

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_{Tc} 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_{Tc} .

However, in the previous discussion that we had before lunch, we already discussed the temperature dependence and the scatter in irradiated K_{Tc} we expect to find, and indeed do find, in our empirical data to be the same as for the un-irradiated K_{Tc} . 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.

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

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

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

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

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

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

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

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

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

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

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

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

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1 .99 versus 1.0 to the toughness shift for welds. For
2 plates the Charpy shift actually under-predicts
3 slightly the shift in toughness.

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

12 CHAIRMAN FORD: Before you go too far --

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

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

16 MR. KIRK: Yes.

17 CHAIRMAN FORD: I have not seen the raw
18 data.

19 MR. KIRK: Yes.

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

23 MR. KIRK: Yes.

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

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

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

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

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

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

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1 should be a square root.

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

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

13 CHAIRMAN FORD: Okay. So what you did was
14 you took a functional form from Bob Odette and you
15 fitted it. It wasn't, therefore, to .5 but came up
16 .46.

17 MR. KIRK: Yes.

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

20 MR. KIRK: Minimizing the residuals, yeah.

21 CHAIRMAN FORD: Okay. I understand.

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

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

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

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

14 MR. HACKETT: The data.

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

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

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

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1 combination of the two.

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

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

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

19 CHAIRMAN FORD: Thanks very much.

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

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

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

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

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

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

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

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

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

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

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

25 We plug in our copper nickel fosh, all the

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

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

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

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

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

23 CHAIRMAN FORD: That formula has got some
24 sort of theoretical basis?

25 MR. KIRK: Yeah. It's making fluence

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1 attenuate like DPA which is believed to be a better
2 physical metric or radiation damage. By the way, it's
3 conservative. This dates back to the Neil Randall
4 technical basis for Reg. Guide 199 Rev. 2 circa mid
5 1980s.

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

8 MR. KIRK: No.

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

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

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

15 MR. KIRK: Yeah.

16 CHAIRMAN FORD: Is there any reason for
17 saying --

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

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

25 CHAIRMAN FORD: Like Yankee Row.

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

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

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

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

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

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

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

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

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1 drop.

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

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

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

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

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

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

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

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

25 Time, again, it depends on who you ask as

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 out the high spots.

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

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

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

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

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1 essentially a similar shape.

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

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

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

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

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

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

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

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

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

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

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

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

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1 the curves should be getting closer together.

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

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

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

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

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

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

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1 index temperature for the irradiated material.

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

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

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

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

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

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

7 MR. KIRK: 82.

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

10 MR. KIRK: The 14.4 --

11 CHAIRMAN FORD: Down on the left-hand
12 side.

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

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

24 CHAIRMAN FORD: Okay.

25 MR. KIRK: Going through the flaws, and

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

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

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

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

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1 nondestructively examined it.

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

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

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

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

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

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

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

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

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

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

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

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

12 CHAIRMAN FORD: Shoreham is higher.

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

15 CHAIRMAN FORD: Oh, I see.

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

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

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

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

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

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

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

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

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

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

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

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1 conservative assumption.

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

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

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

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

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

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

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

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

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

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

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

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

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1 MEMBER SHACK: They only assume
2 fabrication flaws, too.

3 MR. HACKETT: Right.

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

6 MEMBER SHACK: Not contemplated.

7 CHAIRMAN FORD: Not contemplated.

8 MR. HACKETT: That's correct.

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

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

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

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1 data on.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

12 MR. KIRK: Yeah.

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

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

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

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

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1 because I'm having trouble with this one.

2 Fred or Steve, are you there?

3 MR. SIMONEN: Can you hear us?

4 MR. KIRK: Yes, we can, Fred.

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

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

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

21 MR. SIMONEN: Exactly.

22 MEMBER SHACK: Okay. Okay. Thank you.

23 MR. KIRK: Thanks, Fred.

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

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1 MEMBER SHACK: No.

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

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

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

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

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1 physical understanding.

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

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

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

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

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

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

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

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

20 MR. KIRK: Sure. Which one?

21 MEMBER SHACK: Just the size distribution.

22 MR. KIRK: This one?

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

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

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

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

16 MR. KIRK: Yes. That's right.

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

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

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

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

9 MR. KIRK: Yes.

10 MEMBER SHACK: So those are empirically
11 based?

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

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

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

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

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

25 MEMBER SHACK: Can I ask one more

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

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

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

16 MR. DICKSON: I agree with you.

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

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

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1 MR. HACKETT: Is that right, Fred?

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

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

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

16 CHAIRMAN FORD: Bruce.

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

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

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

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1 MEMBER SHACK: This is all epistemic
2 uncertainty.

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

9 Is that right, Fred?

10 MR. SIMONEN: That's correct.

11 CHAIRMAN FORD: Okay. Bill.

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

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

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

18 MR. HACKETT: Okay.

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

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

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

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1 are going to revise the revised schedule.

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

4 CHAIRMAN FORD: We'll talk about --

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 scenario here. We were faced with the task of about
2 10,000 or so, 10 to the fourth, a number of scenarios
3 each of which in principle would be assigned to a
4 specific thermal hydraulic run.

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

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

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

25 A few notes quickly on RELAP model

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

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

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

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

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

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

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

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

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

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

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1 acceptable at this point.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 temperature, pressure, and transfer coefficient.

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

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

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

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

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

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

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

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

25 Then the brown boxes cover the uncertainty

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

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

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

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

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

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

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

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

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

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

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

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

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1 the decay heat decreasing the temperature.

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

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

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

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

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

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

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

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

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

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

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

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

25 And the combinations of the secondary side

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

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

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

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

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

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

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

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

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

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1 FAVOR code was not ready for production use.

2 However, at the end we have this pleasant
3 feedback that the criteria we applied had actually
4 worked and none of the ones that we labeled as
5 insignificant, even though we passed a subset of those
6 two for tests, turned out to be important, about 100,
7 I think, scenarios. I don't know the number. Is it
8 50, Terry, of sensitivity cases?

9 MR. DICKSON: Fifty sensitivity plus 47.

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

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

18 MR. MOSLEH: Yes.

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

22 MR. MOSLEH: Right.

23 MEMBER BONACA: The same initiator.

24 MR. MOSLEH: That's true.

25 MEMBER BONACA: The other question I had

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

4 MR. MOSLEH: Yes, only.

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

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

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

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

17 You can note very quickly that the one
18 that I've highlighted yellow actually contains about
19 94 percent of the total frequency. Of all the
20 scenarios that were identified and analyzed being of
21 PTS significance and applying the screening criteria
22 of 10 to the -8 or less being screened out. When you
23 add all those scenarios, 95 percent of those fall in
24 that particular cell containing a number of dominant
25 scenarios one of which will be the subject of example

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1 tomorrow.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

25 What we did not treat in the second

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

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

11 Nevertheless, we did not treat that source explicitly.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 those.

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

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

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

23 The first step was to go through one-
24 factor-at-a-time kind of analysis sensitivity. About
25 200 such cases were run. Actually 200 plus because

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

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

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

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

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

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

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

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

16 And to get the effect of the simultaneous
17 impact of the factors we came up with another
18 computational trick that I will mention later. This
19 is an example of many, many sensitivity analyses that
20 were performed where an average temperature change,
21 10,000 second average on the downcomer temperature is
22 plotted against the different break sizes going from
23 2.8 inch to 8 inches for two different states of HPI,
24 the nominal and the failed state.

25 We can see easily that after about 6.6

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

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

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

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

20 MR. BESSETTE: I forget.

21 MEMBER SHACK: The gamma.

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

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

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

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

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

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

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

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

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

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1 the effect of the combinations.

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

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

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

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

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

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

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

20 We used this linear average temperature as
21 a guideline in identifying representative thermal
22 hydraulic average temperatures and representative
23 thermal hydraulic scenarios leading to, if you see,
24 that equation on the very last line, $T_{dc}(i)$ and $p(i)$
25 which means this temperature and the corresponding

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

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

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

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

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

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1 through the process I just outlined.

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

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

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

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

14 MEMBER SHACK: The very last view graph.

15 MR. MOSLEH: Okay.

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

19 MR. MOSLEH: Yes.

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

23 MR. MOSLEH: Right.

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

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1 uncertainty over into the frequency space.

2 MR. MOSLEH: Yes.

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

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

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

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

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

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

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

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

8 MR. MOSLEH: This is 18.

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

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

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1 happening.

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

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

14 MR. MOSLEH: Yes, central or median.

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

17 MR. MOSLEH: Yes.

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

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

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

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

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

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

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

23 MEMBER SHACK: But the delta T_i then is
24 the one-at-a-time variation.

25 MR. MOSLEH: Absolutely.

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1 MEMBER SHACK: Give me that range.

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

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

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

8 MEMBER SHACK: Linearize the problem.

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

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

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

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

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

25 MR. MOSLEH: Right. If necessary. In

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1 several cases we ran new cases.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

16 MR. MOSLEH: There are a number of --

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

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

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

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

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

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

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

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

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

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

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

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

9 MR. HACKETT: I think --

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

14 MR. MOSLEH: We did not vary on
15 nodalization. We did not vary on errors a code
16 operator could make. The number was listed as not
17 addressed. We think that in terms of number of nodes
18 250 is not bad. Is node No. 250 or 217 or 156 is the
19 right node and the right exact size and location and
20 all that and there are 50 different ways of doing
21 that, that type of uncertainty we have addressed.

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

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

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

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

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

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

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

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

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

17 CHAIRMAN FORD: Thank you.

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

20 MR. MOSLEH: Okay.

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

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1 frequently.

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

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

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

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

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

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

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

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

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

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

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

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

13 MEMBER BONACA: Also for the main steam
14 line break?

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

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

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1 kind of gets to the next slide.

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

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

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

25 They applied that to all the sequences

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

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

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

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

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

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1 catch-all group per se.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

24 Instrumentation is available and
25 procedures are written to facilitate both the

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

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

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

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1 steam generator and so on and so forth.

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

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

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

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

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

3 In other regions the plates may be up to
4 100 degrees lower so we use the appropriate values.
5 Obviously a much more realistic model and one that
6 removes quite a bit of conservatism.

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

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

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

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

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

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

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

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

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

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

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

15 The single dominant sequence is shown here
16 in the red checkerboard. That is a stuck-open
17 pressurizer safety valve for which the operator does
18 nothing and it finally recloses automatically at the
19 RTS setpoint. For a single transient it was big but
20 overall a fairly small contribution to the crack
21 initiation frequency. Since the pressure stayed so
22 high while the vessel got cold, it contributed roughly
23 30 percent and was a single dominant transient to the
24 failure frequency.

25 Comparison of the red and the green shows

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

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

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

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

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

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

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

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

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

25 I think -- yes. Just to summarize, they

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

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

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

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

22 MR. KOLACZKOWSKI: That is correct.

23 MEMBER BONACA: Okay.

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

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

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

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

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

19 MR. KOLACZKOWSKI: Absolutely.

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

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

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

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

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

11 Mark, if you could put up that last slide.

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

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

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1 some extent, we don't know right now.

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

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

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

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

25 MR. HACKETT: At a minimum it will be

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1 modified.

2 CHAIRMAN FORD: Fort Calhoun.

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

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

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

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

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

15 MR. KOLACZKOWSKI: I heard Ocone was
16 going to ask for 300.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

11 MR. HACKETT: Thank you.

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

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