enabled us to do a very good job of characterizing the uncertainties in a way consistent with PRA. Without that help from EPRI and Dr. Detician this would look much different.

So what we started with were two things. One was what we've been calling a root cause diagram which is really nothing more than just a graphical description of a mathematical process. The benefit here is it allows us to depict very clearly what our current process is.

For those of us that thought we understood the 10 CFR 50.61 process and said, "Oh, that's easy," and then a month later said, "Boy, that was educational," it really allowed us to identify very clearly where the uncertainties were, or are I should say, where the judgements are.

If you can't identify them, you can't quantify them. That was our starting point. That told us where we needed to work. But we've also acknowledged at the start that because we are trying to stick with nontoughness based data to predict toughness data, we've got an estimate.

We've got a model and to assess uncertainty relative to that we need something else. We need something to compare to. What we focused on developing is what we referred to as physically motivated best estimate models.

If you only had data and you've got nothing else, then you've got no way to tell if your data is right or wrong. What we tried to do was to use our physical understanding of defamation of fracture to tell us how the data should behave and then calibrate those models based on available data.

This also allowed us considerable insight into classifying uncertainty type as being either aleatory or epistemic. It really made quite clear those uncertainties that were just inherent properties of the material. No matter how much funding you had and how well you did your experiments, you would never get any better and those situations where the reverse was true.

When we combined these two things, the identification of uncertainties, our ability to classify them relative to a physical model and, indeed, our ability to quantify them relative to that model, that provided us with a means to account for uncertainty.

Down there in the red print on the screen you see sort of our mantra, "Fracture toughness data is truth. Fracture toughness data is truth." We kept getting back to that, that we certainly use the physical understanding to get insight into how the data should behave and that in many ways told us how we should be looking at the data but we kept referencing back to the idea that we are trying to predict K_{Ic} data or K_{Ia} data.

This all enabled us putting all this together to recommend a FAVOR procedure for both a model and an uncertainty treatment for all of these key parameters in the toughness equation, the RT_{NDT} index temperature, the T_{30} shift, the RT arrest shift which gives us the distance between the initiation and the arrest curves and, of course, K_{Ic} and K_{Ia} themselves.

What I'm going to do now, and if my memory serves me, we briefed the committee in detail about this before, is to just go through and hit the high points on each of these models. We have some backup slides that we can refer to if necessary. Certainly there is the draft NUREG that, I believe, was passed on to you.

There are three sequences of a few view graphs here that I'm going to step through. The first step along the way is the model of initiation fracture toughness. As the schematic on your screen shows, there are two parameters in this initiation model.

There's, of course, the toughness value K_{Ic}, and then there's the index temperature RT_{NDT}. We need to have a model and quantify uncertainty in each of these. As I pointed out, to do that we need some independent arbiter of truth because we recognize that RT_{NDT} is most certainly not truth.

So this slide depicts the end result of

our best estimate model. Our understanding of the physics of defamation and fracture suggest that we expect, and indeed observe, a common temperature dependence in the variation of toughness with temperature that is common to all ferritic steels.

That temperature dependence depends only on the lattice structure and so we don't expect it to change with chemistry. We don't expect it to change with the radiation in the range of exposures that we get in reactor vessels.

CHAIRMAN FORD: Sorry. Did you say you wouldn't expect it to change with chemistry?

MR. KIRK: No, not unless the chemistry makes the steel not ferritic. As long as -- I'm sorry. As long as the lattice structure is body centered cubic.

CHAIRMAN FORD: There's a master curve of K_{Ic} versus temperature for all --

MR. KIRK: For all ferritic materials without question. If you change it from being ferritic, it won't follow that master curve.

MR. HACKETT: I guess we don't want to go too far with this. Nothing, I guess, in science is without question.

CHAIRMAN FORD: No, no.

MR. HACKETT: I think what Mark is going to go through is a lot of empirical data backing up those statements and a lot of analyses.

MEMBER SHACK: It does shift with transition temperature.

MR. KIRK: Yes. The effects of irradiation, and I get to that here, when you look at what irradiation does to the material it, of course, hardens the material which leads to a shift in the fracture toughness.

MEMBER SHACK: But even without irradiation you have a shift.

MR. KIRK: You have that index temperature.

MEMBER SHACK: So the master curve is indexed to a temperature.

MR. KIRK: That's right.

MEMBER SHACK: So all ferrictic steels are not the same.

MR. KIRK: No. Absolutely not, but once indexed to a temperature, they all follow a common variation of toughness with temperature.

MEMBER SHACK: Yes. That's correct.

MR. HACKETT: And maybe to pursue it further, because I can Peter still musing over this, and it's been something that has been worked on for a long time. I think Mark answered it correctly. The chemical composition influence would be through its impact on crystal structure.

Obviously there are influences of the chemical composition on the end state crystal structure, but for most ferrictic steels under these conditions, that variability is not there. It could be in other circumstances but not in the particular problem --

MR. KIRK: Not in these conditions. So what we see is certainly for the range of conditions that we're interested in here, the steels, the exposure conditions, the ranges of chemical release, our physical understanding suggest that we should expect to see, as we said, once indexed a common temperature dependence and, indeed, a common scatter to all ferrictic materials of interest.

We find that the effects of irradiation, since they don't affect the crystal structure, and since they don't affect the micro-defects that are leading to the scatter in the K_{Ic} or K_{Ia} data, we effect irradiation to simply produce a shift in that transition temperature as is illustrated by the two graphs which have probably been shrunk too small.

These physical expectations are backed up by, indeed, mountains of data. It's been said that the master curve hasn't yet met a ferrictic steel that it doesn't like and I personally haven't found one.

I should also note that a couple of years ago I made it a quest to find one believing, as any good experimentalist did, that life just can't be that simple. I haven't found one yet. We've adopted as our best estimate to which we will compare are RT_{NDT}, the master curve method, with the T_0.

I'm sorry, Peter. Go ahead.

CHAIRMAN FORD: I must admit I'm still pondering over this master curve business and invariable. If you go back to VG 64, you've got on the left-hand side two examples of two steels, one from Midland and one from HSST program where they are markedly different from this master curve.

MR. KIRK: Well, actually, that's the K_{Ic} curve, but go on. That's the K_{Ic}.

CHAIRMAN FORD: My point is if you just look at it as a reference curve, those gold lines you see there, the same curve.

MR. KIRK: Yes.

CHAIRMAN FORD: Big difference. Two sets of steel. Different T values which is fine. You would expect the difference in the half T and the 3 or 4 T. Big difference.

MR. KIRK: In fact --

CHAIRMAN FORD: What physically is the reason for that?

MR. KIRK: I think -- maybe I'm not understanding what you're saying but, in fact, if you pass the master curve through each of those data sets, which would mean you would have -- if you assume that was the master curve, which it isn't, but if you assume that it was and you indexed it to T_0 and you then did a statistical test to see how many were above the line and how many were below the line, and your statistical expectation would be 50 percent above and 50 percent below, you would find no reason to reject the hypothesis.

CHAIRMAN FORD: So T_0 on your slide VG 69 is not the same as RT_{NDT}.

MR. KIRK: Absolutely not. No.

MR. HACKETT: I was going to add the Midland case was particularly an extreme example illustrating the problems you get into with RT_{NDT} indexing which is one of the things we had attempted to remove from the analysis.

MR. KIRK: Yes. Certainly T_0 is not RT_{NDT}.

If that helps, I should have said it a lot sooner.

CHAIRMAN FORD: I thought I was a metallurgist. I'm really showing my ignorance here. Physically what is T_0?

MR. KIRK: A committee decision. It's physically nothing. It's simply the temperature at which the median toughness is 100 Mega Pascal root meter. That sounded flip but it's the God's honest truth.

CHAIRMAN FORD: Okay.

MR. KIRK: The intention was you wanted to pick a temperature that is sufficiently above the lower shelf. Well, first off, that you're not getting into twining behavior. Secondly, that you've got enough slope that you can make a reliable experimental measurement, and you want to get it far enough off the upper shelf that you're not into terrine. It could have been 80 or it could have 120. Why we picked a temperature of 100 Mega Pascal root meter to combine with a reference thickness of one inch I'll never know.

MR. HACKETT: I think in short we could devote an entire meeting to the master curve easily.

CHAIRMAN FORD: I'm rapidly coming to calibration.

MR. KIRK: Okay, but it's -- while we are on this point, the benefit of the master curve method, relative to RT_{NDT}, of course, is that here the index temperature is defined consistently based on toughness data for all seals. The T_0 is always the temperature at which the median toughness is 100 Mega Pascal root meter.

Forgetting all the physical arguments, if you wish to, it's no huge surprise that T_0 indexes toughness data better than RT_{NDT} which is clearly not toughness. The benefit of using T_0, in addition to all the physical underpinnings, is that it's rigorously consistent for each and every material.

You know the T_0 and that's the point that's made at the bottom bullet is consistently defined for all steels and so it corresponds to the position of the toughness data each and every time, not to some representation of the data that is independently derived.

CHAIRMAN FORD: And this is true only for LEFM conditions?

MR. KIRK: No. Actually it's true for small scale yielding conditions which can go considerably beyond LEFM because LEFM limit in E-3.99 was constructed for mathematical reasons so that linear elastic fracture mechanics would apply irrespective of material.

Actually, the material that set the limit was titanium. If the limit had been -- if the coefficient in the K_{Ic}, which is 2.5, had that been set on ferrictic steels, it would have been set at about 1. There are a whole host of conditions and all the data points here satisfy small scale yielding conditions. You need EPFM to describe and the master curve works under those conditions.

CHAIRMAN FORD: Okay.

MR. KIRK: Next slide. Looking at RT_{NDT}, and this is where my eyes got wide open by the process that Marjory was able to bring to the table because, like I said, I thought I understood this. Here is where I apologize for font too small. The schematic on the right-hand side of your screen shows the 10 CFR 50.61 process for determining RT_{NDT}.

I don't wish to go into the details here but suffice it to say there's a preferred procedure which is the ASME NB-2331 procedure where you test procedure where you test Charpy, test NDTs, and compare them. Then the NRC has adopted and has used for quite sometime two alternative procedures, one involving the use of Charpy data only which is necessary to get RT_{NDT} values for plates.

And a second alternative involving the use of generic data which is just to say if for some reason you didn't happen to measure the RT_{NDT} of your limiting weld, oh gosh, darn, here is a generic distribution that you can go to and use a value.

So there are three different ways to get RT_{NDT} and none of these ways is terribly prescriptive at all. Even if you go through the preferred procedure of NB-2331, it says you test Charpys, you test NDTs, and you compare them, but it's noticeably silent on how many Charpys. There's no statistical analysis of data and so on and so on and so on.

There are many, many engineering judgements in this process. I think I said a myriad of methods and transition temperatures have been used to define RT_{NDT}. If you go up to the MTEB 5.2 procedure, which the NRC adopted in order to have RT_{NDT} values for plates, you see a whole host of index temperatures used, T_{30}, T_{45}, T_{100}, 30 degrees off the upper shelf. All of these things go into the mirace that we call RT_{NDT}.

Now, having said that -- and it's been noted to me by my colleagues that I'm hypercritical of RT_{NDT}, maybe because I know there is something better.

Having said that, RT_{NDT} when combined with the ASME K_{Ic} curve, because of the procedures you have to go through that's reflected in all these boxes to get an RT_{NDT} at every point along the way, you are constrained to make only a conservative judgement. Again, in my quest to find data that violated the master curve, I've also been on a quest to find data that violates the K_{Ic} curve when indexed RT_{NDT}. I can personally tell you that that data doesn't exist.

The methods were set up in the early '70s with the intention of always producing a bounding K_{Ic} curve, and it does. It just simply works. All of these things taken together strongly suggest that the bulk of the uncertainty in RT_{NDT} is epistemic.

If we had more prescriptive procedures, if we had only one procedure, if NTE and Charpy actually meant anything relative to toughness data -- I want to get Bill laughing out loud if I can -- all of these things would serve to reduce the uncertainty in RT_{NDT} and those are all clearly lack of knowledge uncertainties. For the purposes of this analysis, it's pretty clear that the RT_{NDT} uncertainty should be modeled as being epistemic.

Actually, what I would like to do is if you would skip ahead to view graph 73 since we just talked about RT_{NDT}. If we go to view graph 73 I can show you what we've done in terms of treating the uncertainty in RT_{NDT} data. Up at the top again is the mantra, "Toughness data is truth."

What we did was simply to take those datasets for which we had a sufficiently well developed transition curve to have a transition curve and plotted those data as measured relative to an RT_{NDT} indexed K_{Ic} curve. We then slid the curve which is represented here. It was a little more elaborate than this because we employed a statistical treatment, but we basically slid the curve until it bumped into the first data point.

We called that value delta RT and we adopted that delta RT value as a measure of the epistemic uncertainty in the RT_{NDT} data. That's how far off the K_{Ic} curve, which was intended to be a bounding curve was from the data it was intended to represent.

What you see at the bottom of the graph is a statistical representation of all of that data. We had enough data for 18 different RPV steels, both plates and welds. I think there was a forging or two in here to define delta RT values for those different materials and what this distribution tells you is that

RT_{NDT} at its worst is accurate -- I'm sorry, at its best is accurate.

At its worst places the K_{Ic} transition curve perhaps 175 degrees too high which is sort of getting to the Midland situation. And, of course, there is a range of situations in between.

What we've done in the FAVOR code, and this is by far the most significant change in terms of having an effect upon the results, perhaps not the most significant scientific change but the most significant change in terms of changing the numbers is the realization that, oh, RT_{NDT} isn't the transition temperature, the toughness data is and here's how far off it is. In our sampling procedure, which I'll show you in a minute, what we do is we simulate an RT_{NDT} value.

We then simulate a random number from zero to one and that then tells us for that run how far off RT_{NDT} is. Like I said, it could be right, it could be 175 degrees too high. On average this works out to about a 65 degree downward correction in RT_{NDT}.

MR. DUDLEY: Just a minor point. How much better did you get with the three parameter fit than a two?

MR. KIRK: I don't know.

MEMBER SHACK: Nobody uses three parameter.

MR. KIRK: It was a convenient fitting function. One other thing to point out because I know we've gotten this question in the past. The rules of engagement for this analysis would stick with LEFM data because the basis of FAVOR is LEFM.

Having said that, we did that very heavily restricted data set that we could use, as you might expect. Having said that, we have also constructed this cumulative distribution function which is shown on the bottom using EPFM data, using data that would be valid by the master curve method.

What we find out is what that then gives you is something more like 70 materials to put on the lower graph of a much wider variety and you get the same function. I know some concern had been expressed before that. Now the inherent assumption is that distribution represents everything. Admittedly it's a leap to say that 70 represents everything but it's a shorter leap than 18.

So in terms of a practical affect, that's the big change in the initiation toughness model. The other change, which is -- I'm sorry?

CHAIRMAN FORD: I'm struggling, I must

admit. There's a whale of a lot of information here and to make sure that I'm not stamping something that is okay when I honestly don't know what I'm looking at. What other peer reviews has this gone through?

MR. KIRK: Okay. I would be happy to comment on that. The work that you're seeing summarized here was developed by a team that involved both myself, NRC contractors, and indeed EPRI contractors, as I pointed out.

We developed a draft recommendation that has been passed around to members of the industry and, indeed, members of our own contract staff, people like John Murkle have reviewed this, Randy Nanstead.

MR. HACKETT: And Richard Bass.

MR. KIRK: Richard Bass at Oak Ridge National Laboratory. This has been through -- in that group I lot track of my revision count. We went back and forth.

CHAIRMAN FORD: And how about the ASTM bodies?

MR. HACKETT: The ASTM standard has been -- I don't know, again, how many versions that went through. Several of us were involved in that. Mark has been more involved than most. It started in the late '80s and refined into a standard in -- when was it, '97?

MR. KIRK: Yes, ASTM has adopted the master curve testing standard in 1997 so they provided us with a method to measure T_0. In that committee, as you might expect the notion of the universal curve shape. Everybody is going, "What?" Universal scatter. "What?" That was vetted through that committee.

Then following that in 1998 ASME adopted a parameter called RT T_0 which is simply T_0 plus 35 degrees fahrenheit as an alternative to RT_{NDT} to index the K_{Ic} curve. On the outside -- well, external to this project the master curve itself has gone through considerable peer review in both ASTM and ASME.

Here, to be honest, and this is where the initial constraint came in, I said we didn't want -- well, we didn't want to if we didn't have to require the licensees to make new measurements.

As we were discussing yesterday, when you are dealing with uncertainty and, indeed, this is a huge uncertainty, two things you can do. One is to try to mathematically account for it which is what we've done here. The benefit is that enables you to use your old measurements. The detriment is you are introducing that level of uncertainty.

The alternative would be to say, well, let's go make toughness measurements and do this better but that would send everybody scurrying back to reconstitute Charpys and preprac them and test them and hopefully find them in the hot cells.

That is certainly a further improvement than can be made here. Indeed, I know a number of the licensees are interested in that level of improvement. That's not one that we've taken here.

CHAIRMAN FORD: Okay. All of these, as you said, at the very beginning, using a 95 percentile you are going to come up with the end result.

MR. KIRK: No. You're going back to the graph that Ed showed. In this we have quantified -- well, first off -- you told me I was going to get in trouble for that and you were right. We used the 95th percentile for convenience. I think your comment about a one sigma representation is one that I've taken note of.

Throughout here we are defining distributions left and right. In this presentation you're probably not going to be able to keep track of them. But what you'll find out if you refer to our draft NUREG which is the basis for this if you refer to the FAVOR theory manual which shows you what actually got in there is that we only truncate the distributions when it results in a physically unrealistic prediction like a negative transition temperature shift.

I mean, that's not relevant here but, for example. No, we've put the full probability distributions into the paper code.

CHAIRMAN FORD: Okay.

MR. HACKETT: I guess I would just add a comment just sort of almost a philosophical basis here. The buy-in you need for this project on the master curve is really fairly limited, as Mark tried to get to. I mean --

MEMBER SHACK: No. You need to believe it's the truth.

MR. HACKETT: That's what I was coming to. What you are really buying into in this project, and that's why I just wanted to try and make sure the record is straight on this thing, is that the master curve in T_0 is giving you a better representation of the real material behavior than RT_{NDT} for all these variety of reasons we've been through.

Given that, there are whole other levels of things you can do with a master curve concept. I think it's also fair to say it's been an area that's been guilty of some zealotry over time in selling it. But it had to go through a vetting process which I think it largely has been through in terms of ASTM and ASME.

But for application to this project what you are really saying is that has given you better representation of what is the truth for these ferritic materials. Then we are making adjustments to take that conservative bias out, as Mark mentioned, in the RT_{NDT} methodologies. I think that is really all it boils down to for this go-round.

MEMBER SHACK: Well, that and that your 18 material curve augmented by your 70 is universal.

MR. KIRK: Yes.

MR. HACKETT: Yes.

MR. KIRK: And really what that curve represents is nothing more than quantifying what all -- I've got to remember I can't point at the screen -- what all of this gave you. What all of that combination of judgements and guidance or lack of guidance produces.

Indeed, at the beginning of the project it had originally been the hope that we could use, and we have used some of these diagrams directly as mathematical models. In this case it was much easier, much more pragmatic just to rely on the data to tell us what the end result was because propagating all that through just became impossible.

Okay. So we've now discussed in detail one of the parameters which was the index temperature, RT_{NDT}. We can now go on to just briefly discuss K_{Ic}. In terms of significance, this -- well, let me get to the end. The physical understanding of the cleavage fracture process shows us that the noncoherent particles and other barriers that present themselves to dislocation motion are alone responsible for the scatter in K_{Ic}.

That idea coupled with a number of other ideas, mainly that K_{Ic} does not exist as a point property. That is to say, all K_{Ic} values have associated with the milane scale, and that is the crack front length as it is commonly known, suggest that there is only so good that you can measure K_{Ic}.

In other words, I suppose as an experimentalist I like this analogy if we had perfectly machine specimens preprac to exactly the same depth, all tested exactly the same way, you would still get the scatter that alarms most non-metallurgist when you look at this plots.

So that understanding tells us that in a

PRA context this uncertainty that you see here is indeed irreducible and, therefore, should be modeled as aleatory, which is to say this created -- I think this consumed probably about two months of Terry Dickson's life.

In the old version of FAVOR we did an epistemic model of K_{Ic} where we went through our toughness model and we got an applied K at a particular time, temperature, pressure. Then we drew randomly from the K_{Ic} distribution at that temperature. We said K applied, K_{Ic} passed, failed, zero, one binary.

Whereas now, and Terry will get into this in much more detail tomorrow, we recognize that for any applied K_{Ic} that is, in fact, presented to a material which has a distribution of K_{Ic} value so instead of for that given applied K_{Ic} instead of the material having failed or not, what we calculate and indeed carry through the model is some probability of failure.

Instead of having zeros and ones we've got .005s or whatever that then get added up. That was a significant programming change which is indeed a much better -- is consistent with the PRA process, is a much better representation of the physics. What effect that has had on the numbers I honestly couldn't tell you because we haven't done nor would intend to do that comparison.

MEMBER SHACK: One thing is when you come up with the aleatory model for K_{Ic} and you do it this way, you get the three significant figures, the same numbers that Oak Ridge got from their statistical analysis which I thought was based on RT_{NDT}.

MR. KIRK: I'm sorry?

MEMBER SHACK: You have a Weibull model for K_{Ic}, an aleatory distribution. That describes the aleatory behavior of K_{Ic}.

MR. KIRK: Right.

MEMBER SHACK: That's a Weibull model. So say three significant figures I get the same coefficients in there that I got from the Oak Ridge model when I did it with RT_{NDT} that you're doing with RT lower bound.

MR. KIRK: Okay. Let me --

MEMBER SHACK: There is something magical I'm missing here.

MR. KIRK: I think let me clarify. First off, the aleatory model, the other mantra that we kept having in this project was physically motivated and empirically derived, which is to say we got from the underlying physics the notion that there should be an exponential temperature dependence in K_{Ic}. It should have a lower bound value.

Indeed, we took out of the physics that the scatter in the Weibull slope should be four. But having said that, we then fit the temperature dependency. We fit the lower bound values because we didn't have independent estimates from the -- or direct estimates, I should say, from the physics of that.

The reason why the numbers are the same is that we didn't have independent numbers from the physical model. They, in fact, came from the data. The other thing that perhaps isn't clear is Oak Ridge wasn't fitting RT_{NDT} data. They were fitting RT lower bound data. The index temperature they used was RT lower bound.

MEMBER SHACK: Well, that confused me because when I looked in the Oak Ridge manual, the first time it comes up, of course, it's fit to RT_{NDT}. Then they tell me, okay, that's really just, you know, an approximation. I thought in the original fit --

MR. KIRK: In the original fit, yes.

MEMBER SHACK: -- it was RT_{NDT}. Did the numbers change?

MR. KIRK: The numbers changed. And you may have found an error in our work in terms of labeling. Right now the --

MEMBER SHACK: They sort of warn me as I go along here that that is only approximate first up. I figured that was just to trap the reader.

MR. KIRK: No, the fit --

MEMBER SHACK: Okay. The original fit really was different, as I would have expected it to be.

MR. KIRK: The original fit really was different. Indeed, this gets into some of the, well, to me the interesting nuances of why it was good to have the physical model and the empirical models.

When we got to -- when you did just the the empirical fit of the data, you found it doing bizarre and unexpected things like the scatter on the lower shelf was bigger than the scatter on the upper shelf. That was simply an artifact when you fit it versus RT_{NDT}. That was simply an artifact of the fact that you didn't have enough data on the upper shelf and you didn't have enough data to independently estimate the scatter parameter.

So bringing in the elements of the physical knowledge that said, well, first off, RT_{NDT}

isn't the right X-axis normalization. We need to have something tied to the data. That is physical observation No. 1.

Combine that with the notion that there should be a scatter parameter or Weibull slope that is not only universal to the various materials but constant with temperature. Put those two physically motivated constraints on the model and you get out -- I don't know if I can blow this up so that you can actually see it.

You get a K_{Ic} versus temperature model that is in agreement with the physics, is in agreement with what you would "expect it to do" which is to say smaller scatter on the lower shelf, expanding scatter as we go up.

MR. HACKETT: It's an interesting question. I'll just add one more comment. This goes to the root of someone. Mike Mayfield started us down this path many years ago before there was a master curve. If you ask yourself the question, "What if I had never heard of a master curve or there wasn't such a thing, what would I have done in this area?"

I think his vision at the time was as simple as we're just going to get all the appropriate data and analyze it the best way we know how statistically and at least try to have a statistically motivated version of this that wouldn't necessarily have the physical underpinnings that Mark is talking about. But would that have been an improvement? Yeah, absolutely it would have been an improvement. You didn't need the master curve to do this.

It's a refinement that came along in parallel that I think made some things better from a physical understanding basis in addition to some of what was going on in the project anyway. There would have been improvements here I think regardless of the different form.

MR. KIRK: Did that answer your question, though?

MEMBER SHACK: Yes.

MR. KIRK: Okay.

MEMBER SHACK: I'm just trying to -- coming back to Ed's point, what difference would it make in the final? I mean, if I lump all the aleatory and epistemic things together, would it make a difference?

MR. HACKETT: We have not done that in any kind of systematic way. That's not to say we couldn't, although we weren't in the mode of searching for --

MEMBER SHACK: It certainly makes it

easier to understand the Oak Ridge calculation when you draw an epistemic loop and aleatory loop inside.

MR. HACKETT: Yes.

MEMBER SHACK: And that alone is almost worth the price of admission.

MR. KIRK: I think -- I've gone through this debate many times with people who were all trained as engineers so we are all trained to say, "Does it make a difference?" For my money, certainly you want to focus on the things that are most important unquestionably.

But there is also considerable value in doing things right, especially from a regulatory perspective where you get yourself caught in this trap of, "Oh, well. I don't know. Let's make it conservative." That becomes an increasingly in today's world a hard judgement to defend. If conversely you can say this is physically the right thing to do, it makes it much more defensible.

Indeed, it's then something -- the best part about that is I think it sends something that you can change later as your state of knowledge improves because you know why you made the decision and you know where it came in.

MEMBER SHACK: If you lump them together, I'm not sure that you would have been able to do the conditional probability of fracture. That is, that would have included both aleatory and epistemic uncertainties and that really would have been wrong.

MR. KIRK: Well, yeah. And certainly if you lump together you truly haven't gained that much. I'll have to take exception with Ed because the original ASME K_{Ic}/K_{IA} analysis was not a statistical analysis. That clearly needed to be done. But if you just do a statistical analysis of RT_{NDT}, you're stuck. Your hands are tied and you're saying RT_{NDT} is truth.

Now you're stuck with something that clearly is not truth most of the time and you wouldn't have gotten the 65 degree benefit and the conditional probability failure numbers would have been at least one order of magnitude higher. I'm saying that with a fair degree of certainty because it was only recently that Terry implemented this adjustment for epistemic uncertainty in the code.

Indeed, the original Oconee scoping analysis was done without that adjustment in because we didn't have it at the time. When Terry put it in, fortunately or unfortunately, I don't know, this isn't something we have taken the time to document. I recall phone conversations where the comment was made that that change alone drove the failure probabilities down by at least an order of magnitude.

Certainly any licensee that is watching their RT_{PTS} number knows that if they can get, heck, 5 degrees fahrenheit is great. 65 degrees fahrenheit you just forget that you have a problem anymore. There is no question in my mind that a purely statistical analysis would not have netted that benefit because you would be stuck with the notion that RT_{NDT} is truth. You just can't get there from here.

MR. HACKETT: You can see what happens when you get materials and mechanics people going on this stuff. We're going to run out of our time.

CHAIRMAN FORD: I'm looking at the time. I don't want to stop the flow of concentration here. We can go until half past 12:00 before we stop. I don't believe there is any reason why we can't do that. Ed, I'll leave it up to you to decide when we stop for lunch.

MR. HACKETT: I guess I don't see this yet as a good point to stop but I might ask Mark to look at an intermediate juncture.

MR. KIRK: We can -- well, I think we have essentially finished the initiation toughness model.

We've got a couple more logical break points. We need to work through the irradiation shift model and the arrest toughness model. If we can do those two in the next half hour, that will probably be pretty good.

MR. HACKETT: I'd be thinking you could probably do one.

MR. KIRK: Hopefully.

CHAIRMAN FORD: I was hoping for lunchtime to try to catch up.

MR. HACKETT: This could be a good break point since we've since debated the master curve a lot and talked about the initiation toughness model. We're at the next jump-off point which is the irradiation model

CHAIRMAN FORD: If you don't mind.

MR. KIRK: That's fine.

CHAIRMAN FORD: We'll break now. That gives me an hour to try and catch up here.

MR. HACKETT: Drinking from the fire hose.

CHAIRMAN FORD: This is one of those situations I say now I'm a corrosion engineer rather than a metallurgist. We'll recess for an hour and come back at 1:00.

(Whereupon, at 11:59 a.m. the meeting was adjourned for lunch to reconvene at 1:00 p.m.)