MEMBER WALLIS: To do with heat?

MR. HICKLING: Yeah. It starts with the heat, yes. There are other factors involved.

MEMBER WALLIS: It's a production lot.

MR. HICKLING: Correct.

MEMBER WALLIS: It's not a property.

MR. HICKLING: No, not at all. It's just a material identifier. It's a number.

So we finally then get to where we want to go by taking the log normal fit, the ordered median ranking of the alpha values for these 26 heats using standard statistical methods.

I'm not myself a very good statistician. In fact, I'm a pretty awful one. Glenn White, who did the data correlation exercise on this, and with a lot of input from the gentleman on my right who has a very strong grasp of statistics, we tried all sorts of methods, and I think this came out as probably the most valid for looking at this database.

MEMBER WALLIS: So what you're saying here is that the properties of this stuff are very dependent on how it was made.

MR. HICKLING: Correct.

MEMBER WALLIS: And that isn't a variable that's under control or is measured in some quantitative way.

MR. HICKLING: Correct.

MEMBER WALLIS: So there's a tremendous amount of uncertainty about what's going to happen.

MR. HICKLING: Yes. And that's why ultimately there's a limit to how far we can go with a deterministic approach and why we have to get into a probablistic approach.

But this is the result of doing this exercise. What we are actually plotting here is the cumulative distribution of these alpha values for the 26 heats. So every single point here represents one heat.

Now, it may have one specimen. It may have up to the maximum of 32 specimens concealed in that calculated alpha value, and because it's a log normal distribution, of course, it never completely goes to zero or to one. So as you can see, that is this most susceptible heat which was identified, but our curve here is predicting that you could have higher susceptibility heats and you could, in fact, have very, very graphic cracking, which is ultimately going to be physically unreasonable.

There is a limit. It's very hard to define. There's no fully accepted mechanism of Alloy

600 cracking. Therefore, it's very hard for first principles to calculate a physically accepted maximum crack growth rate.

But we all know there has got to be one because otherwise you're getting electro-chemical --

MEMBER WALLIS: What is your access there?

MR. HICKLING: This is the cumulative distribution of the alpha values as a function of the actual values.

MEMBER WALLIS: What does that mean? You're just adding up the number of --

CO-CHAIRMAN SIEBER: It's the probability of this.

MR. HICKLING: Basically it's the probability function.

MEMBER WALLIS: But they all have different origins, and there are 27 tubes for one alpha value, only one for another alpha value. I don't know how you get a --

MEMBER APOSTOLAKIS: Are these points treated as being equivalent?

MR. HICKLING: Yes.

MEMBER WALLIS: But they're not.

MEMBER APOSTOLAKIS: Some of them come from a large number of test, some do not.

MR. HICKLING: Correct.

MEMBER APOSTOLAKIS: So shouldn't that be taken into account?

MR. HICKLING: Well, there's a limit to how you can do that. If you only have one point to test, if the heat --

MEMBER WALLIS: You're looking for a pretty curve, and this looks quite pretty.

MR. HICKLING: No, no, it's not quite that. You're looking to try and represent what you have. What you have is not what you'd like to have, but you're looking to try and represent it in the fairest way possible.

And given the importance of material heat, we would have been much worse off just taking all of the data and ignoring that effect.

Having said that, the full 158 data points for all of the heats feeds straight into the probablistic analysis that Dr. Riccardella will be talking about. He does not use this approach at all for that. He just takes the data as it comes out.

MEMBER SHACK: Which has its own set of problems.

MR. HICKLING: Which has its own set of problems, too.

MEMBER APOSTOLAKIS: But still, you know, some of these points --

MR. HICKLING: Some have much bigger uncertainty than others.

MEMBER APOSTOLAKIS: Yeah.

MR. HICKLING: Correct.

MEMBER APOSTOLAKIS: And why is --

MEMBER SHACK: You can do the analysis estimating the uncertainties in each of the alphas, and you find when you do that that the curve does not shift was much as you would expect.

MR. HICKLING: We have gone through that exercise.

MEMBER APOSTOLAKIS: Now, why do we need one curve?

MR. HICKLING: Because we are trying to propose a single crack growth rate versus K curve appropriate for dispositioning axial internal cracks in the field.

MEMBER APOSTOLAKIS: But why not a family of curves? I mean, I have uncertainty here, don't I?

MR. HICKLING: Well, you don't have enough data to generate a family of curves. Remember what we've done. We've --

MEMBER KRESS: Well, if you factor this

probability in, you in essence have a family of curves.

MR. HICKLING: Yes, you do in that sense, but you don't achieve very much because your uncertainty -- I'm going to come on, if I may. Perhaps we could postpone that question until I get to the applications slide as to how we intend to --

MEMBER APOSTOLAKIS: Why assume the data is constant and focus on the uncertainty in alpha? I mean, do we really know, Peter?

MR. HICKLING: No, we don't know beta at all. Beta is assumed from this other analysis. Beta has been adopted from an analysis from Scott.

MEMBER APOSTOLAKIS: But what alpha did scott use? He varied it?

MR. HICKLING: Yeah, the alpha value -- well, the definition of alpha depends how you mean. On a heat to heat basis, yes. Alpha varies.

MEMBER APOSTOLAKIS: Beta doesn't change from heat to heat?

MR. HICKLING: No.

MEMBER APOSTOLAKIS: There is evidence that that doesn't happen?

MR. HICKLING: I'm not quite sure what question you're asking me here.

MEMBER APOSTOLAKIS: Why do you assume that Beta is constant?

MR. HICKLING: Because you can approach what you're trying -- you've got to remember what you're trying to do. You're trying to define a crack growth rate which is going to vary with stress intensity, first of all.

MEMBER APOSTOLAKIS: Yeah.

MR. HICKLING: There is no reason necessarily that we have to expect that the material properties will affect the dependence on stress intensity per se. They'll affect the propensity to cracking very much, but the actual stress intensity dependence is no reason to assume that that should vary hugely between different materials.

And, in fact, if you do the exercise that Bill is talking about, the fitting to the individual heats and seeing how this curve moves, it doesn't move a whole lot with the probabilities.

In an ideal world, you might only have one heat of material, and then you wouldn't have this problem, but we're trying to tackle a very real problem here with a larger number of heats out in the field.

MEMBER WALLIS: Well, it's a very strange way of doing things. If I understand, you're looking at data from all of the different sources, and then you realize there's a tremendous number of different alphas to correlate those data, and then you are saying that we're not going to use some statistical thing to relate to this to CRDM.

I want to know which one of these data points is most like our CRDM rather than just taking a mean of a lot of things which might be something like it.

MR. HICKLING: Well, it's a good desire, but they all are. They're all from thick section Alloy 600 material. They may just --

MEMBER WALLIS: There must be some reason that they're different by such large factors.

MR. HICKLING: Yes, and the main reason is almost certainly the thermal processing history of the material.

MEMBER SHACK: But if you had a CRDM nozzle picked at random, you don't know whether it comes from the top of that curve --

MR. HICKLING: The middle or the bottom.

MEMBER SHACK: -- or from the middle or from the bottom, except on a probability basis, that it's more likely to come --

MEMBER WALLIS: It's like testing a lot of nails from nail suppliers and measuring something and then saying we're going to apply that to a bridge.

MR. HICKLING: But it's the standard situation you get into in stress corrosion cracking where you're forced to use what's available, what you can generate in terms of data, not what you would like to have, which is for every single heat out in the field archive material with good quality data on it.

MEMBER SHACK: If you knew exactly what caused the spread, like the grain size and the way they cooled it down, starting raw materials, you might be able to go in and characterize a nozzle, but you know, that's asking a lot.

MR. HICKLING: There's a parallel here which is perhaps worth following very, very briefly to a different problem in the BWR industry where stress corrosion cracking has also been studied for very many years, also intragranular, but where the mechanism of cracking has been tied down fairly well and has been linked to exactly the sort of factors you're talking about so that you can tell what difference potential makes, what difference material, what difference the chemistry makes, and so on.

Unfortunately, despite 30 years or more of study, there is still at least three, probably many more, credible mechanisms for primary water stress corrosion cracking of Alloy 600, and so we do not have that in depth understanding at a fundamental level to do that.

MEMBER KRESS: Yeah, nd I think the only recourse is to fall back on a probability.

MR. HICKLING: So where does this get us to? Let's come back to that Christine and just throw up what this actually does.

These are the 158 data points. As I remind you, each one is plotting growth rate in the test against the representative K value for the test, and again, you will notice the bunching between the 20 and 40 values of K, just the odd ones which are higher or lower.

This is the modified -- this Scott curve, called the modified curve, but that's --

MEMBER APOSTOLAKIS: This curve has nothing to do with the previous curve?

MR. HICKLING: Yeah.

MEMBER APOSTOLAKIS: Yeah, what?

MR. HICKLING: This curve is calculated.

MEMBER APOSTOLAKIS: Okay.

MEMBER WALLIS: But the naive observer

would say that the curve has nothing to do with the data whatsoever.

(Laughter.)

MR. HICKLING: Possibly true, possibly true.

CO-CHAIRMAN FORD: But the MRP curve, John, is the mean curve from the previous graph. It's using the alpha mean.

MR. HICKLING: What I'm going back to, it does, of course, have -- if we could just go back to the previous slide.

To get to that curve, we -- let's go back to the curve with the alphas, please. Thank you.

You're basically given the choice here. Once you've determined this dependency, how do you handle the uncertainty, and what value of alpha are you going to use to plot your single curve? Because you need to end up with a single curve in order to do anything sensible in the field.

The value that we've chosen is to use the 75th percentile from this curve for our value of alpha, and this is, in fact, the mean, if you like, of the upper half of the distribution. So it's not the median value here. It's considerably higher than that. There's a reason for this. It's basically that we are trying to make a best estimate of lightly cracked growth rate in the field, and there's obviously no point in going unrealistically low, but there's no point either in going absolutely unrealistically higher for every single heat of material that's out in the field.

The conservatism that you might want to apply, we feel should be added later in the process when you're evaluating and dispositioning an actual crack, and you have plenty of opportunity there to add engineering conservatism rather than adding it in a hidden form at this stage in the data.

And the ASME code gives some basis for this approach of taking the 75th percentile. So this is how we define the value of alpha here that we use when we create that next curve. Okay?

MEMBER APOSTOLAKIS: So this curve then is the Scott curve with alpha equal to this value, the 75th --

MR. HICKLING: No --

MEMBER APOSTOLAKIS: -- beta equal to 116?

MR. HICKLING: The shape is modeled entirely on the Scott curve. So the exponent is derived from the Scott curve, and the nominal threshold is derived from the Scott curve.

MEMBER APOSTOLAKIS: And alpha, too.

MR. HICKLING: No, the alpha is derived from our actual data.

MEMBER APOSTOLAKIS: Yeah, but in this plot it's the 75th percentile of the previous curve.

MR. HICKLING: yes.

MEMBER APOSTOLAKIS: Okay.

MR. HICKLING: But that previous curve is for our own data on the thick section, not for the steam generator.

MEMBER SHACK: But isn't the MRP curve the 75th percentile? The modified Scott was an earlier curve that had been proposed.

MR. HICKLING: Yes, yeah. The MRP curve is what we calculate on that basis.

MEMBER KRESS: Now, the data points --

MR. HICKLING: And it lies -- it's parallel to obviously the Scott curve because it takes the shape from it. It's force fit to it, but it's about 20 percent higher.

MEMBER KRESS: Yeah, but the data points on this curve are the same data points you use to get your probablistic alpha. So it's no surprise that it kind of goes through the mean of them because the 75th on that cumulative is like a mean.

MR. HICKLING: Yeah, it's the mean of the upper half.

MEMBER KRESS: So it's just reflecting the previous curve when you see it do that.

MEMBER BONACA: And I hope the Scott curve had a better fit to data than this.

MR. HICKLING: Well, that's why we used it.

MEMBER KRESS: Well, all this is saying if you go back to that previous curve, it went from ten to the minus 13 up to ten to the minus 11, and you look at the data on this curve. It does the same thing. It's a reflection of this curve right here.

MR. HICKLING: That's right.

MEMBER WALLIS: And any theory that you had that you forced alphas to be like this would go through the data.

MEMBER KRESS: Oh, yeah, absolutely, because you forced it to go through the data. And you forced it to kind of go through that part of the data.

MEMBER WALLIS: Yeah. That conclusion is Scott is wrong. I mean, Scott has nothing to do. Scott --

CO-CHAIRMAN FORD: Scott can't be wrong because it's based on --

(Laughter.)

CO-CHAIRMAN FORD: I'm not saying a Scott can't be wrong. But the Scott curve is an empirical relationship based on field data.

MR. HICKLING: Yes.

CO-CHAIRMAN FORD: But what I'd like to know John is you choose the 75 percentile of alpha according to the MRP curve.

MR. HICKLING: Yes.

CO-CHAIRMAN FORD: But I know there was some data where you should be at the 95th percentile. What was the reasoning behind the choice of 75 over the more conservative 95 percent?

MR. HICKLING: The reasoning is, Peter, quite simple, that we feel that in screening the database we've already applied quite a considerable amount of conservatism. There are a lot of material, as you know. For example, we couldn't consider any heats which didn't show cracking at all. So they're eliminated.

The reasoning is quite simply that we feel that this curve is a good representation, if you like, a conservative representation already of what is actually out in the field.

There will be a lot of heats out in the

field which will crack at very much lower rates than this, and I'm going to come onto a comparison with field data in the next slide.

MEMBER KRESS: You're saying that all the data you threw out would fall below that curve on this plot basically.

MR. HICKLING: In general, in general. There are two types. That would be a little bit too general, that statement. We threw some data out, for example, because it was tested in off chemistry, and that might have been higher, but a lot of the data we threw out would have quite clearly fallen well below this curve.

For example, in some of the wedge overloaded data which we threw out, those points were coming out at least an order of magnitude lower than they probably should have been simply because of problems of artifacts of testing.

MEMBER APOSTOLAKIS: But if you use the 75th percentile of alpha, wouldn't you expect most of the points to be below the curve? That doesn't seem to be --

MR. HICKLING: No.

MEMBER APOSTOLAKIS: -- the case.

MR. HICKLING: It depends entirely on the distribution.

MEMBER KRESS: That distribution, the 75, is actually close to the mean really.

MEMBER SHACK: Well, it's the 75th percentile on the heat. Now, if a susceptible heat has 32 data points, it's going to skew. When you look at data point by data point, it skews the distribution, which is one argument for doing it by heat. Otherwise you overly weight --

MR. HICKLING: Right.

MEMBER KRESS: And so this is a log scale down here

MEMBER APOSTOLAKIS: Wait, wait, wait. I'm speaking of the 75th percentile of this curve, right? If I plotted these points, you know, in the next curve, then I should have most of them below the curve.

MEMBER WALLIS: Yes, but you didn't.

MEMBER SHACK: But you didn't.

MEMBER APOSTOLAKIS: But you didn't.

MEMBER SHACK: You plotted the raw data.

MEMBER APOSTOLAKIS: You plotted the raw data, which now brings you back to the earlier assumption of using these points as being equivalent. Doesn't that tell you something about the uncertainty of each point and how important it is?

The fact that the new curve doesn't seem to be on the high side probably tells you something about the --

MEMBER WALLIS: No, it tells you there were 21 points for heat one and only one for heat 26.

MR. HICKLING: But there's a strong tendency for the laboratory to have tested a susceptible heat if possible. That's true in the whole history. They don't want to get a zero result which is of no use to anybody.

So there is an innate bias in any stress corrosion cracking test data to have chosen usually the most susceptible material they could get their hands on at least initially.

MEMBER SHACK: But the question is: do you want to characterize the variation in the set of test data that you have or in the population of heats of material that you're likely to encounter in the field?

If you want to characterize the variation in your test data, you do your statistics on all of the data points. If you want to do that, except you sort of hope that you have enough data that's really characteristic of the population.

MEMBER APOSTOLAKIS: Let's go to the next curve.

MEMBER SHACK: But what you're looking for is the population.

MEMBER KRESS: Well, why did you feel like you had to not use the whole curve? If you do a probablistic fracture mechanics, you could have used that whole distribution.

MR. HICKLING: We are doing it. The probablistic fracture mechanics uses the whole database and --

MEMBER KRESS: Okay. I feel better about it then.

MEMBER APOSTOLAKIS: So we could have a family of curves here, you know, with some confidence instead of a single curve, and that's what you're going to do in the probablistic --

MR. HICKLING: Exactly, except the probablistic, as I say, is not based on the MRP curve at al. The MRP curve we're trying to achieve is a reasonable representation of what we would expect for crack growth rate already involving some conservatism for heats out in the field.

MEMBER APOSTOLAKIS: So this is a reasonable representation?

MR. HICKLING: As Bill says, of the heats that are likely to be out in the field.

MEMBER SHACK: It's his choice.

MR. HICKLING: This was the expert panel's recommendation.

(Laughter.)

MEMBER SHACK:

MEMBER SHACK: Yet in a deterministic world you pick one curve. Which curve do you want to pick?

They have chosen the 75th percentile for the reasons that John has stated. You could make arguments that it should be the 95th percentile. You want to bound all of the data. You could make it the 50th percentile. You want a representative.

You know, you have to decide in a deterministic world with a lot of scatter. You have to make an argument for which curve you want to pick.

MEMBER APOSTOLAKIS: And the argument is that the points above the curve don't matter that much?

MR. HICKLING: Well, let's develop the argument a little bit more because the test of any curve is does it describe the field observations, and that's the point. It's already indicated a little bit.

There are actually two points written in here, which I'm going to come onto in the next slide what those are. There is very little data available in the U.S. from the field on nozzle cracking where there have been sequential measurements of crack length and depth.

The only data that's available is from one nozzle in D.C. Cook 2 where a crack nozzle was allowed to operate for a certain period of time, and there was increase in the measured length and depth of the crack.

And these two points are plotted here. This is the length increase of that crack, and this is the depth increase.

Now, agreed this is only one isolated indicate, but it is worth noting that both of those points fall very well below that curve.

We go on to the next slide --

CO-CHAIRMAN FORD: Could I just interrupt for one minute, John? I wanted -- this is the reason why we are discussing this data. This is one of the first times that this group has seen these data, and I wanted to be aware of the amount of work that's gone into this area.

However, we could go on forever discussing this, and what I would like to suggest is that we will finish this at 12 o'clock, this particular presentation at 12 o'clock. We will recess for lunch for three quarters of an hour, and we'll come back at quarter to one, and that will give Glenn hopefully time to do his presentation and leave when he wants to do. Yes?

MR. WHITE: Yes.

CO-CHAIRMAN FORD: Will that be okay?

So, John, could you pick and choose and try to finish by --

MR. HICKLING: Yes, we can get through the rest very quickly, I think.

(Laughter.)

CO-CHAIRMAN FORD: Yeah?

MR. HICKLING: With your help. Basically whenever you do a comparison from what you derive from the laboratory data with the field data, we've talked a lot about the uncertainties in the laboratory data, but it's worth remembering that there are very considerable uncertainties in the field data because we're basically talking about differences between two ultrasonic measurements of crack size, and we are really analyzing the difference between the delta between those two measurements.

So there's considerable NDC uncertainty, feeds in straight away here.

Secondly, there are uncertainties in the estimates of K, depending on how you analyze the residual stresses for the particular component concerned, and that's a very significant problem in this area.

And, thirdly, of course, there may be some uncertainty in the actual operating temperature of the nozzle, and we know how corrected these values are for temperature in different plants and in different countries.

I've showed on the previous slide the D.C. Cook data. The main body of field data we have available to compare with our curve is, in fact, French data because the French, once they detected cracking Bouget, did a lot of ultrasonic inspection, and they never had a second leakage.

So there is a lot of field data out there, and we made very considerable efforts to obtain everything we could.

The French reported their data at certain operating temperatures for their plants, and there has been some movement in what they've reported over the years as the operating temperature of different plants.

We have taken the latest report we were able to obtain on individual plants and extrapolated the reported data to a common temperature of 325 degrees Centigrade in order to compare it with our curve.

What we've done, rather than just comparing it simply with the curve, is we decided to go to a statistical approach here to show you how, in fact, the data, the screened data in our database, is going to work. And what we've actually done for the comparison is the following.

For every point where we had a field data point at a particular K value where we could derive a crack growth rate. We've done some random sampling from the upper half of the MRP distribution of crack growth rates, are using the same approach that we got, basically the letters to the 75th percentile, and using the K dependence of the Scott equation.

Let's just put up the results, and then we can come back to that. In this diagram, the black points represent the EDF field data extrapolated to the nominal temperature, 325, from the reported temperature of the head.

The red data points are the data points obtained from our MRP distribution applying this Monte Carlo approach to the top half of the distribution. So every time you did that, you'd get a different set of red points.

But remembering the uncertainties in that field data, we feel that's a more valid comparison than just putting a curve through it, and at first glance you can see that the Monte Carlo does produce some very high crack growth rates, of course, as you'd expect from the MRP distribution, and the agreement doesn't look that bad.

In fact, the next curve shows what that would look like on a cumulative probability plot of the French field data here, the black points, and this statistical treatment of the upper half of the database, which are the red points.

And there's no denying the French field data is higher, showing that the cracks measured in France in the field did grow more rapidly than what we're predicting, and when we consider there are very real reasons for that, as Larry mentioned earlier, we don't think it's just a matter of chance that the French plants have this problem so much earlier.

MEMBER WALLIS: Would you do the exercise of taking random numbers between 20 and 50 for K and between 1E minus 11 and 1E minus nine for crack growth rate?

Just take random numbers, do exactly the same thing you've done here. You'll get the same sort of picture.

MEMBER KRESS: Well, that's what he did, except the random numbers are --

MEMBER WALLIS: But what does it tell me? If the random numbers give the same result as your data, I'm not quite sure I've learned anything from the data.

MR. HICKLING: No, they're not entirely random numbers. It's a Monte Carlo treatment of part of the data.

MEMBER WALLIS: Well, no, I mean if I look at this curve here with this distribution of points.

MEMBER APOSTOLAKIS: Which distribution are we referring to? I haven't seen a single distribution here.

MEMBER WALLIS: If I had random numbers here, I get the same --

MEMBER APOSTOLAKIS: Which distribution? Of the alpha?

MEMBER WALLIS: The alpha.

MEMBER KRESS: Yeah, but they only selected from the top half though.

MR. HICKLING: Correct. It's an attempt to recommend the sort of variation that is inherent in the data, whether it be from the lab or the field.

MEMBER WALLIS: But there are people who have tried to publish reports like this, which show that taking random data on the same graph gives the same result, and that doesn't give me a good feeling at all that it's a useful exercise.

MEMBER KRESS: Well, it's a way to compare the French data to this database that went into making the curve. That's all he's saying. It's a way to compare those two.

MEMBER WALLIS: But if you compare the random numbers thrown at the --

MEMBER KRESS: But he's showing what would happen if you took the French data and put it on this same curve with -- you'd have ended up with a different distribution.

MEMBER APOSTOLAKIS: The French data were not part of the derivation of the curve for alpha?

MEMBER KRESS: No, and they say it's clearly a different set of data, and they have reason to believe it should not be part of the database, and

I think that reason is maybe weird, and that is, well, they started cracking a lot earlier than ours. So it must have been something.

MR. HICKLING: No, no. Excuse me. There are two separate issues here. That is the reasoning why the French data will always come out higher no matter how you treat it, because we do believe that the material susceptibility was higher.

The one thing we do know is that the material processing temperatures in general were much lower in France for that nozzle material, and there's good reason to expect that that would lead to a higher degree of susceptibility.

The second point, the reason why we didn't use the French data, for example, in deriving our curve is that there are uncertainties in the French field data which we cannot fully tie down and which we are ultimately somewhat unhappy about. We've extrapolated up very much in temperature. Whether or not that's fully justified is another issue, and it's an issue we couldn't solve.

MEMBER KRESS: It depends on whether your final product you want to be highly conservative or you want to be a representative value, I guess.

MR. HICKLING: Exactly, and the feeling is that we are trying for a representative curve, and the conservatism which needs to be added is added in the engineering analysis later on and is visible, not hidden in some way.

MEMBER KRESS: Yeah, which the ACRS has said is the way you ought to do things with respect to different issues in the past.

We have always advocated that as the right approach.

MEMBER APOSTOLAKIS: Well, yeah. There was a bullet that said if you did something because it was conservative. I mean, they're not as pure as it would seem.

(Laughter.)

MEMBER APOSTOLAKIS: Right?

MEMBER KRESS: There's always a mixture.

MEMBER APOSTOLAKIS: Yeah.

CO-CHAIRMAN FORD: It was the screening criteria which they said was conservative, and that's why they're using the 75th rather than the 95th percentile for alpha. It's reasonable.

MEMBER APOSTOLAKIS: So what Tom said is not quite accurate.

MEMBER LEITCH: John, one thing that concerns me regarding that French data, I guess, I've always wondered whether -- you know, we spend a lot of time talking about crack growth rate. I'm wondering about the depth of the crack at initiation. In other words, does the crack grow more or less linear? It's how many inches per year from zero, or might it be a fact that instantaneously the crack proceeds to some depth?

MR. HICKLING: No, definitely not instantaneously. You're quite correct. We're not trying to describe that whole phase of initiation and early growth, but all that we know about both primary water stress corrosion cracking in general suggests that the initial phase of crack growth is very, very slow, indeed, and getting the crack -- remember in the field we're not dealing with transgranular fatigue pre-crack which then goes into granular at all. We're dealing with a crack which develops as an intragranular stress corrosion crack at a point in time where you can't calculate it.

And all of the evidence is that a huge part of the lifetime, perhaps as much as 85 percent of the lifetime of the crack, as it were, is developing the initial crack, whatever you'd like to call initiation, and growing it to a level at which you can detect it with NDE methods.

So we're not addressing that whole area at all here. We're just saying what would we do to disposition once we find a flaw which is large enough to be found by NDE.

And I think the one thing that you can be sure about is that there's nothing instantaneous about stress corrosion cracking in that sense.

MEMBER LEITCH: So you're saying that the evidence seems to suggest that that initial phase is relatively slow compared with the ongoing. I was wondering if -- you know, in my mind I had pictured a model that was just the opposite of that. Initially it took a quick depth and then the growth was slow from there.

MR. HICKLING: No. I think you'd find pretty uniform agreement among anyone who's worked on stress corrosion cracking.

MEMBER APOSTOLAKIS: So the growth rate is independent of the size of the crack?

MR. HICKLING: No, it's actually not. It's very dependent upon it.

MEMBER KRESS: It's part of the K

MEMBER APOSTOLAKIS: Oh, K, K.

MR. HICKLING: It's later part of the K, and in the very initial stages, it's more complicated that --

MEMBER APOSTOLAKIS: Now, where do the curves cross up there? Is there any reason why they should do that?

MR. HICKLING: Yes, because the black points, in fact -- well, it's a function, of course, of the sampling that has been applied to the MRP distribution to get these particular set of points, but if we just go back very quickly to that alpha curve, it's a point I'd like to make.

Remember this is a log normal fit which is approaching one exponentially. So you are predicting infinitely high crack growth rates, albeit with a very, very low probability that it will ever occur. So that is physically unreasonable.

And, in fact, as Dr. Riccardella will talk about in the probablistic talk this afternoon, for that purpose you're going to have to truncate this log normal distribution, go to a log triangular because it's physically unreasonable to go to infinity. A stress corrosion crack would never do that. It can't do.

But the effect of using it in the way we've just done it is, of course, it can generate some very high crack growth rates even at low K.

MEMBER APOSTOLAKIS: So how big was your Monte Carlo sample? Was it big enough to pick up those values, the sample?

MR. HICKLING: Yes.

MEMBER APOSTOLAKIS: You said you did a Monte Carlo.

MR. HICKLING: You mean the number of iterations?

MEMBER APOSTOLAKIS: Yeah, yeah, because it will be a very large number to start picking up the very unrealistic --

MR. HICKLING: No, I'm not saying we'd be picking up any which are way out in the table here, but I'm saying it's inherent in the approach that we're using.

MEMBER APOSTOLAKIS: Does that explain why the curves cross?

MR. HICKLING: I think so, yes, because the French field data is real data, albeit with uncertainties.

MEMBER APOSTOLAKIS: Oh, okay. So it's an artifact.

MR. HICKLING: Can we go on quickly?

I want to make one very -- one before, please. Thanks -- I want to make one very important point. In actual fact, in France in the regulatory context, the French finally did not use any of these approaches. The actual French approach that was finally agreed upon was that in no case did the actual measured crack growth rate in the through wall direction of any crack which was found in plant exceed four millimeters per year, and this was actually the figure they adopted irrespective of head temperature as a limit which would allow them to justify continued operation for at least one cycle even with cracks which were already 11 millimeters deep.

MEMBER WALLIS: And your French data plot shows 20 or 30 millimeters a year. That's in the other direction.

MR. HICKLING: No, but that's because it's been temperature corrected, and it's been pushed up a lot in temperature. The reported temperatures for the French plant, as I said, have moved somewhat, but they tended to move down quite low. So we've had to extrapolate up an awful lot, and we're not very happy about having done that, quite frankly.

MEMBER WALLIS: That's one reason they're so high.

MR. HICKLING: Absolutely.

MEMBER WALLIS: Or it is the reason

they're so high.

MR. HICKLING: So moving on to what do we actually intend to do with this curve and why do we think it makes sense, it's intended that it would be used to detect the disposition, PWSCC floors, either if they're axial ID floors or if they're below the J-groove weld, i.e., we're not -- floors which are not part of the pressure boundary.

The main application we see is a deterministic evaluation of axial ID floors which are part of the pressure boundary. We're not intended to use it, as we discussed earlier, at very low K values. Such floors, once detected, will already be well above any K value that you might be looking at here.

And this is to give you a feel for a generic calculation of what that ID axial crack growth would look like. The Y axis here is showing the depth of the axial ID crack initially, and this is showing the calculated operating time to reach a 12 millimeter deep crack, which would be 75 percent through wall acceptance limit in the nozzle, to give you a feel for the sort of way in which this would pan out.

MEMBER WALLIS: There's a lot of uncertainty in this, isn't there?

MR. HICKLING: There's a lot of

assumptions I would say is perhaps a better word as well rather than --

MEMBER WALLIS: I don't know how you can get one curve from that tremendously uncertain data without showing many curves or something.

MR. HICKLING: Well, the way we get to a single curve is defined because of the way we've defined the curve. I think the question is what uncertainty remains in the analysis.

For example, we've assumed in this particular case a particular K value based on a residual stress here. Now, this is a generic calculation. It's purely an example calculation, nothing else.

In any application of this, we'd expect that a found floor would be dispositioned correctly in terms of the best possible stress analysis to reduce the uncertainty, for example, in --

MEMBER KRESS: Yeah, but I would have also expected for a specific case for the decision maker to make an appropriate decision, you would have a set of curves for the distribution of the uncertainty about that curve.

MR. HICKLING: Not in terms of the deterministic approach, no.

MEMBER KRESS: Well, yeah, but that's one of our problems with the deterministic approach. We never know what the uncertainties are, and the uncertainties are what drive our decision making process.

You know, if that curve had uncertainty bounds on it, five and 95 percentile or something, then as a decision maker I'd have enough information to at least think about what decision I want to make, and you could do that with the database you have. It's inherent in it.

Pardon?

MEMBER APOSTOLAKIS: We're going to discuss this this afternoon.

MEMBER LEITCH: I'm afraid I don't understand how that curve would be used. Maybe I don't understand the axes.

MR. HICKLING: In an actual plant situation, you would detect with NDE a crack which you would size as best as you possibly could.

MEMBER LEITCH: Right.

MR. HICKLING: And here we're saying that we might size it as let's take an example and size it at four millimeter depth (phonetic).

MEMBER LEITCH: Okay.

MR. HICKLING: You do the best possible analysis you could of the residual stress driving that crack based on all sorts of things, including nozzle downhill angle and all of the other things you might be able to put into that to get your K value, which would feed into the equation here.

You'd adjust your head temperature to the correct value for the actual plant, and you'd then read across and determine that without adding any subsequent conservatism, which you would almost certainly want to do; the prediction from the MRP crack growth rate curve would be perhaps in that case that you would need something like 16 months or 15 months for that crack to have grown from four millimeters deep to 12 millimeters deep.

MEMBER LEITCH: Okay.

MEMBER KRESS: And that's part of the analysis you make to determine whether you can continue operating in a certain amount of time.

MEMBER BONACA: So that curve will shift.

MEMBER ROSEN: Well, if you have an 18-month cycle, you look on that curve and see if your operating time is greater than 18 months and it says it is; then you can run the cycle.

MEMBER BONACA: Right.

MR. HICKLING: That's a good point. This is the temperature of what we regard as the hottest head, which might be actually applicable.

MEMBER APOSTOLAKIS: But is there any reason to believe that this curve is conservative? I mean, in a deterministic world at least you want to have something conservative.

Is it conservative?

MR. HICKLING: There are some conservatisms inherent in the derivation of the curve. That's the point I was trying to make earlier. Whether or not it's a conservative curve is a global question which is very difficult to answer.

We consider that it's a representative curve for some of the heats which are more likely to crack because remember it's the 75th percentile, not the 50th, of our database.

Could I just go quickly over the very final slide?

There is no intention, I think, in the industry to try and disposition OD cracks which are actually found. Going back to what we talked about right at the very beginning, if we were talking about hypothetical calculations, we would recommend that this factor of two, which represents the uncertainty in the chemical environment be put onto that curve.

And a subgroup of the experts did look at the experience. We still think the arguments as I mentioned that we put forward on the environment are valid in the non-Davis-Besse situation, which we consider to be the usual case which has been found to date.

However, last slide Christine.

It wouldn't be valid, and we're not claiming that it would be if the leak rates were sufficiently high to get a large, local decrease in temperature, cavity formation, and steel.

That brings up the question: what would happen with stress corrosion cracking of Alloy 600 in that case?

And that takes me back to this point I mentioned earlier, that in general, we think of Alloy 600 as being very resistant to cracking in acid media. There's very little data available. What there is shows that in order to get cracking in concentrated boric acid, you need quite high levels of both oxygen and chloride contamination, not just one or the other.

And interestingly, the effects at N was at intermediate temperatures, suggesting that we're now in a different type of Alloy 600 cracking, not the primary water stress corrosion cracking we've been talking about.

And that's all I had.

CO-CHAIRMAN FORD: Thank you very much, John.

MEMBER KRESS: The factor of two that's put on there, because of chemistry uncertainties, strikes me as being a little strange in view of the uncertainties in the data about getting the curve in the first place. It's just overwhelmed by the --

MR. HICKLING: It's handling a different situation.

MEMBER SHACK: It moved the whole population is the theory.

MR. HICKLING: Yes.

MEMBER SHACK: On any crack growth rate of heat, it's insignificant compared to the variation between heats, but if you're moving the whole population.

MEMBER KRESS: I'll have to think about that one. I still think it's gilding the lily.

CO-CHAIRMAN FORD: John, thank you very much indeed.

I'd like us to go into recess until quarter to one when we'll start again. Quarter to one, guys.

(Whereupon, at 12:04 p.m., the meeting was recessed for lunch, to reconvene at 12:45 p.m., the same day.)

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

(12:49 p.m.)

CO-CHAIRMAN FORD: Okay. We're back in session.

We're going to start with the technical assessment of Davis-Besse's degradation. Am I correct?

MS. KING: Yes, you are correct. I do have both presentations for you, and in your packets, this would be Slide 81, about three quarters of the way back. And we will come back to the fracture mechanics.

MS. WESTON: If I may, some of the slides and tables are in your book starting at page 131.

MEMBER APOSTOLAKIS: So when you said the 81?

MEMBER KRESS: The package of slides.

MEMBER APOSTOLAKIS: This package, yes. Okay.

MR. WHITE: Good afternoon, everyone. My name is Glenn White, and I'm with Dominion Engineering.

Since March 22nd, Dominion Engineering has been supporting the Electric Power Research Institute and the Materials Reliability Program on assessing the

Davis-Besse experience. Specifically we've been trying to understand, based on calculations, analysis work, and also looking at experimental data that's available, what the degradation progression was at Davis-Besse.

MS. KING: We're in animate mode. Let me fix it real quick. Go ahead.

MEMBER APOSTOLAKIS: Show without a dimension. See that on the left at the bottom?

MS. KING: Thank you very much.

There we go.

MR. WHITE: Okay. The presentation that I have prepared that's in the packet here is approximately 15 slides of material that summarizes the various mechanisms that could possibly be active and summarizes our conclusions as to what we believe happened at Davis-Besse, what the likely progression of degradation was.

Two weeks ago at an NRC meeting with some of the NRR staff and research staff, I presented a longer presentation, 63 slides. That presentation is available on the NRC Web site, and we have that as back-up material for this discussion.

So if there are questions that get into particular areas, I'm prepared to answer them using that longer presentation, but the original time allotment for my talk was only a half hour. So that's why they're sticking to the 15 slides in the packet.

MS. WESTON: Glenn, they have copies of that package in the notebook.

MR. WHITE: Okay.

MS. WESTON: They have the whole package.

MR. WHITE: Great, perfect.

I'm going to start off talking about the purpose of this work, the approach that is called for, and then get into the individual mechanisms briefly, as I said, and then outline what the likely degradation progression was based on our analysis work, supplemented with experience and experimental results, and then also touch on the most relevant experimental test that had been performed in the past because I think it's important to touch on that.

We've done work to try to quantify the chemical environment and the thermal hydraulic environment along the leak path in the annulus on the OD of the nozzle, and so there are a lot of other analyses that we can get into, as I say.

So if we go to the next slide, the purpose here is to answer two main questions that have been put forth. The first one is if there is significant degradation it will be detectable visually, by doing a visual inspection of the region above the head.

And the unit could be detectable a couple of different ways. One, you might see a void directly so that you could see the wastage directly.

But the other way, you could infer that there might be wastage that would require a closer look if you found a significant amount of deposits, either boron deposits or some corrosion product deposits. So that's the first main question.

The second main question has been put forth is what is the time scale of this process following initiation of a through wall leak. Is there a period of time that we all have assurance that we can't reach unacceptable wastage? That's the second question.

A related question to that is: what is an unacceptable level of wastage, and I'm not directly addressing that in this presentation here because it's a closely related, but a slightly different subject that really goes to the structural stress calculations.

What I'm going to be concentrating on is the degradation progression, the environment in the annulus, and the various corrosion and potentially erosion mechanisms. But on the question of what is acceptable, I will mention that in the early '90s, in the '93 time span, the three owners' groups did finite element analyses taking out a certain volume of the -- actually six cubic inches of volume of the low alloy steel head, and they did that using different geometries of the assumed loss, different aspect ratios of the voids.

And at that time it was determined that six cubic inches allows the code margins to be maintained.

MEMBER WALLIS: It depends how it's removed.

MR. WHITE: It depends how it's removed, but each owners' group took two or three different bounding assumptions. So based on those --

MEMBER WALLIS: But if it's a straight hull, it's very different from taking off six cubic inches all the way around.

MR. WHITE: Yes. For example, it would take all six cubic inches along the other surface, the top surface of the head, or you could take the six cubic inches along the bore, and no matter how they were taken out, the stressors are still within code margins.

MEMBER WALLIS: That's assuming you had a lined head?

MR. WHITE: Assuming different geometries, different bounding geometries is what they did.

Since that time, we have just recently begun to look at this question of what is acceptable wastage, and Dominion Engineering has performed some preliminary finite element analyses, taking out some of the elements that make up the head, and the conclusion from that work is that it's most likely significantly more than six cubic inches can be lost and still the primary membrane stresses will still be below the code allowable stress intensity values.

And just mentioning because this is a related question --

MEMBER WALLIS: This is with the stainless steel liner, cladding?

MR. WHITE: The cladding is a second question. The first thing we did --

MEMBER WALLIS: Well, without your cladding you could make a hole six cubic inches, couldn't you? You could drill a hole through it and remove six cubic inches. You have a small LOCA that's all

MR. WHITE: We've also looked at the issue of the cladding, and I believe later this afternoon there will be some discussion about the margins in terms of the cladding for Davis-Besse, and there, again, there could be a significantly large area where the cladding is retaining the pressure.

MEMBER WALLIS: Okay. So like 200 inches at Davis-Besse?

MR. WHITE: Yes, approaching 200 cubic inches of material loss at Davis-Besse, and I'm going to put that in the context of the progression in some other slides here.

Okay. The basic approach is to examine how the various conceivable mechanisms and material loss change as the leak rate increases. Through our analysis work, what we found is it's really the rate is the controlling parameter for two main reasons which are shown down here.

Number one, the level of cooling. When you start with primary water, it has a certain enthalpy, about 613 BTUs per pound. If you have saturated steam at atmospheric pressure, its enthalpy is higher. So you need to have some heat input in order to completely boil off that primary water.

But the primary water because of the temperature and the pressure, it does have enough enthalpy to boil itself through flashing al the way to 45 percent quality, assuming atmospheric pressure.

To get from the 45 percent quality all the way up to 100 percent quality, you need a heat input, and obviously that heat input is proportional to the size of the leak rate. So the higher the leak rate, the higher the heat sync, the more local cooling. The more local cooling you have, the more ability there is for liquid to exist in that annulus, and it's the liquid environment which is potentially corrosive to the low alloy steel.

The second point are the velocities, the magnitude of the velocities. For very low leak rates, velocity, just a simple average mass balance velocity calculations show very small velocities which are not consistent with erosion or potentially flow accelerated corrosion mechanisms.

So,a gain, the leak rate is the controlling parameter in terms of the potential for erosion or flow accelerated corrosion. So that's why we concentrate on varying the leak rate.

Okay. Go to the next slide.

The leak rate also has another important determining characteristic, and that is the leak rate determines the magnitude of deposits that will exit the pressure boundary. As we've heard, of course, the concentration of boron in the primary waters decreases over the fuel cycle from in the neighborhood of 2,000 ppm down towards 100 ppm, in some cases lower than that, ten, five ppm at some plants right at the end of the fuel cycle.

But if you integrate over the same time period for two different leak rates, you'll get the amount of deposits being proportional to the leak rate.

The bottom line here from the analysis is that we integrate all of the results together to determine the time frame for significant degradation and then correlate the volume of wastage, material loss of the head versus the volume of deposits produced, and, for example, at Davis-Besse it has been reported that there were 900 pounds of boron deposits on top of the head.

So we're trying to do analysis work in order to try to show how much wastage you would expect as the amount of deposits on the head. Obviously hundreds of pounds in deposits should be readily visible on top of the head. Much smaller amounts of deposits may require the insulation to be removed.

All right. The material loss mechanisms.

If we go to the next slide, we start off on the corrosion or the chemical type of mechanisms. The first one here --I'll just briefly touch on each one of these -- boric acid corrosion.

In the leak process, you can have a concentration occurring due to the boiling, flashing and boiling, process which tends to concentrate the boron. So you can end up with a concentrated boric acid solution.

However, if there's no oxygen, typically these sort of de-aerated boric acid tests of low alloy steel show very low corrosion rates. So that's the first thing to keep in mind.

The second potential mechanism here is deposits themselves. Could they be corrosive without liquid?

And there have been some tests that have been attempted with some deposits on top of low alloy steel and found to be very mildly corrosive in a human environment. So that's the second potential mechanism.

Then we do have a crevice geometry here. We have the annulus. So potentially there could be a crevice corrosion mechanism. Crevice corrosion is a mechanism that's of concern in marine applications often. It's also a concern with the waste packaging at Yucca Mountain.

So we've looked at crevice corrosion as a potentially significant mechanism.

We also have, as mentioned before, the low alloy steel is in contact with the Alloy 600 nozzle. So there's a galvanic couple, and perhaps that could drive a corrosion mechanism. Where that coupling, the low alloy steel will raise the corrosion potential or the Alloy 600 will raise the corrosion potential of the low alloy steel and provide the driving force for the corrosion. So we've also looked at that.

Then the next mechanism coming down the list here is classic boric acid corrosion. Now we have an aerated environment. There have been many tests performed in this sort of environment. They're documented in the boric acid corrosion guide book that's been published by EPRI , and you can have up to one to five inches per year of corrosion shown in these tests where you have oxygen that's in the solution.

Lastly here, molten boric acid corrosion. Boric acid deposits have a melting temperature of about 340 Fahrenheit. So even without water, you can have a liquid at the higher temperatures, and the question becomes: how corrosive is that liquid? And so I'll have some comments on that molten salt type corrosion.

And this slide here are the flow type, velocity type mechanisms here, and the first one being flow accelerated corrosion. That's a possibility depending on whether or not there's a magnetite layer that may form on the low alloy steel. This is, of course, a mechanism that is seen on the secondary plant in the piping. So we've examined looking at the possibility of that having an influence on the development of the process.

And then there are more just the straight erosion type mechanisms, flashing induced erosion. If we think about gaskets that can develop leaks, you may have a local region that may be a somewhat analogous situation here with erosion.

You hear the term "steam cutting erosion." That's just really another term for flashing induced erosion. We have water droplets. So, therefore, the term "droplet impingement erosion."

Single phase erosion of steam velocities as you boil water off all of the water content in single phase steam and potentially you might have velocities of the steam and potentially that could lead to a single phase erosion.

So that's an introduction to all of the mechanisms that we could come up with for removing material.

This matrix here is a preliminary take based on the last two months of work on how these mechanisms may stack up in terms of which ones are active. As I mentioned, the first two have low rates. So we don't think they play a major role in the progression.

Then we get to single phase erosion. We start with an initially tight annulus, a gap on the order of 1/1000 of an inch radially there or perhaps tighter. So initially if you have a leak, it may lead to velocities high enough to get erosion.

Now, once that annulus would open up, then the velocities would be reduced because of the greater flow area. So perhaps for the initial tight annulus the single phase erosion could be a factor or impingement erosion also.

I've got full accelerated corrosion listed here if the velocities are high enough. Crevice corrosion. I can say that this is not a classic crevice corrosion type system here because crevice corrosion is typically associated with materials that passivate (phonetic), like stainless steels.

If we had -- crevice corrosion is driven by a chemical process where the anodic corrosion reaction occurs deep down in the crevice, but the cathodic reaction occurs away at the exposed surface on top of the head. If there was a liquid film up on the top surface of the head, potentially you could have the driver for a corrosion circuit from the outside to the inside deep down in the annulus.

However, in our case, if there's going to be a significant water film on the outside of the head, in the top head surface, then we would expect there also to be deposits in an acidic environment, which would lead to significant corrosion rates themselves. So it would act as an anodic site up on the outside. So we don't see this separation of the cathode and anode excites in the low alloy steel due to the crevice corrosion, provided that you have the acidic environment on the outside of the head.

But the next mechanism here, galvanic corrosion in the secluded type geometry may be more of a possibility. We do have the coupling from the low alloy steel to the Alloy 600, and that potentially does give you a driver for the corrosion.

However, there isn't enough data available in the literature to try to quantify the magnitude of that mechanism. There just hasn't been a lot done with low alloy steels and boric acid type environments with things to measure polarization curves and so on. We haven't addressed that from a basic corrosion science standpoint yet.

Molten boric acid corrosion here. I'm saying that it's possible, but we expect lower rates. There isn't a lot of available data experimentally in terms of trying to measure its corrosivity for low alloy steel. However, if we look at the basic corrosion chemistry there, we know that the molten boric acid has a lower -- the solubility of corrosion products are lower in molten boric acid than in aqueous solutions. So that's one factor.

Electrical conductivities are likely to be lower in molten boric acid, and also the oxygen and hydrogen ion concentrations are also likely to be lower in a molten salt type solution.

So for some fundamental reasons we believe that the molten boric acid corrosion is unlikely to produce the one to five inches per year that has been observed with the aerated concentrated boric acid solutions, but it's still something that has to be looked at.

Okay. So that takes care of this slide here. This next slide here really sums up the analyses that have been done in terms of understanding the chemical environment, looking at the pH through multi-Q calculations.

MS. WESTON: It's page 139 in the book.

MR. WHITE: And we've also performed thermal hydraulic calculations and heat transfer calculations to try to quantify the temperature as a function of the leak rate. We've calculated velocities as a function of leak rate, wall sheer stresses, as I mentioned, the pH under various conditions.

So putting all of those things together, we've developed this degradation progression here which really goes from the left side of the slide to the right side of the slide as the leak rate may increase over time.

The top row of boxes here has a nozzle or weld condition. Early in time you would just start out with a leak path to the annulus, but in a very small leak.

As that crack growth continues, that leak -- an axial through wall crack may reach above the top of the weld for a significant distance. At Davis-

Besse, which would be associated with the far right area here, there was an axial through wall crack that reached .9 inches above the top of the weld on the nozzle ID and 1.2 inches above the top of the weld on the nozzle OD.

So there was a leak path that extended all the way through the nozzle a significant distance above the top of the weld and leak rate calculations that we performed as part of this work have shown that should result in a high leak rate, meaning on the order of .1 gpm, which is consistent with all of the evidence for the Davis-Besse nozzle number three.

So we have growing cracks, increasing leak rate as we go from left to right across the page here.

MEMBER SHACK: Now, what does the pressure drop look like, say, with that .9 inch crack and I have a pressure drop across the crack into the annulus and then I have the annulus -- the interference fit to the atmosphere? What's the pressure drop across the crack and then across the interference fit?

MR. WHITE: Well, I do have some slides on that, but I don't want to go right to them. What I would say is initially when you have that very tight initial annulus of a mil, a half a mil or so, you may have also a significant pressure drop in the annulus itself.

But as the annulus tends -- as you begin to have some material loss, very quickly you'll reach a couple mils radial gap and the calculations show that you basically have atmospheric pressure at a very large range of leak rates in that annulus.

So fairly early in the process we believe that we essentially have atmospheric pressure in the annulus, and really the choke point in the flow is at the exit of the crack.

And that's what I'm showing here on this line, is the annulus condition. Here possibly hypothetically starting off clogged, but then opening up and allowing more and more flow through, but it's really the crack that's more the governing resistance to the flow.

Leak rates here. Well, we'll start over there. We have a hypothetical zero leak rate. Contrary to experience, we had a nozzle with a leak path type crack, in other words, a leak path reaching to the annulus, but there was no actual flow making it to the outside to the top of the head. Then we would have a hypothetical zero leak rate, and this column addresses that situation.

As we go to the right, we're increasing in leak rate, .001 gpm, .01 gpm as we move to the right, and then up to the point greater than .1 gpm on the far right.

MEMBER ROSEN: Why do you say in your first column that you will at least have some small amount extruded in that circumstance? This is the classic stealth crack that we worried about.

MR. WHITE: Well, just thinking that if you're going to precipitate and go up the annulus, you should be pushing out a small amount. I'm not claiming how visible that's going to be.

MEMBER ROSEN: Pardon me?

MR. WHITE: I'm not claiming how visible that amount will be. I'm just saying that you have a clogged up annulus with --

MEMBER ROSEN: It could stay subsurface you're saying. It says here at least a small amount is extruded . Presumably you mean outside the crack in the annulus. The extrusion results in deposits that are visible.

MR. WHITE: Right. Well, as I say --

MEMBER ROSEN: It's your chart. I'm just asking what you mean by that.

MR. WHITE: What the real experience has been over here in this chart, in this column over here, we do have small amounts of deposits that come up that correspond to small leak rates.

MEMBER ROSEN: I don't think I agree with you that it's been in the second column. That column has a bottom line of seven pounds, and we've seen pictures where there were very small amounts.

MR. WHITE: Less than seven pounds.

MEMBER ROSEN: Sure.

MR. WHITE: Yeah, much less, yeah.

MEMBER ROSEN: So the column on the left was what was operating in those conditions. We had a lot less than seven pounds, and I'm trying to examine what happens down at the end --

MR. WHITE: Right.

MEMBER ROSEN: -- the boundary of that.

MR. WHITE: So let me talk about if there is a zero leak rate what happens and you don't have significant deposits that come out.

In that situation, in a hypothetical situation, we have no velocity. so you have no erosion type mechanisms that could be active, and you would have no cooling going on. So you would have a crevice environment there that's at 600 degrees approximately, the primary temperature.

But since this is a clogged annulus up to the point where the clogging is, you're going to have pressurized water at the primary pressure. You're not having any boiling going on because there's no flow. If there was boiling, there would have to be a leak that would be actively going to the outside.

So there is no vaporization driven concentration mechanism with no flow at all, and then as we heard earlier in John Hickling's talk, there's not going to be oxygen down in that crevice environment. So there aren't the conditions that would produce -- the corrosion rates would be limited to the low corrosion rate s that had ben measured for de-aerated environments, and without a large concentrating mechanism it should be even less than most of those tests which were done in concentrated boric acid conditions.

MEMBER WALLIS: If I remember correctly, the analyses that were done to preclude oxygen at the bottom of the annulus were done for fairly tight crevices and straight fits that you have --

MR. WHITE: Right.

MEMBER WALLIS: -- in these designs. In order to get to the right of that diagram, to get your temperature down, which you will need for the high corrosion rates, does that preclusion of oxygen analysis still hold for the fairly wide annuli that you're going to need to have the flurry?

MR. WHITE: No. Well, as we move all the way to the right here, aerated boric acid corrosion once you have something that opens up to the problem, you definitely would have the aerated boric acid corrosion as --

MEMBER WALLIS: Oh, okay.

MR. WHITE: The question becomes: at what point does the oxygen get down into the crevice? It's obviously between those two points, and at this point we just can't say exactly where that point is based on the work that's been done so far.

MEMBER WALLIS: Okay.

MR. WHITE: It's just when you're very hot, the hot iron is going to be very efficient at taking out the oxygen. It's when you have the cooling and the opening up together and the higher velocities and the eddies that could form. Then you could start to have oxygen coming down deep into the crevice.

MEMBER ROSEN: So to finish this discussion, the stealth cracking mechanism that has been postulated that what we saw at Davis-Besse could be going on under the surface, in your own words now, how likely is that here?

MR. WHITE: Well, the work shows that it's unlikely, taking in turn first the case of no leak rate at all, having a pressurized annulus with single phase liquid without a big driver for concentration and no oxygen. That would have no active mechanisms.

As we move towards small leak rates, ten to the minus six gpm, ten to the minus five gpm, for much of the cracking we see on the order of a cubic inch of deposits that corresponds to a gallon of leakage in a year. That's two times ten to the minus six gpm.

So as we approach ten to the minus five gpm, when you do the heat transfer calculations, you don't get the cooling. So what's going to happen is that that annulus is going to boil dry immediately right near the bottom of the crack, right near the bottom of the annulus at the crack. So there isn't going to be liquid over a significant volume or height inside that annulus. So that's really what's preventing corrosion mechanisms that may potentially occur in the absence of oxygen from really being significant.

I mean, this is just consistent with all of the experience out there for very small leaks that show minimal material loss. You don't have the velocity mechanisms, and you don't have very much liquid around at all. Perhaps it's all boiling dry low in the annulus, and you need that liquid even to get something like a galvanic type mechanism going.

CO-CHAIRMAN FORD: If I go from the left-hand side and approach the right-hand side, you're having more and more conjoint requirements that are necessary to get a Davis-Besse situation on the right-hand side.

MR. WHITE: Yeah, you're --

CO-CHAIRMAN FORD: And because there are so many conjoint requirements, annulus size, exposed crack length, leak rate into the annulus. So you're precluding the possibility of this being a generic phenomenon.

However, you don't have to be on the right-hand side to have a real bad situation. Those EPRI and CE tests were one inch per year. So you've only got to get over to the middle column before you've got potentially a fleet wide problem.

I use that obviously to make a point. It's not an isolated set of criteria that you need. Am I overstating it?

MR. WHITE: If I were to draw this down here, these tasks that are on the upcoming slides, I would probably draw this more towards this range. We can go over the actual leak rates in these tests, but they were closer to the .01 to .1 gpm.

There was one test down at .002 gpm by Combustion Engineering that had a significantly lower rate of corrosion than the other test at .01 and .1. So it's really the .01 number that I'm taking from those tests as being sort of a critical value based on that.

At Davis-Besse we believe that the leak was between .04 and .15 gpm based on the unidentified leakage, based on the mass of deposits that were observed, and other indicators. So that would put it all the way off to the right there.

MEMBER WALLIS: There seem to be various things I'd like to know more about. I don't know the details of your analysis, but the way in which the annulus clogs or doesn't clog or periodically extrudes whatever is in there, is that just hypothetical?

One could postulate all kinds of things that could happen in an annulus in terms of deposits and the way they can be pushed out or slowly slide out or do various things.

MR. WHITE: Yeah, one possibility is that when the head cools you have the difference in cold fission or thermal expansion. So the annulus tightens up, and as it depressurizes, at that point the annulus comes back to that interference.

MEMBER WALLIS: It could slowly flow out although it's apparently solid. It could slowly be extruded from the --

MR. WHITE: Right, being in molten form. What we're saying though here is we're trying to show that regardless of those details, without having a liquid high up in the annulus and without having any velocities to speak of, there are no credible active mechanisms.

MEMBER WALLIS: Why is the velocity coming out of this hole zero feet a second and not 1,000 feet a second?

MR. WHITE: Well, if you're postulating that the annulus is completely blocked up.

MEMBER WALLIS: No, it's a crack. It's coming out of the crack. The crack tip goes through, and the velocity -- it says liquid velocity exiting the crack. It's coming out of the crack. So if we had a fairly broad crack and as it breaks through, what is it going to say, a sonic flow at the exit poll? Why is it such a low velocity coming out of the crack?

MR. WHITE: As we increase the leak rate, you're asking about the --

MEMBER WALLIS: Well, even at the beginning. I mean at any time why is it so low? Why isn't it -- why couldn't it be much higher at the beginning?

MR. WHITE: Well, if we took the -- should we put up the slide?

MEMBER WALLIS: Maybe even do just a calculation of flow through very long, very fine tube of a flashing liquid. It takes a pretty long tube before you stop getting choking at the exit from the tube.

MR. WHITE: Let's show you what the --

MEMBER WALLIS: Maybe it's too complicated to get into now, but I'm surprised that you couldn't get a much higher velocity under these circumstances.

MR. WHITE: Go to 544 in the other presentation.

MEMBER WALLIS: It depends a bit upon the shape of the crack. You do it as a two-phase calculation of the flow in the crack?

MR. WHITE: Right. What we're really looking at here is we took as a flow area the area opposite the crack.

MEMBER WALLIS: Well, if you're using Moody and Fauske and HEM, aren't those models for choking?

MR. WHITE: No.

MEMBER WALLIS: Critical flow? Yeah, that's what Moody and Fauske deal with, critical flow.

MR. WHITE: These are just slip models we're just using. We're just assuming two-phased flow in a pipe, for example.

MEMBER WALLIS: Well, you're using a square root of density ratio.

MR. WHITE: Yes, right, right. Just to get a handle on the velocities, we were interested in the velocities not right at the crack exit, but --

MEMBER WALLIS: You're basing it on the shape of the crack, not just on the --

MR. WHITE: Yes.

MEMBER WALLIS: -- flow rate.

MR. WHITE: I agree, I agree. As a first cut, we wanted to get some --

MEMBER WALLIS: So you assume something about the shape of the crack?

MR. WHITE: No. All we did was we took the flow area as the area opposite the crack. As the flow turns, it's going to expand.

MEMBER WALLIS: Opposite the crack? No, it isn't. What matters is the flow and the area in the crack itself.

MEMBER SHACK: This chart is showing a leak rate through the crack of a given amount. this is the annulus velocity that you would get.

MR. WHITE: Right, as you're --

MEMBER WALLIS: It says here exiting crack. Maybe it's the words that are wrong.

MR. WHITE: I agree.

MEMBER WALLIS: If you were saying the velocity in the annulus -- I agree the velocity in the annulus could be low, but the jet coming out of that crack could conceivably be sonic, and that's going to do something in that annulus presumably.

MEMBER ROSEN: Graham.

MEMBER WALLIS: Yes.

MEMBER ROSEN: I have a slightly different model that at the crack itself, you know, is a very labyrinth kind of thing, and it functions as a breakdown orifice.

MEMBER WALLIS: It's like a porous median, and then it maybe breaks through the outside, a little hold.

MEMBER ROSEN: Just barely, and there's

almost no -- the pressure drop through this labyrinth and pathway is enough to --

MEMBER WALLIS: Well, maybe it is.

MEMBER ROSEN: -- initially create -- there's no velocity at all. I mean as it first breaks through, it just drips.

MEMBER WALLIS: It certainly doesn't drip. It may come out with steam.

MEMBER ROSEN: Yeah, well, it flashes. I mean a little bit of liquid which is completely broken down; pressure that's completely broken down in this labyrinth --

MEMBER WALLIS: Well, that's your picture of it.

MEMBER ROSEN: -- drips out, drips and flashes.

MEMBER WALLIS: That's your picture of it.

MEMBER ROSEN: Yeah.

MEMBER WALLIS: I'd like to know what the reality is.

MEMBER ROSEN: Well, I'm just saying that there's a way to think about it that creates very low velocity.

MEMBER WALLIS: Yeah, but there's also a way to think about it that gives you 1,000 feet a second or so.

MR. WHITE: We've done calculations to try to calculate the crack opening area. So we can use those to give you some velocities also.

CO-CHAIRMAN FORD: If I could suggest we don't do that right now.

MR. WHITE: Okay.

(Laughter.)

CO-CHAIRMAN FORD: Time and what I want the committee to understand is where they are in this overall approach. There are hundreds of questions, and you get the idea.

MEMBER WALLIS: But then they're off by a factor of 10,000 in velocity, and it's interesting to know.

CO-CHAIRMAN FORD: That's true.

On this diagram here, and it's the last question we'll take on this one.

MR. WHITE: Okay.

CO-CHAIRMAN FORD: It's my understanding the tech spec is one gallon per minute.

MR. WHITE: Right.

CO-CHAIRMAN FORD: And, therefore, all of the operating plants, if they could detect, you could be right over the right-hand side there and have all of your mechanisms which would be -- have one gallon per minute, absolutely okay. That's the only criterion we're taking. That's correct, isn't it?

MR. WHITE: Well --

CO-CHAIRMAN FORD: I'm saying it could be done at -- according to the EPRI --

MR. WHITE: Right.

CO-CHAIRMAN FORD: -- tests of one inch per year, you could be down at .01 gallons.

MR. WHITE: The way that I look at the far right of the chart here is that the calculations show if you have more than .1 gpm of low, you're likely to locally cool all the way down to 212, the metal. So that you can have liquid that's making it all the way out onto the top of the head. So there's going to be a significant amount of boron deposits that are going to be wetted by that liquid that's coming out, and it's going to be colder.

There are liquids that are going to exist over a certain area, and that is going to be similar to situations that we're seeing at plants in the past that had large leaks from up above sources that led to lots of deposits and wetting the top of the head where up to about a half inch of material loss has been seen in the past.

And some plants in Europe have also observed this with large leaks. So it may not matter so much where the leak source is coming from one you have a large leak here, that you can wet the top surface of the head, and you could have corrosion possibly occurring from the top, from the top top.

CO-CHAIRMAN FORD: But my point is right this instant in time. Obviously there's more work that has to be done, but right at this instant of time, if there's any ability to your logic in that diagram --

MR. WHITE: Right.

CO-CHAIRMAN FORD: -- you'd better change your tech specs.

MR. WHITE: Well, what we're --

MS. KING: If you're depending upon leak detection only.

CO-CHAIRMAN FORD: Correct.

MS. KING: If you're looking at leak rate only, and what we're saying here is we expect there to be significant visible evidence during a visual examination of your head.

CO-CHAIRMAN FORD: Okay.

MR. WHITE: Yeah, it's important to put this in the context of a time frame.

CO-CHAIRMAN FORD: Right.

MR. WHITE: And based on the Davis-Besse root cause analysis report work, it's believed that the high corrosion rates were occurring for four years, the last four years, roughly an inch and a half of corrosion rate per year occurring.

But before that, it's believed that another four to six years, and of course, it's not possible based on all of our information to nail down the exact time progression, but it's believed there were another four to six years of leakage that was occurring.

So it makes sense that as those cracks grew and you had more crack opening area along that crack, that you would get the higher leak rate. So there still would have been the four to six years to be able to detect something similar to or larger than the amount of the deposits that were seen at other plants.

MEMBER SHACK: Just coming back to that then, in the four years you've got now a one inch crack above the nozzle, and if you go back four years, how big is the crack when you're getting significant of the low alloy steel?

MR. WHITE: Well, it's something that

could be looked at.

MEMBER SHACK: Well, you know, presumably with this getting -- you know, as I look at your mechanism, I keep coming up to some critical leak rate, which means I need a critical axial crack size, which is far less than any structural limit, and your argument would seem to tell me, you know, one inch minus four years worth of crack growth.

MR. WHITE: Well, for this particular crack at nozzle number three, the evidence indicates led to a leak rate on the order of .1 gpm. It was about an inch above the top of the weld.

Typical growth rates argue for about a millimeter per year to perhaps up to five millimeters per year, perhaps slightly higher based upon the French experiment.

MEMBER SHACK: Yeah, I would say a five millimeter crack.

MR. WHITE: Well, that would say that we went up to about 25 millimeters at five millimeters per year, it would have taken five years to get to that point.

MEMBER SHACK: But significant attack then starts with the five millimeter through wall crack is what you're arguing.

MR. RICCARDELLA: This is Pete Riccardella.

Bill, you know, that crack could have been there for a while though because remember as the crack is growing, it's growing out of the residual stress field and probably slowing down in that axial direction. I don't think a linear assumption on crack growth is fair.

MR. HUNT: Let me just add one other point here. We do have a number of other nozzles in other plants that have cracks just under one inch that seem to be consistent with the lower leak rates over on the left-hand side of the chart, and one of the things that we're looking at from a finite element standpoint is, you know, where the transition occurs in this flow rate.

Steve Hunt, Dominion Engineering.

CO-CHAIRMAN FORD: Could I just in terms of managing time here? I know it's not fair to you. Could you try and finish by quarter to two?

MR. WHITE: Sure.

CO-CHAIRMAN FORD: The other presentation also, the main purpose being to just let everybody around this table know what the concerns are, what you're doing to resolve those concerns.

CO-CHAIRMAN SIEBER: Let me just add one word about the applicability of leak rate tech specs to this situation. If you look at the reactor coolant system, there's a lot of places where it can leak a little bit, and that's through interconnecting valves, through other systems, through safety valves, PRVs, pump seals and so forth, and generally speaking leak rates like you're talking about on the head are very small compared to some of these others.

This chart, which we haven't discussed yet is the one that shows weak rate versus time. If you go back three cycles and you look at the leak rates in those early -- the first two cycles, the leak rate is very low, which is pretty much typical of PWRs, but it's probably enough to support the fact that you might have had crevice leakage and annulus leakage of this nozzle.

So just to clarify that. That's not the only reason for the leak rate tech spec.

MS. KING: I think we discussed these.

MR. WHITE: Yeah, the next slides go over the basic -- this one might be worth touching on here. Again, this is outlining the idea that was put forth by the Davis-Besse root cause team as one possibility, that the material loss for nozzle three occurred more from a top down type mechanism.

Perhaps these mechanisms like galvanic erosion led to some growth down deep in the annulus, deeper than -- greater material loss deep in the annulus than at the outside, but that once the leak rate reached that .1 gpm, then water would reach all the way to the top, and it would have been a top-down type mechanism where you have the aerated concentrated boric acid corrosion.

As the corrosion would have moved downward, then the surface area covered by liquid would be less because now you would be going more into a pool geometry. So this might explain the change in slope at the outside of the cavity.

In other words, the area at the outside of the head is greater than the area as you move down.

And then also what might produce the shape of the cavity, the oblong shape, it being longer in the downhill direction than the transverse direction?

Well, gravity would have displaced that pool, the initial pool on top of the head in the downhill direction, and as you move down, that could explain the shape.

CO-CHAIRMAN FORD: So you're still having no significant velocities in that pool?

MR. WHITE: Well, once things are opened up, velocities are not going to be that high.

CO-CHAIRMAN FORD: So this isn't a solution mining type of thing where you go down there and dissolve the rock with a jet of liquid? It looks like it. I mean if you had a jet coming out of that crack, it would make a cavity something like what was observed, I would think.

MR. WHITE: I'm not completely discounting the erosion type mechanisms. It is a possibility, but we know, therefore, the aerated boric acid, concentrated boric acid conditions, you can have the high corrosion rates.

So it seems consistent that that would have been the primary mechanism when you got there.

CO-CHAIRMAN FORD: Okay. You're about to discuss those two tests. Just let me make sure we know factually where we are right now. Right now you come up with a series of hypotheses, qualitative hypotheses enunciating the conjoint requirements to get the temperature in the annulus down to a lower value necessary to sustain high crack growth rates, and you're relating that to leak rates from a practical point of view.

Are there any tests planned for the near term to qualify that hypothesis?

MR. WHITE: Well, there have been discussions initiated between the MRP and NRC Research as to what tests could be performed, and so we're in discussions with the industry and with the NRC about --

CO-CHAIRMAN FORD: And how urgent are those?

MS. KING: We would expect this work to identify the appropriate tests to go perform, and then we would take immediate action.

CO-CHAIRMAN FORD: Immediate being tomorrow.

MS. KING: Well, he needs to finish first.

CO-CHAIRMAN FORD: Well, I recognize that, but I still say it with some -- quickly.

MS. KING: Quickly, yes. I don't plan to wait until 2005. As soon as we can identify what the appropriate tests would be, we would immediately start to pursue --

CO-CHAIRMAN FORD: And to fill hydraulic analyses. There are obviously a lot of questions on thermal hydraulics in that crevice and how they change with operating conditions, fit up and shrinkage, ovality (phonetic), and the dimensions of annulus as they change in time. All of them will be addressed.

MR. WHITE: If we go to these slides here, we can go over briefly what's been done in the past. This slide just touches on different types of boric acid corrosion tests, but here we see some of what the mock-ups look like for testing that was done sponsored by EPRI back in the '96-'97 time frame with different leak rates simulating an annulus geometry with leakage, and if we could go to the next slide, here's a specimen, one of the specimens from one of the six tests. This was a leak rate of .01 gpm. The actual injection point is here along this hole here. This is a thermal couple probe area here.

But the flow came through here, and then impacted on a stainless steel tube that was inside this hole, and so you can see some of the corrosion that occurred in this test and how it's deeper down in the annulus.

CO-CHAIRMAN FORD: The erosion was not on the impact point at this -- the back side of the impact point.

MR. WHITE: Well, right. Here the flow is coming through --

MEMBER ROSEN: Remember the impact is on the stainless steel tube in this case.

CO-CHAIRMAN FORD: Oh, okay, okay.

MR. WHITE: Here's an example of one test that was performed in the past. Future tests may want to look at the development of the corrosion rate versus time, and there are techniques to try to capture that as a function of time.

They could try to quantify the environment carefully in terms of the temperature along the leak path, in terms of the chemical composition along the leak path and so on., the electrochemical potential.

So that's one area of potential testing. Other areas would go to the properties of molten boric acid, its potential for being corrosive, for looking at the galvanic mechanism.

In this test, one could decouple the stainless steel tube from the low alloy steal, electrically isolate them and see if that was a major factor in terms of the amount of corrosion that would indicate galvanic mechanism here.

This slide here just summarizes that those tests that were performed in the '96-'97 time frame, along with tests that were performed in the late '80s by Combustion Engineering of the pressurizer nozzle geometry, an inverted geometry so that the nozzle was facing down rather than up out of the low alloy steel.

Those tests both produce similar corrosion rates, two to two and a half inches per year, and one key thing form this testing was that for leak rates greater than .01 gpm, as the leak rate was increased, actually the corrosion rate decreased, and the belief is that that tends to indicate a corrosion type mechanism rather than a flow erosion type mechanism and the reason is believed to be that the higher flow rates would flush out the impurities, and you'd get actually a lower boric acid concentration because you'd be with greater flow flushing out the crevice.

These tests had interference or I should say gaps of about 5/1000 of an inch radially, which is larger than the initial fit-ups for the CRDM nozzles, but would be representative after that CRDM nozzle annulus opened up over some time.

MEMBER SHACK: I'm sorry. In the CE test, is that another one where the jet impacts the tube or, no, the flow is coming --

MR. WHITE: That one they actually had a crack in a steam generator tube and let the flow come from inside the steam generator tube and then go into the annulus and then down, and that test, although it had a similar material loss rate, the location of maximum corrosion was at the outside, at the exposed surface down at the bottom.

MEMBER SHACK: But at least that one you did have a potential for erosion of the low allow steel.

MR. WHITE: But the material loss didn't happen up there.

MEMBER WALLIS: Well, this two inches per year is for these particular tests.

MR. WHITE: Right.

MEMBER WALLIS: And I don't know that it's being predicted theoretically.

MR. WHITE: No.

MEMBER WALLIS: So there's no reason to suppose that two inches per year in this test is the same as what you'd get in the reactor situation where flow rates and commissions are not quite the same and the geometry isn't quite the same.

MR. WHITE: Also, I think an important factor is the amount of cooling that you get because obviously it's difficult to mock up the way the reactor heats the head with that large heat source. In these tests, there are cartridge heaters that may be used to do the heating so that the amount -- the temperature drop, the local temperature along the leak path could be much different.

MEMBER WALLIS: So you mean in the absence of a predictive method based on physics and chemistry. I don't quite know what to do with two inches a year from these tests. It could be ten inches a year or .2 inches a year in the reactor for similar conditions because they're not going to scale it to the reactor condition, unless I have some sort of a physical model.

MR. WHITE: Well, the objective of the test was to try to simulate typical conditions as much as possible.

CO-CHAIRMAN FORD: I think the main point here, Graham, is that they've done some preliminary hypothetical work and come up with a potential progression of events, and there's an urgent need to do some confirmatory analyses and tests. I think that's a fair --

MEMBER WALLIS: You're not going to be confirming anything. You're going to be investigating and --

CO-CHAIRMAN FORD: Well, confirming the hypotheses and coming out with a prediction of what happened in the actual plant.

MEMBER WALLIS: That's a long way to go.

MR. WHITE: But so far everything is

consistent with the experience, I'd say. The majority of the leaks we've had on the order of 35 leaking nozzles in the U.S. The large majority of them have had small amounts of boric acid and no measurable wastage or very small amounts of wastage.

That's consistent with not having a lot of liquid in the annulus with having the temperature close to the primary temperature with the low velocities, and we've had the one case, much larger in comparison, where the calculations showed that liquid could make it all the way out into the top of the head, and that's the case where we had the large corrosion.

MEMBER WALLIS: As soon as the inspection shows rivers running down the head instead of just crystals coming out of the ground.

MR. WHITE: Right.

MEMBER WALLIS: That indicates there's liquid up there, doesn't it?

I've seen all sorts of photographs. I've seen these little popcorn around.

MR. WHITE: Right.

MEMBER WALLIS: And I've seen the popcorn with some sort of a river down below it. That indicates that there's liquid. As soon as you see something flowing down from the --

MS. KING: Well, but that doesn't necessarily mean -- in those photographs it does not necessarily mean that the liquid -- the source was the PWSCC crack. It potentially could have come from --

MEMBER WALLIS: What it means is that it was wet boric acid.

MR. WHITE: Also boric acid, you know, melts at 340 degrees, becomes molten. It's very hydroscopic. So it like to pick up whatever moisture is in the air.

MEMBER WALLIS: It dries up as it --

MR. WHITE: So the appearance of deposits and the morphology of them is going to change as the plant cools down.

CO-CHAIRMAN SIEBER: My impression was that the so-called lava flows were mostly iron oxide and molten boric acid because it was hot enough that the liquid containment pressure, that would vaporize right away, and you would end up with crystals which would then melt and form these rivers.

That's at least my first impression of what I saw. Could you comment on whether that was correct or not?

MS. KING: Well, I guess I could comment.

The boric acid crystals initially early on in the videos were brittle, and you could see that as they were being cleaned off the head surface, and it was verbally reported later that they got --

CO-CHAIRMAN SIEBER: Chunks.

MS. KING: -- very hard and very difficult to remove, which would go towards the molten boric acid.

CO-CHAIRMAN FORD: Could I suggest that we call a halt at this point?

MS. KING: Sure.

CO-CHAIRMAN FORD: Before we do that, could I ask just the staff? I know there's a cracking action plan to deal with the cracking issues. Is there going to be an associated degradation action plan?

They talked about in talking to you and talked about the appropriate tests that they want to do to validate these hypotheses. Is there an NRR action plan associated with that?

MR. WHITE: Not specifically at this time. We've asked Research to give some idea of what we'd be looking at with respect to if we wanted to build a mock-up and run a test or something like that, what it would conceptually cost us with respect to dollars, et cetera.

I don't know if there's anybody here from Research. Bill, have you had a chance to look into that at all?

MR. CULLEN: Bill Cullen, Office of Research.

The panel has asked a few times this morning about research that might be considered going forward. Peter himself has specifically asked twice what's going to happen, and just a few minutes ago Christine indicated that on the industry size there's research that's being proposed.

I have received, as you can imagine in the last three months all kinds of proposals from all kinds of people who have considered themselves to be experts in boric acid corrosion. I'm currently going through those things trying to figure out what's good, what's bad, and what would be helpful.

Additionally, as Bill Bateman has just indicated, NRR themselves has asked Research to come up with a plan.

So combining all of those requests and all of that input, I am I would say nearing the end of the line on deciding what it is that we're going to do. Even as we speak there's some proposed funding documents that are circulating in the next building. So we will, I think, know within a very short time how much funding could be made available for Office of Research sponsored funding to look into some of these issues dealing not only with the corrosion aspects of this, but also some nondestructive inspection programs that might, you know, when they get implemented within the industry, serve to help find this sort of situation or this sort of degradation long before it gets as far as it did get in the Davis-Besse.

Also have some other plans about I would like to maybe do some sensor development because I think we've talked about the right element monitors and the containment air coolers being credit up (phonetic), going along that line or down that road.

I think there's some sensor developments, some instrumentation development that could be undertaken that would also help.

CO-CHAIRMAN FORD: Will that be covered in part by Ed? Are you going to be talking later on on the inspection?

MR. CULLEN: It's not my position to say, but Ed Hackett will not be presenting today. Mark Kirk will be presenting a little later on, PFM.

MEMBER BONACA: Since we are on the

subject of inspection, just one thing that is a no brainer, doesn't need the research. Two things actually.

One, are we going to make some criteria on what the licensee has to do when he finds he has flanges leaking?

I mean one of the problems at Davis-Besse is that they manage the leaks. They only fix some, and then they decided to put off until the next outage some.

On deposits the same thing happened. They simply had a certain time allotted for removing deposits. They removed what they could at the time, and then they just started again without removing all deposits.

Are we going to establish some criteria for this? It's not only inspections. It's what we're going to do with what we find after we inspect.

It seems to me that as a minimum one would expect that if you find leakage you fix the flanges before you restate. That's become a priority.

MR. BATEMAN: Yeah.

MEMBER BONACA: Also it would be the removal of boric acid deposits.

MR. BATEMAN: Yes, you're right. If

licensees do find leakage at their flanges, they do repair the leakage before they restate.

We're going to talk a little bit later this afternoon on the inspection plan methods and frequency. So I think that will probably address the other --

MEMBER BONACA: Because, you know, those things, I mean, are no brainers. You don't need research for that. If you do that, you're going to find where the problem is. You know, you're not going to be stumbling and propagate the problem, cascade from cycle to cycle as it happened at Davis-Besse.

I would expect that that would be a requirement that it would be very reasonable, in fact.

MR. BATEMAN: Yeah.

MEMBER BONACA: If you had accumulation of pounds of boric acid crystals on top of the head, I think the requirement should be remove them all. You don't restart until you've done that.

MR. BATEMAN: Yeah.

MEMBER BONACA: I think any licensee who can think with his own head in this situation would do that.

MR. BATEMAN: From the results of Bulletin 2002-01 inspections, I think we feel at this point that Davis-Besse was an anomaly, and I think maybe we have a tendency to try and apply that to everybody else, but we haven't discovered anybody else in the industry who has come anywhere near close to having the boric acid accumulations on their head that Davis-Besse had.

MEMBER BONACA: But you understand the requirements I'm discussing here are reasonable actions.

MR. BATEMAN: Absolutely, absolutely.

MEMBER BONACA: And those could have prevented so much of this pain and attempt on our part now to try to foresee the future. It's going to be very hard to do anyway, but I think fundamentally some basic ground rules, and I would call them almost housekeeping for a plant.

MR. BATEMAN: I think it's basically more of an implementation issue. Licenses have a boric acid inspection corrosion program based on their response to generic letter 8805, and we found with the exception of one licensee that they're implementing it.

I think it was more of an implementation problem as opposed to a program problem. I think the programs are out there. It's how well are they implemented.

CO-CHAIRMAN FORD: Could I ask that we move on at this stage?

And, Larry, could I ask the two of you to do both of your presentations by quarter to, finish them by quarter to three, at which point we'll take a break?

MR. MATHEWS: I really don't have much to say. We could skip the collateral -- it's just two slides.

CO-CHAIRMAN FORD: All right.

MR. MATHEWS: We really haven't done anything since last time.

CO-CHAIRMAN FORD: Okay.

MR. MATHEWS: We can come back later and talk about it.

CO-CHAIRMAN FORD: If the rest of the committee -- if it's okay with them, we will skip Larry Mathews on collateral damage. There's not a lot that has been done since then, since the last time we met.

Pete.

MR. RICCARDELLA: Okay. I'm Pete Riccardella from Structural Integrity Associates.

We were contracted back about September of last year to develop a probablistic fraction mechanic's model for this top head degradation and cracking issue, and the focus of that model is primarily looking at probabilities of the growth of large circumferential cracks and nozzle ejection.

What I plan to present today is somewhat of an overview --

MEMBER APOSTOLAKIS: Excuse me. Where are these slides?

MS. KING: These slides follow the crack growth rate.

MS. WESTON: Page 46 on this handout.

MS. KING: There you.

MS. WESTON: And they start on page 59 in the book.

MR. RICCARDELLA: Are we set?

My purpose today is to present an overview of this model, and I should say that in the period of time since September, we a have had several meetings with the NRC staff. There's been several interactions, both teleconferences and meetings where we've discussed, traded ideas on the methodology.

And in fact, we've gone back and made some modifications, adaptations to the model based on NRC staff input.

So first I'll give an overview of the methodology and then talk about some PFM analyses that we've performed in support of the propose MRP inspection plan.

The key elements of our probablistic fracture mechanics model are listed on this slide. We have an experiential based probability of leakage model. We don't try to model the initial nucleation and growth of the crack. We're basically just going back and looking at based on experience probability of leakage versus time, and then we take it from that point.

We have a fracture mechanics model for stress intensity factor in which we've considered both part through wall and through wall cracks in different nozzles, different locations on the head, different places on the hillside.

But the assumption in our fracture mechanics modeling is, I believe, a conservative one, is that once we detect a leak, we assume that we instantaneously have an axial crack which has branched and turned to a circumferential crack and is already 30 degrees of the circumference.

So that's the starting point for our analysis. We've compared that to looking at a leak and then getting multiple initiations, reinitiating new cracks around the periphery, and we believe that our model instantaneously assuming a 30 degree circumferential crack is both conservative and less arbitrary than you get by trying to model these multiple reinitiations of circ. cracks.

It also agrees at least with the anecdotal evidence of how the circ. cracks developed at the Oconee plant, that they tended to be more like axial cracks that branch into circ. cracks rather than reinitiated circ. cracks.

MEMBER APOSTOLAKIS: Where does the work that was presented earlier on the rate of growth of cracks fit into this?

MR. RICCARDELLA: We have the next slide a key. A key element of our model is the statistics of crack growth, and I'm going to show you how I've used the work that John --

MEMBER APOSTOLAKIS: I'm a little confused by the first bullet there.

MR. RICCARDELLA: Okay. Well --

MEMBER APOSTOLAKIS: What does it mean, that you're already having a leakage?

MR. RICCARDELLA: Well, what we're assuming is that at a certain period of time we have a leak.

MEMBER APOSTOLAKIS: Right.

MR. RICCARDELLA: And then when we have a leak, we assume that at that point in time, we have an axial crack that branches into a 30 degree of circumference circ. crack, and then we use the crack growth to analyze the progression of that 30 degree of circumference crack out to failure, out to 300 or 330 degrees, whatever it is that produces ejection of the nozzle. Okay?

So we are arbitrarily giving up that initial nucleation and growth portion of it.

MEMBER APOSTOLAKIS: Which could have included the crack growth rate again, right?

MR. RICCARDELLA: Some amount of crack growth to get to 30 degrees, but we're kind of giving that away.

MEMBER APOSTOLAKIS: All right.

MR. RICCARDELLA: And we're saying we start there basically.

MEMBER SHACK: Well, you start there, but you take the Weibull model into account.

MR. RICCARDELLA: Yeah.

MEMBER SHACK: So I mean, you accounted for it in a different way.

MR. RICCARDELLA: Exactly.

And then finally in our model we have the ability to look at the effect of inspections on this cracking and on the probability of an ejection. We can look at different inspection intervals, as well as different levels of reliability for different types of inspections, and we have some assumptions we've made regarding the probability of detection.

If you have a leak and you do an inspection, what's the probability of detecting that? Also the probability of detection for ultrasonic or other --

MEMBER APOSTOLAKIS: So where does that go?

MR. RICCARDELLA: Huh?

MEMBER APOSTOLAKIS: The probability of detection?

MR. RICCARDELLA: Down here, effective inspections.

MEMBER APOSTOLAKIS: And you also have a model for the probability of detecting and doing nothing about it?

MR. RICCARDELLA: No, no. The assumption is that --

MS. KING: Do you mean no inspection?

MEMBER APOSTOLAKIS: I know it is there --

MR. RICCARDELLA: -- if it's detected you fix it.

MEMBER APOSTOLAKIS: -- but I don't have time to do anything. I'll work as best as I can.

MS. KING: Essentially I think what you're saying is the effect of not completing, not completing it.

MEMBER APOSTOLAKIS: Not doing anything about it.

MS. KING: Yes. We can take this model and do no inspections, and you can see what the --

MR. RICCARDELLA: Well, no, not fix it. He's saying you detect it and you find it and you don't do anything.

MS. KING: Oh.

MR. RICCARDELLA: We can conservative -- let's just say we'll conservatively bound that in our probability of detection because we're using some pretty low numbers for probability of detection.

CO-CHAIRMAN SIEBER: Let me ask a question about that a little bit. I've been thinking about it since I'm the ultimate determinist. If you find a crack in the through wall or greater than 40 percent and it's not at a mechanical joint, okay, you know, a bolted joint, the code applies to that, does it not? And the code says you've got to repair it.

MR. RICCARDELLA: Oh, yes.

CO-CHAIRMAN SIEBER: Otherwise you're in violation of the boiler and pressure vessel code; is that correct?

MR. RICCARDELLA: Yes.

CO-CHAIRMAN SIEBER: And so everything you find that is greater than 40 percent has to be repaired, right? Is that true?

MR. RICCARDELLA: I think it's 75 percent.

MS. KING: Yes.

MR. RICCARDELLA: It's more like 75 percent than 40.

MEMBER SHACK: I'm not sure there's an exact code section that applies.

CO-CHAIRMAN SIEBER: Well, you can calculate how much margin you have.

MEMBER SHACK: Yeah, and in fact, there was a memo from Jack Strosnider to the MRP that says, "Here's an acceptable set of acceptance criteria until we figure out what's the right thing to do."

CO-CHAIRMAN SIEBER: But a through wall crack you have to repair.

MEMBER SHACK: Yeah, if it's leaking,

yeah.

MS. KING: Actually the flaw acceptance criteria is related to cracks that intersect the pressure boundary at specific depths and the location of that crack.

MEMBER LEITCH: Ninety-five percent limit after the next operating period.

MS. KING: Right.

MR. RICCARDELLA: But the assumption in a probablistic model is that if you inspect and find a crack, you fix it. We take it out of the population as far as possibly proceeding to a nozzle ejection.

MEMBER APOSTOLAKIS: But, I mean, how real is that? I mean, why are we doing it? Is somebody else dealing with the issue of if you find it, you decide to do nothing about it, you know?

MR. RICCARDELLA: No. I think the inspection plan and the code tell you what you have to do if you find it.

MEMBER APOSTOLAKIS: But if the code is implemented correctly, I expect the probabilities to be very low. That doesn't tell me anything. I mean, the finding is the boric acid corrosion control program at the site included both cleaning and inspection requirements, but it was not effectively implemented.

Now, to tell me that, you know, I believe that they will find it and do something about it doesn't address this issue.

CO-CHAIRMAN SIEBER: Well, one of the problems --

MEMBER APOSTOLAKIS: Now, that's not fracture mechanics, but that's the issue.

CO-CHAIRMAN SIEBER: One of the problems with visual inspection is you're beyond 70 percent. You're through wall, and then if you have, for example, the CRD in flange, which I think is a bolted joint, right? We had welded joints, but you know, those by code can leak. Okay?

So the issue is can you do a visual inspection with all of this boric acid that leaked down laying on top of the head, okay, and if it's there, what do you do about it?

I think that's part of the issue, which again tells me that sooner or later you have to go to a volumetric kind of inspection to be able to satisfy the requirements of the code.

MR. RICCARDELLA: Yeah, I think we might be able to address the question a little better if I get a little further into the presentation and talk about exactly how we're using the probablistic fraction.

MEMBER APOSTOLAKIS: Now, on this slide it says theta. What is the expression for the Weibull?

MR. RICCARDELLA: What is the expression?

MEMBER APOSTOLAKIS: Yeah. Do you have the mathematics of it so that I know what theta is?

MR. RICCARDELLA: Give me the one with the curve. I have an actual curve of the Weibull. Okay? This Weibull paper, it's a standard, two parameter. I'm sorry I can't quote it off the top of my head. Perhaps Glenn can bail me out and give me the expression.

It's a standard two parameter Weibull.

MEMBER APOSTOLAKIS: Yeah, but it has several different expressions.

MEMBER SHACK: Theta is like the mean value.

MEMBER APOSTOLAKIS: Well, but it's not.

MEMBER SHACK: It's not, but it's like.

MR. RICCARDELLA: Theta is like the 63 percent cracking, and there's a function of service. Now let's go back to the previous slide.

We have a Weibull analysis that was actually developed by Dominion Engineering based on all of the B&W plants and the cracking that's been experienced in those, and we made the assumption of a Weibull slope of three.

Actually, Christine, if I could go to the next slide, if you look at the actual data, you would actually predict if you just did a pure analysis of the data, you would predict a much steeper Weibull slope, like about a Weibull slope of nine. That's the curve that --

MEMBER WALLIS: Once they get to 20 they're -- oh.

MR. RICCARDELLA: Pardon me?

MEMBER WALLIS: Once they get to 20 it's --

MR. RICCARDELLA: Pretty much, yeah, but we believe that there is something else going on here, that there is somewhat of an inspection transient going on and that some of these were leaking earlier in time, but we didn't start doing inspections until pretty late.

And so --

MEMBER SHACK: Except the Oconee 3.

MEMBER WALLIS: Still it's cumulative.

MR. RICCARDELLA: That's true, that's true. Well, except if I use a steeper slope, I'd get a less conservative result. So we're using the slope of three.

MEMBER SHACK: Steep slope is sort of good news. So it's good news and bad news, but your header, you really meant the slope is three, not theta.

MR. RICCARDELLA: Yes, you're right.

MEMBER SHACK: You've totally confused us here.

MEMBER WALLIS: Weibull analysis is a prediction of these lines. Is that what it is?

MEMBER APOSTOLAKIS: They assume the functional form. That's what it means, Weibull analysis.

MR. RICCARDELLA: Yeah, well, this is actually a Weibull paper. So, you know, the shape of the equation is built into it.

MEMBER APOSTOLAKIS: How many parameters?

MR. RICCARDELLA: Two parameter Weibull.

MEMBER APOSTOLAKIS: Two parameter. And then by fixing the slope, essentially you end up with one parameter.

MR. RICCARDELLA: One parameter. That's right.

MEMBER APOSTOLAKIS: Now, can you tell me what the cumulative fraction of number of leaking CRDM nozzles is? What does that mean?

MEMBER WALLIS: Just what's the fraction of the number of the leak. If there are three out of 100 or --

MEMBER APOSTOLAKIS: So if I have 150 of them, this will tell me the fraction of them that are leaking?

MR. RICCARDELLA: Yeah, Oconee 3 had this fraction leaking, and then the next inspection they had that fraction leaking in that plant.

MEMBER APOSTOLAKIS: So this is between inspections?

MR. RICCARDELLA: Between inspections? There's only one plant on here that's inspected twice, and this is the time period between inspections, from here to here. The others are just until the first inspection. That's the fraction of nozzles that were found. So there --

MEMBER SHACK: With two inspections he can calculate both of the parameters. With one inspection, he has to assume one.

MEMBER APOSTOLAKIS: So let me understand what you just said. The other plants are not inspecting at all?

MR. RICCARDELLA: No. This is just -- this Weibull analysis is just based on the B&W type plants.

MEMBER APOSTOLAKIS: Right.

MR. RICCARDELLA: Because they've had seven out of seven leakers. The others that have been inspected, most of them have had non-leakers. There have only been two other plants that had leakers, and they're not shown on here, but they fit very well into this group of data. That Weibull fit fits all nine of the plants that have leaked, that have had leaks fairly well.

And, in fact, at the time this chart was produced, the Davis-Besse hadn't been inspected yet, and the mean prediction was here, and when we actually did the inspection, they came out -- the point falls very, very close to that.

MEMBER APOSTOLAKIS: But, I mean, this is the result of some inspection scheme, isn't it? So what was that inspection scheme? How often do they inspect these things?

I'm trying to understand that.

MS. KING: This data came from the Bulletin 0101 inspections.

MEMBER APOSTOLAKIS: Yeah, and? And?

Don't assume that I know.

MR. RICCARDELLA: It was just the first inspection of a large number of plants, and what we did was, you know, based on a lot of data from stress corrosion cracking behavior, this type of material, we concluded that the largest slope that we'd expect to see is three, and so we fit the data with a slope of three.

As I said, if you did a pure fit of the data to solve for both slope and theta, you'd end up with a much steeper slope.

MEMBER WALLIS: You don't put in any data at all really.

MR. RICCARDELLA: Pardon me?

MEMBER WALLIS: You're just drawing some lines. You're not fitting any data.

MR. RICCARDELLA: No, not really. No, we're just saying where does the slope of three best fit between that group, that group of data.

And then what we did is we had an upper bound and a lower bound, and in our Monte Carlo modeling we assumed a mean and then a variation about that mean, and you know, we assume a Weibull slope if you go back to the previous --

MEMBER APOSTOLAKIS: So these are --

MR. RICCARDELLA: -- 15 with a -- nine is the worst case and a 21 is the best case.

MEMBER APOSTOLAKIS: And these are the fifth and 95th percentiles? In the Monte Carlo simulation, how will these --

MR. RICCARDELLA: They're triangular actually. We're using a triangular distribution for this particular --

MEMBER APOSTOLAKIS: So these are 100 percent?

MR. RICCARDELLA: Yeah, 100 percent and zero percent.

MEMBER SHACK: Now, Peter Scott with a much larger database to work with comes out with a 1.5 slope, which is in your case more conservative. So your three isn't conservative.

MEMBER APOSTOLAKIS: Where was this information?

MEMBER SHACK: When you do this kind of analysis for the French plants where you actually have a larger set of data so that you don't have to assume the slope, you get a number of 1.5 instead of three.

MEMBER APOSTOLAKIS: So the curve then -- the straight line would be very almost horizontal.

MEMBER SHACK: Well, much closer to --

MR. RICCARDELLA: It would be shallower like that, yeah.

MEMBER SHACK: Which is bad because you get earlier initiation.

MR. WHITE: Bill, can I address that?

Dominion Engineering did the work of determining that three was the appropriate slope to use. One major source is MRP Report 66, which just came out earlier this year. In that work, the investigators looked at a large set of available data mostly for crack initiation, and the best fit Weibull slope to that large set of data, and I can't remember exactly the number of data points, but this was a much larger set, I believe, than Peter Scott was working with.

And the best fit was 2.7 for the slope.

MEMBER SHACK: Was that steam generator tubes or nozzles?

MR. WHITE: It was on all available crack initiation tests for Alloy 600.

MEMBER SHACK: Okay. Because the earlier results from Dominion just looking at steam generator tubes gave numbers much closer to the 1.5.

MR. WHITE: Well, we've also --

MEMBER SHACK: Original Gorman, you know, reports.

MR. WHITE: One of the presentations that I made at the meeting on May 22nd with the NRC went over this in a little more detail, but we looked at the available data, and there is a range that's observed, but if you look at that, there are for some steam generator field experience in some locations in the tubes higher PWSCC slopes than others, and three seems to be appropriate based on that also.

MEMBER SHACK: Well, in the steam generator, it's always conservative to take the higher slope because that's predicting lots of tubes to fail.

MR. WHITE: I'm just talking about actual observed ranges of slopes for role transition PWSCC for various locations, U bands, for example, and I can show you that data if you'd like.

MEMBER SHACK: And compare with my data.

MR. RICCARDELLA: I think we could do the analysis with a slope of 1.5. I don't think it would have a huge effect on the probablistic fraction mechanics results. I've done it for nine and three, and I've found that that was worth maybe about a factor of two. You saw that.

And I think the difference between three and one and a half would be even less of an effect than that.

And considering that I benchmarked it against Oconee, it really wouldn't make that much of a different. Okay?

MEMBER APOSTOLAKIS: Now, you're going to use this for future?

MR. RICCARDELLA: Yeah, we

re using this in our Monte Carlo analysis to create --

MEMBER APOSTOLAKIS: Right, but the inspections will have some sort of a period in the future?

MR. RICCARDELLA: Yeah, we can assume various inspection intervals and --

MEMBER APOSTOLAKIS: So why does this apply?

MR. RICCARDELLA: Well, under the assumption of no inspection. We have a time based Monte Carlo analysis that we start at zero, and we say at so many years we would predict this many leak, and a couple of years later we would predict this many leaks. So we can do a complete analysis with no inspections, and then we can come back and superimpose inspections on top of that and see what the impact is on those results of different inspection intervals.

Okay. So that's just the starting point.

So here is, for example, for a 600 degree plant, beta equal to three, theta 15 plus or minus six. These are the assumed times. So if we're at ten years and we're just the mean case in this particular case, that would be about a 25 percent probability of leakage. If we're out at 16 years, that's a 60 percent probability of leakage, but we vary it between these extremes in the Monte Carlo modeling

So that's the very starting point of the analysis.

MEMBER APOSTOLAKIS: The triangular distribution you mentioned.

MR. RICCARDELLA: Yes, it's between these extremes.

MEMBER APOSTOLAKIS: In the vertical sense or the horizontal sense?

MR. RICCARDELLA: Yes, in the vertical sense.

MEMBER APOSTOLAKIS: In other words, ten effective years has a probability anywhere between .1 and .7.

MR. RICCARDELLA: Yes, and incidentally, this is the probability of first leak in a head of a certain number of nozzles. In other words, it's not the probability of a leaking nozzle for any individual nozzle. It's the binomial probability given 69 nozzles that you'll have at least one leak.

MEMBER APOSTOLAKIS: Oh, so this probability on the left is not the previous probability?

MR. RICCARDELLA: Well --

MEMBER APOSTOLAKIS: The previous was accumulative.

MR. RICCARDELLA: Yeah.

MEMBER APOSTOLAKIS: Now it's something else.

MR. RICCARDELLA: The direct relationship, if you go back --

MEMBER APOSTOLAKIS: Why is that still viable? It's not viable anymore, is it?

MR. RICCARDELLA: Yes, it is. In fact, it is, and the relationship is theta for a leak in a nozzle is equal to -- I'm sorry. Theta for the first leak is equal to theta for a leaking nozzle divided by the beta root of N, where N is the number of nozzles. There's a direct relationship between theta. The slope stays the same, and there's a direct relationship between theta for that given leak.

MEMBER APOSTOLAKIS: Do you have that derivation someplace?

MR. RICCARDELLA: Yeah. Yes, we do.

MEMBER APOSTOLAKIS: And we can look at it?

MR. RICCARDELLA: You certainly can. I have it on my laptop.

MEMBER APOSTOLAKIS: Mag, can you please get that document so we can look at ti?

MS. WESTON: I'm sorry. What is it you want, George?

MEMBER APOSTOLAKIS: The document.

MR. RICCARDELLA: Derivation of the relationship between theta for first leak and theta for nozzle leakage in general.

MS. WESTON: And where do I find it?

MS. KING: On his laptop.

MR. RICCARDELLA: We'll give it to you.

MS. WESTON: Is it in your original presentation that you did to the staff?

MR. RICCARDELLA: No.

MS. KING: No, we haven't had this question yet.

MEMBER APOSTOLAKIS: This is the first question that you get that you haven't had before?

MS. KING: For the derivation. No one has asked to see the derivation yet.

MR. RICCARDELLA: You know, that just gets us to the point where we assume a leaking nozzle. Then we have to say, "Okay. How do we evaluate a nozzle growing from this assumed condition at leakage, which is the 30 degree crack, to a large circ. crack that could potentially lead to ejection?"

And so we've developed a series of finite element models with cracks of different sizes and different depths. This is the model we use for a through wall crack, 180 degrees, and this is a crack that is assumed to initiate on the up hill side of a hillside penetration.

MEMBER APOSTOLAKIS: Again, I'm a little slow here. You said that basically you use a binomial distribution there to get with the expression for the first leak.

MR. RICCARDELLA: Yes.

MEMBER APOSTOLAKIS: Now, what happened at Davis-Besse, as I recall, was that there were three nozzles that were adjacent. Now, in the binomial, of course, you assume independence. So is that a valid assumption?

MR. RICCARDELLA: Yeah, if we look at the distribution over all the plants that have had leaks, it is pretty random distributed around the head.

MEMBER APOSTOLAKIS: No, but when --

MR. RICCARDELLA: Davis-Besse happened to have three, but those happen to be three that were out of the same heat of material that happened to be a particularly susceptible heat of material.

MEMBER APOSTOLAKIS: Right.

MR. RICCARDELLA: Okay? But we believe that as far as time to leakage, there's no geometric dependance at any of the nozzles at any particular location in the head. The Oconee nozzles, the ones that leaked, tended to be toward the periphery. The Davis-Besse nozzles tended to be near the top dead center.

Now, in terms of the tendency to develop large circ. cracks, we believe there is a dependence on where you are on the head, but the time to leakage, we believe, is pretty independent.

MEMBER APOSTOLAKIS: Okay.

MR. RICCARDELLA: So let's go back to that one. This is the model we use for the through wall crack initiating here, running parallel to the weld. The red Xes that you see in the bottom of the condition we applied for the J-groove weld, and here's the crack tip. You see the added refinement that we put in the mesh of the crack tip.

And this model enables us to calculate the stress intensity factor at that crack tip, and we've run this for 30 degrees up through 330 degrees, and we have K versus crack size.

We've run it for nozzles of different angles, you know, top dead center going all the way out to the hillside like this, and we also use gap elements on the back side of the crack to represent the constraint provided by the vessel wall. This is where it shrunk fit into the vessel.

Next.

We also have part through wall crack model where we consider an axial crack that is branching and turning into a circ. crack, and this is what we use for the shallower crack configurations to calculate the K.

So this is what we use to calculate the stress intensity factor K that we use in conjunction with the crack growth expressions that John Hickling presented. You have to get a stress intensity framework, and John said, well, it's typically 25 to 30 or 35. These are the models that we use to develop that stress --

MEMBER WALLIS: How independent is this model that you choose? I mean you could have chosen really different geometry, couldn't you?

MR. RICCARDELLA: Yes. Could I have the next slide?

What we've done, we've assumed a -- for most of the analysis I'm going to present now and really all that we have done right now is the B&W type plant. Okay? So we specified that type of geometry, and then we've looked at nozzles of zero, 18, 28, and 38 degree angles, and so that takes us from top dead center to the most hillside.

And in the Monte Carlo analysis, we bend the nozzles into one of these four categories. You have a nozzle for that and like every single category, and what we find -- and then we've also looked at cracks emanating from the uphill side, growing down, and then also emanating from the downhill side and growing up.

And for this particular geometry, what we found is that the uphill side is much worse and also that the stress intensity factors get higher particularly for the longer cracks as you get further away from the center.

MEMBER WALLIS: They depend upon what you are assuming about the shape of that?

MR. RICCARDELLA: No -- well, yes, they do to some extent. They depend upon that. We've made what I believe is a conservative assumption on the shape of the crack. They also depend on the residual stress, which is the size of the weld and the welding parameters, and we currently have underway analyses of a CE type head and of a Westinghouse type head so far.

MEMBER WALLIS: So with these assumptions you have to make about the crack shape and all of that stuff, what's the uncertainty in these Ks?

MR. RICCARDELLA: I would say it could be as much as a factor of two.

MEMBER WALLIS: A factor of two. So that covers pretty well the range of the data that we were looking at this morning.

MR. RICCARDELLA: No, but you'll see that when we get into the Monte Carlo modeling that the effect of the uncertainty in crack growth rate is like factors of ten to 20, and they tend to overwhelm. You know, the scatter in that lot normal or distribution overwhelms, and remember we're using --

PARTICIPANT: The variation in --

MR. RICCARDELLA: Yes, the variability. And remember there's an exponent of one. So it's pretty much a one-to-one relationship between K and crack growth rate. So I think that, you know, further sharpening the pencil on stress intensity factors isn't going to make that big an effect.

MEMBER WALLIS: -- aware of how uncertain it is.

MR. RICCARDELLA: Yeah, I would say it could be as much as a factor --

MEMBER WALLIS: Well, when you give us 26.9, it's probably anything between 20 and 35 or something like that.

MR. RICCARDELLA: We have analysis -- the highest number that we have anywhere here is this 38. We have an analysis for another plant where that's as high as 60, okay, for a different plant type, different residual stress.

And then we went ahead and did the probablistic fracture mechanics on that, and it had maybe a factor of two influence on the probability of nozzle ejection.

MEMBER SHACK: Zero angle nozzles. So I mean this is a cylinder under pressure. Pi squared over pi R squared P. Why am I not getting at least a pressure K that's going up by the time I'm getting the 300 degrees?

MR. RICCARDELLA: I'm not sure. That's something I've got to go back and check into. I think it has to do with the distribution of the street. You know, it's a through wall where you've got residual -- it's residual plus.

MEMBER SHACK: I've even got a large gap here. So I've really got bending at this point, right? You're letting this sucker bend.

MR. RICCARDELLA: No, that's not --

MEMBER SHACK: Isn't that what the large gap means, that the nozzle is free to bend? It's not constrained in the axial?

MR. RICCARDELLA: Yeah, but we don't have the -- this case here -- well, yeah, a large gap. It's still got some interference.

MEMBER WALLIS: Why does it leap from 20 to .6 in 160 and 180 degrees? Is that a typo?

MR. RICCARDELLA: No, that's the change in the model from --

MEMBER WALLIS: On the top there, the fourth line.

MR. RICCARDELLA: I understand, yeah. It's the change in the mode from the part through wall crack to the through wall crack.

MEMBER WALLIS: So by changing the model you make all of the difference in the world.

MR. RICCARDELLA: Yeah.

CO-CHAIRMAN FORD: Could I make a comment? Again, it's on terms of time management here. Could I request that the members kind of --

MEMBER WALLIS: We're trying to establish credibility. That's all.

CO-CHAIRMAN FORD: You can take credibility as 100 percent.

(Laughter.)

CO-CHAIRMAN FORD: Okay. Ninety-nine percent. My point is that we requested this so that the members would understand the approach that was taken, the completeness of the approach and where we're heading. Obviously this is not finished. There's no way this is finished, I am assuming, in its entirety.

I just wanted the members to understand the depth of what's being done here. So, Pete --

MR. RICCARDELLA: We have some very interesting conclusions and observations on the basis of what we've done, and I'd really like to get to that because I think there will be a lot of interest --

MEMBER KRESS: Before you go though, one more question.

(Laughter.)

MEMBER KRESS: Your K is a strong function of the residual stresses.

MR. RICCARDELLA: Yes, sir.

MEMBER KRESS: How do you get those?

MR. RICCARDELLA: We have residual stress analyses that were performed, elastic, plastic residual stress analyses of a nozzle that --

MEMBER KRESS: Based on how it was welded?

MR. RICCARDELLA: Yeah, un-huh, based on weld size. We didn't take into account that much heat rates and things of that sort. I think standard heat rates were used, but as I said, I believed there could be an uncertainty as high as a factor of two on these results.

And they tend not to dominate the probablistic fracture mechanics results because of the slope of the curve.

Okay. The next is how we use the crack growth data that John Hickling presented, and here you'll recognize this upper plot with the black as being the fit. This is the cumulative distribution function for that constant alpha, and the black points are the heat by heat data.

So each of these points represents the average of those groups of heat. You remember some heats had 27 specimens; some had one; some had two.

The lower curve with the red data is the integral of all of the data points, the individual data points, and as you can see that's more conservative. The mean is a higher alpha for those, and John discussed why that is. It's because there's more testing that has been performed on the higher rate heats of material than the other.

What we've chosen to do in our analysis is to use both variabilities, and this is basically as a result of some comments at the meeting that we had at the NRC where we look at heat to heat variability, and then we superimpose upon that within heat variability, and we can specify. For example, you specify 69 nozzles in a head. You could say, well, that head consists of three heats. Twenty of the nozzles are from one heat, 30 are from another, and ten are from a third heat.

So we picked from this distribution for the heat, and then we sample again for the individual nozzles in that heat, and we look at the heat to heat scatter in that analysis.

And another parameter that we've taken into account, and again as a result of our interactions with the NRC, is a correlation effect between crack initiation and crack growth. The comment was that you shouldn't just go in and randomly for a time to a leakage from the Weibull distribution and then pick a second completely random parameter because if you have a nozzle that leaks, chances are that's a bad actor.

So you have higher crack growths in the ones that leaked than the ones that don't leak. And so what we've done is we've built into our sampling scheme in the Monte Carlo analysis an ability to correlate the random number for leakage with the random number for crack growth. Okay?

And this particular slide shows a .8. It's a minus point eight because it turns out that a high random number for leakage means a long time until leakage, a high time until leakage. A high number for cracked growth means a high cracked growth rate.

So if we have a heat of material that's out here in the .8 for crack initiation, then we're going to sample from this narrower set of data for crack propagation, and the .8 is an input parameter. We can input zero. They'd be totally independent. We can input one and be totally correlated. Okay?

So basically it's a knob that we have in our analysis to calibrate or benchmark our analyses against real behavior. Okay?

MEMBER WALLIS: What are the crosses?

MR. RICCARDELLA: Oh, that's just the randomly generated -- go ahead and put in the .99. this is the actual spreadsheet that does it.

MEMBER WALLIS: Randomly generated numbers?

MR. RICCARDELLA: Yeah, these are -- let me show you. These are the random numbers that actually go into the distributions to pick our crack growth rate, to pick our time to leakage, see.

So if I assume .99 or a very high correlation, basically I'm using the same random -- it's like using one random number to pick both parameters. They're very, very highly correlated. Okay?

If I put zero, they're totally independent. So by going from zero to one, I can span the entire range from no correlation between crack growth, time to crack growth, and time to initiate in crack growth to very highly correlated time to initiate a crack growth. Okay?

And then we've got to go back. Thank you.

MS. KING: Sure.

MR. RICCARDELLA: Okay. This shows just some typical results of this analysis. So this is a typical probablistic fracture mechanics analysis. What I'm creating is the probability of net section collapse with no inspections for a 602 degree Fahrenheit head as a function of EFPYs. Okay? Starting in about 20 years, and I've got several cases here.

First, the difference between these two parameters here represent the difference between assuming that the head is made up of three heats and the --

CO-CHAIRMAN SIEBER: Could you move closer to the microphone, please?

MR. RICCARDELLA: I'm sorry.

That the head is made up of three heats or 69 heats. In other words, every nozzle is an individual heat, and that addresses a specific question that came up at an NRC meeting about is it appropriate to sample each tube individually or should you be sampling them in groups. It turns out that it really doesn't have a significant effect.

The other thing we looked at was a log triangular versus a log normal fit of the data. Did we not go through the -- actually this got a little bit out of sequence. Let's go to the next two slides.

This is the distribution, again, showing the distribution of that parameter alpha by heat. The black data points are by heat, and I show a blue curve, which is the log normal basically that John Hickling presented earlier. The red curve is the log triangular, is a log triangular fit for that same group of data where it's truncated at two extremes.

So we don't get into these very, very high crack growth rates in the very tails of the distribution.

Okay. This is what we're doing for the heat to heat variation, and then the next slide shows we took the entire population of data and looked at each data point relative to the mean of its heat, and we developed basically a deviation from the mean in terms of the multiplication of one for every data point.

And so you see that we get about a plus or minus six multiplier for within heat variation. So as you go through the Monte Carlo simulation, we say, "Okay. I have at least 20 tubes in the header out of one heat."

We pick a heat from the previous chart, and for each of those 20 tubes, we sample from this distribution to say where that -- you know, to get the actual crack growth rate for that tube, and we correlate that to the time to crack initiation from the Weibull.

MEMBER KRESS: So for a log triangular, shouldn't you get a discontinuity at .5 in the slope?

MR. RICCARDELLA: No, I don't think so. At .5?

MEMBER KRESS: That's where the triangle turns around and goes down the other way.

MR. RICCARDELLA: Yeah, but still, 50 -- there might actually be a -- if you look real closely there might actually be a discontinuity.

MEMBER KRESS: Okay. Maybe it's just my eyes.

MR. RICCARDELLA: Okay.

MEMBER SHACK: But that's sort of good because those are the sort of two bounding distributions that you would pick.

MR. RICCARDELLA: Yes, right.

MEMBER SHACK: And you're not seeing all that much.

MR. RICCARDELLA: What we find is about a factor of two, which I think is kind of within the levels of uncertainty of this type of an analysis really.

MEMBER SHACK: Considering, you know,

you'll never determine those tails. So in one case you've chopped them off and in the other you've let them run to infinity.

MR. RICCARDELLA: Yeah. Okay. Next slide.

Okay. Now, the real key, I think, to this whole analysis is we've made an attempt to benchmark the results with respect to the B&M plants, and so what I'm showing here is -- the previous slides were probably density functions. This is cumulative probability. Okay?

So this is cumulative probability assuming no inspection for a plant operating at 602 degrees, like the B&W plants.

This is the cumulative probability of leakage versus time, the cumulative probability of large circ. crack versus time, and the bottom is the probability of net section collapse versus time.

And what this slide says is that for that group of the seven B&W plants operated at approximately this temperature, they had about, at 20 years, those have about greater than a 90 percent probability of at least one leak, and that's fairly consistent with the operation. Seven out of seven of them had leaks.

MEMBER SHACK: Of course, since that's how you determine the Weibull, I would hope it would --

MR. RICCARDELLA: Yeah, right. That's a good observation. But then more significantly, one out of those seven plants had a large circ. crack. And now when we integrate the fracture mechanics into it, we're predicting about an 11 or 12 percent probability of a large circ. crack, and then that drops down to what the actual probability of net section collapse would have been at that time, assuming no inspections.

Now, as soon as we do inspections, of course, we change that probability of a large circ. crack.

MEMBER WALLIS: What does net section collapse mean?

MR. RICCARDELLA: Net section collapse basically means nozzle ejection. The same terminology.

MEMBER WALLIS: The same, okay.

MR. RICCARDELLA: Okay. Now, with that as the methodology, now we've used this model as a method to basically assess and provide a technical basis for our proposed inspection plan. Okay?

And the method we've used for this is to, first of all, start with the benchmarked analysis parameters that I've just described. Okay. So we've somewhat benchmarked the analysis.

Analyze different plants at various head temperatures, and what we've done is we've set risk categories based on both the probability of net section collapse per year and based on the cumulative probability of leakage. Okay?

And then we've also set inspection intervals looking at the effects of inspection based on probabilities of net section collapse, based on the impact of inspections on probability of net section collapse.

So I'm going to run through some of the results that we have and then later this afternoon or this evening, Michael Lashley will talk about the resulting inspection plan that's resulted from this.

MEMBER WALLIS: Inspections result in a change in the profile though because you do something as a result of what you find?

MR. RICCARDELLA: Yeah, the assumption is that if you find it you fix it, and so that particular nozzle no longer has a chance to propagate to ejection. You fix it or do something to take it out of the mix.

MS. KING: Well, the assumption and the experience to date is that you find it and you do something about it.

MR. RICCARDELLA: Okay. Just to review the analysis parameters, we've used the head temperature. We've analyzed ranging from 560 to 605 degrees Fahrenheit. I mentioned already the Weibull parameters of slope of three with a beta and a theta of 15 plus or minus six, and it's assumed to be a triangular distribution.

The crack growth rate statistics we've discussed. We're using the log triangular for both heat to heat and within heat variation.

We've used this cracked growth versus leakage correlation factors. We've used minus .8 for both the heat to heat and within heat, and --

MEMBER WALLIS: Is this something you thought was real?

MR. RICCARDELLA: Yeah, well, you know, it's kind of the knob that I used to make it match the results on the B&W plant.

MEMBER WALLIS: I thought it was. There was a dial

MR. RICCARDELLA: It is, in fact, yeah.

Okay, but you know, the real use of this type of analysis is to make apples to apples comparisons of different things. So I think it's appropriate to pick a set of numbers.

MEMBER SHACK: It also doesn't seem physically unreasonable.

MR. RICCARDELLA: Yeah.

MS. KING: Right.

MR. RICCARDELLA: You know, if you told me that you wanted me to use log normal and it doubled the probability, that probably lowered that correlation factor a little bit because, you know, in the end you want to agree to reality, to what we've observed in reality, and if reality changes, if we make some inspections and find some additional unexpected results, we'll have to go back and recalibrate, I guess.

And then we need some sort of acceptability criteria, and just for purposes of this inspection plan, what we're using is sort of an acceptable level would be a probability density function for a nozzle ejection of one times ten to the minus third, and that's consistent with the most predictions of the consequential core damage frequency, given a nozzle ejection is also about ten to the minus third.

So we've got a couple of plots here. This is a plot for a lot of different temperatures, 570 up to 605. The probability of net section collapse versus EFPYs at different temperatures, and you see two lines on here. You see the 1E to the minus three that I've talked about as being the acceptability limit. There's also one down here at 1E to the minus four.

You can see that these tend to jump around a little bit. Let me show you. Go two ahead to the conversion study.

Here's a convergence study that we did on one particular case with a 600 degree F. where I've run these with 10,000 -- this is the same thing, probability net section collapse versus EFPYs, assuming no inspection. I ran them with 10,000 Monte Carlo simulations, 100,000, and then a million Monte Carlo simulations. Essentially that would take about a ten-hour run to do the million Monte Carlo simulation.

But you can see that even though you get this jumpy curve, if you pass kind of a best fit through the jumpy curve, you'd predict about the same time to one times ten to the minus third. Okay?

All of the cases I showed earlier were run with 100,000, the middle of those three. Also you see that in terms of the probability of leakage, it has very little effect, the probability of leakage. Because it's a higher probability number, basically it converges much faster.

This is the cumulative probability of a leak, assuming no inspections, again, versus EFPYs for a bunch of different head temperatures. And, again, I've drawn two horizontal lines on here, one at 75 percent probability of leakage, and one at 20 percent probability of leakage.

Now, what I've done in the next plot is I've taken the intersections of those horizontal lines with the results of the analysis and created a locus of basically a time versus temperature locus of that data. So these upper two curves correspond to the net section collapse of one times ten to the minus three. That's the red chain link curve, and the 75 percent probability of leakage in terms of a time-temperature domain.

The lower two curves represent one times ten to the minus four, probability of net section collapse, and that just approximately corresponds to a probability of leakage of about 20 percent.

So what we have here basically is the

temperature, the heat temperature, versus EFPYs of operation. However, as somebody mentioned earlier, some plants have operated at different head temperatures. They operates for a while at 600, and then they dropped it to 570.

And so this has been integrated into the number of -- the EFPYs are the effective EFPYs for those plants that have had multiple head temperatures, assuming that it has always been operating at the current head temperature.

CO-CHAIRMAN SIEBER: Now, these susceptibilities do not incorporate any knowledge you might have about the susceptibility of different heats.

MR. RICCARDELLA: No.

CO-CHAIRMAN SIEBER: Okay.

MR. RICCARDELLA: No, we haven't taken that -- this is still generic. Basically it's a time-temperature, the same type of time-temperature correlation that Larry was talking about earlier. I've broken it into two, and, in fact, these are exactly the data points that Larry showed on his plot earlier of the actual plant.

So we show where the actual plants lie, the 69 plants lie on this time-temperature domain.

CO-CHAIRMAN SIEBER: I have another question. You have a susceptibility that's a function of temperature which you've described in everything you've done so far, but we also know that there is a susceptibility due to the heat. Which is the more predominant effect as far as determining how long it will be until section collapse?

For example, all of the leakage we've seen so far came out of one heat, right?

MR. RICCARDELLA: No.

CO-CHAIRMAN SIEBER: No?

MR. RICCARDELLA: A couple of them came out of welds, I guess.

CO-CHAIRMAN SIEBER: All right, okay.

MEMBER SHACK: And there were more than one heat.

MR. RICCARDELLA: There was more than one heat.

MS. KING: There's more than one heat that has leaked.

MR. RICCARDELLA: There is a strong heat-to-heat sensitivity, as there is a strong temperature effect. Right now I can't say which is more important, but, you know, the heat-to-heat variability though, that variability is built into the distributions that we've used in our analysis.

And what we're trying to present here for purposes of the inspection plan is sort of a summary of the fleet or, you know, a simulation of the entire fleet.

CO-CHAIRMAN SIEBER: I can appreciate that, but when we started out, we used the temperature data as a basis for ranking the plants and saying these are the high susceptibility plants; these are moderate; these are low.

And then you put them in order, and that tells the agency who to go after first. If it doesn't consider the heat data, it's not totally clear to me that we're capturing everything that needs to be captured to do that ranking.

And this going back to --

MR. RICCARDELLA: But what the problem is is that we really don't have much information about the susceptibility of the individual heats in the individual plants. So, I mean, even if -- is that what you were going to say, Larry?

MR. MATHEWS: Well, what I was going to say is by ranking them the way we did, just based on time and temperature, there's an inherent assumption in that process that every plant has that same bad material that Oconee 3 had, and it's very likely that many plants aren't nearly that bad.

CO-CHAIRMAN SIEBER: I guess that's one of my problems, that when you put that assumption in there, then the ranking is less accurate than it would be if you took that effect into account.

For example, if you buy a deep draft pump and there was an instance with an information notice about ten years ago where the heat of some pump couplings in the shaft was not good, and that became a shut down your plant deal, and they were able to identify where the bad couplings were depending on when they were made and who you bought them from.

And so they ought to be able to tell where all of these nozzles came from, right?

MR. RICCARDELLA: Oh, we have information, but we don't have information on the susceptibility. You know, as we continue to do more inspections and collect more data, if some form of correlation becomes apparent, we'll take that into account in the model.

We could adjust this model so that we favor, you know -- so that we could analyze individual groups of material that are on the bad side or on the moderate side or on the good side, and if we start to see those --

CO-CHAIRMAN SIEBER: It seems to me that if we continue on this methodology of inspections and so forth and rankings that you ought to maybe do that.

MS. KING: Yeah, currently we are tracking the inspection data to the heat, but right now our stance is we don't have enough data to differentiate.

CO-CHAIRMAN SIEBER: To do something real --

MS. KING: And I'm not turning away from that. It's just that at this point we don't have enough data to differentiate between the heats.

CO-CHAIRMAN SIEBER: Well, if I were in your place rather than mine, I would be looking to trying to do that, to give me a better picture as to what's going on as time goes on and you collect the data.

MS. KING: Right.

MEMBER KRESS: Let me see if I can understand the basis behind your one times ten to the minus three acceptance criteria. If you have that happen, it means you have a small break LOCA.

MR. RICCARDELLA: Yes.

MEMBER KRESS: And you can't put in one rod.

MS. KING: Essentially.

MEMBER KRESS: Essentially.

MR. RICCARDELLA: Essentially, yeah.

MEMBER KRESS: And that has a conditional core damage probability of probably ten to the minus three itself.

MR. RICCARDELLA: Yeah.

MEMBER KRESS: So you're talking about one times ten to the minus six core damage frequency.

MR. RICCARDELLA: Core damage frequency.

MEMBER KRESS: As your acceptance criteria.

CO-CHAIRMAN SIEBER: That's right, or whatever it comes out when you add on all the other mitigation you get out of the plant.

MEMBER KRESS: Yeah, but that should be at a conditional already.

MR. RICCARDELLA: I guess the other thing, too, is we're really not using that as acceptance. We're really saying that that's the limit that defines when we proceed from the moderate risk region into a high risk region, which is using this to set --

MEMBER KRESS: That's kind of a definition.

MR. RICCARDELLA: -- to set inspection requirements.

MEMBER KRESS: Yeah.

MR. RICCARDELLA: But that's where it comes from, exactly.

MS. KING: And you'll see it hopefully before this evening. We'll get to show you what these inspection requirements are.

MR. RICCARDELLA: And, you know, we're doing inspections to try to make sure that we never get to that point. We're starting to do inspections, you know, even in the low risk regime. We have different inspection levels, but they're graduated as plants move up from one regime to the other.

So you can see that a high risk model, basically it captured -- there's a total of nine red points that were leaders. Okay? And all but basically one of those red points is either on or above our high risk line.

And also I should say that all of these data points are about a year old. So they're all really going to move up about a year, and actually this data point here is three plants. There's one right on top of another. So you can't see them.

The three points where there were inspections that found cracks but no leaks are the three yellow points, one, two, and three, and then there's a whole group of plants that have done inspections and found nothing that are shown.

So it has really taken the plot that Larry presented earlier and breaking it into a two dimensional plot so that you can really see where these plants lie, time and temperature.

And now the plants progress upward on this line in real time, not in dog years, but in real years.

(Laughter.)

MEMBER SHACK: What I'm looking at now, when you have those dots, isn't that a median value for a plant with that temperature?

And if I ranged it up to the 95th percent, as I go through and I vary different heat assumptions, that low temperature plant is going to get better, and it's going to get worse, you know.

I've done a bunch of Monte Carlo runs. What's being plotted here? Is that the median value from that?

MR. RICCARDELLA: This actual data point?

PARTICIPANT: No, the third, the chain link line.

MS. KING: Oh, the chain link line.

MEMBER SHACK: The lines.

MR. RICCARDELLA: The lines are the median results from my Monte Carlo analysis. The data points are just time and temperature.

MEMBER SHACK: But where would the 95 percentile of the curve be?

MR. RICCARDELLA: I haven't really put confidence bounds yet on the Monte Carlo analysis. That requires some assumptions about, you know, the confidence in the various assumptions that occurred.

MEMBER SHACK: I mean, for a given temperature you get a distribution of failures, right?

MR. RICCARDELLA: Yes.

MEMBER SHACK: Well, you can take the fifth to 95th to that.

MR. RICCARDELLA: This is all of -- this 100 percent of that.

MEMBER SHACK: It's 100 percent?

MR. RICCARDELLA: There's no -- you know, in terms of putting confidence bounds, I think you'd have to look at uncertainties in your various assumptions that went into, you know, the analysis.

MEMBER SHACK: This is all of the failures.

MR. RICCARDELLA: This is all of the failures, yeah.

CO-CHAIRMAN FORD: Could I make a suggestion?

MEMBER ROSEN: One question. What would a failure look like? Where would you plot a low risk plant that had inspected and found the crack? What color would that be and where would it be? Say it was a 600 degree --

MR. RICCARDELLA: Well, anyone who has inspected and found leakage is a red dot. Anyone that has inspected and found cracks is the yellow circle.

MEMBER ROSEN: So it doesn't matter whether you're a low risk or a high risk plant.

MR. RICCARDELLA: No, not in how you plot the individual points. I mean, if --

MEMBER ROSEN: So if I'm to take any comfort from this plot at all in terms of stuff, you know, if they're falling within the boundaries, because you're by definition saying if you get a crack or a leak and you're a 600 degree plant, you're right there.MR. RICCARDELLA: Yeah.

MEMBER ROSEN: And you may need a brand new plant, maybe one of the youngest plants that --

MR. RICCARDELLA: Well, no, no, no. The probability of leakage for a 600 degree plant, let's say a 602 degree plant, your probability of leakage hits 20 percent at about eight years in accordance with this, and then you continue to operate that plant. It gets higher and higher.

By the time you hit 18 years, it's 75 percent. This is Davis-Besse. It had, you know, at the time of that inspection about a 75 percent probability of leakage in accordance with this model.

MEMBER ROSEN: I'm trying to figure out where a point would be on this chart that would not be consistent with your model.

MR. RICCARDELLA: Oh, if one of these guys comes out as a red triangle, we're back to the drawing board, okay, or a circ. crack or anything like that. I mean, then it's reevaluating the whole model.

CO-CHAIRMAN SIEBER: That would tell you your temperature correlation is no good.

MR. RICCARDELLA: Yeah. It might be that the temperature or estimates of that head is wrong. We don't have absolutely certainty in our estimate of the head operating temperatures.

CO-CHAIRMAN FORD: Again, Pete, I hate to do this to you, but could you just move straight to your conclusions?

MS. KING: I guess could we show a couple of slides on the effect of inspections?

CO-CHAIRMAN FORD: Please.

MR. RICCARDELLA: Let's just real quickly show the next one. All we've done here is to take those same -- that same chart and put on lines of constant EDYs, which is degradation, and it just shows that's how we get the 18 and the ten basically, because those are the ones that fall on top of our risk curves.

Okay? All right?

MS. KING: Now we'll go a couple ahead.

MR. RICCARDELLA: Now what we do is I've taken that same analysis, same model and said here's the probability of net section collapse versus time. This is run at 600 degrees. So this is actually EDY. It says EFPYs, but in this case EFPYs equal EDYs.

And at the time that I get to 18 years, which is approximately that one times ten to the minus three, I assume inspections of various levels. I assume a bare metal visual, and there's three curves. One is a bare metal visual every refueling outage. One is every two EDYs. One is every four EDYs. Okay? And what's the effect of those?

And we made some assumptions about probability of detection, which I think we should cover.

CO-CHAIRMAN SIEBER: Point, six.

MR. RICCARDELLA: Yeah, we assumed .6, which means --

MS. KING: I think that's hanging up.

MR. RICCARDELLA: I think you've got to go up. No, one more up.

CO-CHAIRMAN SIEBER: We'll trust you it's .6.

MR. RICCARDELLA: Point, six, no, but there's something else I wanted to point out, was that what we assumed also that was for subsequent exams, if you missed a leak in a nozzle, we applied a factor of .2 on that. So it's really only .12, is what we're assuming for subsequent exams of a nozzle.

The comment came from one of our interactions with the NRC that if you do an inspection and you miss it, it might be because that's a particularly difficult nozzle to inspect, and you have a higher probability of missing it the next time.

So we put the ability to input a knockdown factor on the POD or --

CO-CHAIRMAN SIEBER: You can't find it because it's covered up by boron crystals, right?

MR. RICCARDELLA: Yeah, something like that, or difficult access or tight shrink fit or all kinds of things. So we think it's a fairly conservative assumption as to what the POD is, and then we had a second POD set of assumptions for nondestructive volumetric examination, and that's a curve of probability of detection versus crack size.

CO-CHAIRMAN SIEBER: Okay.

MR. RICCARDELLA: So if we go to the volumetric, this is the same kind of curve again. We assume we do the inspection at one times ten to the minus three or at 18 EDYs, and then what the effects of NDEs at four and eight years are on that.

CO-CHAIRMAN SIEBER: Do you have one that shows the comparison between a visual and a volumetric?

MR. RICCARDELLA: The next one sort of shows that.

MS. KING: Kind of, yeah.

CO-CHAIRMAN SIEBER: Okay.

MR. RICCARDELLA: Here's the case of starting the inspections earlier. You know, we are proposing some inspections for the moderate category. We're not proposing that people just operate without any inspections until they get to high risk. We're actually specifying inspections in the low risk and also in the moderate risk.

These are the moderate risk recommendations, and it's either a visual at two EDY or an NDE at four. So you can see the effect of the two. Obviously the NDE is more effective as the large curve.

The NDE at four is more effective than the visual at two.

CO-CHAIRMAN SIEBER: Yeah, that gets back to my earlier point. If you really want to find them, it ought to be volumetric.

MEMBER ROSEN: It's a question of whether you want to find that or whether you want them to find us.

CO-CHAIRMAN SIEBER: Well, I think that's well put.

MR. RICCARDELLA: Okay. Let's just go to conclusions.

CO-CHAIRMAN SIEBER: You don't have anything with NDE and visuals at the same intervals, right? That would be a yes or a no.

MR. RICCARDELLA: No.

MS. KING: No, we do not.

MR. RICCARDELLA: No. We could back it out from the previous two curves if you want. The NDE is --

CO-CHAIRMAN SIEBER: I think I know enough.

MR. RICCARDELLA: The NDE is much more effective because it's finding -- first of all, we're using a higher POD for the NDE, and secondly, it's finding things even before they leak.

CO-CHAIRMAN SIEBER: And it also helps you to some extent to see if you've got a cavity somehow in the ferritic material that you can't see from the surface, if you're good enough at looking at it.

CO-CHAIRMAN FORD: I'm going to let the members read the conclusions during their break time.

CO-CHAIRMAN SIEBER: Our understanding is more important.

MR. RICCARDELLA: This is the key though. I think when Michael gets up later to present the inspection plan, you're going to see that this is the basic result of this analysis, is we've got low risk, medium risk, and high risk categories that correlate to those EFIs, and I've kind of explained where those different categories come from.

CO-CHAIRMAN FORD: Thank you very much, indeed. I appreciate it.

CO-CHAIRMAN SIEBER: Well done.

CO-CHAIRMAN FORD: We'll recess until

quarter past three.

(Whereupon, the foregoing matter went off the record at 3:00 p.m. and went back on the record at 3:15 p.m.)

CO-CHAIRMAN FORD: Mark, you're up.

MR. KIRK: Okay. Thank you.

Is that working?

MS. WESTON: Yes.

MR. KIRK: Okay. The title of this presentation is NRC assessment of the margin available at Davis-Besse. My name is Mark Kirk. I'll be making the presentation for the NRC Office of Research.

What I'll be presenting in the next 40 minutes or so represents the collaboration of a whole host of people, and I don't think I have all the names on the top slide.

Wally Norris is another, like myself, is another program manager in the Office of Research. He manages the work at the Engineering Mechanics Corporation of Columbus, who has done some of the finite element analysis. I manage the work at the Oak Ridge National Laboratory under the HSST program.

Of course, Bill Cullen is leading the Davis-Besse effort within the Office of Research.

Nilerh Chokshi is the head of the

Materials and Engineering Branch.

At Oak Ridge, Paul Williams and Richard Bass have been doing the finite element analysis. At the Engineering Mechanics Corporation of Columbus, the work there has been led by Gary Wilkowski and Dave Rudland.

CO-CHAIRMAN FORD: Could you just -- we've also got a quorum of people here. I just want to interrupt for one second just to let everyone know that at the rate we're going, in case you have to make family arrangements, et cetera, it might be a quarter to seven or seven o'clock before we're finished, if we keep up the density of questions.

MEMBER BONACA: Tonight?

CO-CHAIRMAN FORD: Tonight.

(Laughter.)

CO-CHAIRMAN FORD: Sorry, Mark.

MR. KIRK: Okay. And I'll apologize in advance. In order to give you the most up-to-date information, we've revised these slides since I provided them to Mike at the end of last week. I do not have handouts right now, but I will. We'll make them and we'll get them to you. They are probably about twenty percent changed.

What we'll be talking about today is

mainly a discussion of our deterministic assessment of margins in the condition that existed at Davis-Besse at the time of the March shutdown.

We'll also be giving you some views on the next steps in this analysis which include some further refinements of this deterministic assessment and also moving on to do a probablistic analysis.

The scope of our deterministic assessment was first to asses the margin to rupture of the exposed cladding left in the condition that existed at the March '02 shutdown.

The next step was to determine how much of either -- how much over-pressure it would have taken to rupture the cladding in that condition or how much more wastage would it have taken to rupture the cladding at operating pressure.

Finally, we had planned to assess various weld repair options.

The red text, it's just up here to provide you a perspective of where we are, well, where we thought we were last Friday. We've had an increased level of understanding which I think I should say is a reduced level of eposemic (phonetic) uncertainty regarding our failure criteria. So we are going back and redoing some calculations, but I think that's all for the better.

We're still working on the middle bullet. The last bullet we are not going to do because repair isn't being considered at this time.

This slide provides you with an overall perspective of the analytical tools we've been using. We've been using to different sorts of finite element models.

At Oak Ridge National Laboratory, we've constructed a full 3-D finite element model where we've got a global model. It includes the specific head geometry as installed at Davis-Besse. It includes all the control drive penetrations.

That global model, when subject to internal pressure, establishes the boundary condition on a sub-model which then means that we get a much more refined representation of the head wastage at least at best we can tell at the time. So this is the model that we would regard as giving us the answers that are the closest to reality.

We also have been using an axi-symmetric finite element model. That was constructed at MC^2. Because of the limitations of axi-symmetric modeling, the wastage had to be modeled as a spherical pit at the top of the head.

Obviously, that's geometrically not a completely accurate representation, but the reason why we wanted to do that was to enable us to do some quicker parametric studies about increase growth and so on. Moreover, Gary Wilkowski, who's been doing the analysis at MC^2, has considerable background in modeling of corrosion damage in gas pipelines and so is familiar with some of the approximations that is used in that industry.

But in any event, in the end we'll be reporting and relying on the results of the 3-D model. We've used the axi-symmetric model largely to help guide the 3-D modeling effort and provide quicker results at the time.

This table provides just some details of the analysis and the various inputs that we've used. The loading in these analyses has been either the design pressure or in cases where we've tried to calculate the over-pressure margin, obviously, we've ramped that up.

The temperature has been the operating temperature and we've not considered any temperature gradients because none exist at operating, at least in any practical sense.

I'll show you more about the material

properties and the local geometry that we've modeled on the following slide. That's a new slide that is not in your pack.

On failure criteria, I'm going to give you a little of a now-and-then flavor because this is the area where we've done some refinements in the last few days.

Up until last Friday, we considered -- or we defined, I should say -- failure to occur when the average through thickness plastic strain in the exposed cladding area exceeded 5.5 percent, with the 5.5 percent corresponding to the strain at the beginning of plastic instability.

That was derived from uni-axial tension data that showed an 11 percent strain at max. load, and furthered the assumption that failure occurs at the same stress level under uni-axial and bi-axial loading.

I want to stress that is an assumption that maybe isn't as coupled as well as it should be to the actual ductile failure mode.

We'd honestly never been completely satisfied with that as a failure criteria because up until last Friday, we hadn't known of the existence of any better data to calibrate to. But I'll be discussing how we've changed that shortly.

MEMBER WALLIS: Don't you have stress concentration around the edge where there is sort of a sharp edge?

MR. KIRK: Yeah. Yes, you do. And that is considered in the geometric finite element model, yeah.

MEMBER SHACK: Why would you even start with that assumption, Mark?

MR. KIRK: Start with what assumption?

MEMBER SHACK: That failure occurs at the same stress into the uni-axial and bi-axial loading.

MR. KIRK: If you want the straight and unvarnished answer, because it made the math work easily. But don't go there too far because everything has changed.

MEMBER SHACK: Okay.

MR. KIRK: Okay. This just shows the material properties that we've been using, just simply appropriate properties for the RPV steel and for the 308 cladding.

I'm now going to give you a short time history of the geometries we've assumed. Our first cut at this, when this all hit the fan back in March and the Office of Research was asked to assist, we had to get some cut on the geometry.

So we took one of the photographs that was taken in the vary initial inspections. It was a head-down shot of the cavity. At that point, the brown that you see at the bottom of the cavity, that was water sitting in the cladding.

We used the diameter of the hole as a dimensional reference and simply digitized the shape of that cavity.

Our current model reflects the results of Figure 13 which is shown in the licensee's root cause document. It's our understanding that that's the best current representation of the cavity.

What we've incorporated into our model is a -- I think everybody here has also seen the companion profile view which shows the nose in the RPV steel. However, we don't believe or we don't have any reason to believe that that contributes significantly to the load carrying capacity of the membrane. So we haven't included that in our model.

Basically, the 3-D model that we're using now has a hole in it down to the cladding along the green contour.

MEMBER WALLIS: What's the boundary condition on the control rod drive cylinder there?

I understand the boundary condition around your green line, but what about the boundary where there's a gray? What's your boundary condition for the cladding there?

MR. KIRK: I'm not -- I mean it's a -- it's hooked to the rest of the head and you don't apply a boundary condition there. You apply a boundary condition remote to the head.

MEMBER KRESS: It's free to move there.

MR. KIRK: It's free to move, yes. It's not constrained. But I'm not sure I'm answering the question.

MEMBER KRESS: It's hooked at the corners where the green --

MR. KIRK: Yes.

MEMBER WALLIS: So where there isn't green, where that round grey thing is; it's free there?

MEMBER KRESS: Yeah. It says free-floating membrane, a free-floating area. No constraint to it.

MEMBER WALLIS: Oh, it can't be. From there?

MR. KIRK: From there, yes. That just expands with pressure.

MEMBER SHACK: No, but the displacement of the cladding is constrained to be the displacement at the nozzle?

MR. KIRK: Yes.

MEMBER SHACK: It's not free-floating?

MEMBER WALLIS: It's not. It rests on the nozzle.

MEMBER SHACK: And it's attached?

MR. KIRK: This is where the nozzle attaches at. That's correct. Yes.

MEMBER BONACA: I thought the portion of the cladding was exposed within and beyond the image that you have from a picture taken above.

MR. KIRK: I believe that's what's reflected -- well, these are two different --

MEMBER BONACA: I understand.

MR. KIRK: Better -- presumably better knowledge going from here to here. I believe there is -- I should say I believe because nobody is going to band-saw through this thing and cut it open for all to see.

My understanding from what I have seen -- and I think, you know, yours too -- is that there is exposed cladding back here.

MEMBER BONACA: Exactly.

MR. KIRK: I believe this contour here which I have not outlined is what you would -- what I'm trying to say --

MEMBER BONACA: Oh, that's what you're seeing?

MR. KIRK: -- is that if you're looking down from the top there's metal here. This is the position of the nose. That's where the cladding will dispose.

Now what's really there, we still don't know. I think that's fair to point that out, that in our current calculations -- in anybody's current calculations -- what the actual geometry is is, indeed, unknown. I mean we're getting better and better representations of it. But I think it is important to point out that the first order effects that are important is the overall exposed area.

The shape of that, obviously it's different if it is a perfect circle than if it is along the ellipsoid. Also the details of the thickness, overall thickness of the cladding and thickness variations.

We don't know all those. You know, those are, to borrow a phrase that I've learned from our PRA friends, "those are in principle knowable," but we don't know them right now.

(Laughter.)

MR. KIRK: So we like everyone are proceeding with our best current information which to my understanding is this right here. But if anybody in the audience can tell me later about better information, I'd greatly appreciate it because we're about, as you'll learn later, we're about to embark on finite element analysis to drive some probablistic calculations. If we can go into that with a better knowledge of the geometry, that would be certainly desirable.

MEMBER KRESS: I have a little bit of a strange question. Why do you want to do this?

MR. KIRK: Because my boss asked me to.

(Laughter.)

MR. KIRK: No, you had a serious question.

MEMBER KRESS: Right, seriously. I mean, you're asking -- this is kind of a what-if question. How close were we to disaster?

MR. KIRK: Exactly.

MEMBER KRESS: Is there some use for that information?

MR. KIRK: In terms of -- do you mean in terms of the probablistic analysis or the -- we've been doing the deterministic analysis, I think, just to -- my understanding would be to satisfy that question of how close were we. Were we really close or were we not so close at all?

MEMBER KRESS: You just want to know that?

MR. KIRK: Yeah.

MEMBER KRESS: Is there some use for that information?

MR. KIRK: Now, going to the probablistic calculation -- and I'll give the short answer and some of my colleagues in the back can perhaps give a more detailed answer -- the probablistic calculation is being used as one of the inputs to NRR's safety determination process.

MEMBER KRESS: The old ASP type thing or?

MR. KIRK: Steve, do you want to take a cut at that so I don't use the wrong acronyms?

MR. LONG: This is Steve Long.

Significant?

MR. KIRK: Yes. Yes, significant. See, I knew I'd do it wrong.

MEMBER KRESS: Okay. I understand that.

CO-CHAIRMAN SIEBER: The way you can do that is to assume it fails and look at what mitigating systems were in service and what the failing duct you had, which comes out to what, three times ten to the minus three or something like that for CDF?

If you assume the failure frequency is one, that's the first cut.

MR. KIRK: What I'm going to show you is a series of slides that summarize our current results, and some of these are as current as just this morning. So you are getting the latest and best.

What the contour plot shows you here is the equivalent plastic straining contours in the cladding.

We've removed all of the reactor pressure vessel head so you can see what's going on. We've taken this up to the operating pressure of 2165 psi. At that pressure we get the highest strain somewhere around about the center of the wastage cavity, and the peak strain is somewhere between 2.5 and three percent.

We've been going through extensive debates, as I think most of the committee members are aware, with the industry over what an appropriate failure criteria is, but I don't think anybody has ever presumed that it would be as low as this.

The finite element model with the best representation of the geometry as showing us that at operating pressure we wouldn't really expect it to fail. Indeed, it did not fail.

CO-CHAIRMAN SIEBER: Let me ask you a question. How did you model the cladding itself? Cladding is not a plate. It is a series of weld stripes, which to me would seem to be weaker than a solid piece of material that was just a plate there.

Did you treat the cladding differently --

MR. KIRK: No.

CO-CHAIRMAN SIEBER: -- than you would have as a solid metal?

MR. KIRK: No. Right now -- well, it's weld strip cladding. So we've assumed -- I mean, it's been modeled as a plate. So you've implicitly assumed that there are no flaws in it and that you've got no significant lack of inner rod penetration.

CO-CHAIRMAN SIEBER: Do you feel comfortable with that?

MR. KIRK: Yeah. I feel reasonably comfortable with that. The only further modification that I would think would be appropriate at some point -- and again, this gets to the question of why are you doing this -- is how refined a model do you want to get to get a warm, fuzzy feeling that you weren't that close after all.

You might want to include the natural undulations that result from the welding process. I would personally take the position that I wouldn't want to do that until I had a lot better picture of what those undulations were. I don't have that right now.

MEMBER ROSEN: You'd just be making it up.

MR. KIRK: Yeah. Right now I would be forced to make it up. That's right.

MEMBER ROSEN: I'm not sure. You need to be careful about assuming that because it's weld metal that it's weaker than a plate. There's lots of evidence that they think it might actually be stronger.

MR. KIRK: Well, just in terms of the --

CO-CHAIRMAN SIEBER: Wait a minute. It seems to me that I've seen weld overlays on various vessels where it wasn't continuous. I've seen places where the weld didn't --

PARTICIPANT: Didn't overlap.

CO-CHAIRMAN SIEBER: -- and the undulations actually exist because they are crud-trapped. That's what makes all these clad vessels, unless they're micro-polished, so radioactively hot.

MEMBER ROSEN: But would you agree with me that we don't know -- a priori we don't know whether it's stronger or weaker?

CO-CHAIRMAN SIEBER: I think that you would say it was weaker if you knew exactly what the weld metal was and the temperature conditions as --

MEMBER ROSEN: But we don't.

CO-CHAIRMAN SIEBER: -- how it was laid down. You would know something about it, but it would be a guess. It really would.

MEMBER ROSEN: I'm just trying to make the point that we don't know whether it's stronger or weaker than a model plate because we don't know what the configuration is (a), and (b) we don't know whether a weld metal deposited that way is, in fact, weaker or stronger.

MR. POWERS: This is Jim Powers from FENOC.

MEMBER KRESS: The question is: to what detail do you think you all have to go to with this.

MR. KIRK: One of the things that we will be doing -- and I'll get to this in a bit in the probablistic analysis -- is we will certainly be including -- because we know from measurements that were reported in Figure 14 of the licensee's root cause report; we know there are measurable variations in the cladding thickness.

And so in our probablistic analysis, I can say with a fair degree of certainty that variations in a uniformed plate model of thickness will be included. Whether we need to, want to, whether it's warranted to go to the next step and include the details of the undulations is, indeed, up for questioning.

Like I said, I wouldn't -- I, personally, wouldn't want to do that until I had a much better picture. By that, I do mean something like a photograph and profilometry of what's actually there because otherwise I'm just guessing.

CO-CHAIRMAN SIEBER: Okay.

MR. POWERS: This is Jim Powers from FENOC.

We do have some undulations on the surface, but it's relatively smooth. There is no separation of contact bead to bead. It was a six wire sub-arc application of the clad. We PT-tested that clad area and found no indications in situ.

So we had some degree of confidence in its continuity.

MEMBER WALLIS: There is a measure of the residual bulging, isn't there, in this?

CO-CHAIRMAN SIEBER: Yes.

MEMBER WALLIS: Does that check your analysis? I mean the actual movement of the center from --

MR. KIRK: I don't have those figures reported here, but my memory is from an early analysis that they were -- given the approximations in the analysis and the difficulty attendant to measuring a set deformation off of initially curved surface, that if you will forgive the phrase, "they were close enough for government work."

(Laughter.)

MEMBER WALLIS: That's your predictions, or the measurements?

(Laughter.)

MR. KIRK: Both.

(Laughter.)

MR. KIRK: In this case, the measurements weren't reality either.

I mean, remember those measurements were made in an environment where they were trying to minimize man REM so it wasn't exactly like somebody got down there with a micrometer and made a measurement that was good to the mil.

I think we're in the position the piece is now cut out. I apologize because I don't know where it is. Clearly somebody in this room does. But, you know, we're in the position of making much less equivocal measurements.

MEMBER WALLIS: Is this going to the Smithsonian or somewhere, is it?

(Laughter.)

MR. KIRK: I don't know.

CO-CHAIRMAN SIEBER: We probably are dwelling on this more than is necessary. So I think at least I know in my own mind what was done and how it was modeled and that's good enough for me.

MEMBER KRESS: A strain is a measure in the change in length divided by the original length.

MR. KIRK: Right.

MEMBER KRESS: Your original length, is that your finite element node that you use? You get a change in that finite element node?

MR. KIRK: Yes. Yeah.

MEMBER KRESS: Okay.

MR. KIRK: This is the slide there where --

PARTICIPANT: I could put cartoons on there.

MR. KIRK: This is the slide where we've had some significant changes and, I think, changes for much the better in our predictions of margin on over-pressure. All the predictions that we've made, that anybody's made obviously depend upon how you modeled it and what you've assumed for failure.

In particular, the assumed failure criteria, the failure strain makes significant differences in how much pressure you think you can withstand.

There was considerable discussion given in earlier presentations of this work that the industry analyses performed by Dr. Riccardella were predicting considerably higher over-pressure margins than our analyses. It's not hard to see that was related to differences in the failure strains we were using.

One thing I would point out is that even with our at the time more pessimistic view of the strain that the material could withstand before failure ensued is all of our over-pressure margins exceeded the SRV set-point of 110 percent. So something even with the very pessimistic view that we took initially on what the material could take, a controlled SRV trip would have happened before we would have expected the SRV set-point to have been reached, before the membrane would have blown.

However, as I said, we've been having

continuing discussions between ourselves and the industry regarding the issue of the failure criteria. We've recognized from the beginning that the failure criteria that we took on was somewhat arbitrary.

Pete pointed out to us -- pointed us back to a paper that he had presented way back in 1972 at PVP, where experiments had been run on, among other things, burst discs of 304-stainless steel. I have diagramed the experiment here.

The disc had a thickness of both an eighth inch and a quarter of an inch. It was a six-inch diameter exposed area, and it was subjected to pressure on the backside until it ruptured.

Now, to quote Richard Bass, who has looked at this over the weekend, in fact, if I were to design an experiment to calibrate the failure criteria in my finite element model, I would have designed this experiment.

So over the weekend, once we finally got the peculiarities of electronic data transfer perfected and actually got a copy of the paper, Paul Williams and Richard Bass at Oak Ridge modeled this geometry, which is very conveniently axi-symmetric, and used it to calibrate a failure model that we would use in the Davis-Besse analysis.

We believe that these experiments are extremely relevant and appropriate to this end because the experiments have a similar material, have a similar thickness, and have a very similar exposed area to the conditions of interest at Davis-Besse.

By calibrating the failure criteria to these experiments, we are able to significantly reduce our uncertainty in the failure criteria, of course, by referencing the relevant experiment data.

In doing these analyses, we've reached the same conclusion that was reached back in 1972, that disc rupture occurs shortly after the finite element solution fails to converge under pressure loading, of course. What that means, physically, is that the elements -- we're doing large deformation, large plasticity, finite element analyses -- the elements have been stretched so far that you can't maintain -- you can't reach an equilibrium condition.

This, of course, produces what we'll call an NRC failure criteria, which is much, much closer to that that's been advocated by the industry for quite some time now.

In exercising this new failure criteria, we're using a new sub-model of the wastage area just based on our most recent geometric understanding and also including more refinement through the cladding thickness.

MEMBER KRESS: I have a little problem with that criterion. Doesn't the failure to converge of your finite element model depend on the size of your finite elements that you choose?

MR. KIRK: Yeah, yes it does. We've done the studies on that. But the more -- the less refined your model, the stiffer the model becomes. In other words, our initial model included only one element through the thickness of the cladding.

MEMBER KRESS: Okay. You mean --

MR. KIRK: That --

MEMBER KRESS: -- you only had sort of a surface?

MR. KIRK: Yeah. Yeah.

MEMBER KRESS: That went all the way through?

MR. KIRK: That's right. That model will fail to converge at a lower pressure than a more refined model. So the -- if you fail to refine adequately, you will --

MEMBER KRESS: But, how about in the other two dimensions? You can make that smaller and smaller. On the top.

MR. KIRK: Yeah. Yes, you're right. You're absolutely right that that will depend upon the level of mesh refinement.

What I'd like to point out is that if you under-refine the mesh, which is the only error that you can make because it is an inherently discrete model, that will lead to an under-prediction of the true failure pressure, not an over-prediction.

MEMBER KRESS: You may be right. I am still bothered by having a failure criteria that's tied to how well my finite element model behaves. It seems a little strange to me, but I'll buy what you say.

MEMBER SHACK: The system is too stiff. Therefore, I'm going to get less deflection than I would for a given load.

MR. KIRK: That's right.

MEMBER SHACK: Wouldn't that tell me I'm -- I'm getting less deflection so if I go -- the strain I'm predicting is really too small, right?

MR. KIRK: I'd have to check, Bill. I think it goes -- oh, what I do remember is --

MEMBER SHACK: But your going to run the mesh refinement?

MR. KIRK: We're running the mesh

refinement. We know that if we have four elements through the thickness, we get to a higher plastic strain before we can converge them with one element through the thickness. As of 0800 this morning, we had a pressure of 3.5 ksi or 60 percent above design without failure, and the model continues to run.

MEMBER ROSEN: Why wouldn't you let the licensee do any more? They want to get more and more margins. You know, let them do it. You're done with the problem as far as I'm concerned.

MR. KIRK: I'll give you a list of people I'd like you to say that too, if you would.

MEMBER ROSEN: I just said it.

(Laughter.)

MEMBER WALLIS: No, I don't think your done with the problem because the public is going to ask this question, the newspaper reporters, all kinds of people.

Have you done the ASME diaphragm tests? Have you predicted that too?

MR. KIRK: I'm not familiar.

MEMBER WALLIS: The one you just drew. The one you showed us -- the pictures.

MR. KIRK: Yes. Yes, that's the -- that was the -- I don't have that here, but those results, we were able to predict the results in the paper.

MEMBER WALLIS: You did a good job on that?

MR. KIRK: Yeah, within ten percent of the -- we systematically under-predicted the true burst pressure of those experiments by a factor of ten percent.

MR. POWERS: Jim Powers from FENOC again.

From a licensee's perspective, we have a very short presentation that we'd like to do that shows what we did in terms of optimization of the node, numbers of nodes for the modeling, as well as a correlation to the disc burst criteria and shows how we selected our failure criteria. We have about a dozen slides, if we could respond afterwards.

MR. KIRK: So as I said, these are very new results. This gives you a sense of the line that we're trying to pursue.

Also, right now the only information that we have on the additional -- how much bigger the cavity would have had to have been in order to fail comes out of our axi-symmetric model that was done at MC^2.

We haven't yet gotten this into the 3-D model. What we did is we just expanded the diameter of the pit at the top of the axi-symmetric head until the failure pressure, and I should emphasize this is a failure pressure at assuming the old 5.5 percent strain failure criteria. So the newer, better, updated version should be bigger.

In any event, based on that criteria, we calculated that we needed, in round terms, two more inches of wastage along the main axis in order to fail at the operating pressure. Given the changes that I just reflected and our understanding of an appropriate failure criteria, I would expect that when we do this with the 3-D model, with the new failure criteria, the amount of the additional wastage could indeed be considerably more.

MEMBER KRESS: Are you going to convert that into how much time was left before the --

MR. KIRK: Yes. We do consultation with Bill Cullen. Yes. Yes.

MEMBER ROSEN: Did you get any information about what would happen to the rod after failure? Would it eject?

MR. KIRK: That's not part of our current analysis. I'll throw that open to anybody else in the room if anybody -- do you know is anybody considering what would happen to the rod if this membrane ruptured? Anybody?

I haven't heard about that, but --

MR. POWERS: Jim Powers, from FENOC.

We submitted in our safety analysis of this rod ejection effects. We described those the last time we came to this subcommittee in terms of the shield above the rod housing area and lateral loads from jet, cavity loads on adjacent rods, and the fact that they'd remain in the elastic range and should function properly.

So we had submitted that previously.

MR. KIRK: This is my -- the last slide that I was planning on presenting. It just gives you a perspective on where we're going. I put on the slide last week that we were looking at a better definition of the failure criteria. That, based on the work over the weekend, is now well under way.

On that basis, we intend to recalculate the margin on over-pressure and the additional cavity growth needed to fail using the new failure criteria and the 3-D model.

As I indicated before, we've begun the FE analysis to support -- to generate the inputs needed for a probablistic analysis that's needed to support NRs, and now I've got a wrong again, significance determination process.

MEMBER WALLIS: If your cavity grows enough, then the liner is actually holding the nozzle.

MR. KIRK: That's correct.

MEMBER BONACA: In fact, a similar question I have is this analysis clearly is looking at the strain in the material and the ability of what it would take to rupture.

MR. KIRK: Yeah.

MEMBER BONACA: In reality, during the clean-up of the head, there was work being done on the nozzle from below and that's when the tube moved.

So I guess the question I have is: how well attached is this nozzle to the cladding, okay, that would result in that being the weak link?

So, therefore, the cladding probably could have still survived, but the nozzle would be ejected. I don't understand what caused them to do that.

MR. POWERS: Jim Powers from FENOC.

What caused the rotation is we were going into the repair methodology for the J-groove weld cracking phenomenon. So we machine-up through that weld and actually separate from it. Then it wasn't supported up above due to the cavity and it tipped a bit.