A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(1:04 p.m.)

CHAIRMAN FORD: Let us reconvene. You're on 74.

MR. KIRK: Okay. Where we left off when last you tuned in. We are now going to talk about the irradiation shift model which starts on view graph 74 in your packet. This is the way that we estimate how far upward in temperature we shift the un-irradiated K_{Ic} curve.

We've already talked -- the parameters of this model are, of course, the shift value which we currently estimate using the Charpy 30-foot pound transition temperature, the delta T_{30} value, and the distribution of irradiated K_{Ic}.

However, in the previous discussion that we had before lunch, we already discussed the temperature dependence and the scatter in irradiated K_{Ic} we expect to find, and indeed do find, in our empirical data to be the same as for the un-irradiated K_{Ic}. All we really have left to talk about is the shift value.

Now I'm going to violate a fundamental rule of presentations and show an un-Godly complex equation and then say a little bit about it.

This is the model that the staff has decided to use to represent, or I should say to relate the shift in the Charpy 30-foot pound value with other variables that we know about, the reactors like the irradiation they run at -- I'm sorry, the temperature they run at, phosphorus, fluence, nickel, copper, and so on.

This is an equation that has been developed by two of our contractors, Ernie Eason and Bob Odette. It's currently a fit -- I should say, again, it's a physically motivated empirically calibrated fit to all of the data that was available from the domestic nuclear reactor surveillance programs as of, I think, about two years ago.

I did not intend to go into this in any level of detail. Just suffice it to say that this relates variables of chemistry and irradiation exposure to the Charpy shift value and that it's a fit to the available evidence that we have for operating domestic commercial reactors, and that the functional forms have been selected consistent with a physical understanding of irradiation damage.

There are two points I would like to carry away from this slide. One is in the upper right-hand corner. If you go to any meeting in this country or,

indeed, anywhere in the world and discuss embrittlement -- this would be referred to as embrittlement trend curve -- you can get engaged in some very spirited debates about whether this is the right form or somebody else's equation is the right form or does phosphorus belong in there at all or should there be a flex effect and so on and so on and so on.

Needless to say there is considerable disagreement among the experts in this field and perhaps it is just a sign that we have too many experts as to what the right form of this equation is. Indeed, just from a statistical perspective it's the devil's own because of all the scatter in the data.

Having said that, the uncertainty here is clearly epistemic. We are suffering from a lack of complete knowledge about what some of the underlying phenomena are and under what regimes of chemical composition and irradiation exposure they're active. Obviously we need to make that characterization in order to use this equation properly in the reevaluation effort.

The other thing that we need to know that I would like to focus on a little bit here is down in your lower left-hand corner, and that is the question

of does delta T_{30}, which is a Charpy shift, have any relationship to delta T_0 or the toughness shift. In all previous calculations that have been done of this type, the implicit assumption has been made that, in fact, the Charpy shift is equal to the shift in the toughness curve because no adjustments or corrections, or what have you, have ever been made.

Indeed, if you look at the physics that guides toughness shift and Charpy shift, leaving aside some of the finer details, you decide that, yeah, increases in strength should cause increases in toughness shift and, indeed, increases in Charpy shift so they should be related. Exactly what the relationship is is perhaps in question in terms of what the coefficient is.

To quantify what that relationship is for this project, we went into our empirical database of shift data for reactor vessel steels which is illustrated here and simply developed a winter regression fit between the Charpy shift value on the x-axis which is what we'll know from the embrittlement trend curve relative to the fracture toughness shift on the vertical axis.

Indeed, more by luck than anything else the 30-foot pound Charpy shift is roughly equivalent

.99 versus 1.0 to the toughness shift for welds. For plates the Charpy shift actually under-predicts slightly the shift in toughness.

In forgings there's a co-efficient here but we don't adopt that in the FAVOR code because there were such a small amount of forging data to fit. Moreover, it's not going to make a big difference anyway because there is only one forging in the whole evaluation and it's such a low embrittlement I don't think it matters. So what you can take away from this slide, which indeed this is a big version now --

CHAIRMAN FORD: Before you go too far --

MR. KIRK: I'm sorry. Go ahead.

CHAIRMAN FORD: -- would you mind going back to the one with all the equations on it?

MR. KIRK: Yes.

CHAIRMAN FORD: I have not seen the raw data.

MR. KIRK: Yes.

CHAIRMAN FORD: In many of the relationship correlations, the correlation factor is very low.

MR. KIRK: Yes.

CHAIRMAN FORD: What you've done is when you said that Ernie Eason was working with Bob Odette,

that he was taking the functional form of what you might expect from theory and force fitting it, if you like, to what Ernie Eason was seeing from his correlation between observations.

MR. KIRK: As an example, perhaps an easy one to look at is in the age hardening, or perhaps more commonly called the copper rich precipitive term. You've got the fluence function here that is fit to a Tan H.

Not that there's anything in the theory that says we should have a Tan H but the theory does say that we should expect the effects of age hardening and once you've precipitated all the copper out of the matrix, the mechanism should stop.

That is what the Tan H does. Yes, you're right. You come away from the physical understanding with some very basic observations and then try to find those functional forms or, indeed, lack of functional forms in the equation.

Another one to perhaps point out, and this reflects a decision that was made and equally a different decision could have been made, I think it's fairly well accepted, perhaps the only thing that is commonly well accepted in this community, is that the coefficient on the matrix hardening term in fluence

should be a square root.

I'm really waiting for somebody to get after me about significant figures, but a statistical estimate of that value is .4601, very close to one-half. We elected a decision but clearly a different decision could have been made to stick with the empirical fit.

Equally you could have said, "Well, I've got sufficient weight of physical understanding to say that this should be a square root term, force that coefficient to be .5, and then the effect would be taken up in the other terms.

CHAIRMAN FORD: Okay. So what you did was you took a functional form from Bob Odette and you fitted it. It wasn't, therefore, to .5 but came up .46.

MR. KIRK: Yes.

CHAIRMAN FORD: Because that came out of minimizing your --

MR. KIRK: Minimizing the residuals, yeah.

CHAIRMAN FORD: Okay. I understand.

MR. HACKETT: Some of this, Peter, is also worked in reverse where you start with the empirical data and, in the case of Ernie's analyses, he would have applied some statistical evaluation procedures

that show a trend. Then the challenge would be to Odette and others in the mechanistic community to explain that trend physically.

In some cases we could. In at least one or two cases there weren't real good physical explanations. Then we are stuck with a difficult decision of whether to include or not include that effect on the model.

CHAIRMAN FORD: Like in all such model derivations where you are basing it on existing data, obviously you are going to get a good correlation, or within a certain error, between the model and the data.

MR. HACKETT: The data.

CHAIRMAN FORD: You're doing a circular argument. So how can you validate such a model until you get new data presumably?

MR. HACKETT: Very difficult. For instance, the long-term bias term Mark has on here, you can see that this data is accumulated on materials that have been aged over 97,000 hours obviously. By definition you're talking a pretty small population.

I think in some cases you are really stuck with expert judgement whether it's statistical judgement or physical judgement, or hopefully some

combination of the two.

I think part of what we get into, especially more so in this area than any other area like Mark was alluding to, is the statistical judgements are inherently offensive to scientists. You have this data that indicates this trend. No one can explain it but, then again, no one is going to argue statistically it's not there.

Then you are backed up to the sort of what Jack Strosneyder would say if he were here. The regulatory approach is we're regulators, at least our job, and we see a trend in this data. We need to account for it somehow.

Then you're stuck with not having pure science of some sort to back that up so you're in a weaker spot than you would like to be but it's incumbent on you to do something. I think these are the things that make this area especially difficult.

CHAIRMAN FORD: Thanks very much.

MR. KIRK: Okay. So the end result of all of this is the model that was coded in FAVOR where we start with generic distribution. Well, we start with mean composition values of copper, nickel, and phosphorus that have been recorded in the docket by the plants.

In most cases we don't have -- I should say in most cases we don't have enough data on the particular heats of steel to construct a reliable statistical distribution of copper, nickel, or phosphorus. I have this here if you wish to go into it in detail but I wasn't planning on showing it.

We, therefore, have done some work to construct generic distributions of cooper, nickel, and phosphorus that are then sampled on an epistemic basis. Those go into the embrittlement trend curve.

Peter, if you're interested in the data, if you refer to this tiny little postage stamp figure, the cloud disturbs people and perhaps it should but equally perhaps it should provide a challenge to future work. That is the embrittlement trend curve with all the data plotted over it.

Also, another thing to point out, as I suggested earlier, that intention of this is to be informative and that sometimes means it shows some things that you haven't done a darn thing on. In order to figure out what the embrittlement shift is for a particular location into the vessel wall, you need to attenuate the fluence through the wall.

Unfortunately, well, just the way it is, not sufficient work has been done in that area in the

past decade, or decade and a half, to really provide a good basis for any revision of the plant attenuation function. I think, again, if you go into the community you would find lots of arguments waged against this attenuation function and, indeed, I could make a few myself.

Having said that, if you say what new do we know, the answer would be we don't know a lot more so we're just sticking with that one for right now.

Having said that, here in PTS where the flaws that get you are invariably pretty close to the ID vessel surface anyway, what particular attenuation function you pick isn't going to be that important.

Stepping outside of this framework here, it is going to be critically important when we go to implementing this embrittlement trend curve for regulation of heat-up and cool-down limits because there right now you need to calculate embrittlement shift at the quarter T and three-quarter T locations.

Once you start attenuating the three-quarter T, you pick up some pretty substantial effects. There I think we certainly should pay much more attention to it but right now we're sticking with the -.24 exponential exponent.

We plug in our copper nickel fosh, all the

other variables into the embrittlement trend curve, get out a predicted delta T_{30} at the cracked tip, convert that to a toughness shift and, indeed, sample epistemically on this uncertainty and what that shift value is.

In terms of what's new relative to what we've done, we've got a new embrittlement trend curve here but in total on average relative to the old embrittlement trend curve, it's different by six percent. Will that have a big influence on the results? My guess is no.

The new thing here is that we recognize the delta T_{30} isn't a toughness shifting converted. Having said that, the conversion is very close one to one. Again, it's the appropriate thing to do from the viewpoint of doing this all right. I personally don't think it's going to have a big effect.

CHAIRMAN FORD: Just for interest, how many data points is that attenuation curve based upon?

MR. KIRK: It's a judgement. That might be a little bit unfair but the number is certainly not bigger than about this.

CHAIRMAN FORD: That formula has got some sort of theorical basis?

MR. KIRK: Yeah. It's making fluence

attenuate like DPA which is believed to be a better physical metric or radiation damage. By the way, it's conservative. This dates back to the Neil Randall technical basis for Reg. Guide 199 Rev. 2 circa mid 1980s.

MEMBER SHACK: But didn't Eason and Odette come up with a different attenuation?

MR. KIRK: No.

MEMBER SHACK: A different thru-wall variation of toughness?

MR. KIRK: No. We have not looked at that.

CHAIRMAN FORD: I've certainly seen attenuation curves with data but for stainless steel.

MR. KIRK: Yeah.

CHAIRMAN FORD: Is there any reason for saying --

MR. KIRK: There is some very limited data for the Gundremmigan vessel and there was --

MR. HACKETT: Also, this is an interesting but, again, whole other topic we could get into in great detail. One of the goals of the research program in this area for a long time has been to try to do this type of validation on a retired vessel.

CHAIRMAN FORD: Like Yankee Row.

MR. HACKETT: Well, Yankee Row may have been more of an anomalous example but we have made attempts with San Onofre, with Zion. Unfortunately, we haven't met with success in any of these based on the schedules for decommissioning and the economics involved.

The other one Mark was going to refer to probably was the Japan power demonstration reactor. We had a collaborative program with the Japanese to do that and attenuation has been mapped through the JPDR wall. Unfortunately, that was, again, a demonstration reactor. It's fluence levels were not typical of what we would see in our commercial operation. Also the wall was thin. There were a lot of things that made it atypical.

This is an area where work remains to be done and we have been trying to cooperate with Bob Hardy and others in the MRP to try and somewhere in the future get samples. Our last attempt was San Onofre and my understanding is that was a budget breaker. We're trying to do that.

CHAIRMAN FORD: Could yo mention -- it just struck me about something else. You said it didn't really matter. Is that in relation to the attenuation?

MR. KIRK: In relation to the attenuation in that once the flaw starts to bury itself in the vessel thickness, the applied Ks aren't high enough to cause crack initiation. Our benchmark here is the Charpy shift on the inner diameter because that is where all the surveillance data is.

Obviously the further you go into the vessel, the more any differences in insenuation function are going to show up. If you attenuate only over, say, the first 10th of the vessel wall thickness, the difference between this and other proposals, I think, the other candidate proposal was a -.36 coefficient.

Over the first 10th of the vessel thickness it doesn't really make a huge difference at all. If you go out to three-quarter T where you need to be to assess your heat-up and cool-down curves, it makes a whole big lot of difference. I mean, that's just the mathematics of it and the fact that you are extrapolating data further.

One other thing to point out with respect to trying to validate this experimentally using mechanical property data is that it's a very difficult experiment to do from the viewpoint that any of the candidate coefficients here, it's not a precipitous

drop.

Remember that you are going to try to resolve this with either Charpy or perhaps T_0 transition data and then you are -- so you are going to have an uncertainty in that of plus or -20 degrees C and you've got a huge signal to noise problem.

Then you couple that with the fact that you're talking about material that's been irradiated in a power reactor so you're not going to get a whole huge big chunk of it. You're going to get a little bitty something. Certainly the industry, the NRC, has been interested for a long time in getting a chunk of one of these things because everybody likes to cut up real structures.

Having said that, I think before somebody makes that investment, it's incumbent on us to think about what the effect is we're trying to resolve relative to our ability to measure that. What you ideally would want to get is something that has been -- what you would really want is something if you go way far out on the embrittlement curve it would all be embrittled to about the same. You want to get something around the need.

But designing the right experiment here, having the money to do it, and having enough material

to make -- I was going to use the word unequivocal but that is perhaps delusional -- less than uncertain measurements is very, very difficult.

CHAIRMAN FORD: Looking at this -- just getting off that subject and going on to the database for the effect of embrittlement, fluence on K_{Ic}, do we have any data going out to fluence levels appropriate for end of life for extended license?

MR. KIRK: For extended license there's a good question. Without remembering the specific numbers, I don't think the fluence is so much the problem. The area of question, and it depends on who you talk to as to how big a question this is, certainly we've got fluences out to EOL and NEOLE because the capsules all have a lead factor.

The difficulty is that now some folks believe that there is some either independent or synergistic temperature effect that is occurring at long times that could be giving us an uptake in the embrittlement that you simply wouldn't observe in the accelerated capsule positions, or that it would take -- well, it would take a long time to observe. I think more data is always better but I think we have covered that parameter space adequately well.

Time, again, it depends on who you ask as

to how important you believe that to be. Certainly if you look at it from a purely thermal embrittlement viewpoint, these reactors operate at 550 and you have to be there for a hell of a long time and ferritic steel to get anything really going on. Like I said, some people believe in a synergistic effect and some don't.

MEMBER SHACK: Just coming back to your attenuation again, suppose you rethought the quarter T flaw. If I look at your statistics, my chances of a quarter T flaw don't look real good.

MR. KIRK: As a member of the ACRS, I think you should make that a recommendation. Indeed, just to digress a little bit, one of the points that Mike Mayfield had when we prebriefed him on this is he said, "If things keep going the way they look like they're going and we are able to raise the PTS screening criteria by the amount that these analyses seem to suggest, then the conservatisms that are buried in your Appendix G calcs for heat up and cool down are going to make routine operations more limiting than PTS so somebody better we thinking about that."

So, yeah, I think -- and that is a very good point and one that I think you people have

recognized is that with -- and we are soon getting into talking about the flaw data. With the flaw data that we have, which is, indeed, not nearly as extensive a database as this but bar more extensive than we ever had before, I personally hope we are developing the basis to sway some of the minds on ASME to say that, yeah, quarter T flaw is just a tad bit out there. We clearly make up some problems for ourselves here.

That concludes the irradiation shift section. Now the last part is the arrest fracture toughness model. We need to deal with the arrest model because our criteria for vessel failure is that you punch a crack through the outside. Again, that is an engineering decision.

Certainly in other countries, I know France and I believe other countries in Europe, have adopted an initiation based failure criteria. This has been what we have historically done in the U.S., which is not to say that it's right.

We have revisited that a number of times both within the NRC and outside with the industry and the consensus has -- well, there has never been sufficient consensus to move us towards an initiation based criteria basically because I don't think anybody

wanted to give up whatever extra bit we got out of this.

The parameters of the arrest model are as illustrated here. We need a distribution of K_{Ia}, and we need to know how much the crack arrest curve shifts outward from the K_{Ic} curve due to the higher strain rate characteristic of arrest than initiation.

The current arrest fracture toughness model if you look at K_{Ia} it's got a fit temperature dependence and scatter that since it was a fit made independently of K_{Ic} data is completely independent of the K_{Ic} data. It's not related at all. Whereas when you look at the underlying physics, you say maybe they should be.

RT_{NDT} we've already beat on enough so I won't go there. And in the current ASME code and, indeed, in the model that we've used in our former PTS calculations, the shift between K_{Ic} and K_{Ia} has been fixed for all material conditions irrespective of irradiation.

Those are just merely the qualitative characteristics of the current model. To try to put all on one slide where we got between working with the industry, with Marjory and ourselves, this is the summary. I'll try to step you through this to point

out the high spots.

We start off on the left with the physical observation and in the middle tell you what that would suggest we should see in the empirical data. Then on the right we can show you the data.

Starting in the green we start with the observation that all ferrictic steels have the same lattice structure. That suggest looking at a dislocation based constituent model that we should have nearly identical temperature dependence for initiation and arrest. I say nearly instead of exactly because, of course, there's a coupled rate temperature effect term.

Clearly K_{Ia} occurs at a different rate than K_{Ic} but the rate effect is small. We also expect that the temperature dependency again since it's controlled by the lattice structure should not be influenced by radiation composition and heat treatment.

Over on the right you see a plot of mean curves that were fitted through the K_{Ic} and K_{Ia} databases just as fit curves unconstrained by any physical observation. Indeed, we find out quite happily that the data are doing what we thought they should do and that the K_{Ia} and K_{Ic} curves do have

essentially a similar shape.

As you can see, the physical understanding gets rolled into the empirical model with different -- I shouldn't say this in this crowd maybe but Dr. Apostolakis is not here -- to different degrees of certainty based on how good we feel our physical insights are.

The middle box I have a graphic on that I can show you if you're interested but when we look at the micro-features of the steel control initiation and arrest. If you look at crack initiation, crack initiation starts with things like carbides and they have a fairly wide spacing in the ferrite matrix relative to the factors that control crack arrest which is more characteristic of the matrix itself.

We should expect to see that the scatter in the K_{Ia} data because the crack initiators are spaced wider that allows more stress variation and consequently you would expect to have more scatter than we see in the K_{Ia} data. Indeed, the empirical evidence bears that out again. We've got a graphic on that in the report. It's not featured here because it's a fairly minor effect.

The third point which is, I suppose, the big change in this model is that we come with a

physical observation that there is a hardening curve that is universal to all steels, or maybe I should say all ferritic steels.

The best 25 word description I can come up with for that is if you take a tensile specimen in the laboratory, whatever steel, I don't care, load it up past the yield point, unload it, then reload it, it doesn't start flowing again until it passes the point where you started to unload.

You can unload all you want and it just keeps marching up the same hardening curve. This idea gets to the point that, okay, you are mechanically cold working that material. This concept gets to the idea that hardening is hardening is hardening is dislocation piling up in the material so that you would expect to see all materials tracking up one universal stress strain curve.

What that leads us to is the idea that we should expect to see the initiation curve approach the arrest curve as the material damage increases for the following reason. If you have, say, un-irradiated material that hasn't been irradiation hardened, say it's a 60 yield material, you apply a rate characteristic of crack initiation and you get one transition temperature.

You apply a crack arrest type loading rate which is very, very fast. You get an elevation in the yield strength. You get a hardening due to the rate effect because the dislocations can't move fast enough so you get a big uptake in the yield strength and consequently a big increase in the crack arrest transition temperature.

Contrast that with the situation that you find if you take the material that has been irradiation hardened to a considerable degree you've already exhausted in that material a considerable amount of your hardening capacity so the same increase in loading rate will produce a much, much lower increase, elevation in yield strength and, consequently, a much, much smaller increase in the transition temperature from initiation to arrest.

When we examine the experimental database which is shown down here in the blue graph, delta RT arrest is just the difference in transition temperature between initiation and arrest and T_0 would be the crack initiation transition temperature.

Down here you have materials that haven't been significantly worked. Up here you have materials that have been. Again, you see what you qualitatively expect from the physical understanding which is that

the curves should be getting closer together.

CHAIRMAN FORD: There is statistical justification for drawing those curves?

MR. KIRK: Yes. Those are statistically drawn curves.

CHAIRMAN FORD: Two points to the right and quite a lot to the left.

MR. KIRK: It's a log normal fit which I think is causing that to happen, but yes. This now becomes -- I'm going to keep this -- well, we can go to this. This shows again the overall sort of high-level flow chart of how we model initiation and arrest.

In terms of looking at what's different, the point I would like to again focus on is this graph. As I pointed out before, in the -- look down here now. In the existing model, the model we've used before of crack initiation and arrest, the K_{Ic} and K_{Ia} curve were fixed at a separation of 55 degrees fahrenheit, about 20 degrees C, and they always stayed that way irrespective of how irradiated you were.

Previously the curves were fixed with this kind of separation down here. What we've done here, we've implemented this in the model so we go through and start off by figuring out the crack initiation

index temperature for the irradiated material.

Then we come into this curve and then randomly pick what the shift is based on these data. What this is saying is that now for materials that have been heavily irradiated we'll probably get about the same numbers we've always used before. For materials that haven't been heavily irradiated we'll get a much bigger number.

What that means is for materials that haven't been heavily irradiated, the crack arrest curve will be shifted out further than it ever was before which makes that material less arrest tolerant. That was actually a nonconservatism in the old analysis.

However -- way that quickly -- having said that, this is again one of those things that it's appropriate to include to get the model right but I don't think has a big practical influence because what you're saying here is I have a material, let's say, at an un-irradiated transition temperature of -100 degree C, I'm going to shift its arrest curve out further but I don't care because it's got so much initiation toughness the cracks never started anyway.

This has been implemented in the model and is, indeed, a substantial change relative to the

earlier model and it's a change that we believe to be physically appropriate but I don't think it's going to make a big difference in the result of the calculations.

CHAIRMAN FORD: I'm going back to reading this again. On 82?

MR. KIRK: 82.

CHAIRMAN FORD: Just a small detail. Where does the 14.4 come from?

MR. KIRK: The 14.4 --

CHAIRMAN FORD: Down on the left-hand side.

MR. KIRK: I know. I need to refresh my memory. The 14.4 -- here is one of those things that we haven't talked about in detail here. T_0 which is -- okay. This is a relationship indexed to T_0 which is measured by the ASTM standard. That has a size associated with it of one-inch thick.

The 14.4 reflects the fact that in general, of course, the vessels we're assessing are thicker so you need to shift that out a bit. That is the short answer. The more detailed answer we'll talk about later.

CHAIRMAN FORD: Okay.

MR. KIRK: Going through the flaws, and

this now -- so that completes the toughness and embrittlement model. We now get on to the flaw model. The graphic in the upper left-hand corner shows you where the flaw data comes in. This actually goes into the stress intensity factors so this is now on the driving force side.

We've developed distributions of flaws in fabrication welds, repair welds, cladding welds, and plate materials. Each of those distributions includes a description of flaw density, flaw size, flaw location, and orientation.

The information that we've used to construct these statistical models include experimental data and data from two expert sources including a model called PRODIGAL which is a computer code that actually arose out of an expert elicitation and, indeed, from an expert elicitation that Debbie Jackson and Lee Abramson completed probably about two years ago now.

This table describes the sources of experimental data. This has been a major program with the NRC department of research where we have actually gone out and gotten either ex-vessel material or material that was supposed to go in a vessel that never made it, cut it up, and destructively and

nondestructively examined it.

To follow the outline, I would like to start by talking about the assumptions and the process for model building. The assumptions fall into three categories; basic assumptions, assumptions necessitated by procedure, and assumptions based on observation under physical understanding.

I'll start with the basic first. The basic assumption is that except in limited cases we will employ no theoretical or physical model of flaw formation. That is not to say that some don't exist out there. It is to say that we didn't find them sufficiently well developed or have the ability to develop them sufficiently for this purpose.

Therefore, we are treating our experimental data as the highest truth so, consequently, since we've only got data, we don't have an independent model. These uncertainties will have to be treated epistemically. We employ expert opinions and models only when necessary and they have kept in to a small extent basically where we didn't have enough data.

The third point is that the inspected material, therefore, must be assumed to represent adequately all the material conditions of interest.

As a consequence, when we look back we said we had data looking at welds which is the most important thing in this analysis. We have weld data from two vessels, P.B. Rough and Shoreham.

When it came to deciding on the flaw distributions that we would use, you could take those two and put them together and essentially take an average out of them, or you could treat them individually and do what regulators are prone to do and use the most conservative one.

In this case we've used our most conservative data and assumed it to represent the whole of the population. The data is not average the reason being that even though -- certainly not to be overly critical because, as I mentioned to Bill before lunch, we certainly have much more data than we had here before.

Having said that, we've got extensive data on two samples of material. We are expanding that to all the vessels in service and we don't have a physical model to say if that is appropriate or not.

You certainly would hope that it would be appropriate because all these materials were procedured in the same standard. They were all inspected nominally the same way and so on and so on.

The judgement that's been made, you can characterize that as being conservative and you can say that it's conservative for the available data. As I mentioned before, we don't really have a good way to project to other data sets.

CHAIRMAN FORD: So when you look at this data, you are saying that, in fact, it's that?

MR. KIRK: No. I'm saying -- Dr. Ford is showing a graph of flaw size for the Shoreham and P. B. Rough and I think Shoreham was -- I can't remember which was higher.

CHAIRMAN FORD: Shoreham is higher.

MR. KIRK: Shoreham is higher. We use the Shoreham curve. We don't assume --

CHAIRMAN FORD: Oh, I see.

MR. KIRK: We use the curve. We use the distribution, yes. Looking at the procedural assumption, I'll try to go through these very quickly but feel free to stop me when you want. The largest flaws have been the subject of our destructive inspection recognizing that the large flaws will be driving the failures.

The small flaws have only been destructively inspected on a sampling basis and in constructing our distributions we've combined the

larger flaw destructive evaluation data with the small flaw NDE data in construction of those distributions.

All reported defects in FAVOR have been modeled as sharp cracks. I know the folks at PNNL are on the line. They designed their procedures to minimize the number of volumetric defects like porosity that were actually reported. However, having said that, they also wanted to be careful that no nonvolumetric defects were missed.

Consequently, certainly some volumetric defects were included in the characterization. In FAVOR we model those as sharp cracks and that is clearly a conservative assumption.

We've idealized complex clusters of flaws, porosity, so on, into single, simple, plainer elliptical or semi-elliptical cracks. That's certainly a customary procedure within ASME code. It's become a customary procedure because it's conservative in virtually every case.

However, I'm not going to sit here and swear to you that I couldn't come up with some complex cluster of flaws for which the K at some point at some location of the flaw front isn't bigger than the superscribed ellipse but we have adopted that characterization.

Flaws are measured in their true size and shape in PNNL's inspection, but in FAVOR they are assumed to lie only in the axial or circumferential direction simply because with FAVOR we had no intention of it ever doing mixed mode fracture calculations. Again, that is a customary procedure in engineering assessment, broadly conservative but I'm sure there are one or two exceptions to the rule.

We'll get to this on the next slide but we found that virtually all the flaws that we found in the weld metal were, in fact, on the fusion line. FAVOR assumes a bi-material -- well, it doesn't characterize the heat affected zone. We have weld metal properties. We have plate or forging properties. There's no properties in between.

However, all the flaws, or most all of the flaws, are physically found to lie in the fusion line so judgement had to be made regarding what properties, what chemical composition, to assign to those flaws in assessing our embrittlement level.

Lacking any better knowledge and not being prepared to model anything other than a bi-material, the decision was made to assume that the fusion line flaws were controlled by the more embrittled of the surrounding materials and that is obviously a

conservative assumption.

The weld flaw distribution that is actually used in any particular vessel analysis is based on a rule of mixtures from the different weld process constituents. In other words, if you go in detail into the PNNL report, you will find that they reported to us different flaw distributions for SMAW, SAW, and repair welds.

Within FAVOR we don't map where the different weld types are. We just know that there's a weld there. So what we've done, for example, most of the vessel is SAW so the result in flaw distribution is, I think, 96 percent SAW, one percent SMAW, three percent repair and that is assessed on a vessel-by-vessel basis but it's just a simple role of mixtures based on a random draw from the experimental data.

Obviously all of those things -- well, some of those things could be done better. Some of them perhaps could not be done better but all of them are outside of the scope of the current project.

Moving on to assumptions that have been based either on observation or physical understanding, we establish -- of course, we get these statistical distributions of flaws with tails that go on forever.

As Lee Abramson, our statistician, points out, just because you've never seen a quarter T flaw doesn't mean one might not exist somewhere, which is unlikely but perhaps true.

In any event, one needs to decide if you are just going to not truncate the distribution and have ever diminishing probabilities for very large flaws, or if you are going to truncate it somewhere. We took the decision to truncate it and we established limits based on physical arguments.

For example, the largest flaw that is allowed to exist in a weld is the maximum extent of a weld repair cavity. We picked those limits based on physical arguments like that that we believe to be conservative.

We have since done sensitivity studies in favor and found out that picking the limit, half as much or 10 times more, didn't make a difference at all. We believe it to be a realistically conservative characterization that really has no affect on the results. I've already discussed this before.

CHAIRMAN FORD: That rather assumes that your NDE inspection capability and the porosity of which you do it is capable of finding all those. Is that, in fact, the case?

MR. KIRK: Actually, that's a whole other interesting point, Peter, because we don't right now -- this has always been a subject of debate between us and the industry and the ASME code. There really is no explicit credit given for the inspections other than that it gives everybody a warm feeling that things are the way they expected them to be.

But it isn't like Mark referred to heat-up and cool-down curves earlier which were all based on the quarter T flaw or flaws at quarter T, three-quarter T, a good portion of the way through the wall. Somebody could right now inspect their vessel with the best available UT technology and show to themselves at least convincingly that sort of thing just isn't there.

The regulation still requires them to do that calculation as if the crack were there. In that sense, there's no credit given, nor is there credit given for vessel specific inspections as part of this project. That would be something again if we were talking somebody came in on a 1154 specific analysis, then the regulator would be stuck with trying to decide what level of effectiveness and probabilities of detection and sizing to apply for that specific case on a vessel specific basis.

MEMBER SHACK: They only assume fabrication flaws, too.

MR. HACKETT: Right.

CHAIRMAN FORD: So you're not going to be a stress corrosion crack. That's not even --

MEMBER SHACK: Not contemplated.

CHAIRMAN FORD: Not contemplated.

MR. HACKETT: That's correct.

MR. KIRK: I've already touched on the second row. Since virtually all of the weld flaws were found on the fusion line and that's where you would expect them to be anyway. That's where they've all been placed.

All flaws in the cladding have been assumed to exist parallel to the welding direction because that's where we found them all and because that's where you expect them to be. They are normally lack of enteron fusion. This is a change from the previous code where cladding flaws were all stuck in the axial direction because, well, heck, it was conservative.

Here we've got a very easy physical observation to make. They just took all those surface laws and essentially made them benign. The distribution of cladding flaws we've got very limited

data on.

We have a few observations so we base the overall trend on the model coming out of the elicitation code PRODIGAL relative to our experimental data that is providing what would appear to be a fit to the data. It basically goes through the data cloud.

Same thing with plate flaws. We have some data on plate flaws and we're getting more. However, the dataset wasn't large enough for our statistical people to feel comfortable basing it on the data alone coming out of the expert judgement process that Debbie and Lee conducted.

The experts told us that they expected small flaws which in plates were defined as flaws less than a quarter inch to have one-tenth of the density to be density, density, density.

If you had 10 flaws in a weld, you would expect to find one in a plate for small and flaws and one-fortieth of the weld density for large flaws. So we've used the weld flaw distribution and adjusted it down by those factors.

It turns out -- and this is perhaps just luck. It had to be because the data wasn't available -- that goes through the data. Then the last part is

that when we have flaws in axial welds or circumferential welds where cladding, we know based on the weld what way the flaw is going to go. Axial welds have axial flaws, cladding and circ welds have circ flaws.

In plate we don't have that preferred orientation and, indeed, the inspections of the plate other than laminar flaws, which we have discarded because they don't provide a fracture concern, there's no preferential orientation in the plate.

We found no preferential orientation in the experimental data so the FAVOR code when it generates a plate flaw has a subroutine that just does a coin toss and 50 percent of the time it's an axial flaw and 50 percent of the time it's a circ flaw.

Again, for the results you're looking at now for Oconee we could put them all axially and it wouldn't matter. For Beaver Valley it's obviously going to be a very relevant judgement to make because they are plate limited.

In the interest of time, I'll just show one result from the data. This shows the flawed densities that are being used. It's kind of a busy graph. I apologize for that. On the horizontal axis we have normalized the flaw density to the median flaw

density and the median flaw density is shown in the bar chart.

We are seeing out of the data what you would think you would see, that the small flaw, small being less than one bead size, less than one bead depth, I suppose, the small flaws occur with much greater frequency than the large flaws.

However, if you refer to the accumulated distribution function, the small flaws are much more tightly grouped. There is much less uncertainty about the size of those flaws than there is the large flaws.

This is just a diagrammatic representation of some of the information out of the experimental data. There are whole books on this so I won't go into that in detail but just some of the information that's going into the FAVOR code.

In terms of how we do the flaw model in FAVOR, as I pointed out before, the distribution used is either based on a rule of mixtures for the different weld types or unbounding cases in terms of what data we decided to use as noted previously.

In terms of how we treat uncertainty, since we have no independent physical model, the statistical uncertainty in the data is the only uncertainty that we explicit account for in the model.

We quantify that uncertainty by generating 1,000 different input files and randomly drawing from those input files when we go through a FAVOR run.

Certainly the algorithm to generate the flaw distribution could have been coded into FAVOR. This was simply an expediency to say, well, it's easier to write a preprocessor and treat this as input data. As I mentioned before, the uncertainty has been modeled as epistemic.

MEMBER SHACK: Can you come back to that flaw density curve?

MR. KIRK: Yeah.

MEMBER SHACK: How did you -- you take different pieces of the weld and you find the flaw density in that and then you measure them to the median flaw density for a given weld? What does this mean?

MR. KIRK: I'm sorry. Your question is how do you measure the flaw density?

MEMBER SHACK: Yeah. How is this determined from the experiment? I mean, is it saying that if I have three feet of weld, there's some small cubic location that actually has a much higher density of flaws and that is where I get the two?

MR. KIRK: I'm hoping PNNL is on the line

because I'm having trouble with this one.

Fred or Steve, are you there?

MR. SIMONEN: Can you hear us?

MR. KIRK: Yes, we can, Fred.

MR. SIMONEN: For the flaw density we might have examined, let's say, .05 cubic meters of repair metal. We found, say, six flaws, large repair flaws within that repair metal. Nominally it would be, you know, six flaws and divided by that amount of metal you get the average density.

Statistical uncertainty is that if you only find a few flaws in there, you don't know if you would have examined 50 times more metal that you might have found a different value. If you only find one or two flaws in a volume of metal, you can't say too much about the average density. There are statistical equations to characterize the uncertainty due to the small sample size.

MEMBER SHACK: Oh. So this is basically a sample size correction then?

MR. SIMONEN: Exactly.

MEMBER SHACK: Okay. Okay. Thank you.

MR. KIRK: Thanks, Fred.

We covered that. I'm sorry. Bill, did you have another question?

MEMBER SHACK: No.

MR. KIRK: In terms of looking at the flaw model and significant changes from the earlier analysis, on this graph the mean flaw distribution curves that were used in the Oconee analysis indicated as being for the weld metal, the base metal, surface flaws in the cladding. You can compare that to the Marshall distribution labelled M which was used in the earlier 1980s analysis.

Two relevant things to point out in comparing them with now is the Marshall distribution had in some cases bigger flaws than we have now but much more significantly all the flaws in the Marshall distribution are surface breaking flaws. Of course, that is a much more severe flaw than an embedded flaw.

Whereas now virtually all the flaws in the weld metal, virtually all the flaws in the base metal are embedded so there's a substantial change that has a substantial effect on the calculated probabilities of initiation and failure.

Finally, my wrap-up slide. This is the message or things we would like you to remember coming away from the PFM summary. In the area of toughness models, we have referenced all of our models and our uncertainty analysis to both toughness data and

physical understanding.

We have removed a significant conservative bias in the un-irradiated index temperature. We have removed small non-conservatism in the arrest model. And we have explicitly treated the aleatory nature of toughness uncertainty where we've quantified it and it's being treated in FAVOR.

In the embrittlement model, again we have referenced to, in this case, Charpy data. I should say in physical understanding. We have a correlation with an improved empirical and physical basis and we have corrected for slight biases in the Charpy-based shift estimates.

In fluence, which we didn't get into in detail but it certainly bears mentioning, in the earlier analyses the peak fluence in the vessel was assumed to exist everywhere on the vessel.

Now we use a fluence map that's calculated by Brookhaven National Lab which shows that the peak fluence, of course, only occurs at very limited locations in the vessel so by allowing the fluence to vary over the vessel surface in a realistic way, essentially huge portions of the vessel the radiation levels drop substantially and effectively drop out of the picture. That is a substantial change.

Flaw distributions while based on limited data are, indeed, based on significantly more data than before. Most flaws are embedded rather than being on the surface. However, I should indicate that relative to the previous analyses where we would put the circa 1980s analyses would put one considerably larger flaw in each of the subregions. Now we've got many, many more flaws than before.

The vessels are now seeded with, depending upon the analysis, somewhere between 5,000 and 10,000 flaws which is indeed appropriate based on our experimental data a significant departure from before but it appears that the effect of making those flaws buried and making them smaller has more than overcome the effect of having many, many more of them.

That's the summary for PFM. I appreciate your endurance.

MEMBER SHACK: Can we go back to the model for just a second?

MR. KIRK: Sure. Which one?

MEMBER SHACK: Just the size distribution.

MR. KIRK: This one?

MEMBER SHACK: No, the next one. There's an expert judgement that the distribution is truncated, for example, at 23 percent for the welds?

MR. KIRK: Yes, 23 percent. I believe the number was based on the extent of a weld repair cavity that is allowed by ASME and I believe that was two inches. It happens to be 23 percent of the Oconee vessel wall. I'm trying to remember the logic we had on the base metal cut. I think we all rationalized that it should be less than weld metal and I think that was about it.

MR. HACKETT: I think the base metal was a judgement amongst sort of the working group that was involved with that.

MEMBER SHACK: What we're seeing here is some sort of a multiplication, right? There's a size factor times the density distribution to get the number.

MR. KIRK: Yes. That's right.

MEMBER SHACK: The size distributions, those are going to be universal and the density will be -- oh, the density is universal, too, because we don't have an individual vessel type.

MR. KIRK: That's right. The density that we measured on the Shoreham vessel will be assumed to apply to all the vessels. The size distribution will vary slightly, and I emphasize a slightly bit, relative to the ratio of SMAW to SAW to repair. That

changes the rule of mixtures adding that gets in here. Since the vessels are made virtually all from SAW, that's what's dominating the distribution. Again, essentially, yeah, a universal size distribution as well.

MEMBER SHACK: Now, until we get to the cut-off, are those slopes basically coming out of the analysis of the data from P.B. Rough and Shoreham?

MR. KIRK: Yes.

MEMBER SHACK: So those are empirically based?

MR. KIRK: Those are empirically based. That's right.

MEMBER SHACK: And then an expert elicitation says it sort of really tails off pretty steeply at some point?

MR. KIRK: Well, the expert elicitation was do you expect to find a flaw of infinite size? Well, no. Okay. Well, how much smaller? Well, that's why we're experts. Pay us more.

MR. HACKETT: I guess I would make the suggestion this is another good break point.

CHAIRMAN FORD: I was about to say exactly the same thing.

MEMBER SHACK: Can I ask one more

question? Is 1,000 samples enough? I mean, does that quantify your -- you know, what uncertainty does that give you if I sort of do my order statistics? Is that what I do to get confidence bounds on these things?

MR. KIRK: Yeah, I think the -- well, I'll take a cut at it and maybe Terry or Fred would like to chime in. I mean, I think of it as like two certainly isn't enough, 30 certainly isn't enough. You want to take enough draws from the mathematical model that we have constructed to reproduce the scatter that we've seen in the flaw data. Certainly 1,000 covers that to me. A more mathematical explanation you're going to have to ask somebody else.

I don't know. Terry, do you have a thought on that?

MR. DICKSON: I agree with you.

MEMBER SHACK: Okay. So you weren't aiming for some percentile confidence bound by some order statistics argument?

MR. DICKSON: Terry Dickson, Oak Ridge National Laboratory. Actually, I'll defer to Fred Simonen for the definitive answer, but I believe he's going to tell you that he did a Monte Carlo analysis basically with the intent to reproduce the scatter in the data that was seen.

MR. HACKETT: Is that right, Fred?

MR. SIMONEN: Yes. We thought 1,000 was more than enough samples. Just given the number of actual observations we had, I don't think we could say if we get 10,000 we could be doing anything more realistic than the amount of data we had collected on the flaws.

MEMBER SHACK: That's true. I guess there is no point --

MR. SIMONEN: I guess we didn't want to go beyond that. It just generated a lot more data for Terry to handle internally within his computer code. That is kind of a judgement as to what would be a reasonable number of files to give Terry to manipulate within his FAVOR code.

CHAIRMAN FORD: Bruce.

MR. BISHOP: Bruce Bishop at Westinghouse. What that is is 1,000 different flaw distributions. The flaw distribution is sort of the measurement of the uncertainty in the density and the flaw size. Actually what you're saying is we don't know that exactly.

MEMBER SHACK: There's only one flaw distribution, right?

MR. BISHOP: No, there's a 1,000.

MEMBER SHACK: This is all epistemic uncertainty.

MR. BISHOP: What we're saying is we are representing that uncertainty as we're not sure that we know that one distribution exactly so we're going to simulate using Monte Carlo simulation the uncertainties around that distribution and that's where the 1,000 distributions come from.

Is that right, Fred?

MR. SIMONEN: That's correct.

CHAIRMAN FORD: Okay. Bill.

MEMBER SHACK: Yes. I'll think about it some more.

MR. HACKETT: And next we'll come back and pick up the thermal hydraulics portion.

CHAIRMAN FORD: I'd like to talk with you and the colleagues here about that.

MR. HACKETT: Okay.

CHAIRMAN FORD: Let's recess until 25 past. Then we'll find out the best way of doing the rest of the time we have.

(Whereupon, at 2:13 p.m. off the record until 2:33 p.m.)

CHAIRMAN FORD: Okay. We have decided to keep to the schedule, albeit a revised schedule. We

are going to revise the revised schedule.

MEMBER SHACK: You even said that with a straight face.

CHAIRMAN FORD: We'll talk about --

MR. HACKETT: I think this means we'll be picking up on slide 27 in everyone's package.

Professor Mosleh from University of Maryland will be making a presentation.

MR. MOSLEH: Yes. I am actually representing a large group of people who work on this part of the problem. As Alan mentioned in the morning, this has been truly a interdisciplinary, interactive work.

I would like to acknowledge the contributions by other team members, Kasyz Almenas, Professor Almenas for the University of Maryland, an expert in thermal hydraulics, Bill Arcieri from ISL, an expert in thermal hydraulics, Dave Bessette from NRC, expert in thermal hydraulics, Dr. James Chang, an expert, among other things, in thermal hydraulics at the University of Maryland.

If I were to kind of divide the areas of expertise among all these thermal hydraulic experts I'm the least experienced and that's probably why they asked me to be the presenter. If you divide the areas

of expertise to even substantive expertise, I have the normative part and the rest of the team has the subcentive expertise so I will defer the questions on that subject to my colleagues and team members.

This as a way of introduction, we are in the middle block, thermal hydraulic analysis. But as a major block within all other pieces or aspects of this problem, one of the key requirements from day 1 was to develop a consistent, internally and externally consistent set of methods and techniques with respect to treatment of the value modeling issues, as well as uncertainty and primary uncertainty and consistent within disciplines and compatible across disciplines.

Then you see examples where compatibility became actually a constraint or a driving source or mechanism for our analysis as a boundary condition.

Back to what Dr. Ford brought up this morning which was an important aspect to address, and that is managing such a large effort with respect to interactions among the team members. This was not easy and not obvious from the beginning.

As a result, a number of activities went in parallel. At the end or the middle when we compared notes, we recognized that the need to be a certain type of interaction, lessons learned from the

work, that, indeed, toward the end it became almost one team effectively frequently interacting and exchanging notes and providing feedback and feed forward to different pieces of the methodology and then the process. That in itself deserves, I think, much attention as a model for future activities.

One aspect of this was recognition, of course, of the key issue and uncertainty assessment which we obviously had the intention and then we did an assessment of uncertainties that we could indeed address explicitly.

But also part of the uncertainty assessment became uncertainty management in the way that Alan mentioned this morning where we would actually refine the models to remove the sources of uncertainty and, therefore, improve the modeling process. That was also, I think, an important aspect of our experience.

With this introduction, I would like to give you an overview of the presentation. Talking about initially in the beginning constraints and assumptions of the model and the entire process. Some notes about RELAP model which is a core of our analysis, the method that we use. We call it top-down. Probably not the right terminology for that.

Method of defining plant states that are PTS significant to enable us to identify what areas to focus on, what accidents and areas made sense, and what things we need to hand to the PFM folks for further analysis. In addition, obviously, we also had to devise a method of identifying which RELAP runs, which hydraulic runs we needed.

With that, our job being primarily on the uncertainty side, which is my part, method of identification of dominant sources of uncertainties and the characterization of such uncertainties.

Finally, applying these tools and techniques that we developed throughout we ended up with a set of thermal hydraulic runs that represented not only the best estimate but also the uncertainties with the corresponding frequency distribution.

Constraints and assumptions, kind of a high-level view of the issues that we have to wrestle with. I'm sure some of you are familiar with this in Europe and also in the past in the U.S. for characterizing and addressing uncertainty thermal hydraulic.

You know that much of the effort has been focused single scenarios and we can do quite a reasonable job by focusing narrowly on a single

scenario here. We were faced with the task of about 10,000 or so, 10 to the fourth, a number of scenarios each of which in principle would be assigned to a specific thermal hydraulic run.

We had to reduce that to about 100 or less and that was the concern at the beginning imposed by obviously resources as well as input requirements or formats for the FAVOR code.

In addition due to the complexity of thermal hydraulic models, as you know, and inherent nonlinearities, we had to simplify the process using screening criteria, screening tools to help us focus on those aspects of uncertainty that merit consideration and explicit modeling avoiding the nonlinearity to put kind of almost a panaverbal barrier between us and the solution or the answer we were interested in.

One of the other assumptions that we made was that RELAP was essentially a proper model and subject to the variabilities and uncertainties that we recognize when addressed to the extent possible that that tool provided a reasonably good answer to the characterization of the pressure and temperature traces that we were interested in.

A few notes quickly on RELAP model

description. The experts among the team who ran RELAP started with the Oconee model developed by INEL in the early '80s in the original PTS evaluation study, introduced some setpoint changes to update it to the current plant values.

One thing is that they change the nominal value for the temperature or that the temperature was set at 70 rather than 90 which was in the '80 study. We used that, by the way, as a valuable in our uncertainty assessment later.

Some control models were added to enable simulating operator response and action times which Alan mentioned in the morning also. A few other model corrections were introduced. For instance, to test the problem with the overfilling of the intact steam generator they introduced a control or EFW flow so that they could be controlled independently.

As kind of a step toward refining and making the code more accurate, they added two dimensional downcomer model as opposed to the 1D which is applied to the rest of the code.

Do you have a comment, Dave, on this? No? Okay.

What is different in this particular study? Well, the fact that we had a faster running

code and something that we could manage a few hundred runs was a tremendous help compared to the 10 or so runs that was the basis of the 1980 study. This allowed us to run many, many cases and do sensitivity and parametric studies to help us screen out factors that did not matter and zoom in on things that contributed significantly to the uncertainty and the basic characterization of the plant thermal hydraulic response.

In the AF input preparation there is not much change. It is still time consuming. However, the graphical output capabilities have improved significantly. We use that a lot during our assessment of the scenarios and thermal hydraulic behavior.

What's different, again, and probably the most important part of this presentation, that they allowed us to do uncertainty evaluations and uncertainty assessments, an integral part of this round as compared to the 1980.

And some new insights with respect to the validity of the 1D model based on exponential results and some other computation for dynamic results were basically things that enabled us to convince ourselves that certain assumptions we were making were valid or

acceptable at this point.

Other things is that the new code has the capability of addressing the two-phase flow versus the old five equation models that we have been using in the past routinely. That is an expanded or enhanced capability in that sense.

This is an overview and I would like to spend a little bit of time on that because it is going to be helpful in understanding how the whole process actually generated the results. Even though this has been kind of shown before, I would like to point out specific things that relate to the term hydraulic uncertainty.

On the left under PRA event tree side, the yellow box on the screen, you see scenarios coming from PRA and each scenario has an associated uncertainty on the frequency or probability of occurrence so those little curves that you see on the side of each scenario is the uncertainty distribution or the frequency of occurrence of the event.

That is about 20,000, 30,000 or more. I don't have the most recent figure but there are many such scenarios obviously. One step in reduction happened in the interface between the yellow box and the green box, namely we have to map tens of thousands

of scenarios to a limited number of thermal hydraulic runs. That's one reduction process that we had to go through.

What you see as a distribution on that side of run 1, run 2, etc., is an aggregation or the sum of all the frequencies of all scenarios that have been assigned to that particular bin. You see the same thing done for the remaining runs, thermal hydraulic runs.

MEMBER SHACK: Now, run here means bin, right?

MR. MOSLEH: It's a bin that initially assigned the most representative distribution of thermal hydraulic run to and then we go through an expansion asking whether that is kind of sufficient representation of that particular bin. In some cases we added more runs and in some cases the bin remained one run bin.

Initially we had about 300 cases to play with in terms of runs, although we were shooting or focusing on reducing it to less than 100.

Now, at that point, addressing the uncertainties added -- you know, the requirement for adding runs or expanding the existing pool. Please note that while we were looking at mapping the PRA

event tree sequences to the thermal hydraulic runs, we identify additional runs we needed to do and then identify sources of uncertainty that we could actually put back into the event three and, therefore, expanding the event trees and reducing the number of uncertainty parameters we had to consider.

Back and forth between the green and the yellow box we were iterating to optimize what we would then consider in the thermal hydraulic uncertainty case.

The reason being is for each scenario, as you will see, we identified multiple sources of uncertainty and variability that needed to be considered on top in addition to the characteristics of the scenarios going into those bins and the few the better because you are talking about the communitorial explosion if you have many, many factors. Each factor sometimes is a continuous variable so we have many values to consider and so on and so forth.

Moving from the RELAP characterization of bins and assigning them to multiple scenarios, we have one uncertainty analysis task starting with these bins and adding flavors of various parameters producing multiple curves, multiple versions or variations of the initial set of thermal hydraulic results;

temperature, pressure, and transfer coefficient.

What you see in the middle box, the brown box, the first of one group, you see three curves resulting from one such uncertainty consideration and the corresponding fraction of times, P1, P2, P3, that each of those situations will emerge. I'll go through these in more detail but this is an overview.

At the end, obviously, you naturally need to modify the initial frequencies coming from the PRA by the probabilities that reflect the uncertainties that now we have added. You have gone from one thermal hydraulic run to three thermal hydraulic runs each of which has a certain probability of occurrence, P1, P2, P3.

You modify the frequencies coming from the PRA by these probabilities, P1, P2, P3, and you generate a new frequency curve that is assigned now to each of those sets of thermal hydraulic traces and those are passed on to the PFM analysis where the frequency distributions and the conditional probability of failure of the vessel will then generate when summed over all scenarios the total frequency of vessel rupture as a result of PTS type event and that is the very last distribution that you see in the blue box.

You have expansion and contraction process taking place addressing different types and flavors of uncertainties, variability, spectrum of scenarios, and so on and so forth. That's an overview.

The process is outlined in this set of blocks. I just wanted to comment on a couple of things. The yellow boxes, the lighter shade, essentially referred to the process of identifying what mattered in PTS from a top-down approach.

You have a disturbance in a plant and what kind of situations can you get to a potential PTS scenario. This is the type of thing that is typically done in a PRA space.

In this particular case for ensure that we have a comprehensive and complete coverage of PTS scenarios we have this top-down approach utilizing basic principles, heat balance in the primary system and applying certain characteristics of the plant itself, in this case Oconee to identify the classes and categories of PTS potential scenarios.

That is kind of the yellow boxes. That parallels and shares many steps, many phases with what Alan presented this morning in his functional event tree perspective.

Then the brown boxes cover the uncertainty

layer on this. Initially even though we went through a significant reduction in the number of scenarios, still there was no point or no net gain from addressing uncertainty on everything so we went through further reduction to zoom in on those cases where uncertainty assessment will actually be cost beneficial.

Through that process we have identified important sources of uncertainty, reduced the number of parameters that we had identified initially to fewer so that the problem would be manageable in size. Size management in this case was a complex process, not very obvious as to what one would actually use and rely on to reduce the number of runs, cases, variables.

We went through the typical screening that we do in PRA, namely frequency screening. Whenever the frequency dropped below a certain level, we just did not consider those. And also with qualitative criteria engineering judgement about the impact of various scenarios.

The starting point that was put together by Professor Almanas initially was this very simple but very informative model of a reactor where we have the core and the reactor vessel, hot leg and steam

generator, the pressurizer, the steam generator secondary side, and the part that we're interested in, namely the reactor vessel downcomer in the yellow highlighted area and various links and lines of the cold leg, the hot leg, and the various sinks and sources of heat and mass and energy.

As a guideline you could look at this and say what could happen that would get us to a situation that has a PTS potential? What are the heat sources? What are the heat sinks? Then from this we identified a number of parameters or characteristics or events in PTS scenarios that would impact the temperature and pressure.

Remember, we are not interested in all possible set of thermal hydraulic characteristics and what happens to them in these scenarios. We are interested in the impact on the temperature and pressure, the downcomer.

Things that affect the temperature, I don't want to go through this. These are kind of fairly obvious in a way, some of them at least. Things that would affect the temperature are the heat capacity and heat source and heat sinks, such as core flood tank and high pressure injection steam generators.

The coolant flow rate affects the temperature, namely the RCP state, the reactor coolant pump. A couple of things that are Oconee specific, the possibility of mixing of core water into downcomer due to the event valve that they have in Oconee, and the number of phenomena that people have identified in the past as possible source of impact on the temperature, the flow resumption and interruption and the boiling condensation.

On the pressure side you have some of the same phenomena and same contributors, the mass, the energy change in the RCS, and the mixing of the core water and the downcomer, so on and so forth.

Through this process of kind of a simple picture that is the beginning of any serious work in thermal hydraulics basic principles we identify these sets of variables and we know that you are dealing with large RCS heat capacity and these significant heat capacities require a large heat loss to decrease the downcomer temperature fast enough to be of PTS concern.

This is an example to show graphically and numerically that the heat sources are smaller than the dominant heat sinks that we have identified, namely a two-inch surge line break which can easily overcome

the decay heat decreasing the temperature.

On the left-hand side you see the primary side break case and the impact of a two-inch or eight-inch break, compared to the black line going up that shows the net affect of the decay. The heat sinks, the dominant ones, basically can actually put us in a PTS situation by themselves.

On the second side we have a similar situation, main steam line break and the SRV stuck-open case. These are cases that could get you to also a cooling effect that is not compensated by the decay heat. These are obviously the dominant sources of heat or other minor sources of heat such as the energy that the pumps put in but they are not dominant obviously.

This was mentioned earlier this morning. To characterize scenarios on a screening level, we looked at things that would give you a cool-down ramp of more than 100 degrees and temperatures falling -- downcomer temperature falling below 400. As was mentioned several times, we did not use these as a sole screening criteria.

In every case we had our eyes on the pressure behavior making sure that we are not missing anything that would be the result of rapid change or

significant change in the RCS pressure that combined with the temperature behavior would get us into trouble.

In general we realize that the tendency was that the thing that PTS was most sensitive to were the temperature. If you look at the three variables, temperature, pressure, and the heat transfer coefficient, if you were to rank those, I would rank them as -- we would rank them as temperature first, pressure, and then the heat transfer coefficient. It turned out actually the heat transfer coefficient is not a factor at all. We can vary it by some factors and not get any significant impact or effect on the result.

These things we are using as guidelines to help us kind of put our arms around the problem, the multi-dimensional problem of how many variables do you change at a time. Well, you know, first we look at temperature and then we look at pressure. In that order we can reduce the number of factors to consider at a given time to a smaller number.

The earlier presentation so far as focused on kind of general characteristics, general considerations. At this point I'm just showing that there are certain peculiar features of, in this case,

Oconee that needed to be considered. In this particular case the existence of the vent valve that could create a flow path for the hot water or steam getting to the downcomer. That was a factor in our analysis.

As a result of these considerations looking at the various factors, the team came up with a matrix representation of the -- this is an event space where you look at the primary and the secondary states and you then look at various combinations of those. On the primary side we have the primary side intact or a break or breach in the integrity of the primary and there is a dividing line between at 1.5 inch break size.

That dividing line is based in the capacity of HPI to compensate RCP trip set points. The dividing line also gives us a zone on the left-hand side below 1.5 inch versus above 1.5 actually decoupled the primary and the secondary.

On the secondary side you have things such as whether you have breach of one steam generator or two, or you have an overfeed case for one of the steam generators and the combination of these so there are a number of possible states.

And the combinations of the secondary side

and the primary side the state gives us the matrix. Some cases obviously are not of PTS concern, not a concern at all such as intact on the primary side and nominal situation for -- a normal situation for the second side.

As we go through the matrix we can identify certain characteristics that are unique to the cells of this matrix. Each cell is further divided to subcells, depending on state of high-pressure injection system ,that plays an important role in some of the same areas.

We use this as a guideline and, at the same time, a more event tree driven not quite bottom-up but event driven picture on the left-hand side that was displayed in the morning by Alan.

The insides and characteristic stat were identified on the right-hand side through this top-down approach to PTS event matrix were consistent with the results of the bottom-up approach. Collectively we decided that we have a reasonable coverage of value scenarios as classes of scenarios at this point.

Each cell in the matrix or each branch in the functional event tree on the left represent and in principle contain a number of T-H classes. We populate the matrix with T-H, thermal hydraulic runs.

Each thermal hydraulic run represent many, many scenarios. Sometimes nine or 100, 1,000 scenarios were grouped into one thermal hydraulic bin because of their common characteristics.

Once you have this matrix through an iterative process, we combine cells based on similarity on PTS characteristics. If there are certain things that tell us, well, you know, cell X and cell Y could be combined and represented by the same classes or groups of thermal hydraulic runs, we did so.

In this process we reduced the number of thermal hydraulic runs we needed and also identified new runs that we did not have because we saw an obvious gap or hole. Then we mapped the PRA event tree sequences, those tens of thousands of sequences, individual sequences or groups of sequences, into the T-H runs that we identified at that point.

We went through a series of frequency and PTS impact engineering judgement and engineering assessment to screen the number of scenarios that we needed to consider basically. It's a typical screening that, again, was mentioned this morning based on frequency and impact without having the benefit of the PFM runs at that point because the

FAVOR code was not ready for production use.

However, at the end we have this pleasant feedback that the criteria we applied had actually worked and none of the ones that we labeled as insignificant, even though we passed a subset of those two for tests, turned out to be important, about 100, I think, scenarios. I don't know the number. Is it 50, Terry, of sensitivity cases?

MR. DICKSON: Fifty sensitivity plus 47.

MR. MOSLEH: Right. Fifty sensitivity plus 47 actual base scenarios that were used in FAVOR code to generate PFM results indicated that the ones that we label as insignificant turn out to be insignificant.

MEMBER BONACA: Okay. Just a second. The previous view graph, just to make sure I am still following you, this is still in the binning process?

MR. MOSLEH: Yes.

MEMBER BONACA: Okay. So some of these branches, in fact, will end up in some bins that are not particularly starting with the same sequence.

MR. MOSLEH: Right.

MEMBER BONACA: The same initiator.

MR. MOSLEH: That's true.

MEMBER BONACA: The other question I had

was you presented before a simplified model to highlight what the controlling parameters would be. Again, it was always used for the purpose.

MR. MOSLEH: Yes, only.

MEMBER BONACA: Okay. At this stage you are back now into the more sophisticated RELAP5 model.

MR. MOSLEH: Absolutely. These are just guidelines for us to think through the problem.

MEMBER BONACA: Okay. Good. I just wanted to make sure I am not losing the process.

MR. MOSLEH: So at the end of this process we had a number of scenarios, groups of scenarios mapped into the cells of this matrix. The numbers that you see in this matrix represent the sum of the frequencies of events going into various categories in these cells.

You can note very quickly that the one that I've highlighted yellow actually contains about 94 percent of the total frequency. Of all the scenarios that were identified and analyzed being of PTS significance and applying the screening criteria of 10 to the -8 or less being screened out. When you add all those scenarios, 95 percent of those fall in that particular cell containing a number of dominant scenarios one of which will be the subject of example

tomorrow.

The reason we were interested in identifying such a cell and you are looking for something like that was that instead of now doing uncertainty analysis on all the cells with or without number, we could focus on that particular box.

Covering 95 percent is well within the range of things that we do routinely in PRA. We do uncertainty on the dominant scenarios rather than everything because that's where you see the impact on the results.

Taking that as a guideline, this particular cell includes a number of classes of scenarios including stuck-open SRVs, whether they remain open or reclose, two different flavors of the scenario. LOCAs between 1.5 inch and 4 inch in diameter and LOCAs between 4 inch and 8 eight in diameter are among the things within that particular cell, the yellow cell.

Then we start looking more seriously and all these things in a way happen in parallel more or less because while you were doing the mapping and classification and characterization we were looking at the various sources of uncertainty and variability. We identified the sources of uncertainty and grouped

them into two classes, two groups, those that deal with the structure of the model and the modeling process.

Those which are not in the first group and they were easily characterized as parameters of the models. The line is somewhat arbitrary in a theoretical discussion between model and parameter but, you know, what we have listed there as model uncertainty is what we mean by model uncertainty.

Namely, the process of event sequence modeling and mapping scenarios to T-H run. That process is a source of uncertainty, the process of constructing the PRA model and the mapping and assigning thermal hydraulic runs.

Then they have for each T-H run that you're running and assigning to different scenarios you have sources of uncertainty associated with the RELAP code itself and regroup them into the so-called internal uncertainties and RELAP input deck preparation and nodalization.

What you see here, if I go back from the top, on the first sub-bullet, even sequence modeling and mapping to T-H runs, the first is the fact that you don't have all the possible details in the event trees.

That was mentioned in the morning extensively and discussed by Alan that there are certain inherent assumptions that you are making in constructing event sequences.

What we did here, and I think it's an improvement over the conventional way of doing PRAs and, for that matter, actually the first PTS study, is that whenever we identify things that -- sources of uncertainty, model uncertainty, sources of ambiguity and variability that was best addressed by adding and refining the event trees we did.

We would negotiate back and forth and Alan and the team will go back and add things to the event tree or they will identify things that would help reduce the uncertainty variability so now you are removing sources of uncertainty. The most important ones, the most relevant ones I would say are already addressed by improving and adding details to the model.

Second sub-bullet, assignment of event tree scenarios to T-H bins. In the first round of assignment of T-H bins to the scenarios, we were looking at explicit signature, very specific signature of an event for which we would run a thermal hydraulic run. That initial assignment carries very little

uncertainty because the term hydraulic run is actually tailor made for the scenario.

The second stat you now expand and you have a number of representative T-H runs for a spectrum of scenarios that are going to the bin. This is the part that required judgement and discussion and assessment. There are a number of explicitly treated parameters there that we considered such as operator timing or equipment operation timing, parameters external to the core and to the reactor affecting the thermal hydraulic behavior of the reactor.

These were types of things that help us to have a more explicit treatment and representation of this source of uncertainty, namely the assignment of representative runs. That's where we actually will show you examples of how we expanded the thermal hydraulic runs to address the source of uncertainty availability.

RELAP code. I have a list of relevant modeling uncertainties in the following view graph. It is a computational model. We identified a number of sources of model uncertainty in there. We think we treated the most important ones. That's our judgement.

What we did not treat in the second

bullet, nodalization, is not treated explicitly but we used ISL and NRC used a fairly detailed nodal model. I think it's a realistic level of detail. I'm told that if he had gone to 500 nodes, it would not have significantly improved.

Certainly we are not staying at the 10 node level so there is an optimum level of nodalization that has been used and there is a reasonably high degree of confidence about the quality of such model.

Nevertheless, we did not treat that source explicitly.

Parameter uncertainty, all of the things that I don't cover in the first such as boundary conditions, are included in the parameter uncertainty such as, say, temperature of the RWST. The important ones were treated explicitly.

Now, once you go from the top to the bottom progressively, most of the time with a few exceptions, you are going from primary sources of uncertainty or variability to the second resource of primary -- sources of uncertainty primary to secondary. In what sense? In the following sense.

We are trying to address or quantify address most dominant sources of uncertainty. There are many other sources of uncertainty that their

impact is not significant. We were not planning or did not intend to cover all.

In terms of net effect from the top to bottom, you see cases where you have the scenarios that define a spectrum of scenarios that are mapped into a particular cell in that matrix that I showed earlier. The variation from scenario to scenario is a bigger source of uncertainty than the variation in, say, some coefficient within RELAP. Okay?

As such, I call one primary and the other secondary. Not in terms of complexity because the second one is a much more complex problem to handle. Fortunately, the impact was smaller than we initially thought and that way we were able to address the uncertainties without solving all the problems associated with modeling in thermal hydraulic.

A few comments about what I mentioned earlier, and I think it was in response to a question Dave addressed in the morning. Also we are concerned about the 1D volume averaging nature of the code so there is a downcomer to the modification that has taken place.

We use experimental results, the Oregon State APEX facility and computation flow dynamic runs and results to get a sense of validity of the 1D

computation back to the scenarios that we're interested in. The judgement of the experts within our team is such that that issue is not a significant one.

Empirical correlations, a number of which are listed in the next view graph, was a source of concern and we thought that they do have an impact on the results so we treated the most important one of those explicitly. That in a nutshell is a list of things that at the end of this process we zoomed in on.

On the parametric side we also sometimes call them boundary conditions because they are not inside the code or RELAP run. They are the primary size break, the primary size break location, decay heat, season, like winter, fall, the temperature outside.

HPI state up or down, partially function, HPI flow rate, and the core flood tank pressure were the ones that we characterized as boundary condition or parametric uncertainty and they turn out to be all aleatory in nature because there are externally controlled or variable in nature so to speak.

There is more of an inherent variability in the character than the things on the left-hand side

which are internal to RELAP code and they are modeling in nature. The amount of circulation that we get in the primary is addressed by a modeling trick using the vent valve as a way of simulating circulation.

In fact, what I list here as sources of uncertainty are also surrogate ways of addressing the uncertainty. A number of things such as the component heat transfer, which is a calculated number based on a model within RELAP, we address that using surrogate modeling methods.

Flow resistance is the same. Break flow area rate calculation in RELAP is a source of uncertainty. We came up with a way of addressing that through surrogate models. This computational mixing of fluid, the reverse flow in the cold leg, which is the result of a numerical fact inside the code, was also a source of concern that we tried to address.

So we have a classification of aleatory, epistemic in terms of the character of this uncertainty, whether all the modeling ones are more epistemic because we claim that we better models with developing and utilizing more refined models you remove that source of uncertainty so it's knowledge driven. The other ones on the right hand side are nature driven so we don't have much control over

those.

So as you can see in this view graph, you have, I don't know, maybe 10, 12, 15 parameters to vary and each parameter has a certain range. How do we then identify and actually get the impact of these factors combined when we vary them at the same time?

That's a very difficult problem to address given not fundamentally or in principle because you can run the code billions of times and get an answer for each possible combination and come up with maybe a little bit more efficient sampling techniques and then reduce the number of runs that you make. Nevertheless, it was much more than we could afford and much more than we could actually afford to give to the PFM folks for analysis.

So we decided to use ways of simplifying the problem, reducing the number of variations that we needed to consider, and find indirect ways of identifying representative thermal hydraulic scenarios -- few representative thermal hydraulic scenarios for this massive number of communications that you could possibly get.

The first step was to go through one-factor-at-a-time kind of analysis sensitivity. About 200 such cases were run. Actually 200 plus because

sometimes we had a base case run, one of these 200 cases, changing one parameter, but we kind of had a sense of how RELAP responded to changes in input values so we developed additional sensitivity cases by extrapolating from existing runs so there were a large number of sensitivities that we did to get a sense of the impact of individual factors on the result, on the temperature and pressure.

Primarily focusing on temperatures, I mentioned earlier, we looked at the temperature first and then pressure so one factor at a time. It was a method of looking at the influence of individual factors.

Now, each of these sources of variability or uncertainty such as decay heat or season or HPI state, functional state of the high-pressure pumps, some of them are continuous random variables and some are discrete.

Like the state of HPI is off/on or partially functioning four or five states. Some deal with temperatures that go from 20 degrees to 80 degrees so it continues. We couldn't run all these small increments on the values of the parameters.

We picked representative values from the range and for each range we had an uncertainty

distribution, epistemic uncertainty distribution so temperature could range from 20 to 80 and you have a normal distribution centered around maybe 4560. Then we say let's represent this curve using a discrete set of points, 3, 4, 5 points. We did that and started varying the parameters.

MEMBER SHACK: So that's like saying you took the 5 percent, the median, and the 90.

MR. MOSLEH: Something like that. Or a DPD which meant we collapsed the total probability into three discrete points with probability mass so 30 percent of the mass is centered on one point, say 40 percent on another one, and one minus the rest which would be on the third one. This is called DPD or discrete probability distribution.

And to get the effect of the simultaneous impact of the factors we came up with another computational trick that I will mention later. This is an example of many, many sensitivity analyses that were performed where an average temperature change, 10,000 second average on the downcomer temperature is plotted against the different break sizes going from 2.8 inch to 8 inches for two different states of HPI, the nominal and the failed state.

We can see easily that after about 6.6

inches or 5.7 inches the state of HPI doesn't matter whether it's functioning or failed. Similar sensitivities, tons of those were performed to give us a sense of how things change.

This was to have a sense of the parametric variables. On the modeling side I mentioned a kind of surrogate model. This is a case where we recognize -- the experts recognize that the treatment of break flow rate in RELAP is a source of uncertainty.

In fact, the current version of RELAP calculates what we have shown here with the red line second from top. For different break sizes that's the flow rate that the code calculates at a certain upsteam pressure, I think is 71 bars.

There according to older version of RELAP choose the Ransom trap model. That's the green one. The new one is, I think, Henry-Fowski model we've been using since '98 in RELAP in the alpha -- what is the new version, gamma version?

MR. BESSETTE: I forget.

MEMBER SHACK: The gamma.

MR. MOSLEH: The gamma version. The red one is the one that all our thermal hydraulic runs are run with that. However, we recognize that because of the uncertainty in the computation of the break flow

rate, that if you go to two extremes, namely to the black line, which is a frozen model, and the hem line, which is the blue one right there at the homogeneous equilibrium, mixing that, in fact, just for model uncertainty associated with the break flow rate, if you have four different, at least, variations, four different options, two granted or extreme and the ones in the middle are more realistic, nevertheless, there is a range.

Now, we couldn't really change the code and the computational capabilities of the code so we came up with a surrogate way of addressing the effect of the break flow rate computation. If you note that we can get the same variation in the mass flow rate by changing the size across. You change the size horizontally and you get the effect of the mass flow rate change vertically.

There are a number, four or five such numerical surrogates or methods that we have to have to represent these modeling uncertainty sources within RELAP.

This is a graph that shows on an average temperature sense an average over 10,000. You can look at one of the cases, a 2.8 inch surge line LOCA that if you keep the size at 2.8 inch and vary all

other parameters ranging from cold leg break location all the way to break area temperature in the outside, winter, summer, whether you have the event at the hot zero power or at power, HPI failing or functioning, you can see that the net effect of many different parameters in one graph and very quickly decide that in an average sense only a subset of those are important, not all.

When you go to the commutorial kind of question when you need to combine 15, you can maybe settle for five instead of 15 because the others have negligible impact or effect. That was another set of sizes for problem reduction.

Still you have a number of formulas to vary and then we didn't want to run, like if you take a few of these and a few values per variable, sometimes you get 5,000 combination. We didn't want to run 5,000 cases. We made an assumption that maybe the next effect of these variables, the combined effect of them, would be an linear additive of the individual impact.

Remember that we are focusing on this particular cell, this particular yellow cell. We said it would be ideal if we could add the net effect of the individual temperature changes together and get

the effect of the combinations.

We tested that so what you see as T equals sum of delta T so let's add the delta Ts coming from different sources of uncertainty. The probability of such temperature situation would be the product of the probability of different variables on your uncertainty rate.

As a way of testing this assumption in this limited scope of applicability with no claim of generality that in this limited scope of applicability for the cells we looked at, the cell that was the focus of our uncertainty analysis, this graph shows that for five cases when we actually ran RELAP, and the cases are listed 1 through 5, for various values of the parameters the RELAP calculated average downcomer temperature listed in the last column was not very different and, in fact, was very close to the average temperature from the linear additive assumption.

At least in this case, based on this sample randomly selected set of runs, we felt a little bit more comfortable about the idea that maybe the temperatures can be added.

I must add very quickly something to this, that we did not use the result of the sum of this

temperature as a substitute for thermal hydraulic run. Nor did we pass such information to the PFM. Actually, these were used as indicators, as ways of identifying a few representative curves.

We said, look, if I can identify from this population of many, many possible combinations plotted in terms of average temperature and the frequency we observe on the left-hand side and the cumulative distribution version is on the right-hand side.

If I read the temperature, the total temperature using my additive model, pick three or four representative temperatures from this distribution and the corresponding probability that comes from all possible combinations of these variables, that those indeed are reasonable representation of the spectrum of these massive nonexistent number of runs that we would have otherwise needed to represent the variability of these various parameters.

We used this linear average temperature as a guideline in identifying representative thermal hydraulic average temperatures and representative thermal hydraulic scenarios leading to, if you see, that equation on the very last line, Tdc (i) and p(i) which means this temperature and the corresponding

probability is the fraction of probability contained in this particular bin, this pair of information.

Once we run the corresponding thermal hydraulic runs using RELAP for the combination that represents that particular total average temperature, then we have not only the traces coming from RELAP run, but we also have the corresponding probabilities P1, P2, P3 that can be used to modify the frequencies coming from the PRA.

We have the frequency phi equal to theta coming from the sum of the frequencies from PRA multiplied by the probability P1, P2, or P3 or could go all the way to P5, I think, five different cases coming from selecting representative runs from this type of process.

This process helped us identify the average behavior of the representative runs and then we went and identified the representative runs from the actual thermal hydraulic runs, the ones that came closest to having a similar behavior.

At the end, this is the same matrix, two pages, with the yellow highlighting the cases for which we did uncertainty. It actually list all the cases that we identified and put in that particular cell. That represents the uncertainties addressed

through the process I just outlined.

At the end, what was sent to the PFM analysis were in this particular format of single sets of downcomer temperature, pressure, and heat transfer coefficient and corresponding frequency.

MEMBER SHACK: Okay. I'm up to my eyeballs here trying to follow this. Let me just come back one step at a time.

Now, if I go back to the matrix -- no, wait. I just one to go back one step. Your list of results generated as input to PFM analysis where you've broken the runs down now.

MR. MOSLEH: Oh, the very last view graph.

MEMBER SHACK: The very last view graph.

MR. MOSLEH: Okay.

MEMBER SHACK: Now, the frequencies I see here are the PRA frequencies times your P1, P2, P3 frequencies?

MR. MOSLEH: Yes.

MEMBER SHACK: And the case that I see is the actual case where you've given them the temperature and the number?

MR. MOSLEH: Right.

MEMBER SHACK: So they get the temperature and the pressure for that and you've taken the

uncertainty over into the frequency space.

MR. MOSLEH: Yes.

MEMBER SHACK: Okay. I've got that part. Can I go back one more?

MR. MOSLEH: The reason for this particular format of result was the input requirement. There are many ways of presenting uncertainty but this particular one was dictated by the interface with FAVOR code.

MEMBER SHACK: Now, I take a particular scenario here and I do -- I get the linear sum by looking at the parameters that effect that scenario and I do my linear summation to get my cumulative damage or my cumulative distribution function. Then I pick three cases out of that by weighting it.

MR. MOSLEH: Right. And these are just guidelines for what kind of average temperature behavior you would expect and you find the thermal hydraulic run that comes closest from the set that you have at your disposal.

MEMBER SHACK: Can I go back one more? When do I start doing this? I have a scenario. I have some PRA scenario that I'm following here.

MR. MOSLEH: Class. And then you have typically at least one thermal hydraulic

representative. I don't want to use the best estimate but the one that comes closest as representing the entire class as a single PH run. You have that.

MR. KOLACZKOWSKI: This is Alan Kolaczkowski. Go back to slide 18 way early in the presentation package. I don't know if this will answer Bill's question or not.

MR. MOSLEH: This is 18.

MR. KOLACZKOWSKI: Okay. Thank you. I put this up to show the PRA process started with a bunch of information and then we started building the event tree models and we did some preliminary quantification. We went back to Oconee as well as internally did a review. We modified that stuff. At that point we revised the model. We requantified it. Now we start refining the binning and so on.

It's really in a way at the start of that step. It's late in the process that now that we are beginning to understand what's important; that is, that 94 percent of the risk is in that one bin and before we actually get the final quantification, if you look at the bottom yellow box, event sequence refinement arising from the uncertainty considerations. That is where a lot of this uncertainty process that Ali is explaining is

happening.

It's a refinement on the course binning that has been done up to that point in the process and now we are refining the binning based on the things that really matter from an uncertainty perspective and, if necessary, redefining bins, creating new bins, collapsing old bins, whatever it might be. That's when that refinement step is taking place. I don't know if that answers your question.

MEMBER SHACK: Yes, but when I finally get to a refined bin, does a thermal hydraulic bin define sort of a median set of boundary conditions that I used for that?

MR. MOSLEH: Yes, central or median.

MEMBER SHACK: It predicts some sort of central, median, whatever we want to call it.

MR. MOSLEH: Yes.

MEMBER SHACK: A set of thermal hydraulic conditions that I'm going to use for that bin.

MR. MOSLEH: Yes. Yes. There is a thermal hydraulic signature for that bin.

MEMBER SHACK: There is a thermal hydraulic signature for that bin. Now, how do I define the parameter range? The epistemic uncertainties in the model I think I can understand.

Those are kind of constant. It's the aleatory uncertainties in the boundary conditions for that central condition. How do I define those?

MR. MOSLEH: Well, for each of the variables that we identified as being a source of uncertainty whether epistemic or aleatory. Within the main characteristics of that central median thermal hydraulic signature we start varying those uncertainty parameters the ranges of which are defined by either the nature of the uncertainty.

For instance, if you are dealing with outside temperature we had four possible cases; winter, summer, spring, and fall. Some other ones the exact range and reason for the range and the values and distributions assigned were subject of analysis discussion, an expert assessment.

But at the end you end up with the ranges for each of those uncertainty sources. Those then are used to vary the relevant characteristics of that central T-H case. By the way, I must say also that sometimes a T-H bin was represented by more than one T-H run.

MEMBER SHACK: But the delta Ti then is the one-at-a-time variation.

MR. MOSLEH: Absolutely.

MEMBER SHACK: Give me that range.

MR. MOSLEH: One. That's the effect.

MEMBER SHACK: Okay. And then I get that effect. I get the delta Ti from that. I guess I can see how that works.

MR. MOSLEH: Then you say what if I change all of them together and this is a nonlinear --

MEMBER SHACK: Linearize the problem.

MR. MOSLEH: Linearize them and you test it in that limited context and know that it worked.

MEMBER SHACK: Okay. Then I convert that into a cumulative distribution.

MR. MOSLEH: Right. Of all possible combinations you put in the distribution how many cases do you see in average temperature of this magnitude. What fraction of time you see an average temperature of some other magnitude.

That is the nature of your distribution. You say from that distribution I'm going to pick three or four or five values average temperature. I look at my table of thermal hydraulic runs and find something closest.

MEMBER SHACK: Okay. Or, if necessary, add a new T-H run.

MR. MOSLEH: Right. If necessary. In

several cases we ran new cases.

CHAIRMAN FORD: I have an even more fundamental question. I can understand roughly all these questions about uncertainty in the model and the boundary conditions. Are we satisfied that the RELAP model, in fact, is accurate?

For instance, I noticed that in one of the documents we're talking about modifying the Oregon State experimental facility for PTS conditions. Have those experiments been done? Has the RELAP5 code been verified against those data?

MR. MOSLEH: Dave is the right person to ask.

MR. BESSETTE: Well, yes. Every time you pick up a code like RELAP and use it, you are faced with that question. Is it adequate for the purpose I'm using it. You have to go back and look at the code assessment, code validation.

Early on we decided that associated with the current PTS effort that we would run an experimental program at Oregon State University APEX facility dedicated to PTS experiments. We ran a series of 20 experiments separate fractured integral and one of the purposes was to look at phenomena, look at mixing and things like that.

The other purpose was to provide data on important PTS transients to assess relap. We haven't run RELAP against all 20 experiments but we have run them against about five experiments so we've done that assessment.

At one point we had some view graphs in here dedicated to that but we took it out because we probably wouldn't have time to show it.

CHAIRMAN FORD: It would have been really helpful actually to have shown it because it would have given a baseline confidence that you are starting from something solid and then you can start to build up on, well, what is my uncertainty in conditions, etc., etc.

Since we don't have it on paper in front of us, are there good correlations between observation and theory?

MR. BESSETTE: Yes, particularly with respect to these fairly global parameters like pressure and temperature. We got very good agreement.

CHAIRMAN FORD: When you say very good agreement, when you fed in basic uncertainties rather than all this stuff. I don't mean that derogatory. I mean, this PRA stuff, would it have changed the results when it comes down to you would have a thru-

wall crack, the frequency of that data? Do you understand what I'm saying?

The basic uncertainty or inaccuracy, if you like, of code itself as based on experiment at Oregon State, how much would that have affected the end result of the frequency of thru-wall crack as a function of temperature?

MR. HACKETT: I would guess -- let me venture something out just to sort of frame the thing. I would guess the short answer is we probably don't know without having gone through that in an interactive sense through completion of even FAVOR runs with the final impact on thru-wall cracking frequency.

Dave might be able to say how much -- what sort of confidence band we had with the data from APEX being predicted by RELAP. Was that within five percent or one percent on balance? As Peter said, not having the curves in front of us here, were they that close? Was it that good or were there some difficulties?

MR. BESSETTE: Specifically in terms that are RELAP comparisons with Oregon State data, the results -- we are still looking at -- whenever you compare the code with the data, there may be

disagreements and we are still sorting out -- I can't give an exact number because we are still sorting out the information.

MR. HACKETT: I think maybe what we could do we could take an action to get that information.

MEMBER BONACA: But it seems to me that, for example, in the selection that you made going from view graph 46 to 48, when you are looking at code model uncertainties you were choosing those parameters that would affect the code respondency between that behavior that is predicted RELAP5 with the sequence that you have in the PRA.

The code is respondency between actual performance. For example, whether or not RPV valves -- what is the state of those. I have the same concern about characterizing the RELAP5 predicting but I think the bigger issue here is you can always postulate that you have a transient that will give you that kind of thermal hydraulic behavior.

The question is does it correspond really to a certain frequency for certain scenario of the PRA that has characterization of frequency, the proper representation corresponding. It seems to me that we are focusing on those elements that would affect correspondence between the two of them.

MR. BESSETTE: In this uncertainty assessment what we tried to do was to identify the dominant boundary conditions and dominant modeling aspects of RELAP in terms of the uncertainty and varying all those parameters.

MEMBER BONACA: I guess I'm trying to understand myself. It seems to me that when you take a certain thermal hydraulic sequence that you have probably a number of ways to get there. One of those is some bias in RELAP5 that will give you that.

The response shouldn't be that one but that kind of temperature pressure profile. The biggest issue is that the correct pressure temperature profile for that specific sequence in the PRA. That's really what you're looking for, right?

MR. MOSLEH: There are a number of --

MEMBER BONACA: Is that really what limits your epistemic uncertainty?

MR. MOSLEH: Yes, that's a contributor to the correlation between the scenario characteristics and the thermal hydraulic results coming from RELAP and, for that matter, with reality. It's an epistemic source of uncertainty. We think that the judgement of this exercise is that we capture the dominant sources of uncertainty.

Even within RELAP in terms of its prediction capability, wherever we find the source that would result in a different temperature or pressure trace, that we try to address those.

We think that those are of secondary importance in the sense that the scenario to scenario variability that will give us a bigger spectrum of thermal hydraulic response and reduction of those to a few cells for the PFM analysis was a bigger source of uncertainty than some other concerns about the accuracy of a RELAP code. Obviously we did not close the book on issues regarding RELAP and it's correlation with the --

MR. BESSETTE: The APEX results. Thus far we have never attempted to define the uncertainty of RELAP with respect to comparison between the code and individual integral system experiments. We have avoided that kind of thing in the past in favor of doing uncertainty quantification based on the dominant phenomenonlogical uncertainties in the calculation.

One of the reasons we did that is that the facilities are scaled so just because we define a comparison, you can define a comparison between RELAP and individual experiment but that, we don't believe, gives us a real uncertain quantification for the code.

CHAIRMAN FORD: But surely if you -- I mean, when a licensee comes along with a predictive code of some sort we ask them to verify it and the same should apply here. If you are going to use a code like RELAP5, you should be fairly comfortable.

If you have a known set of inputs into the code, you will get a verifiable output from the code like temperature of the material. Then you can take that known code and apply all these uncertainties of the inputs but you've got to go through that first step. I have been assuming since it's a fairly old code and it has been verified sometime before this --

MR. MOSLEH: I referred to a number of studies here and in other countries. I mentioned Germany and Italy in comparing the results both at the input level and the output level of the code. What parameters we are assuming ranges inside the code and how do they correspond to measurements and actual experiment results and what comes out of the code to compare to the measures.

There have been a number of studies. The work in Italy you are probably familiar with Professor Darios' work, Ed Hoffer at GRS. There is work but it is not really conclusive in the sense that this code has been verified against a carefully selected set of

experiments run to verify and confirm all its results and all ranges.

MR. BESSETTE: It doesn't help right now but we did go over some of that information. We had a thermal hydraulic subcommittee meeting back in July of last year at Oregon when we went through some of those results. Unfortunately it doesn't help you today.

MR. HACKETT: I think --

MEMBER BONACA: I think also the nodalization. I mean, I didn't see anything in the selection of epistemic parameters here that would indicate that you were looking at that.

MR. MOSLEH: We did not vary on nodalization. We did not vary on errors a code operator could make. The number was listed as not addressed. We think that in terms of number of nodes 250 is not bad. Is node No. 250 or 217 or 156 is the right node and the right exact size and location and all that and there are 50 different ways of doing that, that type of uncertainty we have addressed.

MEMBER BONACA: You see, yes, but I'm just struggling with that. Maybe you're right but assume, for example, that you had a model that consistently under-predicts your cool-down. Now you are having

this frequency coming out for the sequences and you have now this look that goes to the probabalistic fracture mechanics that will tend to give you optimistic results. I'm just making a hypothetical. Assume that you have a bias in the code that will give you consistent with that.

MR. HACKETT: I'll interject at the risk that I'm not an expert in this area. Obviously I think what Dr. Ford is bringing up and what the committee is discussing is very reasonable. Obviously we are also in an enviable position here with RELAP that there are cases you can benchmark, too.

I think what I'm hearing is that is not the ultimate test of the code but that, at least, needs to be documented and presented to the committee again at some point. We'll take an action to come back.

MEMBER BONACA: Most of all, I mean, I'm not at all saying -- you know, I think actually RELAP5 has a lot of credibility as a computer program but there is so much that goes into the actual modeling with the program, so on and so forth.

When you go from view graph 46 to 48, I was left at the beginning with the question of why only these epistemic parameters. I'm sure you have

plenty of reasons why you haven't included others but it will be valuable for us to know where they are.

MR. MOSLEH: The specific ones list that and the exceptions are based on analysis we have done. Obviously there are cases where we say we did not address this particular source of uncertainty assumed to be good or we have reasons to believe they are good such as nodalization.

MR. HACKETT: I was going to say at this point, too, based on discussion with the chairman previously we were going to try to fit in what was originally item 3 on the agenda which was to go over the Oconee results in terms of introducing tomorrow's example problem. If that's okay with the committee, we'll go ahead and do that at this point and try and finish that as expeditiously as we can.

CHAIRMAN FORD: Thank you.

MR. HACKETT: And that will be largely Mark and Alan, I think.

MR. MOSLEH: Okay.

MR. KIRK: Okay. Let me find where we are Okay. Go to view graph 101 and 102 in your slide pack. Actually, view graph 102 you have seen before. People pick on me for taking too long to make slides so to reduce their unit cost, I need to use them

frequently.

The Oconee I results that we would like to talk about again. This is sort of a quick view and a high-level summary which is not to say that we have a detailed summary that we haven't showed you because we're in the process of putting that together right now.

We did after multiple years of talking about process and procedure and aleatory and epistemic we thought that everyone would find it refreshing to actually see something that sort of looks like results. We'll talk a little bit about some vessel specific inputs that we have.

Again, as before, highlight where there might be some significant differences relative to the earlier 1980s analysis, the generic inputs of flaw distribution and toughness distribution and, indeed, fluence we have addressed earlier. Then we would like to share with you some outputs and interpretations that we have developed to date.

As you previously have been warned, but I'm obligated under contract to tell you again, all of these are preliminary. In terms of the PRA and thermal hydraulic inputs to this analysis, we have sort of gone through this before.

Starting from the 1E-4, 1E-5 number of scenarios, that got down to approximately 150 total transients that were analyzed by RELAP of which 50 were screened, eliminated by inspection based on the criteria that has been discussed earlier.

Fifty have been assigned to what we call our base case which is in the process of being revised, and 50 were used as thermal hydraulic sensitivity cases to address some of the uncertainty concerns that Professor Mosleh was just discussing. It's my understanding that those results are still being processed.

The initiating event frequencies that we're looking at, and this is just, you know, a very broad brush range from E-9 to E-4. The only reason to point that out is to say -- and, of course, this reflects all of the current data.

The reason to point that out is simply to say we are not getting low-vessel failure probabilities simply because we have low-balled the initiating event frequencies. That's not the case. We have tried to take as realistic a view as possible.

Again, reflect the most recently available data regarding operator training procedures, actuarial data and so on. Just as a point to make relative to

where we've been before, some initiating event frequencies are considerably lower than their 1980s brethren. For example, the main steam line break dropped from around an E-4 to E-6 event.

MEMBER BONACA: Just a question I had on that. The range of frequencies there includes also those -- it's just the initiating event, right?

MR. KOLACZKOWSKI: Yeah. There's a little bit of a perhaps confusion in terms. It's an initiation event from the PFM perspective. It is the whole scenario frequency. It's the bin frequency. I apologize for that.

MEMBER BONACA: Also for the main steam line break?

MR. KOLACZKOWSKI: In terms of the main steam line break same thing. We are really talking about a main steam line break that becomes a very severe cooling event. I'll get to this point in just a minute but in the early work where they essentially took no credit for operator action, you are almost left with just the frequency of the break.

But if you allow for operator credit and, so sad, it becomes a severe event, you drop that scenario's frequency by like two orders of magnitude by giving credit to the operator. I guess that really

kind of gets to the next slide.

Just a couple of things I want to mention quickly from the PRA perspective in the Oconee analysis. What you see on this slide is a summary and then some comments on that, a summary of what were the two dominant PTS risk groups of scenarios, if you will, from the early 1980 work.

The one on the left was, in fact, the most dominant and it was called the residual group. It was, as we call it here, the everything else group. It was made up primarily of a collection of relatively small frequency sequences, sort of the residuals from a number of the hundreds of thousands of sequences that they analyze.

If it was fairly small frequency, they kept throwing it into this "residual group." It got to the point where that residual group's frequency got to be pretty large. On top of that they ten applied the worse case conditional probability of vessel failure to all the sequences in that group, the worse CPF that they had calculated for the 10 or 12 or whatever T-H runs that they had made, which was a conditional probability of failure of 5.4 times 10 to the -3.

They applied that to all the sequences

that were in this residual group so a very conservative treatment. This accounted for roughly half of all the PTS risk in the early '80s work. Also note that little to no -- there were a few exceptions -- little to no human actions were credited in the analysis for the residual group at all.

Comparing that to the current study, again, we already pointed this out, we're using latest frequencies and equipment failure probabilities, etc., based on experience up through about 1999 in the industry and Oconee specific where we could.

I pointed out already that the number of transients we're having today per year is a lot less than number of transients we were having in the '70s. Clearly that has had an impact here.

We've already talked many, many times about the fact that we didn't have a catch-all group. We have rather than binning everything into 10 bins, we have binned them into 140 bins or whatever, some of which were screened.

More refined binning and, therefore, didn't have to do this very gross binning and treat small main steam line breaks in with the large main steam line breaks, for instance. Just the process itself obviously is a lot better and we didn't have a

catch-all group per se.

Human actions accredited realistically. Then for all the bins that we have sent to the PFM folks, of course, they calculated CPIs and CPFs for each of those bins rather than throwing everything into a residual bin and then applying the worse case CPF to that entire bin. Very different process.

Because of the changes in process, because of using the latest frequencies, probabilities, giving human credit, etc., it really made that residual group essentially go away and get replaced by more accurate -- because of more refined binning more accurate assessment of what the PTS risk really is.

A word about steam generator tube ruptures because that was also among some of the dominant scenarios in the early work. The more likely steam generator tube rupture types of sequences did have low CPFs in the original work and they still do on our work. But, again, many of the less likely; that is, the lower frequency types of steam generator tube rupture events got dumped into this residual group and then treated with the worse case CPF.

Rather than really treating it as a steam generator tube rupture, it was treated more like a main steam line break so clearly it overestimated the

risk contribution from those lower frequency steam generator tube rupture events. Whereas, again, we are treating those as a bin unto themselves. Clearly our work agrees that you have to remember if you really treat it as a true steam generator tube rupture, the break is fairly small.

One or two tubes is the assumption here. The amount of cooling you actually get in the scenario is pretty low and it drops the amount from significance. Now, a few words about the main steam line break the remainder of this slide and the next slide. Then I'll pass it off to somebody else.

The main steam line break represented nearly all of the remaining PTS risk in the early work beyond the residual group. Again, looking at comparisons between the old or older work and this work, of course, in this study I've pointed out that we have allowed human credit for rapid isolation of the feed to the faulted steam generator and also for throttling high-pressure injection once the throttling criteria are met.

Why did we do that? Well, again, really looking at the way today's training and procedures are and crediting appropriately certainly suggest that is the appropriate way that we ought to be treating this

kind of scenario. Whereas in the original Oconee work there was almost no human credit given to perform those actions.

Main steam line break happened. It caused a severe cooling event. There was no credit for the operator. Not surprising it was a dominant PTS risk contributor.

You did see on the third sub-bullet there under the human credit just some typical human error values for failure to isolate the faulted steam generator by 10 minutes time into the event. Also the failure to throttle the injection 10 minutes after meeting the throttling criteria for injection, you did see some typical human error values there.

Again, that is why the main steam line break suddenly goes from 10 to the -4 event to a 10 to the -6. You're taking a 10 to the -2 credit for the operator to stop the feed of the faulted steam generator which effectively stops the cooling of the event.

So, therefore, the sequence frequencies are relatively low for a severe cool-down event due to a main steam line break. Again, what the actions effectively do is isolating feed limits the cool-down rate. Throttling the HPI limits any repressurization

issue. The more likely scenarios which involve successful actions by the operator become essentially relatively benign cool-down events so the main steam line break kind of goes away.

And I guess that's reinforced just a little bit. I'm sorry. I went the wrong way. Reinforce just a little bit in the next slide why crediting operator actions during main steam line break. Again, this is just meant to be an example of all the actions that we credited where we thought it was appropriate.

Overcooling prevention and control are an integral part of the Oconee crew training. Oconee operators are sensitive to overcooling scenarios, especially because of the once-through steam generator design. They are pretty sensitive to knowing that if they don't control feed properly, that they can get a serious overcooling event.

They are very sensitive to the fact that use of the once-through steam generators, etc., they need to get on things like a main steam line break scenario fairly quickly, isolate that faulted steam generator and stop the cooling.

Instrumentation is available and procedures are written to facilitate both the

identification of an excessive steam demand. The way the procedures are written and the way they jump into certain EOPs there is a bias towards, for instance, when you do see a main steam line break or the indications thereof, the procedures are written such that they can immediately go to a step in the procedure that says isolate that faulted steam generator right now. We don't wait until we get to step 42. We can do step 42 right now. That's the way the procedures are written.

Additionally, there are warnings to throttle the HPI which appear in numerous points throughout the procedures and, in fact, is a so-called continuous action step. They look for those throttling criteria. Once they are reached they can begin the throttle regardless of where they actually are in the EOPs as they are responding to the event.

Then finally I pointed out the importance of going to Oconee and observing on the simulator some actual overcooling events. We felt much more comfortable with our human error estimates because they were based in part on observations that we saw, the way the crews actually respond to a main steam line break event and how quickly, or not quickly, they can get to certain steps in the process to isolate the

steam generator and so on and so forth.

There's a nutshell of at least some of the examples on why, for instance, in this case the two most dominate PTS scenarios in the early work have now kind of gone away based on differences between the old work and the present work.

MR. KIRK: Okay. Back to some materials changes. This gets to the way we model the vessel. What you see here is an unwrapped view of the Oconee vessel with various plates and welds indicated. The point I would like you to take away here is in the earlier analysis the whole vessel would have been assumed to have been made out of the most embrittled material, in this case being the circ weld that's indicated in green.

That is very different than what we do today. What we do today is we have in documented information that the licensees have supplied, the material properties, the copper, the nickel, the RT_{NDT}, the Charpy shift, and so on, for each of the different plates and welds and we use those in the analysis.

I haven't indicated the least embrittle material here but previously the whole vessel would have assumed to have been RT_{NDT} at the end of Oconee's original licensee which was 40 years of 183

fahrenheit. That would have been this whole picture, whereas now it's only this little slice through here.

In other regions the plates may be up to 100 degrees lower so we use the appropriate values. Obviously a much more realistic model and one that removes quite a bit of conservatism.

This slide in your packet has been modified -- this slide that is on the screen has been modified from the one that is in the packet to hopefully prevent you from asking me a question I have now been asked two times and it was misleading on the slide.

This is an attempt to summarize both the PRA input to the PFM analysis, that being the initiating event frequencies on the horizontal axis. Just to remind you, call attention to the word "mean" because each of these initiating events, of course, has a histogram associated with it.

And then on the vertical axis you see the output of the PFM code and that should, again, say -- I think that is actually the 95th percentile conditional thru-wall crack probability.

Be that as it may, a couple of things to take away. One is that if you go along the horizontal axis I broke the logarithmic scale so the horizontal

axis is zero. Not a very small number but zero.

There have been quite a few, in fact, about half of the base case sequences that we analyzed in PFM had no probability of failure because the applied K never got up into the K_{Ic} distribution so by definition no risk. About half of the sequences had some risk associated with them.

Of course, it's the product of the X and the Y variable that gives you your risk metric, at least in conceptual terms. What you want to look for are vents up here in the upper right-hand corner and those will be our dominant events.

What you see are a whole bunch of LOCAs populating that area, some stuck-open valves on the primary side which is kind of like a LOCA except that the operator might be able to do something about it, whereas a LOCA they can't.

Also call your attention to two points that were just made previously. Steam generator tube ruptures, the red circle is down here. No contribution in this case. Main steam line break which, as was just discussed, used to be a dominant contributor way down here and so what you'll find out is on the next graph main steam line break doesn't even show up.

What this is is again another very high-level condensation of the results of the preliminary Oconee analysis. The bar graph on the left-hand side of your screen shows the contribution of several different classes of events to the probability of crack initiation. What you see is all those LOCAs act together and they make up about 70 percent of the cracks that have initiated in the vessel.

However, in general, of course, and with increasing breaking size the LOCA pressure drops quite dramatically. It doesn't form -- it is still a dominate contribution to the probability of failure but doesn't make up nearly as much as the initiation part.

The single dominant sequence is shown here in the red checkerboard. That is a stuck-open pressurizer safety valve for which the operator does nothing and it finally recloses automatically at the RTS setpoint. For a single transient it was big but overall a fairly small contribution to the crack initiation frequency. Since the pressure stayed so high while the vessel got cold, it contributed roughly 30 percent and was a single dominant transient to the failure frequency.

Comparison of the red and the green shows

you the benefit, at least in this one instance, of operator action in that the green represents two variants on the red scenario where the operator took control of throttling the HPI. One scenario was at one minute. One scenario was at 10 minutes.

Obviously those two scenarios combined contributed more to the crack initiation frequency but, happily, it severely mitigated -- severely being a good word in this case -- it significantly mitigated the number of those cracks that went through the vessel wall.

Here we have two scenarios that contributed a little under 20 percent to the initiation frequency, but that only increased to roughly 30 percent of the failure frequency, whereas that same scenario without operator intervention, perhaps only five percent of the initiation frequency during into almost 30 percent of the failure frequency.

MR. KOLACZKOWSKI: Let me point out that tomorrow when we go through the example it is focused on this SRV reclosure and operator actions, etc., etc.

MR. KIRK: Observations which, I think, we have already made. Dominant scenarios are all primary system LOCAs. By realistic accounting of operator

action we have been able to mitigate significantly the influence of secondary system events on the total probability of failure.

In fact, it's fair to say that secondary system events haven't played a role at all in Oconee, at least at this stage. Finally, the time of SRV closure and, thus, re-pressurization has a significant influence on event severity. That is a pretty obvious observation.

Ed showed you this at the beginning and promised it was where we would get to at the end and it hasn't changed all day. Really great how that works. We are currently in the process of scrubbing this and hopefully removing the pink preliminary. We know now that there are things that we certainly do need to change.

We found some things that will drive these results in an upward direction and some in a downward direction. I was keeping count on the back of a matchbook and I finally gave up. Of course we're going to rerun it again but I've given up even trying to predict. Certainly the news is positive at this point and I don't think anything that we found is going to change that overall positive view.

I think -- yes. Just to summarize, they

look promising. This all leads us to the perception that the risk of vessel failure is lower than we previously believed it to be, previously believing circa 1980s. Obviously we still have a considerable amount of work to do.

Specifically we need to establish a new risk goal so we know where to draw that horizontal line. We need to assess the contribution of external events to overall risk. Of course, we need to complete and finalize the analysis for Oconee and complete the analysis of the other three plants to make this a done deal.

That's the end of the slide set on Oconee.

MEMBER BONACA: A question I had was tomorrow when we have the example, it would be interesting to have a sense for this curve that you showed us, you were pointing out that the main steam line break sequence has been essentially eliminated because you went from 10 to the -4 to the -6. If I understand, Oconee has no main steam selection box so it's all manual.

MR. KOLACZKOWSKI: That is correct.

MEMBER BONACA: Okay.

MR. KOLACZKOWSKI: I mean, they have turbine stop valves, etc., if the break was really

down right by the turbine but it is true if the break was in essentially a large portion of the steam line itself, there are no MSIVs that would automatically close off the event. It also relies on operator action.

MEMBER BONACA: It would be interesting to understand the sensitivity of these results on that kind of range of parameters, how they propagate through.

MR. KOLACZKOWSKI: Again, we will do our best to address that. I mean, the example is really aimed at the LOCA scenarios because that's where the dominant results are. I think we can certainly digress what we need to and talk a little bit about, therefore, why is the main steam line break still going away. We can certainly attempt to do that.

MEMBER BONACA: Because, I mean, that's an important element.

MR. KOLACZKOWSKI: Absolutely.

MEMBER BONACA: If it doesn't go away for some reason, then, you know, that pointer comes up somewhat and I would like to understand. Before, I mean, I get all excited because this is exciting.

MR. KOLACZKOWSKI: We feel confident that the main steam line break is going to go away.

MEMBER BONACA: But there is an issue of this kind of uncertainty.

CHAIRMAN FORD: Ed, you said, and I don't think you said it flippantly, in the beginning this turns out to be a correct scenario for a majority of the stations nowadays, "PTS situation goes away." Could you expand on that?

MR. HACKETT: Yes. In fact, one of the things I was going to mention, a conversation with Ron Gambol during the lunch break.

Mark, if you could put up that last slide.

We had a conference call with the industry before we did this. This is about a week ago now. Just focusing up there on the one that says, and we carefully worded this, "Leads to perception that the risk of vessel failure is lower."

Ron was reminding us on that call, and reminded me again today, to be careful about overselling this at this point. Also that this risk perception is a relative thing. Oconee's risk was low to start with. If there are Oconee people present or on the line, their risk was lower to start with. We think now it's lower still. But whether it's two orders of magnitude or four, or maybe by the time we get done with all of this it's different than that to

some extent, we don't know right now.

You asked a difficult question, though. Maybe to kind of repose your question does this rule go away if we can show three orders of magnitude improvement and is there any need for a PTS rule if that sort of thing can be demonstrated.

We haven't decided that yet and that's a debate I know we've had. Mike Mayfield challenged us with that when we dry-ran this presentation. What if you are -- for instance, what if we haven't rung everything out of this and when we do, we are three orders of magnitude better than what we thought we were, four or five as the case may be, depending on what you are comparing to.

Then I think it is incumbent on us as regulators to look real hard at whether or not the resources are necessary to go through with the revised rulemaking. I think it's very premature at this point to say that. My personal view is I would be surprised if it goes away to that extent as opposed to being a modified criteria.

CHAIRMAN FORD: It will probably have to be a modified criteria surely because of Palisades and Beaver Valley.

MR. HACKETT: At a minimum it will be

modified.

CHAIRMAN FORD: Fort Calhoun.

MR. HACKETT: Having it go away, I think, would be probably optimistic.

MEMBER SHACK: What's the difference in raising it 50 degrees and making it go away? I mean, for all practical purposes?

MR. HACKETT: Good question there, too, because we had predicted in advance of this that, first off, we just about eliminated anyone from major concern.

MEMBER SHACK: Unless we came back and said for license renewal to 120 years.

MR. HACKETT: Right. That's where I --

MR. KOLACZKOWSKI: I heard Oconee was going to ask for 300.

MR. HACKETT: We still had as of the last go-around on this, and I think NRR -- I don't know if there are any NRR reps here late today but NRR had looked at their reactor vessel integrity database and projected forward to at least the 20-year renewal period.

There were still potentially for or five plants that could be impacted in that time frame depending on how this thing goes. Less of a concern

than it was before but it is yet to be determined exactly where we end up. I know there's a lot of hedging going on there but that's kind of where we are at the moment.

MEMBER SHACK: Those will be impacted if the rule didn't change if the screening criteria stayed right where it was but I don't think you have to shift it very much.

MR. HACKETT: You wouldn't have to shift it very much. In fact, I think on the order of 20 degrees or more would probably take care of the 20-year renewal period is my recollection.

CHAIRMAN FORD: You mentioned also during the break something about the EPRI verification and validation exercise.

MR. HACKETT: Yeah. I guess there are several items in that regard. We are pursuing to the extent possible, and I'm also not the right person, not the right QA person, but we have enlisted the support of folks internal to the NRC and also folks who have done this type of work for EPRI to do verification and validation in accordance with, I believe, we're using an IEEE standard.

MR. MALIK: We are going through the VNB process. On December 6th and 7th we had a public

meeting involving the industry contractors and we were doing the validation part of the process. In that we have taken several aspects of it, PI aspects of it and PFM aspects of it.

There are four or five different teams that are each work. They have come up with a set of problem statements. They will be using those to come up with a solution from independent methodology and compare with what the FAVOR code is giving it and comparing the two.

During the months of January, February, and March all this activity. In April we will meet again and there we will correlate all the results and see how they are coming up with the validation criteria. We will be monitoring the progress.

MR. HACKETT: It's pretty obvious we're at some risk here because in an ideal sense, we would have had this completed before we started runs for Oconee or any of the other plants. The schedule pressure on the project has precluded that so we are running it in parallel with a lot of hope and crossed fingered that we're not going to show up any major show stoppers in the QA.

If we do, we're going to have to circle back on ourselves to see what that means. So far so

good but it remains as sort of a stay-tuned situation to see how that comes out.

I guess that concludes the staff and contractor presentation for today. We'll go through the detailed example tomorrow. Hopefully we have enough time to do that. It looks like we're not quite back on schedule but we're close.

CHAIRMAN FORD: Ed, thank you very much indeed. Are there any other questions for today? We're in recess until tomorrow at 8:30.

MR. HACKETT: Thank you.

(Whereupon, at 4:33 p.m. the meeting was adjourned.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: ACRS Materials and Metallurgy Subcommittee

Docket Number: (Not Applicable)

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Pippa Antonio

Pippa Antonio
Official Reporter
Neal R. Gross & Co., Inc.