And in the power uprates, it is also where people put numbers there, you know, and some of us objected, but I wonder whether it is worth pursuing this anymore. If we manage to get an upper bound, that is good enough. Maybe an expert opinion elicitation is the answer.

MR. CUNNINGHAM: It may be, and I guess I am not quite sure where you are going.

MEMBER APOSTOLAKIS: Where I am going is that we don't have a model, but yet people are coming in here for important issues and nobody says I cannot do this because there is no model. Everybody does something and people seem to say okay, that is reasonable.

MR. CUNNINGHAM: Well, we do have models, and part of what we are doing now is trying to be -- as Alan was talking about, in terms of the quantification process, I am not sure you would say that we have a model there.

But we are trying to take something and make it more systematic if you will, and so you can in a sense call it a model.

MEMBER ROSEN: I don't know if it is called a model really. It is a method.

MR. CUNNINGHAM: It is a method.

MEMBER ROSEN: And Alan described it in some detail for the subcommittee.

MEMBER APOSTOLAKIS: But basically the way that I understand it is that people are happy that they have a description of the context, and then you have a number of experts, and they tell you what the number is.

MEMBER ROSEN: It is more complicated than that, but yes.

MEMBER APOSTOLAKIS: It is always more complicated.

MR. SIU: If I may, you know, clearly in this project we tried to exercise with the tools that we had, and we have some belief that the results that we are getting are reasonable and useful for the decision at hand.

It is not to say that improvements in these tools won't lead to better decisions later on. We just don't have such better tools at this point. So I guess I would argue that we are not necessarily at a state where we should be freezing development on these methods and tools.

We always learn, and the project that you see in front of you now, where HRA is just a part, we have done a lot of work on fracture mechanics, and we have done work on thermal-hydraulics, and have done work on PRA and a culmination of all of that is for example, this particular -- this is one product of such an integrated process.

If we had said back in the '80s, well, we can make decisions, and you have seen the tools that we have now, and that is the current rule. So now we are in a position to better that.

MEMBER APOSTOLAKIS: Well, it is hard to generalize. A lot of things were done conservatively and so on, but it is a real issue, and a major intellectual challenge to develop a model that will give you the probability of time-dependent human actions. So let's recognize that.

MR. SIU: Yes.

MEMBER APOSTOLAKIS: I mean, ATHEANA tried, and it really didn't lead anywhere. I mean, it did a lot of qualitative work, but not the quantitative. And then at the same time we see the staff coming here, and both of them do research at NRR, and they seem to find reasonable things like asking experts, and looking at upper-bounds, and so on.

So it really makes you wonder whether it is worth pursuing an HRA effort now. Maybe 10 years from now, after again we find that a lot of things were wrong and very conservative, because I don't know whether if we lead anywhere, and people do things, but don't make them unhappy.

They don't make them happy, but they don't make them unhappy.

MR. CUNNINGHAM: If we could go back to the HRA program that we have got planned over the next couple of years. I think we have talked to the committee that one element of the expert elicitation process is what kind of experimental information could you provide on human performance insert context.

And I think that is a big element of what the staff is proposing, in terms of research, and getting back to trying to collect more, if you will, empirical evidence or experimental evidence, to support an exert elicitation process.

MEMBER SHACK: We are sort of a quarter of the way through, and so I think we had better move on.

MR. HACKETT: I think I will just add one final comment specific to this project in HRA. One of the slides that we will come to is showing that a lot of the risk is dominated by LOCA and then the HRA is not a huge contributor in that regard. We can get into that further.

MEMBER APOSTOLAKIS: Which LOCA is that?

MR. HACKETT: LOCAs in general.

MEMBER APOSTOLAKIS: Really.

MR. HACKETT: We have got a slide on that. Another motivation was the fact that to quantify some plants are predicted to be close to the screening criteria at EOL, and so sort of this red band that Mark Kirk had here on the slide.

And, you know, starting out towards the end of this decade that you are starting to see some plants that are beginning to impact this criterion. And so their interest level -- and our industry colleagues are not here today by and large, but that gets their interest level up pretty quickly when they are starting to look at making cases for license renewal man, many years in advance.

So that is another major motivator, and also another major motivator --

MEMBER APOSTOLAKIS: Let me understand. Some plants close to the screening criterion?

MR. HACKETT: Right.

MEMBER APOSTOLAKIS: And which ones are

these?

MR. HACKETT: Arbitrarily, what Mark did on this slide is that he is showing a band that is within about 50 degrees of, say, the 270 or the 300 criterion.

And then basically what you are getting towards are --

MEMBER APOSTOLAKIS: Oh, this is from --

MR. HACKETT: Right, exactly. Exactly. So the bottom line is that we are trying to show the interest level, and I think we skipped over one. No, not yet.

MEMBER POWERS: The more I think about this, I didn't understand it at all. Could you focus us here on at least that first one?

MR. HACKETT: Sure.

MEMBER APOSTOLAKIS: The previous one you mean?

MEMBER POWERS: Yes.

MEMBER WALLIS: That is the simplest slide he has got I think, is that one.

MR. HACKETT: Yes, really this is just in simplicity, these are the number of degrees that you are from the screening, and it should say criterion. But from the 270 or the 300, and so it is just showing you that there is a grouping of plants here, especially when you are getting out towards where folks are considering license renewal, where we are starting to get into increasing numbers.

And not that anybody is in any particular difficulty when they are 50 degrees away from the limit. But it certainly is going to make --

MEMBER POWERS: But a lot of them are at zero.

MEMBER WALLIS: Not at the end of the license period or that time.

MR. HACKETT: At the end of the license. There actually should be two.

MEMBER APOSTOLAKIS: What is the point of showing the years there?

MEMBER WALLIS: That's when they get there.

MR. HACKETT: That's just when they get there. That is when they are predicted to get there. This in particular would be Palisades, and I believe that would likely to be Beaver Valley. I can't say for sure, but this one is certainly Palisades. They hit their criterion in 2011.

MEMBER POWERS: Who is the guy at 2035? Is that --

MR. CUNNINGHAM: At 2012, they would be at -- they could not operate beyond --

MEMBER POWERS: He is in a world of hurt.

MR. CUNNINGHAM: They could not operate beyond 2012 because of the embrittlement of the vessel under the current rules.

MR. HACKETT: That was another primary motivation. And in terms of the scope of the analysis --

MEMBER APOSTOLAKIS: That sounds kind of funny to me, but why are you doing the work and not them?

MR. HACKETT: Well, in the next slide, we will come to that. They are indeed doing a lot of work, and working with us on this. In terms of the scope of the analysis, we have analyzed three plans which would be Palisades, Beaver Valley, and Oconee.

Two of those are among the most embrittled at EOL,which would be Palisades and Beaver Valley, and they are both in about a degree of the screening limit at EOL.

We have all the PWR manufacturers represented in two plants from the original study, and which would be Oconee and Beaver Valley, or Oconee and Calvert Cliffs. I'm sorry.

And two plants close to the screening criterion which I mentioned, and caveat this, you know, as Mark has done before, and we said -- these are all that we are aware of, when all significant and potential initiating event sequences are considered.

That is not to imply that there aren't some that could be out there that we missed.

MEMBER ROSEN: We have spent a lot of time talking about model uncertainty yesterday.

MR. HACKETT: Yes.

MEMBER APOSTOLAKIS: And you will again.

MR. HACKETT: This is just to get to Professor Apostolakis' point. The conduct of the project has --

MEMBER APOSTOLAKIS: And you will gather facts and conclusions to report to the full committee?

MEMBER WALLIS: We gathered estimates and --

MEMBER POWERS: And idle speculation.

MEMBER APOSTOLAKIS: It seems to me that if you want to form a peer review group, you are going to have a hell of a problem.

MR. HACKETT: We are working on that. I agree, and we are working on that right now. That is one of the slides that you will see that we will get to, in terms of things that still need to be done.

MEMBER POWERS: Let me assure the committee that I have no idea what Sandia is doing on this.

MEMBER APOSTOLAKIS: Yes, I mean, you are creating --

MEMBER POWERS: I have no idea what they are doing.

MEMBER SHACK: I mean, who is the cognizant Federal employee here?

DR. LARKINS: I guess I am.

CHAIRMAN BONACA: Yes, John Larkins is the Cognizant Federal Employee.

MEMBER APOSTOLAKIS: Well, maybe I should -- can I talk to you?

DR. LARKINS: Sure.

MEMBER APOSTOLAKIS: Not on the transcript.

CHAIRMAN BONACA: Can we proceed.

MR. HACKETT: In addition, I will mention that this also does not indicate public participation, but we have had some significant participation from the public. At least not a lot lately, but definitely some since then.

In terms of how the analysis is conducted, there are two main components. There is the estimation of the plant, which TWC stands for is through wall cracking.

And then you compare that to an acceptable frequency of through wall cracking, which is what we spent one of the previous slides talking about.

And this is how you get there, going through the three major disciplines, from PRA event sequence analysis, to combinations of those running through the thermal hydraulics, and getting the inputs from thermal hydraulics feeding into a probablistic fraction mechanics assessment.

And that addresses the materials aspects and things like flaw distribution. And what you get coming out of all of this is a conditional probability or yearly frequency of through wall cracking. And that then you are going to compare with the limit.

MEMBER APOSTOLAKIS: And when you quantify uncertainties, don't you address them? I mean, can you quantify uncertainties without addressing them? Why do you say address, then quantify?

MR. HACKETT: Okay. Address, then quantify. No, in fact, maybe it should be written that in a lot of cases that you can't get there. The acceptance criterion, bottom line, is that we feel, or at least the team feels, that we are consistent with the Commission's safety goal policy statement, the SRM that was issued after Yankee Rowe, and in general the principles of Reg Guide 1.174.

And then the way that this thing pans out for you is --

MEMBER WALLIS: Excuse me, but when you say through wall cracking and vessel failure, that means the same thing?

MR. HACKETT: That means the same thing, reactor vessel failure frequency, or frequency of through wall cracking, and that is going to get you to the establishment of a limit and the comparison with the curve for the material behavior.

MEMBER APOSTOLAKIS: Without adding anything to it?

MR. HACKETT: Without adding anything in. This part at least is just schematic, and so we are not even going to get into whether degrees F, or C, or RTNDT.

MEMBER WALLIS: But you are going to define it in your report?

MR. HACKETT: It is defined in the report, and obviously I think that is an area where we are going to need to have some clarify.

MEMBER APOSTOLAKIS: When you say in your report that your results indicate that you may increase the screening limit by 80 --

MR. HACKETT: By 80 to 110 degrees.

MEMBER APOSTOLAKIS: You are referring to the 270?

MR. HACKETT: That's right.

MEMBER APOSTOLAKIS: So that becomes 350?

MR. HACKETT: 350 to 380 or so.

MEMBER APOSTOLAKIS: And calculated the way the regulatory guide says?

MEMBER WALLIS: I don't think that is true. No, that is not true.

MEMBER APOSTOLAKIS: So you have a new method for the screening criterion, but the old method for developing your --

MR. HACKETT: Let me see if I can take a crack at that, and we may be back in the same place we were for --

MEMBER APOSTOLAKIS: It not a simple deal.

MEMBER SHACK: Sure it is.

MR. HACKETT: All we are doing there is that you will see a new metric for RTNDT, which we will call an RTNDT star, and I will try to explain that a little bit later how that compares with the current criterion.

And so we are trying to compare apples to apples and you are exactly right. We should try 80 to 110 degrees fahrenheit, and you are adding that on to the screening criterion. So what was 270 becomes nominally 350 to 380.

MEMBER APOSTOLAKIS: Okay. That is one issue. But the other issue is that you are using a more sophisticated methodology now to come up with a screening criterion. Yet the licensee would be using the old approach to come up with the RTNDT?

MR. HACKETT: i see your point.

MEMBER APOSTOLAKIS: And compared to the new screening criterion?

MR. HACKETT: That was one of the things that we addressed. The answer to that is really no. They will be using an RTNDT based approach, and the only thing they will have to adjust for is basically going to be the weighting of this RTNDT for weld type, and weld length, and fluence.

I will try and explain that a little bit better. In practice, they won't have to do anything. If we set the criterion out, all they need to demonstrate is that they are that far back from it, and there won't be any need for any plant specific analysis.

MEMBER APOSTOLAKIS: Yes, but the question is how do you demonstrate?

MR. HACKETT: Well, the only change in --

MEMBER APOSTOLAKIS: Is it from the old approach?

MR. HACKETT: The only change in regulatory space that they would need -- for instance, here are a few things that they would need to know. They would need to know details of the fluence analysis for their vessel, and they will need to know weld type and length that are limiting, and they have that information now.

So we are not imposing anything new in regulatory space.

MEMBER ROSEN: They won't have to worry about it until they are running out about 200 years anyways.

MEMBER WALLIS: Well, that assumes that all the statistical stuff that you are doing is typical of all plants.

MR. HACKETT: Right. It is assuming a generalization. That's right.

MEMBER APOSTOLAKIS: But the earlier argument that it doesn't really matter that we honor the 60 degrees, because there is a compensating addition on the calculational side.

Now you are changing the screening criteria and making it more realistic.

MR. HACKETT: No.

MEMBER APOSTOLAKIS: Aren't you going to touch the other one?

MEMBER SHACK: The screening limit before and we will now make it 290, and we added 60 degrees to the 210 to get 270, and we will add 60 degrees to the 290 to get 350.

So you do the two exactly the same way, just so you don't change anything that the licensee does. He will compute the number and exact --

MEMBER APOSTOLAKIS: So we are doing a good analysis here, and then we will make it bad based on the calculations?

MEMBER SHACK: No. Let's move on.

MEMBER WALLIS: This is all going to be clear when they rewrite the report so that it is clear. It all will be clear when they rewrite the report so that these 6 or 7 RTNDTs are all very clearly defined, and we know what is going on.

MEMBER APOSTOLAKIS: And also when they do page numbers. I was so scared on the plane yesterday.

MR. CUNNINGHAM: If I can go back just a second.

MEMBER APOSTOLAKIS: Yes.

MR. CUNNINGHAM: We are proposing a technical basis for a rule change.

MEMBER APOSTOLAKIS: Yes.

MR. CUNNINGHAM: And the folks at NRR will be looking at rule, as well as reg guide changes, possible reg guide changes.

MEMBER APOSTOLAKIS: Okay. All right.

That is a better answer.

MR. CUNNINGHAM: I don't want to commit Matt to saying that absolutely he is going to do this or that, or whatever.

MEMBER APOSTOLAKIS: Yes, sir?

MR. MITCHELL: Again, Matt Mitchell, NRR. The only thing I would say is we will ensure as we go forward with any proposed rule change that the way that licensees would analyze the actual material properties or vessel is completely consistent with the basis upon which the screening criteria is established.

I mean, that is incumbent in the way that we would modify the rule. So weighted average used -- and which I Ed is going to get to -- to try to enumerate a screening criteria, weighted average, for evaluating the vessel.

MR. HACKETT: What we are hoping is that as a resource that a --

MEMBER WALLIS: Wait a minute. I'm sorry. The present RTNDT is not a weighted average. It is a bounding curve. So you are changing the definition if you go to a weighted average. You won't just be using the --

MEMBER SHACK: But that is only

proposed.

MR. HACKETT: That is proposed right now, and it would be changing it in a way that they would be able --

MEMBER WALLIS: And all of this will be clear when you rewrite it to make it clearer?

MR. HACKETT: That would be our goal.

MEMBER WALLIS: All right. Thank you.

MR. HACKETT: Let's move on to some results. The bottom line is that over the realistic operational time frames, and we tried to show that, and some of this is really extending out too far, but that is just the way that the mathematics went.

But over realistic operational lifetime, the through wall cracking frequency that we are finding coming out of the FAVOR code is very small, and by that we mean somewhere between E minus 8, E minus 9, range.

And you can see that on the slide here, and at the current screening criteria the yearly through wall cracking frequency in a generalized sense is on the order of 1 times 10 to the minus 8.

And then it is important to note here that two of the plants that we use to try and set this up are among the most embrittled that have been evaluated. So we feel we are well below.

MEMBER APOSTOLAKIS: Well, that is confusing, and so let's talk about this figure. When you say the mean of the 95th person, I was looking for those. Where do I find them?

The only difference in the product is the plants.

MEMBER SHACK: They are the same.

MR. HACKETT: Those are the same basically. they are skewed.

MR. CUNNINGHAM: The calculation results, as they are essentially -- the mean is at the 95th percentile.

MEMBER APOSTOLAKIS: And that is mentioned somewhere in here?

MR. CUNNINGHAM: I am sure it is.

MEMBER APOSTOLAKIS: It is? Well, I missed it. Not hear the figure.

MEMBER SHACK: In some of the figures you can almost see a shadow of your --

MR. HACKETT: The second major result is looking at what are the dominant contributors to risk and what the team has found is that its LOCAs are the dominant contributor to risk, as opposed to stuck-open safety valves, which are actually a contributor as you can see here for Oconee, and for the B&W type design.

But an important feature is that secondary side breaks in general are not contributing the way that they were during the original study. There are a couple of reasons for that, and a lot of it goes to the severity in binning, and again the team can correct me if I am wrong on any of this.

But in terms of the binning on the secondary side previously it used to be that everything was binned with the severity of the main steam line break is my understanding.

Also, they are just not as severe a challenge as are the LOCAs, in terms of the thermal transient, and then of course you have the piece that we talked about previously, and some credit applied now for operator action that was not applied previously, or the three main elements don't affect the --

MEMBER WALLIS: So if we actually took the importance of the things which are thought to be important 20 years ago, they seem to be like 1 or 2 percent of the thing now?

MR. HACKETT: Very small.

MEMBER WALLIS: And so in fact you have not only gained a factor of 10 to the 4th, you have gained a factor of 10 to the 6th, because the things that you thought were important have now decreased to 1 percent of what matters. This is even more remarkable.

MR. HACKETT: I think it is remarkable.

MEMBER ROSEN: And things that you have ignored.

MEMBER WALLIS: The things that you have ignored have come up to be important, but they went down. They really were important before because you had the factor of 10 to the whatever.

MR. SIU: Or perhaps even a different way of looking at it is that the things that we ignored are still unimportant in an absolute sense. The numbers are small.

MEMBER WALLIS: But for different reasons.

MR. SIU: But they are high in proportion to what you have got left.

MEMBER WALLIS: But if you had not considered the LOCAs and just used the same basis 20 years ago, you would have been picking up another factor of 10 squared.

MR. HACKETT: And the purpose of the following slide here is to show that we are trying -- we tried to, and we think that we have achieved balance in the project, and in the execution of the project, and that the contribution of the initiating event frequency, and the conditional probability of failure is somewhat balanced.

And the analogy here is, you know, the idea that the initiating event frequency were so, so low that maybe you could operate a plant with a glass reactor vessel.

MEMBER APOSTOLAKIS: Let me understand. What is that figure showing?

MR. HACKETT: What it is really showing here, which is the X-factor, which is the initiating event frequency. The Y-axis is the conditional probability of failure given that event.

MEMBER APOSTOLAKIS: Failure of what, the vessel?

MR. HACKETT: Of the vessel, and that you would not want to see this laying over too much either way, and it is especially skewed to me towards the initiating event frequency side.

MEMBER APOSTOLAKIS: Well, is the initiating event frequency goes to 10 to the minus

2, and the condition probability goes also to 10 to the minus 2?

MEMBER WALLIS: No, no, the other event doesn't mean anything really.

MR. SIU: The question is whether the small numbers that I showed you on the previous slide are coming solely from, let's say, small initiating event frequencies, or solely from the condition of probability of vessel failure.

And what the slide is showing is that by and large for most important sequences there is a roughly equal contribution.

MR. HACKETT: In terms of the materials aspects on the slide that you are seeing here, what we have seen, which is not at all surprising to those of us who have been associated with this for a while, axial welds tracks way dominate the through wall cracking frequency on the order of over 90 percent.

And in this case it is the axial weld, RTNDT, or the adjacent plate RTNDT that is governing. The circumferential weld cracks play a minor role, and in a lot of cases we have seen significantly less than 10 percent.

And in that case you are looking at the

circ weld RTNDT, or the plate, or the forging situation governing. Cracking plates and forgings by and large are too small to play a role.

What you are really seeing -- and Terry can give you the details on this, but you have to have cracks that are probably more than a quarter of an inch or so, or I think what I remember from runs that I have done in the past were things on the order of a quarter-of-an-inch to three-quarters-of-an-inch to really be contributors.

And what you see from our flaw density and distribution that was developed is that you see a lot of flaws on the weld fusion lines, but they are a lot on the order of these two millimeter characteristic flaws. They are very small.

So when you hit those with a PTS transient, by and large they don't participate in contributing to --

MEMBER WALLIS: When you calculate your RTNDT star, you had a weighting factor for axial welds.

MR. HACKETT: Right.

MEMBER WALLIS: Now, I don't really remember, but I think it was independent of plant, and it looks as if the weighting factor here should not be independent of the plant.

It is very different for the Palisades than it is for Oconee.

MR. HACKETT: Yes. In fact, if you look at Beaver Valley, is a plate-dominated plant and so this actually is probably a pretty good place to take that kind of question as a lead-in to the weighted RTNDT.

The reason that -- and Mark Kirk developed that, and again at this point it is a proposal, as a way that you could proceed to recognize exactly this piece here.

That there is not an equivalence in how these things are initiating, and so it was a good idea to try and bring that data scatter today to try and weight these.

MEMBER WALLIS: But that is for different plants, and that is the thing that I wasn't sure about.

MR. HACKETT: It will be different depending on the material condition.

MEMBER WALLIS: So you calculate your weighting factor .

MR. HACKETT: Correct.

MR. SIU: That's right. I think you

could view what he has as a curve fit for the three plants, and now we are doing Calvert and there will obviously be a check on that.

MEMBER WALLIS: So since you had three weighting factors at three plants, and that seems to be --

MEMBER SIEBER: Could you tell me why Beaver Valley is different than the others in that it is plate dominated?

MR. HACKETT: It really comes down to being as simple as their welds are in good shape. So they don't have --

MEMBER SIEBER: That is a high copper plant.

MR. HACKETT: They don't have high copper welds. They have a plate in this case that -- and I may have to turn to Matt for the exact reason. I don't know the exact answer to your question.

MEMBER FORD: Wasn't one of the reasons is that the axial welds were not at peak flux azimuth of the core?

MR. HACKETT: Matt, is that the correct answer?

MR. MITCHELL: Yes, what it comes down

to is that the plates at Beaver Valley are -- one might consider them atypically high in copper when compared to other plates around the industry.

And the way that the core management scheme has been conducted at Beaver Valley has tended to put the flux peaks on the plates rather than on the axial welds.

MEMBER SIEBER: I did that, too.

MEMBER WALLIS: It is not just core management. It is design. You have got a core which is square inches, and you have got a round vessel and where the square points come close to the vessel is where you have a high fluence, and put their welds on the flat part.

MR. HACKETT: That is also true.

MEMBER SIEBER: Well, it was done intentionally at that plant.

MEMBER WALLIS: Well, you don't -- it is inherent in the design, and you don't manage anything after that.

MR. HACKETT: There would be certain limitations as to how much you could change it with the core design versus inherent construction.

MEMBER SIEBER: Well, that plant always had a low-leakage core and the idea wa to keep the fluence to the welds down, and we did that by zoning fuel. So that is how --

MR. HACKETT: Prior -- and that is a good question, but prior to the conduct of this project, I think there was a concern that with the plate being the embrittlement concern, and the material concern, you now have this very large surface area, and then if you were to sum up all the flaws that you might expect over that surface area, you might back yourself into a problem.

Instead, what you find is you find again that the flaws are focused on the weld fusion line, and the plates by and large aren't defective.

MEMBER SIEBER: Yes, I would suspect that most of the flaws are initiated in the welds.

MR. HACKETT: Right.

MEMBER SIEBER: And the density of the flaw initiators in the plates should be very low by orders of magnitude.

MR. HACKETT: That's exactly what we are finding.

MEMBER SIEBER: Okay.

MR. HACKETT: This next slide gets into basically -- well, it does not get into much. Mark Kirk is supposed to be here for that, and we had some -- we even had some audio for that. But the bottom line of this is looking at the containment as a system and its performance in terms of PTS and PTS impact on containment performance, is that the system energy for these types of situations are lower at the time of RPV failure, and so you have a limited mechanical impulse, and you have a limit to the containment pressurization.

And I think we have another graphic here. There it is. I think that Dave and Nathan can help me through this if I don't get it quite right. But I think what David did here was put a line on showing basically water at 212 degrees as a base line for energy, and then showing that particularly in the case of LOCAs, and this is a 16 inch LOCA here.

But the LOCAs drop very quickly and then the energy that you are at is much lower. So the whole bottom line is that the design bounds this type of -- the design being basically to take the double-ended guillotine break from LOCA for containment performance is something that initially in this type of scenario should not present any extra challenge to the containment.

And with some dependency if you are

looking at containment sprays, and we are looking at a situation where we have done at least a qualitative analysis and there is not a missile threat or other threat that would hopefully in a dependent way take out containment sprays.

Another element would be the fuel cooling, depending on the reactor cavity design. Some of the cavities are designed and would be flooded in the event of a significant LOCA.

And then obviously that goes towards your fuel performance or any core melt characteristics. This one I know the committee heard this morning about GSI-191, and there is obviously some dependence in here with regard to 191 and some strainer blockage.

MEMBER POWERS: Are you arguing that if you flood the cavity that the core won't melt?

MR. SIU: We are arguing that the probability of core damage is significantly less if the cavity is flooded, yes. We are not saying -- we just have not carried the analysis all the way through, but you are in a situation where you have got lots of cold water.

You have dumped the RWST, and in some of these plants the water level will rise above the top of the active fuel. In other plants, it won't.

MEMBER KRESS: There is a whole there to get the water into it?

MR. SIU: Yes, it is pouring out of the reactor pressure vessel. This is after the reactor pressure vessel has failed.

MEMBER POWERS: But you are not circulating it.

MR. SIU: It will heat up, but --

MEMBER WALLIS: Even if it doesn't completely cover the core as a pool, you will get two-way effects from spitting and steam cooling, and all that kind of thing.

MR. SIU: Yes.

MR. HACKETT: I guess I hesitate to go back to this type of slide, but -- well, there is one more piece here and this is basically Nathan's point here, is that this is addressed in the sequence analysis in detail for going through this type of scenario for the tree.

This was the one that I was hesitating to get back into, because this tries to resummarize sort of everywhere where we have been. But just going through the bullets, you know, and we have said this before, but very low predicted through wall cracking frequency values, and this is our bottom line, is suggesting that a revision of these criteria is warranted.

Basically this reactor vessel failure frequency set at 1 times 10 to the minus 6, will correspond to this weighted RTNDT value of 290 fahrenheit. Now, again we are back into this where it does not compare directly to the ASME or the regulatory RTNDT.

This is a weighted RTNDT, and it was described in your report, and unfortunately I don't have -- we have some backup slides that get into that with a lot of algebra on i showing that it is weighted basically by weld type in the case of axial circumferential weld length. And also the fluence specifics, and the --

MEMBER WALLIS: For the benefit if Professor Apostolakis, you should point out that it takes account of the epistemic and aleatory uncertainties in RTNDT.

MEMBER APOSTOLAKIS: Yes, we will come to that.

MEMBER WALLIS: Oh, you will come to that, but this RTNDT star is supposed to take account of that or not.

MR. HACKETT: We feel that it does.

MEMBER WALLIS: Well, maybe not. It doesn't. I'm sorry, I'm wrong. It is in evaluating the mean of the TWCF that you take account of that.

MR. HACKETT: Yes, that is correct. In this case, we --

MEMBER APOSTOLAKIS: This is weighted over what again?

MR. HACKETT: This is basically to try and do like the layman's view of this thing. This is taking the RTNDT and going back to that slide that I had showed you that breaks down where the -- I think like Marsh liked to put it yesterday, where do you assign the blame.

And where you assign the blame for failure of these things is failure of axial welds for the most part. So it is trying to weight it where the meat is. So largely weighted towards axial welds, but it will be weighted both in terms of the type of weld, axial versus circumferential, and the weld length.

MR. CUNNINGHAM: So it is the weld length.

MR. HACKETT: And the way the fluence is delineated. So it is a function of those things.

MEMBER APOSTOLAKIS: There was an argument made, which I can't find now, is on page X, and that if a particular utility does not necessarily know what kinds of axial rods it has,a nd that is what it says here, and that is why you are taking the weighted average.

And you have a generic average of 10 percent of them, and what is that called, heating, or heat something?

MR. HACKETT: A heat analysis?

MEMBER APOSTOLAKIS: Yes.

MR. HACKETT: There are obvious different heats of weld material.

MEMBER APOSTOLAKIS: Yes, and they don't know, right?

MR. HACKETT: Actually, they have everything, and this gets back to the discussion that we had earlier. They would have everything. If you were to get into the plant specifics, they have everything that they need to address the weighted value also.

MEMBER APOSTOLAKIS: So if they haver everything, they will not need to use a weighted value, and that is where I am going. Why would they need a weighted value?

MEMBER WALLIS: No, no, a weighted value takes account of the composition.

MR. CUNNINGHAM: The variability of materials and welds within a given plant. The weighting is all for one plant.

MEMBER APOSTOLAKIS: Within a plant.

MR. CUNNINGHAM: Within a plant.

MR. HACKETT: Now, if you were to get to -- and Professor Apostolakis may be going beyond to -- if you were to get to a plant specific analysis, and if your question is can they make this case, and can they calculate this parameter, again it is just a proposal at this point, but yes, they could, because the know the weld types that are limiting, and they know the weld lengths, and the geometry.

And they have the detailed fluence map of their vessel. So they could argue on that basis if they needed to. And the chances are that if this project is successful, they won't need to. Hopefully you won't ever need to.

But that is there if it had to come out. The last point really goes to this issue here, this RTNDT star that we have been talking about, and we have RTPTS,, which is RTNDT, but that is the way that it is calculated currently.

There is a difference of on the order of 80 to 110 degrees F. to compare apples to apples. So like what we were talking about before, what this means in the end is that a 290 F. screening limit on RTNDT star corresponds to the current regulatory limit moving out to 350 or more, depending on exactly where we end up.

And then that then has the effect of pushing out the operation for -- and I think that is my next slide in fact.

MEMBER APOSTOLAKIS: Yes.

MR. HACKETT: Well, maybe not, but the bottom line is that the plants are grouped here and it takes them for even coming close to impacting this revised screening criteria for many years.

At least it looks like for the license renewal period, and probably beyond, and Mark has the graphic down here saying 60 to 80 years potentially.

It may be getting to the point of eliminating this as a real regulatory concern.

MEMBER WALLIS: Mark also pointed out that the highest value you have for Beaver was something like a thousand years or something like that.

MR. HACKETT: They ran the analysis out pretty far I think.

MEMBER WALLIS: So for 60 to 80 in the yellow region, but if you start and kind of go up to the 10 to the minus 6, you have got to go out for hundreds or thousands of years.

MR. HACKETT: We did get into some discussion yesterday, and again --

MEMBER POWERS: We will never get out of the license renewal business.

MEMBER SIEBER: By then it will have corroded through.

MR. HACKETT: So I think our conclusions we have pretty much been through most of that. I think we have covered most of this. There is a question that Mark Cunningham raised about the reg guide.

Certainly we feel that we have a tech basis to go forward with the rule revision. Whether or not we engage in revision of the reg guide is probably going to be a resource issue largely. Nathan mentioned and talked about the reactor vessel failure frequency.

And the metric that we are talking about that is proposed here is that that is equivalent to the through all cracking frequency, and other options were evaluated.

And that that failure frequency would be set at 1 times 10 to the minus 6 per reactor year, and we think that is consistent with the guidance that we received from the committee, and previous foundation for the PTS rule, and also the quantitative health objectives.

The analysis supports this revised screening limit, and in this case the 290 on the weighted basis, which is equivalent to this 350 plus number. in terms of what we are used to thinking about.

MEMBER WALLIS: Well, I am just wondering about you screening them, which is such that they will never reach it. So there ought to be some regulatory check on what is going on with embrittlement.

MR. HACKETT: Before then.

MEMBER WALLIS: Before that, and how are you going to do that?

MR. HACKETT: A couple of things that I could comment on, and I am glad that you brought that up because we have gone through this so fast that we didn't bring up some of the other issues.

One effect that this will have is that we have to now go back and look at the companion in Appendix G for the operational limits. I know that we talked about that yesterday, but we should get into that here, too.

So we have an activity that is looking into the effects on Appendix G for heat up and cool down curves,a nd that is probably more likely to be where we will shift some of the limiting concerns here.

MEMBER WALLIS: But maybe this should also be an ongoing effort to evaluate some of the key assumptions that got you to this wonderful immortal vessel as you go along.

So that you say, oh, well, yeah, we made these big changes in what was assumed about flaws on the basis of the knowledge that we gained. And as we gain more knowledge, do we have to go back on that because of the extra knowledge that we are getting, and say maybe we were too optimistic about flaws or something.

MR. HACKETT: Yes, absolutely. That one is a key one that Dr. Ford mentioned yesterday. The potential or at least we have looked at for fairly near term, and any possibility for any active advancement of these fabrication flaws.

We think the answer is no, and we have data that says that it should be no, but that is not to say that is true for all time.

MEMBER WALLIS: And how about this noble chem thing? Suppose they come up with some new kind of chemical treatment for the water, and is this going to do anything about the surface flaws and all of that? Are we going to have to revisit this?

MR. HACKETT: We are going to have to continue to monitor those types of developments, and then maybe we will finish up and take any other questions with where we are going.

MEMBER APOSTOLAKIS: Oh, I thought you were finished.

MR. HACKETT: As I said, maybe to revisit where Mark started us off, and we feel that we have this interim product that we have shared here with the committee that has been forwarded to the NRR for detailed comments.

And that describes a lot of activities in the Office of Research from all three of the divisions. There is also that NRR has been involved while we have been doing this.

But in terms of the things that we still need to do, the Calvert Cliffs analysis, or the analysis of the Calvert Cliffs plan is not complete, and we should complete that in 2003, and that is a big aid in helping us with number two, in terms of the generalization of what we have done here to other plants, and to all plants.

We do have some sensitivity studies to work on, and one of them involves the flaw density and distribution. We have been challenged with some what if's there.

We feel that we have a pretty solid basis for that, but you can always second-guess what we have done so far, because there is a limited amount of data there like in a lot of cases.

There is verification and validation of the FAVOR code, which has been ongoing, and a lot of which has been completed. A lot of interaction with the industry on that.

Professor Apostolakis mentioned the peer review, and it is a challenge to get people, and it is almost like an O.J. Simpson jury. You know, you are looking at trying to find people who have not been involved in this thing in the United States, and it is not easy.

So we do have that as a take away, and

that we have got an external peer review, and I think in Mr. Mr. Thadani's letter, he had indicated that the ACRS was sort of subbing for -- and I don't know if that is the right word, but there was some discussion yesterday about ACRS substituting for an external peer review, and that is not the case.

As always, we have gotten many useful comments from the committee, and we think that we have addressed a lot of them. We have more to detail with, but it is not substituting for an external peer review, and so we will have that going.

The implications of the operational limits, we talked just briefly about that here. That is something that we still need to address. We have a user request from NRR to get into that area, and we are budgeted to do work in that area in 2004, I believe.

And Matt can get into any other details on the NRR activities, but just briefly here this was sent on -- we actually made a New Year's Eve deadline, which is maybe the first time in my career that we actually did that.

But Shipp (phonetic) was here, and he signed it out, and it went over to NRR on New Year's

Eve. We have to have our comments back by the end of March, and then looking at decision to proceed with rule making, which is -- we talked a lot about that yesterday, too.

We feel that it is warranted technically and there are obviously a lot of other concerns at NRR that we will have to consider with regard to engaging rule making activities. So that will be their decision.

Preliminary indications from discussions with the EDO and NRR are that they feel pretty strongly about this, and so that is likely to go forward hopefully in the near term here.

And that is pretty much the end of our prepared remarks, and we are happy to take any questions.

MEMBER APOSTOLAKIS: Okay. I have a few questions on the uncertainly analysis that is described in Chapter 2 of this report. In Section 2.1.6.1, it says that -- it describes how aleatory uncertainties are handled, and I understand the aleatory problem.

But then much to my surprise, it says that model uncertainties are aleatory, and also uncertainties due to incompleteness are also aleatory. So 2.1.6.1.

And I have always believed or thought that model uncertainties were part of the epistemic uncertainties. Now, you might say all you have to do is take these two paragraphs and move them to the other section that talks about epistemic uncertainties.

But actually there is more to it than that, because somewhere else it says that in 2.26, I believe, it says that parameter uncertainties which are classified as epistemic the only epistemic uncertainty in the report is the parameter uncertainties.

Now, propagated using Monte Carlo and Latin Hypercubes. The other, the aleatory, are handled by considering a best estimate, lower and upper bound, and you put some subjective probabilities.

And then there is Table 2.3 that lists some of these aleatory uncertainties. For example, the break location. We don't know what it is. The season. It says there is one-quarter probability of it being winter, and .5 being spring or fall; and .2 5 being the summer, which I think I know where it comes from.

So these are aleatory and they are random, and you can't do anything about them. But the same table is the RELAP-5 code model uncertainty is an aleatory uncertainty.

So that tells me now that if I run the code a thousand times I will get random results because it is a random code, and then if I go to what Nathan wrote in Appendix B, which was written some time ago, the interpretation that Nathan used for aleatory and epistemic, which I agree with, is inconsistent with this, because I can't believe that the code is --

MR. SIU: George, if I made, I will give my interpretation of what I see written here. And then, James, I don't know if you want to add anything to that.

I think they were referring to model uncertainty in a very limited sense, and in models in a very limited sense. They were talking about the input parameters, such as the valve area.

And when you say the valve has failed, what does that mean? So you look at different openings. That is an aleatory --

MEMBER APOSTOLAKIS: So it is the event that is --

MR. SIU: It is a boundary condition. So you could say that is part of the model.

MEMBER APOSTOLAKIS: But that's not aleatory. I mean, that is not model uncertainty.

MR. SIU: Well, that is what I am saying, is how I was reading that particular model uncertainty, as opposed to saying RELAP is off by -- you know, let's pick an arbitrary number, which may not be real at all, and let's say 10 degrees, plus or minus, standard deviation. That is differently than what this is trying to reflect.

MEMBER APOSTOLAKIS:

MEMBER APOSTOLAKIS: What is says, for example -- are you there, Vic? Table 2.3. I need you guys to look at it. For 2.3, there is no page.

MEMBER RANSOM: It must be missing.

MEMBER APOSTOLAKIS: If it is messed up, you will never fix it. Does anyone on the table have 2.3? Okay. So that I can understand the valve state, now where it says component heat transfer rate, can that be an aleatory variable?

I mean, the heat transfer rate, what does that mean, the heat transfer coefficient? Yes, sir, what is it?

DR. CHANG: This is James Chang from the

University of Maryland. When we modeled this, we considered that there is the uncertainty in the measurement of the heat transfer rate. So in our --

MEMBER APOSTOLAKIS: What heat transfer rate is that? Where?

DR. CHANG: It is the heat transfer -- well --

MEMBER ROSEN: From the fluid to the wall.

MEMBER APOSTOLAKIS: Okay.

DR. CHANG: Yes, but in doing so, we are not able to change the unified equation. Instead, we changed the heat transfer area by --

MEMBER APOSTOLAKIS: And what equation is that? You said that you cannot change the equation. What equation is that? Is it the heat equation in the code?

DR. CHANG: Yes.

MEMBER APOSTOLAKIS: Okay. So that will give you the nominal value, right?

DR. CHANG: Yes.

MEMBER APOSTOLAKIS: And you say that I believe that equation that the code uses only .9 percent of the time, but 10 percent or .8 percent of the time. And 10 percent of the time, I believe it is 30 percent less, and 10 percent of the time I believe it is 30 percent more. That is what the table says.

So there are two questions now. The first is what is the basis for these assessments, and second is that aleatory. In other words, for the same sequence and for the phenomena, 10 percent of the time it would be underestimated, and 10 percent of the time it would be overestimated? That doesn't make sense.

It is always the same value, but you just don't know what it is. So it is a mistake. It shouldn't be the same table as the others,a nd again if it is a matter of removing it from the table, I wouldn't mind that much, but you used it in your calculations.

You combined it with an aleatory, and now I don't know what happened to all of this.

MEMBER WALLIS: This concerned me, too, and when you do this, and when you make a calculation with RELAP, you get the temperature going down like this on a curve.

If you use the aleatory, it jumps around as it comes down the curve and that changes the thermal testing. Well, it doesn't jump around as it comes down.

MEMBER SHACK: Well, no, it predicts a heat transfer coefficient which you are going to use in favor.

MEMBER WALLIS: And then do you stick to that, or as it randomly changes as --

MEMBER SHACK: No, in some codes or in some cases they use the predicted value, and they say there is some uncertainty in that value, and so sometimes they use a higher value, and sometimes they use a lower value.

MEMBER WALLIS: But they use it throughout all the time, this correction?

MEMBER SHACK: No, but --

MEMBER WALLIS: Oh, you don't change it from time to time?

MR. BESSETTE: No, and so let's say we have a heat transfer coefficient for a convection model and so we put a multiplier on that of 1.3 or .7.

MEMBER WALLIS: So it is always off in the same direction? The thing that we are looking for --

MEMBER APOSTOLAKIS: No, no, and if you go to Appendix B, Nathan has a very nice figure of how aleatory uncertainties is handled. It is inside in a loop, and then the epistemic are on top.

This cannot be part of the loop, period. It is epistemic.

MR. BESSETTE: This particular table is everything that we varied, and so it is not intended to be an aleatory table.

MEMBER APOSTOLAKIS: It is not in terms of what?

MEMBER SHACK: Separate the table in two if it makes you happier, George.

MEMBER APOSTOLAKIS: Yes, but the calculation --

MEMBER SHACK: Split the table.

MEMBER APOSTOLAKIS: No, because the text says that all of these are aleatory and they are treated as such, because the epistemic are treated via the Monte Carlo. It is not just a table. The text says this is what we do.

MR. BESSETTE: Yes, and so none of these things are treated in a Monte Carlo sense. These are all treated as --

MEMBER APOSTOLAKIS: It is random, and we are taking -- right? What else?

MEMBER RANSOM: I think they made

sensitivity studies, and so they made parametric studies, although I don't understand why 9/10ths of the time that --

MEMBER APOSTOLAKIS: Well, that is another issue, but the other issue is the process issue. I mean, to put in a table things like I don't know what season of the year it will be, right, and so it is that one-quarter of it is winter. I understand that.

And then to say that the coefficient will be treated the same way, that just does not make sense to me.

MEMBER WALLIS: Well, there is a bigger question than that, is that if you are going to make this correction to the heat transfer coefficient throughout the whole transient, then you simply displace everything.

But in reality RELAP could be critically too high a heat transfer coefficient at the beginning, and to low a coefficient at the end. And that is where you get a transient with a steeper time variation of temperature.

MEMBER APOSTOLAKIS: Right.

MR. BESSETTE: Well, you know, we deal with this single -- let's say convective model. I mean, so RELAP can be wrong im the sense that it is calculating the wrong fluid velocity, which gives you -- maybe you say how can RELAP be wrong in different directions at different times in a different transient, and it is.

MEMBER WALLIS: It is wrong.

MR. BESSETTE: The way that you would obtain that in practice is somehow if RELAP is sometimes toggling too high a fluid velocity,a nd sometimes too low.

MEMBER WALLIS: Well, what I was looking for is that you said you drew these curves for RELAP predictions versus the data, which is fine. And then you have to say intellectually how am I going to represent this difference between the two.

How am I going to do that given that it has certain features, and some of it is above and some of it is below, and with time the deviation goes plus or minus. How am I going to represent that?

How do I go from that to whether it is epistemic or aleatory, and how do I treat it? And all that logic could somehow come out in the report.

MEMBER APOSTOLAKIS: And aren't you actually -- well, admittedly you are doing sensitivity analyses?

MR. BESSETTE: Yes.

MEMBER APOSTOLAKIS: How do you do that? Do you do it one parameter at a time? How do you conclude that the LOCA between 1-1/2 inch and 4 inches is a dominant scenario?

I mean, you have some something, and all you are saying in the report is that for each key PTS contributing parameter, typically three representative values are presented lower, nominal, and upper bound with corresponding predetermined probabilities are used for the assessment of their (inaudible) sensitivity indicator.

But it does not tell me how. So are you taking all the possible combinations of this table and run the code and see what happens, or are you doing one parameter at a time?

DR. CHANG: We do think one parameter at a time. So we fix -- at first we fix the break size and we select 1.5 inches, and 2 inches, and 2.8 inches, and 4 inches, and 5.7 inches, and 8 inches.

So for each break size, I varied the parameter, and at that time we changed a few other EOC water temperature, from the spring time temperature to the winter time, and then see the difference.

MEMBER APOSTOLAKIS: So when you change the component heat transfer rate, you assume that there is perennial summer, because you don't change that. If you are unlucky to have a different heat transfer rate, and it happens in the winter, then you are in trouble, because you are using nominal values for the other parameters, which really goes against this aleatory business.

Aleatory means that things are random and all sort of combinations.

MEMBER WALLIS: And you need 59 combinations.

MEMBER APOSTOLAKIS: Well, whatever it is, yes. We were all very happy when we saw what is now Appendix B that Nathan wrote 3 years ago, or 4 years ago, because that was logical, and explained how things were going to happen. But now they didn't happen that way.

MR. CUNNINGHAM: It is clear, Dr. Apostolakis, that we need to go back and look at this, and either clarify --

MEMBER APOSTOLAKIS: I thought you said Appendix B was clear, yes.

MR. CUNNINGHAM: If Appendix B was

clear, yes.

MEMBER APOSTOLAKIS: I was completely confused by this discussion here, and I thought again, thinking of my colleagues' shock, that maybe I was overreacting and that this was academic, and that you actually did things like that. So it matters this time.

MEMBER SHACK: They have the main sequence, and at least as I understand it, the thermal-hydraulics, they have been in the PRA, and that is how they get those sequences that they considered.

Then they want to consider the uncertainty associated with each of those main sequences. So they take the one-inch break, and --

MEMBER APOSTOLAKIS: No, that is not what it says. They want to characterize the variables.

MEMBER SHACK: But you do that because you are representing this whole set of scenarios by a thermal hydraulic sequence, but that one thermal-hydraulic sequence doesn't account for all the uncertainty that you have in it.

So you account for that uncertainty by considering the range of variables over which that scenario really covers for you representing 15,000 thermal-hydraulic sequences by one, but that really corresponds to a range of variables.

There is the aleatory representation that you have, because the break could occur anywhere. It could occur in winter and in the summer, and there is also the epistemic problem that RELAP may not be calculating the heat transfer coefficient properly.

MEMBER APOSTOLAKIS: Right.

MEMBER SHACK: So you include an uncertainty for that. In that sense that you have included when you do the hydraulics for that bin, you have included the thermal-hydraulic uncertainties covering the fact that you are representing 15,000 sequences by one thermal-hydraulic sequence.

And that there are things that you don't know about the -- and even if you had all 15,000 sequences, there is still things that you don't know about the sequence, like when it is going to happen in the year. And the fact that RELAP could be wrong.

MEMBER APOSTOLAKIS: I understand all of this. The question is what do you do about it? And that is not what is --

MEMBER SHACK: Well, today you have to look that it favors --

MEMBER APOSTOLAKIS: No, no, no. I am looking at 2.6.

MEMBER SHACK: Well, it is a question of how he does it in the calculation.

MEMBER APOSTOLAKIS: Yes.

MEMBER SHACK: Is he picking it randomly within -- I mean, what Monte Carlo loop is he within, and I believe that he does it so that he treats the RELAP uncertainties as epistemic, and the other uncertainties as Aleatory.

MEMBER APOSTOLAKIS: All the indications --

MEMBER SHACK: But he is probably the best --

MEMBER APOSTOLAKIS: Why do you believe that when the author says that they treat them as aleatory? I mean, why do you believe that?

MEMBER SHACK: Well, personally I don't believe when I read that report the figure of 1.1.

MEMBER WALLIS: But, George, there is another point that needs clarification. Is that when the thermal hydraulics result goes to the next step, it is treated as being a deterministic result, and it is one curve. It is not a curve, plus uncertainties.

So I am not quite sure then how the thermal hydraulic uncertainties propagate through to influence the final answer.

MEMBER APOSTOLAKIS: Okay. So there are several issues here. One is the issue of how did you come up with the 30 percent more or 30 percent less with the probability of .1.

MEMBER SHACK: Well, that is a judgment.

MEMBER APOSTOLAKIS: Right, but it can be questioned by experts in that field. Secondly, why do mix aleatory and epistemic; and why do you do a sensitivity analysis one variable at a time?

MEMBER POWERS: Because you are an idiot. It is the wrong way to do it. No, it is easy to do.

MEMBER APOSTOLAKIS: It is easy to do.

MEMBER SHACK: Sure. It is easier to do it at multi-variables at a time than it is one variable at a time.

MEMBER APOSTOLAKIS: So they chose the hard way?

MEMBER SHACK: I bet that they did.

DR. CHANG: Well, I say it is the Table 2.3 here where we changed one variable at a time,a nd then we used the first 10,000 seconds, the downcomer average as a sensitivity indictor, and from here we used a single probe to mix all of them.

MEMBER APOSTOLAKIS: You mixed them? When? I thought you said you do it one at a time.

DR. CHANG: Yes, one at a time, and that is the first set, doing the sensitivity of one parameter uncertainty, and how it could affect the PTS, yes.

And then the second step is that now we have the sensitivity of one parameter, and then all the associate probabilities, and that probability is assigned here.

And then through the all the parameters combined --

MEMBER APOSTOLAKIS: So you are going by the probability?

DR. CHANG: Yes.

MEMBER APOSTOLAKIS: But them that assumes that the dependence of the 30 models in the code is linear, because if it is not linear, then you can't do that.

DR. CHANG: Yes.

MEMBER APOSTOLAKIS: Are they linear?

DR. CHANG: Because the sensitivity would be indicated, we choose for the first and second parameter checks an average of --

MEMBER APOSTOLAKIS: Well, there again you have a problem again because you are saying now that I will take the weighted average.

So I will take 70 percent of the nominal heat transfer coefficient with a probability of .1, and multiply that by .1, and take the results for winter and multiply them by five and add the two. Well, winter is aleatory, and it is really --

MEMBER WALLIS: It is average behavior through the year.

MEMBER APOSTOLAKIS: Average is everything. Anyway, I think Mark is right.

MR. CUNNINGHAM: We need to go back and look at this, and look at it further.

MR. ROSENTHAL: This is Jack Rosenthal, Safety Systems Analysis Branch. I agree with Mark that we have to go back and regroup on this issue. Nevertheless, in preparation for this, I asked Dave please help me as we continue on.

And he pointed out to me that if you take the water from the refueling water storage tank, and you pump it through the system, and you throw it against the wall. And in the winter it is 40 F., and in the summer it is 80 F.

So that delta-40 ends up with almost the delta 40 on the wall. So we take these values, and the delta 40 F. is long compared to at least on an RMS basis how we did between RELAP and the developmental assessment calcs, and we run it through FAVOR.

And what you get is a low number in favor either way. So I acknowledge that there is some real methodology things that we have to straighten out with the report, and I think we can do it right, but my basic understanding is that we have done enough variation of parameters, and done enough FAVOR runs that the basic conclusion that we have that the PTS risk is small is robust.

MEMBER WALLIS: Jack, that's why we need some numbers of these green and red arrows, and my impression is that the effect of this thermal-hydraulics is probably a 10 or 20 percent effect.

And the effect of what you assume about the flaws is a factor of 20 to 70, and so one overwhelms the other completely. If we make that clearer, we might have more perspective on what we ought to concentrate on.

MR. ROSENTHAL: Fair enough.

CHAIRMAN BONACA: I think so.

MR. ROSENTHAL: I figured that the probablistic fracture mechanics is maybe three, or what is the magnitude on the thermal-hydraulics, and yes, we will acknowledge that we need to go back and rewrite the document better.

MEMBER WALLIS: You really need this overview document which puts the whole thing in perspective, all these things in perspective.

CHAIRMAN BONACA: I wanted to ask another question. Just because it is a rather significant contributor that has been eliminated, and we discussed this before, but I did not attend the whole meeting yesterday.

You concluded secondary side breaks are not important. So now I remember one of the dominant breaks assumed for a B&W plant in the previous analysis, and that was a steamline break, and we had run out of feedwater, and tried to isolate the primary system pressure drops.

And you had this ECCS injection, and further cooldown, and repressurization, and now you have this very severe condition. Now, I grant that there is no operator actions being assumed there, and failure of the (inaudible) isolation, and so that is understandable in that scenario, for example.

But how do you eliminate that being any contributor? Just because of operator actions in the procedures? Yesterday, you pointed out that it was not only operator actions.

MR. KOLACZKOWSKI: There are three reasons which Ed mentioned, and we will go over that again, I guess. Hopefully it will be clearer. As we pointed out in the early work, and of course the Oconee analysis that was done in '81 or '82, or whenever it was, the early '80s, that was the one that really showed the main steamline break was important.

If you go in and look at that analysis, you find that because we are dealing today in doing a 150 thermal-hydraulic bins, or as back then it was more like about a dozen, as Ed pointed out, that if you go look at the analysis, you find that essentially they took all the frequencies of things like main steamline break, and maybe a couple of multiples,a nd stuck-open turbine bypass valves, and small steamline break,a nd treated all of those events as if it was a main steamline break.

MEMBER APOSTOLAKIS: Okay.

MR. KOLACZKOWSKI: So from a thermal-hydraulics standpoint, we get this very rapid cooldown, so on and so forth, and they are dumping all these frequencies into that bin, and then obviously applying a very high, or relatively high, CPF.

That is, a conditional probability of vessel failure, because they were treating it like it was all a main steamline break. So first of all, we come along and we say we are not going to treat it that way. We are going to take a main steamline break, and we are going to put it in its bin, and have its frequency.

And that will still give us a high, or relatively high, CPF, but the frequency if we had not dumped in all these other things as if they are all main steamline breaks.

And then we have a multiple turbine bypass valve bin, and we say, okay, we are going to get its frequency, but you know what? That is a much smaller break, and so even though the frequency is higher, the CPF is a lot lower because we don't get much cooldown.

So first of all the binning, and the fact that we are not using as gross bins, everything else equal, you have already lowered it a lot because we are not treating all these frequencies like they are all a main steamline

MEMBER APOSTOLAKIS: I understand.

MR. KOLACZKOWSKI: And so that is reason number one.

MEMBER ROSEN: You're not treating all of them with the steamline breaks degree of overcooling?

MR. KOLACZKOWSKI: That's right.

MEMBER APOSTOLAKIS: So the frequency of that particular event is much lower now because of --

MR. KOLACZKOWSKI: Yes, that is reason number one. The bining itself, and the process itself, changed the numbers.

The second thing is if you just look at -- and now with all the changes that have occurred in FAVOR code and so on, and so forth, removing all these conservatisms, et cetera, if you were to take the same main steamline break back in 1980 with today's code, and now do the analysis with today's code, what you would find is that the CPFs were grossly over-estimated because of the old -- well, whatever was the precursor to the current FAVOR code.

In other words the CPF that was being predicted back in 1984 for a main steamline break, are higher than the CPF we would predict today with today's version of the FAVOR code, just because of the fact that we have removed a lot of those conservatisms in the fracture mechanics part of the analysis.

So that has lowered the main steamline break. And then finally the third thing is as you have already pointed out, Dr. Bonaca, is that the early analysis gave little to no credit for isolating, let's say, a faulty steam generator because they didn't want this to rely on necessarily human action or whatever.

And we said, okay, but we are trying to do a best estimate with uncertainty bounds on things. So as a result, we want to acknowledge that operators just aren't going to watch a steam generator blowdown and continue to feed for 30 minutes and not do anything about it.

And so we said, okay, let's give -- well, whatever we felt was the appropriate credit, and it went through the systematic process, ATHEANA, and expert elicitation, to try to put some, we hope, realistic values on what is the chance that operators would not isolate a steam generator by 30 minutes into this event.

And we all believe that probability of failure is not 1.0 based on the simulations that we have seen, and based on EOPS today, based on where EOPs were back in 1970, late, when those early analyses were done. and based on current training today, et cetera.

And that there are real reasons to provide some credit for operator error.

MEMBER ROSEN: The big change is in systematic procedures, right?

MR. KOLACZKOWSKI: Sure.

MEMBER ROSEN: Since 1970.

MR. KOLACZKOWSKI: Clearly. I mean, the systematic procedures, and so on and so forth of the higher sensitivity to PTS that we have today than we had back in 1981 when this was first all coming up, et cetera.

MEMBER ROSEN: The operators don't have to diagnose what it is. They just look at symptoms.

CHAIRMAN BONACA: And I thank you very

much for bringing that out.

MR. KOLACZKOWSKI: And I don't want to over-emphasize the --

CHAIRMAN BONACA: No, no, let me just say that for the purpose or the point that Dr. Wallis was making before, these are pluses and minuses contributors. This was a very important presentation to me, because it tells me that we are not just relying on operator action judgments, and there are other factors.

And again in the context of a report, it would be valuable to understand roughly what kind of contribution we had from these considerations. And that would take the issue off the table and convincing say, yes, let's just forget about the secondary side and cooldown, because even if what was said about human reliability is wrong, still it is a small contributor, or a smaller contributor than we thought.

MEMBER APOSTOLAKIS: I think in that context, you know, I think we were promised more than a year ago a walk through calculation. I don't think we ever saw that or I ever saw that.

So I have two comments here. One is that Mark Cunningham said earlier that this is a summary report, and so there will be a bigger report somewhere else?

MR. CUNNINGHAM: There will be supporting reports behind this, yes.

MEMBER APOSTOLAKIS: But still though I think it would be useful for the summary report to be a little more explicit.

MR. CUNNINGHAM: Yes.

MEMBER APOSTOLAKIS: Now, in addition to what I said earlier, in 2.3, it just says that we formed a team, a party, a working party, that was able to distinguish between aleatory and epistemic, period. Thank you very much.

Well, give me something, you know. And also the emphasis is too heavy on the process. We formed the party and the party did this or the party did that. I don't care what the party did. What is the method.

Second, I really would like to see a chapter or a presentation on how figure B.4 in Nathan's appendix was actually used. If you do that, I think it would go a long way towards explaining everything that was done. B.4.

MEMBER WALLIS: Well, George, there has to be a much more extensive summary of what were the procedures, and how it all hangs together, and what thermal shock is, and the fact that you have to calculate wall temperatures and so on.

And a lot of the stuff which is very good, you don't get until you get to the appendix. It has got to be right up front, and this is how we did it.

MEMBER APOSTOLAKIS: I think that figure is great. It tells how we did this, and how we did that. Let's make a sequence or something, whatever is convenient, and demonstrate how that figure was implemented, and then show the susceptibility results and the whole works.

Don't just tell me that the working party went and ate dinner last night. I mean, that is what it says in Chapter 3. Not dinner, but we formed a party to understand the physics, because this is important.

Well, you know, I never knew that the physics was important. But this is full of that.

MR. CUNNINGHAM: Between yesterday and today, we have gotten a lot of constructive comments on ways to improve the report, and we appreciate that, and we will take it to heart.

MEMBER POWERS: Let me ask a question.

I hope that I don't get over-interpreted, as it is not intended as a criticism. It is curiosity on my part. At constructing this undertaking, you did a lot of calculations on binned interim results, and then you did subsequent calculations. Why did you bin interim results?

MR. KOLACZKOWSKI: Resources. Learning as we go, and recognition that if it was pretty clear to us that some things were going to be not important at one stage, then we could begin to screen out certain portions of things that we had to model in more detail.

And/or perhaps we learned that the binning was too crude in some places, and more than what we needed in other places, and so therefore we could redo or reshuffle some of the binning, et cetera.

But clearly at the beginning, Oconee had 181,000 over-cooling sequences in the PRA model

MEMBER POWERS: Right.

MR. KOLACZKOWSKI: We could not do 181,000 thermal-hydraulic calculations and avoid binning.

MEMBER POWERS: Why couldn't you do 181,000 thermal-hydraulic calculations?

MR. ROSENTHAL: I think surely you can, and I just got new linux clusters up today, and so we can or must pull the rip cord and let it run. But would it be meaningful?

You know, I am starting out with a -- well, I don't know what, maybe 530 or 550 F. And I am not bringing it in any lower than 212 F, and so about 300 degrees, and I am doing this over a period of two hours or so.

And by the time that I have calculated a hundred ways of going from stake point A to stake point B, and I don't know if it is winter or summertime anyway outside, I would say this would be overkill on just running RELAP.

MEMBER POWERS: I said don't over-interpret my question.

MEMBER WALLIS: But there must be a systematic way of calculating 180,000 sequences to find out the reasons where --

MR. ROSENTHAL: Right.

DR. KORSAH: And to find out a grid.

MR. ROSENTHAL: Right. And I will stop after this, but in fact we did that. And the reality was that we guessed some sequences, and we were off building decks and writing models.

Then we had some PRA input, and then based on that we ran some more cases, and then as a function of time, we started getting fracture mechanics results back.

And then we had already done a fair amount of arithmetic, and we then had an integral finally closed system,a nd this was a function of time.

And at that point the PRA guys started refining their models, because now they had the fracture mechanics, and the end answer, and asking us to do more thermal-hydraulics. And that is what happened with --

MR. BESSETTE: Our first consideration at Oconee, for example, we had 20 bins, 20 RELAP bins, and this process of refinement and deciding how many we needed, we went from 20 to ultimately to about 200.

MEMBER WALLIS: Do these bins take care of the uncertainties in RELAP?

MR. BESSETTE: Well --

MEMBER WALLIS: Do the bins somehow take account of the uncertainties? The next step is a deterministic calculation.

MR. KOLACZKOWSKI: The bins really

representing the uncertainty in the event, because there is randomness in the event, and we don't know if the break is really going to be 1.8 inches or 1.9 inches.

MEMBER WALLIS: I know that, but there is a whole chapter in this report which claims that you have taken account of the RELAP uncertainties.

MEMBER APOSTOLAKIS: And that should be on top of these uncertainties, and what Alan is talking about is the aleatory, and you don't know the size and you don't know the place.

MR. KOLACZKOWSKI: Yes.

MR. BESSETTE: So we had all these bins, and what we did is that we picked the let dominant bins in which to do further uncertainty analysis with RELAP,

MEMBER POWERS: Let me just ask another question again. This is not a criticism of this particular study, but you did a lot of calculations for Oconee, and that means that you had to set up an Oconee deck. If I asked you to do a lot of calculations on Commanche Peak, how long does it take to set up the deck?

MR. BESSETTE: Well, to set up a deck, or to set up a new deck from scratch is about -- I would say two man years of work.

MEMBER POWERS: Two man years of work?

MR. BESSETTE: Yes.

MEMBER WALLIS: Doesn't the Commanche people already have a RELAP deck?

MR. BESSETTE: No.

MEMBER WALLIS: But they have a deck of some sort.

MR. BESSETTE: We don't, no. They don't have a deck.

MEMBER WALLIS: They don't have it?

MR. BESSETTE: No.

MEMBER SHACK: So even after you get TRAC-M, you still have to wait years to point out decks to --

MR. BESSETTE: Well, we don't come anywhere close to having a deck for each plant. We have decks for perhaps 10 plants or so.

MEMBER SIEBER: Even that is a lot.

MR. KOLACZKOWSKI: Let me make a comment about this and why we make the statement that the T-H uncertainties are covered, and I agree that we have not probably proved the point.

But let me just say that I think we believe that the uncertainties in RELAP and its ability to really match experiments, we believe that uncertainty is small, and I grant you that we absolutely have not proved that point sufficiently.

But we believe it is small compared to these things like is the break really 2 inches or 4 inches. That is going to so swamp we believe the uncertainties of the T-H calculation of what a 2 inch response should be, or what a 4 inch response should be, that from that sense, that is why we are qualitatively saying in the report that we believe that the T-H uncertainties have already been enveloped by the ones that we have looked at, because we believe those are larger, and have a greater effect.

MEMBER WALLIS: It is just a question of shielding?

MR. KOLACZKOWSKI: I understand that, and that's why I am saying that I think that we have not proved the point, but I think that is why the statement is there, is that we believe that the T-H uncertainties, in terms of the code uncertainties, are small relative to this randomness of is the peak really going to be six inches or three inches.

MEMBER APOSTOLAKIS: Does this apply also to the probablistic fracture mechanics uncertainties? Are there any uncertainties there? I mean, I appreciate the Marshall distribution, the flaw distribution, but are there any model uncertainties?

MEMBER WALLIS: If you look at the RELAP clause, and any other data --

MEMBER APOSTOLAKIS: What kind of model of uncertainties would you have?

MR. HACKETT: I would take a crack at that. The model uncertainty there is several sources, One, of course, is the one that has been referred to most often here today, would be the flaw density and distribution, and we do have a model there that does explicitly address uncertainties.

And as well as we could do it weighted on the data that we had, as opposed to extrapolations with expert codes, or expect elicitation. That is one. The other model is of course the one that we have spent a lot of time debating here today, and that is on the toughness model and that we did not get into that today, as opposed to what is the measure of truth in this situation.

And the bottom line there is that we did go into this in a fair bit of detail yesterday and you are trying to get an estimate of the fractured toughness of this material, for which RTNDT is but a -- I have to admit is a bad surrogate for that here.

It is what you are stuck with by the historical way this thing played out. So you are trying to get to fracture toughness with this RTNDT, and the imperfections that lie therein.

And there is a model that goes with that, which ultimately traces back to the development of the master curve approach for fracture toughness. And we could spend a lot of time on that,

but there is a model there, and epistemic and aleatory uncertainties that go along with that. The last major piece would be --

MEMBER APOSTOLAKIS: And these are represented somewhere?

MR. HACKETT: Yes, they are in Appendix A.

MEMBER APOSTOLAKIS: Appendix A?

MR. HACKETT: That's right. The last major piece I will just mention is the embrittlement model. which we have spent more time than anything else on between us and the industry.

And in terms of how do you get from

throwing neutrons at a vessel of certain composition and how embrittled it ends up and we have that covered in there, too.

MEMBER SHACK: However, they do believe that fracture mechanics is written in stone. That when Kmaterial equals Kapplied, things break.

MR. HACKETT: Correct.

MEMBER APOSTOLAKIS: And these uncertainties are evaluated?

MEMBER SHACK: When you look at the uncertainties in the embrittlement model, and the uncertainties in the material toughness model, you can make Alan's argument that they ought to swamp any other model.

MEMBER WALLIS: Just look at some of the parts, George. I mean, you have a curve and you have the data, and just take a look at those.

MEMBER APOSTOLAKIS: Yes, but I thought that what Alan and others were saying was that the aleatory uncertainties are overwhelming here. But there is epistemic and aleatory?

MEMBER SHACK: There is aleatory and epistemic.

MEMBER APOSTOLAKIS: But the epistemic I would suspect would be more significant there.

MEMBER POWERS: To be precise, there are aleatory uncertainties in the material properties, and there are epistemic uncertainties in fracture mechanics models.

MEMBER APOSTOLAKIS: yes, yes.

MEMBER WALLIS: And most of the RTNDTs are a very weak surrogate for toughness, but it is the thing that is being used.

MEMBER APOSTOLAKIS: Yes, but what I am asking is the argument that was made that the thermal-hydraulic uncertainties are overwhelmed by the uncertainties in the LOCA size and so on, right?

MR. KOLACZKOWSKI: And perhaps other things in the fracture mechanics.

MEMBER APOSTOLAKIS: So the fracture mechanics are up there? Okay.

MR. HACKETT: In that case the modeling for the flaw density and distribution, and the toughness, I think overwhelm that, too. And we do -- and Dr. Shack raises a good point, in terms of in the fracture mechanics, you are assuming that the fracture mechanics truth in this thing is still a Kapplied versus a Klc type of thing, which takes you back 20 or 30 years in fracture mechanics technology.

And Professor Apostolakis asked a good question there, too, that in terms of -- well, does that work pretty well for this case, and we feel that it does, because you have got a big thick vessel that is about the best way of coming at that type of fracture mechanics that you are going to get, a big thick vessel with a thermal shock.

And that is not to say that you couldn't apply elastic plastic fracture mechanics as a refinement to this thing. And we do in fact do that when we look at low upper shelf welds, for instance.

And that is a whole different problem, but when you are looking at cleavage fracture in a big thick steel component, that is probably still pretty good.

MEMBER POWERS: When are we going to be able to do elastic plastic fracture mechanics routinely?

MR. HACKETT: We do it now. I think we are back to the same kind of point that Jack was making on the binning. It is really a resources issue more than anything.

And Terry Dickson is at the microphone, and I think I can say that by adding elastic plastic fracture mechanics into FAVOR would -- and I will let Terry comment, but it would greatly complicate the computational aspects of the analysis. Terry, did you have some comments?

MR. DICKSON: Yes, but to my knowledge that is on the agenda to do. That is where we kind of go from here. Everything that has been discussed here is based on a linear elastic plastic fracture mechanics model.

And I was going to address the question by Dr. Apostolakis --

MEMBER POWERS: Before you go on to that, do you have some sort of -- is there somewhere a strategy written down on how to evolve our fracture mechanics?

MR. DICKSON: We are working on that right now. But the expectation is that by including the higher constraint plasticity models is that that will be a removal of conservatisms,a nd that these numbers will go down. That is the expectation going in.

MR. HACKETT: Let me come to a little bit more background on that, because the elastic plastic fracture mechanics has also been around for 20 plus years at least, and there are some major analyses that the NRC and the industry have done in terms of qualifying low upper shelf welds for operational performance that is governed by 10 CFR 50, Appendix G, that are indeed based on elastic plastic fracture mechanics.

And with this case there just was not a need to go there as Terry is indicating, but that is future work.

MEMBER POWERS: That's fine. What I am really asking about is what is the Agency's plan to develop its fracture mechanics technology, and whether or not it is applicable to this problem.

MR. HACKETT: Correct. Yes.

MR. DICKSON: I can't speak for the NRC, as I work at Oak Ridge National Laboratories, and we are a contractor, but I know that our plan, and I believe it has been coordinated with the NRC, is that we will be developing a version of FAVOR that includes elastic plastic fracture.

MEMBER POWERS: If there is some sort of a plan on this, it would just be interesting for me to see.

MR. HACKETT: We will make note of that and we will -- Mark Kirk in fact has the lead for developing that right now, and we will make sure that we bring that forward.

MEMBER POWERS: I mean, it is one of those areas that if we are to be supportive, it would be nice to know what the plan is. And it may not be this year, or next year, or five years, but if we have a plan, then we can do things that are supported.

MEMBER WALLIS: Plastic is fine, but then you will get down to the business of what is a flaw, and you said you were using the worst flaw, which is this sort of a razor-like atomic sized flaw that cuts its way through in the worst possible way.

MR. HACKETT: That's correct.

MEMBER WALLIS: And that must be a very conservative assumption.

MR. HACKETT: It is certainly a conservative assumption. Even elastic plastic fracture mechanics does not address that. You are still assuming these atomistically sharp flaws. So that is probably there for the foreseeable future.

MEMBER WALLIS: But that is a conservative assumption?

MR. HACKETT: Yes.

MEMBER WALLIS: George seems to be satisfied, and I would only add to your statement, George, that you need to be shown the thermal-hydraulic uncertainties are swamped by these other ones. But it has to be shown though. It can't just be stated. There has to be a rationale.

MEMBER APOSTOLAKIS: I would like to see though a sequence of calculations all the way through the beginning to the end.

MR. HACKETT: And just as a comment, I have the same recollection as Dr. Apostolakis, and I have been off on another rotation loop here at the NRC, and I have been out of the loop in this project for a while, but I do recall a commitment that we had to do that with the Committee.

And I don't believe for some variety of reasons that never happened.

MEMBER APOSTOLAKIS: It never happened. I am not chairing.

MEMBER WALLIS: How far along are we in this presentation?>

MR. CUNNINGHAM: I guess we are -- I guess if I could wrap up again. We talked earlier that we were interested in a letter from the committee, and we are at the point where we think we have a reasonable technical basis to recommend to NRR that they proceed to rule making to make some changes to the pressurized thermal shock rule to reflect over what we have learned over the last X years in terms of the frequencies of PTS types of events.

So we would be interested in a letter from the committee either endorsing this research idea, and that it is a good idea to proceed to rule making, or some such thing. And again any other comments that you have in that regard, we would be happy to get them.

I am sure that we will be back talking to you, and perhaps Matt and the NRR folks will be the lead the next time we are here.

MEMBER WALLIS: Well, when we were waiting for the train last night, we said what you really need is sort of an external writing committee, which is not so tied up with the work, and just see the details of what you have been doing, and they can present the whole thing in a way that is sort of a half-inch report that tells the whole story.

MR. CUNNINGHAM: Okay. We will look into it.

MEMBER WALLIS: And if you want to know the details, you look somewhere else.

MR. CUNNINGHAM: Okay. We are going to

look into that.

MEMBER POWERS: Mark, one of the hallmarks of this PTS work has been bringing together experts in PRA fracture mechanics, human factors, thermal-hydraulics, people that ordinarily don't speak even similar languages, and producing a product.

And I guess I have been unabashed in my admiration about the way that that was done. Have you had a chance, or will you take the time to go back and assess how easy that is, and what would facilitate those things, and the multidisciplinary activities?

I think you have done this one extraordinarily well, and it sets a high standard for subsequent people coming along, and it might well be useful to set down for people who subsequently try to organize these efforts things that make this an attractable approach

MR. CUNNINGHAM: I think that is a great idea. I think we obviously -- or maybe you didn't see it, but there was some rocky times in this project trying to interweave different disciplines. Many people speaking many languages if you will, and I think we can learn from that.

MEMBER POWERS: I think it is one of the few instances where I have seen matrixing actually work, and that comes from a laboratory that prides itself on doing that, and I don't think we did it as well as you guys did for this particular study.

MEMBER WALLIS: Well, I take a bit of issue with you. Almost all engineering is interdisciplinary in some degree, and you can over-estimate or over-state this division between disciplines, and the different languages.

And in fact it is possible for someone knowing a PRA to have some idea on what is going on in thermal-hydraulics and so on. There are lots of common approaches in all engineering.

MEMBER POWERS: Well, as I said, I spent most of my working career at a laboratory where we try to do a lot of that, and I am always stunned at how difficult it seems to be to do these multidisciplinary things, and I think this team has really done an outstanding job on this.

I attribute it a lot to the personalities involved, and Ashok, I think you are to be congratulated for a heck of a good undertaking here.

MR. THADANI: Thank you.

MEMBER POWERS: Thank you.

MR. HACKETT: I think a comment that I would add, because I see that Dr. Powers' comment is going towards sort of a managerial issue, too, and this in my opinion has been one of the better efforts, if not the best effort that I have seen managed from within the Office of Research.

And in that regard a lot of credit does go to Ashok Thadani's management team, in terms of providing the resources and lining things up so that other things got out of the way when it came time --

MEMBER POWERS: We would never say something like that. It would go to their head, and they would be insufferable.

MEMBER WALLIS: I am astonished by you are saying that this is one of the difficult interdisciplinary projects, and that it is managed better than one of the purely disciplinary ones. I don't think you mean that.

MEMBER APOSTOLAKIS: Say thank you very much.

MR. HACKETT: I will say thank you.

MEMBER SHACK: We are ready to wrap it up.

MEMBER ROSEN: Are we going to have a

committee discussion?

MEMBER SHACK: We will have it later on today as we get ready to consider the letter, and we will have a discussion.

CHAIRMAN BONACA: So at this time we will just recess for 15 minutes until 3:15.

(Whereupon, at 2:59 p.m., the meeting was recessed and resumed at 3:17 p.m.)

CHAIRMAN BONACA: Okay. The meeting will come back to order. And we have now a review of the draft final version of Regulatory Guide DG-1077, Guidelines for Environmental Qualification of Microprocessor-Based Equipment Important to Safety in Nuclear Power Plants, and I believe that John Sieber is going to walk us through.

MEMBER SIEBER: Okay. Thank you, Mr. Chairman. As Mario said, we are going to consider draft Regulatory Guides DG-1077, and the title is, "Guidelines for Environmental Qualification of Microprocessor-Based Equipment Important to Safety in Nuclear Power Plants.

This draft reg guide builds on the environmental qualification guidelines and the rule to which it all refers is 10 CFR 50.49, and Reg Guides 1.89, and 1.180, and IEEE Standard 323-1983, and the International Electrotechnical Commission Standard 60780, all apply.

And the foundation work is contained in two Oak Ridge studies, NEUREG CR 6741, and 6479. The staff provided the ACRS a copy of the draft regulatory guide on June 8th, 2001 prior to publishing for public comments.

At that time the ACRS declined to review it, deciding instead to wait until the comments were received and incorporated. And so now we have come to that point in time.

So the ACRS, other than through mailings has really not had a chance to review the draft regulatory guide that is the basis of these documents except for what we will have this afternoon.

There actually were a significant number of comments received by the staff from 11 commenters, and there is a staff analysis which is proprietary and therefore not a public document, which includes the technical analysis of the comments, and a description of changes that were made to the draft reg guide to bring it to its final form as it is today.

Among those 11 commenters, one that had

a particular large number was Winston & Strawn, which is a Washington law firm that represents the Nuclear Utility Group on Environmental Qualification.

And there were a number of comments which the staff's resolution and technical analysis took about 29 single-spaced typed pages. And so those are listed there.

Winston & Strawn has asked for time to make a statement during this meeting, and I think I will call upon them right now to make that statement.

MR. HORIN: Good afternoon. I appreciate the opportunity to provide a brief statement with respect to our comments on this draft guide. As mentioned, Winston & Strawn represents the Nuclear Utility Group on Equipment Qualification.

We are a group of utilities that are comprised of over 90 of the operating power reactors in the United States.

We are supported by a technical consultant who has been involved in environmental qualification of electrical equipment for over decades, and is the author of a number of papers, the EQ Reference Manual, published by EPRI.

We submitted comments as mentioned, and we have not had the opportunity to see the resolution of those comments. So I want to keep my statement brief here, and hopefully we will have an opportunity to look at the resolution of the comments prior to any finalization of this draft reg guide.

Unfortunately, our technical consultant is out of the country and cannot be here, and so I am standing in as a lawyer, and so I will limit my brief comments to a couple of regulatory points.

We have provided copies of our comments to the committee, and as mentioned, they were rather extensive and dealt with a number of technical issues, and a number of regulatory questions.

I wanted to make a couple of key points, and then I will sit back and listen to see where the reg guide has gone in a revised state. I think most fundamental to our comments is a concern that there has been an approach taken in the draft guide which would confuse the overall regulatory scheme with respect to the environmental qualification of electrical equipment under 10 CFR 50.49.

And again I am referring to the draft

guide that was issued for public comment. Principally among those concerns have to do with the confusion of the applicability of 50.49 to equipment that is in mild environments, versus equipment that is in harsh environments.

50.49 applies to electrical equipment that is in harsh environments, which is specifically defined in that guide regulation as environments which are significantly more severe following a design basis event than during normal operation of, and we are not talking about environments or conditions which are slightly different, or not any different at all.

They are -- 50.49 is geared towards the harsh environment qualification. Secondly, with respect to mild environment qualification, there is guidance, and there is a clear direction within the current regulatory scheme with respect to mild environment qualification.

That guidance is contained in the Standard Review Plan, and that guidance is part and parcel of an overall scheme that would apply to quality assurance criteria, design control criteria under Appendix B, coupled with design analyses for particular applications that are already within the regulatory scheme.

So we had some fundamental problems with the way that the draft guide characterized certain effects as being either aging effects, or effects that would be seen that would create a harsh environment, because they are effects which are not necessarily more severe following a design basis event.

So those type of clarifications are important, because we think that if they are not clarified, and if there is not a clear distinction maintained between harsh and mild equipment, this draft guide, again as we saw it, would be wholly inconsistent with 50.49.

And to the extent that there was an attempt to proceed along those lines would direct or practically necessitate that there would be a whole rule change under 50.49.

So we don't see that as drafted that this was consistent with the existing regulatory scheme. We have some comments with respect to backfit issues, and we will make sure that those are addressed in the context of CRGR, and fundamentally our recommendation here was that certainly as drafted this guide should be withdrawn as a reg guide.

It just simply did not provide a clarity of direction or consistency with the existing regulatory scheme necessary to on its own address these issues.

Alternatives may be whether it is issued as a separate NEUREG document, or perhaps an RIS to address some of these questions, but nonetheless, we felt that this was not an appropriate mechanism to apply these particular considerations.

And we also -- and I don't want to go through all of it this afternoon, but there is an extensive number of comments that sounds as though there has been an extensive resolution, or at least an effort to address those, but again we have not seen that.

So we don't know whether it ends us. But I appreciate the opportunity just to point this out to the committee. Hopefully we will have an opportunity to take a look at how these comments have been addressed in the past. Thank you very much.

MEMBER SIEBER: Okay.

MEMBER WALLIS: I am wondering if you planned that this whole thing is unnecessary and unwarranted, it would seem that no change to the draft would satisfy you.

MR. HORIN: We think that the use of this as a regulatory guide without significant modifications to make it consistent with the existing regulatory scheme would make it unwarranted.

MEMBER WALLIS: You see to claim that the resisting scheme is so good that we don't need to do anything.

MR. HORIN: I think if you read our comments that there are a few elements that really establish matters that cannot already be addressed under the existing design processes for nuclear power plants.

MEMBER SIEBER: I perhaps should not give advice here, but we are going to give advice anyway later on, is that it is either come out with a new guide or modify the existing guides, because there are some differences.

And I think that is pretty well established through the work, and so what I would like to do is to introduce our speakers, and after I give your names, please correct me after I am done, and except for Mr. Wood, where I think I am on safe ground. But Christina Antonescu; is that correct?

MS. ANTONESCU: That's right.

MEMBER SIEBER: And you are from NRR.

MS. ANTONESCU: No, from Research.

MEMBER SIEBER: Okay. And Kori Korsah; is that correct?

DR. KORSAH: Yes.

MEMBER SIEBER: I got it right. How about that, and they will be our speakers this afternoon. One of the things that I would like to ask you to do is that the significant part of what we are about this afternoon will be to address these comments, and so to the extent that you can do that.

And there are too many of them to do them all, and that you may want to choose some of the more important points that have been made by the public to actually explain what it is that you did, and what the staffs position is on that, and why you think that we ought to agree with you.

So with that, Christina, I would like for you to begin.

MS. ANTONESCU: Before I introduce myself, I would just like to let you know that the presentations were organized such that we address the resolution of the public comments, and the subsequent viewgraph presentations will actually address most of these questions.

And if you will allow us, then we can proceed with an overview of the reg guide, and most of your questions will be answered as well.

MEMBER SIEBER: I think that would be helpful

MS. ANTONESCU: Good afternoon. My name is Christina Antonescu, and I am in the Engineering Research Application Branch in the Division of Engineering within the Office of Research.

My background is in electrical engineering, and I have worked at NRC as a project manager in the field of instrumentation and control for the past 11 years.

I am here today to present to you DG-1077, and DG-1077 describes an acceptable method for environmental qualification for microprocessor-based systems.

The draft guide was released for public comments on October 14th, 2001, and we received 11 submissions from the public. After interaction among the staff, the technical support contractors at Oak Ridge National Lab, and industry stakeholders, the draft was revised to reflect resolution of the public comments.

So the purpose here today is to present to you the guidance contained with this DG-1077, which describes the need and the benefits of the guide. And at the end of our presentation, we would like to request a letter from the Committee endorsing publication of the final effective guide.

Before I proceed, I would like to introduce other branch members in attendance. Mr. Steven Arndt, who is the team leader in the I&C Group, and our branch chief, Mr. Dan Dorman.

And our counterparts in NRR I think is represented by Mr. Paul Loeser today. And again I would like to briefly introduce our supporting contractors, Dr. Richard Wood and Dr. Korsah Kofi, from Oak Ridge National Lab.

Dr. Wood is the project manager for the I&C projects that we sponsor at Oak Ridge. He has a Ph.D. degree in nuclear engineering from the University of Tennessee, and has 20 years of experience with instrumentation and control technology.

Dr. Wood is currently contributing to an advisory committee of I&C experts that is providing research recommendations to the Office of Nuclear

Energy in the Department of Energy.

And Dr. Korsah is an investigator for the I&C Qualification Project at Oak Ridge National Lab. He received his Ph.D. in nuclear engineering from the University of Missouri, and has 30 years experience in the I&C Research and Applications.

In additional, Dr. Korsah has served as a member of IEEE working groups on criteria for computers and safety systems IEEE 7.4.3.2, and for environmental qualification IEEE 323-1983.

Following these remarks, I will present an overview of the draft reg guide, and Dr. Wood will describe the technical basis supporting this guidance.

We do appreciate the opportunity to appear before you today, and we look forward receiving the benefit of your insight. So if there are no other questions, I would like to give you a brief presentation or highlights of DG-1077.

The first part of this high level introduction is the overall of the reg guide and follow-up by the technical basis for environmental qualification that Dr. Wood will present. And then Dr. Korsah will summarize th value of DG-1077 and its benefits.

Let me give you a high level on what BG does, and the main scope and what it applies to. It endorses current consensus of environmental qualification standards for safety related microprocessors of these systems.

And the main regulatory position in endorsing the guidance in IEEE 323-1983 for qualification of safety related microprocessor basic equipment for service in nuclear power plants that are subject to conditions and clarification.

And it also endorses the guidance of IEC 60780, and so DG-1077 applies to new or modified safety related systems in existing or future nuclear power plants that employ microprocessors equipment, or not already applied to installed equipment.

MEMBER WALLIS: Could you explain -- one of the criticisms of the previous speaker was that this was unnecessary ,and that you already had sufficient rules and guidance, and so why is it that this is necessary in view of what the present system is, and what are the inadequacies in the present system?

MS. ANTONESCU: If you look at the subsequent view graph presentations, they will clarify your question.

MEMBER WALLIS: You will clarify that question later on.

MS. ANTONESCU: So if we can proceed, then we can systematically go.

MEMBER WALLIS: That seems to me to be the main thing on whether or not it endorses, and what problem does it solve is the real question.

MS. ANTONESCU: Right, and we are going to answer all your questions.

MEMBER SIEBER: There is an interesting aspect to this. Right now in U.S. nuclear power plants, there is not to my knowledge any safety related microprocessor based equipment and harsh environments. Is that correct?

MEMBER WALLIS: That's true.

MEMBER SIEBER: So this really applies to modifications, upgrades, and totally new construction of advanced reactors, and I think that one of the reasons here that you endorsed an IEC 60780, which is a European standard, and I think based mainly on the fact that suppliers may be of European heritage.

And therefore equipment that is built in Europe to satisfy European requirements can't be used in the U.S. unless we endorse the standard, or they change their standards.

So this is the use of an international consensus standard as a way to allow for a greater degree of competition, and choice among licensees. And lacking that, I think that the only thing that would apply is 323, which may require some changes or upgrades in that equipment. Is that correct?

MS. ANTONESCU: Well, I just want to reiterate that if you allow us to go through that you will understand the reason why we find it necessary to also present to you for our endorsement or to provide you the technical basis for endorsement of IEC 60780.

DR. WOOD: I think your comment about the European suppliers is valid, and that was one of the motivations as to why we needed to or we felt the need to also look at the European standards.

There is also a move within the entire U.S. Government to look at more than just national standards, and I wanted to take this opportunity to point out that this is not specifically to satisfy the Code of Federal Regulations 50.49, because the environmental qualification is not limited to the rules and regulations within 50.49.

So that is why we have this and we will

talk about that later.

MEMBER SIEBER: There is a general design criteria that says that this stuff has to work during an accident, and so that is really what the basis is in my view.

DR. WOOD: And there is even more than that, and we will talk about that in the presentation.

MEMBER SIEBER: All right. Go ahead.

MS. ANTONESCU: So why do we need to review DG-1077? We will talk about these things in more detail in our presentation, but I wanted to let you know up front what DG-1077 can address. It is a response to a user need request and --

MEMBER WALLIS: But your response could have been that you don't need a new reg guide.

DR. WOOD: had that proven to be the case, that would have been the response.

MS. ANTONESCU: Yes. It addresses unique characteristics of microprocessor-based equipment that we think should be addressed, and it endorses consensus of national and international standards, and existing reg guides limit the scope to harsh environments, but we want to include all environments.

And also potentially regulatory burden arises from case by case treatment of qualifications from the environments. A recent review of topical reports continue on a case by case qualification from environments, and vendor qualification programs were accepted under three separate SERs; from Tricon, Common Q, and Teleperm.

So instead of having one process, at this point we are reviewing it case by case. The resolution of public comments, we had again 11 public comments submitting comments on DG-1077, and the public comments can be grouped into a group of categories, and we tried to group them into four categories.

And these will be addressed in subsequent slides. The need for guidance, and whether the existing guidance is sufficient, and the application of location categories, and how location categories tend to be applied.

And the scope of qualification, and that is the full scope of environment conditions, mild and harsh. And the backfit analysis. The staff's position is that there are no backfit associated with this guide, and as described in 10 CFR 50.109, because there is no change in licensing basis for existing equipment.

And it only applies to new equipment, and voluntary modifications. And now I would like to turn the next presentation to Dr. Wood.

DR. WOOD: Thank you. I think that the comment that we received prior to these presentations highlighted perhaps one of the most frequent comment that were received in the public comment and that deals with the need for guidance.

So I thought for the technical basis that we would start with the basis for qualification, and walk through that, and then hopefully illustrate why the staff believes that this guide is both necessary and useful.

So to begin with the Code of Federal Regulations, Title 10, Part 50, requires environmental qualifications of safety related systems.

Specifically, structures, systems, and components important to safety must be designed to accommodate the effects of and be compatible with the environmental conditions which they will face.

And design control measures such as testing and other quality control activities should be used to verify the use of that design. The primary -- I'm sorry, that would make it a little easier to follow me. The other way. Sorry.

In any event the discussion in the regulatory guide was modified from the version that was released for public comment to try to more systematically step through the current regulatory requirements and the guidance that is given for those, and then highlight the need for this particular guide.

Part 50.55(a) dealing with protection systems provides embedded requirements for environmental qualification of all systems important to safety, and all protection systems.

And in that it by reference includes the requirements of IEEE 603, which specifically states that environmental qualifications shall be performed to confirm the conservative nature of the design and that it can accommodate the environmental conditions.

Then the specific rule that was mentioned in the comments prior to these presentations, Part 50.49, deals with environmental qualifications of electric equipment important to safety that are to be implemented in harsh environments.

And we will talk a little later about the scope of 50.49, and we are not intending to expand the scope of 50.49. Our purpose is to address the full scope of all of the regulations that are --

MEMBER POWERS: As I understand it, there are no microprocessor-based systems in harsh environments now; is that correct?

MEMBER SIEBER: yes, but it is just a matter of time.

MEMBER POWERS: So that means that arguments that the current regulatory process is stable is not applicable here; is that correct?

DR. WOOD: That is I guess part of our belief.

MEMBER WALLIS: Are these harsh environments under normal operations or under accident conditions, or what?

DR. WOOD: Harsh environments that are addressed under 10 CFR 50.49 are severe environments that are subject to design basis accidents.

MEMBER WALLIS: So something like a LOCA break?

DR. WOOD: Yes. Things that are characterized as mild environments, some of them we would consider severe environments.

MEMBER WALLIS: Temperature and humidity, and things like that.

DR. WOOD: Well, mild covers a big range, and that is one of the areas that we will talk about a little later.

MEMBER SIEBER: I guess to my mind that is why you ended up with three different categorizations.

DR. WOOD: Exactly.

MEMBER SIEBER: As opposed to two, which is what, 323.

DR. WOOD: That's right, and I will talk a little later about how the intent of that is to provide some --

MS. ANTONESCU: Relaxation of 323 for mild environments.

DR. WOOD: Exactly.

MEMBER POWERS: When I search out to apply 50.49 and to understand what a harsh environment is, I should take into account LOCA kinds of accidents and what not. Do I also take into account anticipated fires?

DR. WOOD: That I would have to defer to some of our colleagues. It is not specifically identified, and there is no definition within the Code of Federal Regulations of a harsh environment.

There is a definition of a mild environment, and fires are mentioned.

MEMBER SIEBER: In your report, you mentioned the effects of smoke.

DR. WOOD: Yes.

MEMBER SIEBER: On the other hand, you don't qualify to a fire environment as I read it.

MEMBER POWERS: That is what I was going to get out. Your report is remarkable to me, in that you come along and say, gee, smoke can affect these things, and we know that, but we don't know how to test for that.

You know, we don't have a standardized test for that, and so we are going to ignore the issue, and have you punted on the most important issue here?

MS. ANTONESCU: We are going to minimize it and treat it under design, minimize the susceptibility, and treat it as a design issue.

DR. KORSAH: Also, the other thing is that qualification against fire and so forth, but fire basis is under Appendix R of the Code. So that is --

MEMBER POWERS: Appendix R does not address smoke issues outside the immediate fire zone. And one of the things that this committee has kept asking about repeatedly is that if we have a fire and we disperse smoke beyond the fire zone into the regions where you have digital electronic equipment, do you have a long term problem.

And do the components of the smoke cause a long term degradation of these low voltage systems such that we encounter a difficulty not at the time of the fire, but 6 months later.

DR. WOOD: I think that -- of course, we address how we had originally intended to deal with smoke in a position that was subsequently deleted, because in response to public comments, and that dealt with multi-tiered protection.

Design and implementation approaches that could be utilized to minimize the potential susceptibility of equipment to things like smoke.

MS. ANTONESCU: The intent was to take credit for the specific design approaches that can mitigate the susceptibility to environmental effects.

DR. WOOD: The difficulty that we faced in taking the research information, the findings, and converting that into relevant guidance for the industry is that as you mentioned.

There is no means right now to test whether or not a piece of equipment or in its installed configuration is or is not susceptible to smoke, because there is so many variables that can't be controlled.

However, the other difficulty that was presented is that while the research indicated that certain implementation techniques would be of benefit, there hasn't been a full-scale investigation of all of the possible ramifications of certain things, such as conformal coding, and what might that do to temperature susceptibility.

So it is difficult to recommend implementation guidelines.

MEMBER POWERS: I think I am very sympathetic with the challenge it had there, because as I look at the experimental database that is available, it looks at a very acute smoke exposure, and my reaction to it is fine.

You know, I am glad that you found this stuff out, but when I read Appendix R, I have wiped that equipment out anyway. It doesn't seem to address this long term chronic problem where I have smoke constituents degrading contacts, et cetera, with these materials and what not.

And so I think I must appreciate our argument that says we just have not found the information that is of the breadth that we need for this kind of guidance. I think I am much more sympathetic with that than the apparent wording that says we are going to punt on this, okay?

On the other hand, I say, gee, I have people from the Navy and people from the Army telling me that we don't want smoke to affect our systems, and I see novel designs, especially for surface naval vessels now, where they are confronting this issue in novel ways that I won't go into here on the public record.

But I see other people confronting it, and it might be something that you can put on your to do list, and not for this regulatory guide, but maybe for the next one and what not, because it looks like people are trying to confront this issue.

MEMBER SIEBER: Well, maybe I could give my thought here a little bit. It seems to me that long term failures due to smoke would be very random in nature, you know.

A piece of the equipment would fail

today and another piece two weeks from now and so forth, and the single failure criteria would seem to me to provide a sufficient degree of defense in depth.

DR. WOOD: I can give an example of how that very point was considered. In the research, different fire scenarios were investigated to determine which were the most credible, and then assessed to determine which would provide the most harsh smoke environment.

And a small in-cabinet fire provided the most severe conditions.

MEMBER SIEBER: That's right.

DR. WOOD: And that would be localized.

MEMBER POWERS: Ask the people at Oconee.

DR. WOOD: Yes, I know. Exactly.

MEMBER SIEBER: The density is --

DR. WOOD: Yes, I know, and for reactor protection systems that would affect one channel, and the general fires, because of the fire protection that is engaged, would be detected early. There would at least be knowledge that they had occurred, and then maintenance practices could assess whether or not any of the electronics had been affected by smoke.

The one where you might not know it had happened, and it might not detect it until something failed, would be int eh in-cabinet fire, but that would be in most instances, unless you have an extreme coincidence, localized to the one cabinet.

MEMBER POWERS: Yes, but is a localized one cabinet, and if you produce a lot of smoke and it gets distributed by the HVAC system either during the event or in the subsequent recovery, then is it a more broad issue then?

DR. WOOD: There you run into the separation of the air supplies among different cabinets. You might affect two cabinets, but not all four, but certainly we recognize that there are still a lot of questions that could be asked in investigations that could be conducted.

MEMBER SIEBER: It seems to me --

MEMBER WALLIS: Tell me about the smoke, and what was referred to as specific components in the smoke, and presumably there are aerosols that have water and carbon particles, and so forth. Will they cause effects of electrical coactivity on this rather small space component, and parts of these components?

Do they penetrate and cause local corrosion of structural circuits?

DR. WOOD: Yes, it is conceivable that those things could happen. What we found int he actual physical tests of equipment exposed to smoke is that high density particles or high density of particles of where the effects occurred, and very low density tended -- the equipment tended to be fairly robust.

MEMBER WALLIS: But density you mean the number of particles per cubic meter in the smoke or something like that?

DR. WOOD: Yes.

MEMBER WALLIS: And does size matter?

DR. WOOD: I can't say based on my recollection whether there was any investigation on the size of the particles themselves. Different materials were burned and so there were different sized chemicals and particles released.

MEMBER WALLIS: There was a scientific basis for evaluating these effects then?

DR. WOOD: The telecommunications industry does a lot of research about the susceptibility of equipment and corrosion effects that would occur in the long term.

DR. KORSAH: And also typically during the measurement of doing the scientific measurement is try to make a second -- you know, leakage currents and so forth, and so forth and so on. The other effect is the smoke in conjunction with the humidity and the environment would form some kind of acid, and corrode the metal interconnections and so forth. So that is another effect of the smoke.

MEMBER SIEBER: On the other hand, most of these components -- computer chips, for example, are coded to avoid contact between the smokey atmosphere and the metallic portion of the circuit.

And they also try it seems to me to make more low impedance of the circuits than low impedance circuits so that leakage of currents don't have the impact that they would if you were involved in all high resistance circuits.

DR. WOOD: And I think that highlights some of the implementation of things that can be done, and that was the motivation for that position that I mentioned that was deleted in this version.

MEMBER SIEBER: It would be difficult to test for, because there are so many variables, and there are different kinds of smoke, and different humidity conditions, and different air flows, and so it would be a complex test.

MS. ANTONESCU: Exactly.

MEMBER POWERS: All you are telling me is don't use microprocessor systems.

MEMBER SIEBER: Right now they aren't.

DR. WOOD: I think what we should highlight is that we didn't investigate as a purpose the susceptibility of analog components, but by no means are we saying that digital or microprocessor-based components are more susceptible by definition.

MEMBER WALLIS: Is there a short statement that you have about the need for this new guide?

DR. WOOD: A short statement?

MEMBER WALLIS: To impress upon us quickly about the need for this new guide?

DR. WOOD: Let's see. I have a tendency to be long-winded, and so it is very difficult for me.

MEMBER POWERS: I think -- I'm operating from my recollection, but I think if we look at the Digital Electronics Research Plan that they had a nice piffy paragraph that explained why this work was being done, and maybe Steve could recall that from memory.

DR. WOOD: I can give you our short statement here that Ms. Antonescu went over. First off, we feel that the unique characteristics of microprocessor-based systems need to be addressed, and I have a subsequent slide that talks about those unique characteristics.

So one thing that this guide does is provide that specific guidance in one location. Some of that guidance is scattered among various guidance documents.

We feel like that leads to a case by case basis as everybody discovers in each application what it is that I need to do. Instead of being able to go to a specific guide. There is no existing endorsement of the current national or international consensus standards. That is one thing that this guide provides.

MEMBER WALLIS: And these are specific standards for microprocessor equipment.

DR. WOOD: These are specific standards for qualification of equipment.

MEMBER WALLIS: Microprocessor.

DR. WOOD: Of equipment.

MR. DORMAN: Just to clarify. This is Dan Dorman, Research. It is no endorsement of those consensus standards for microprocessor-based equipment for the range of environments that are considered in this guide.

DR. WOOD: Yes. If you take all of these together, you get the bigger picture, and I will show you the bigger picture is a few words as soon as I finish this discussion.

The comprehensive regulatory guide as Dan mentioned dealing with all environments, there is that comprehensive guide dealing with harsh environments, Reg Guide 1.89.

But as it was mentioned applications currently today of microprocessor-based equipment are in what are called model environments. We visited Taiwan last fall, and they are working on a microprocessor-based system for containment environments.

It is not in the far-distant future when microprocessors will move into containment, and then the other issue was the case by case basis. But these last four bullets are the reasons that motivated the development of this guide.

And so rather than going through all of these in detail, these next two viewgraphs basically highlight the distribution of guidance among different documents, and I won't go through this in detail, but I would like to point out the last bullet on this slide.

The DG-1077 is intended to provide a road map for existing guidance that is applicable to microprocessor-based equipment. So you go to one source, and there it is. You don't have to decide should I infer from the guidance to the reviewer in the standard review plan some things that I needed to do.

Do I have to go to the staff position in NEUREG-0588 and derive some additional information; and then do I go to IEEE323, and then what do I do for model environments. Chapter 3 and Chapter 7 have some differences in what they do, because they apply to different kinds of equipment, and that is in the standard review plan.

CHAIRMAN BONACA: Now, the letter from (inaudible) does not object to having a regulatory guide as an umbrella. The next two specific objections says that new regulatory positions contained in the draft guide include expanding the scope of 10 CFR 50.49 to apply to (inaudible) model environments.

And concluding that EMI/RFI is both an

environmental condition and a significant aging mechanism. Those are two specific objections.

DR. WOOD: Those two specific objections, the objection about the expansion of the scope of 10 CFR 50.49 resulted from a result of a lack of clarify in what the guidance that went out for public comment, and the public comment highlighted to us the need the make it more systematic in the presentation of what is the purpose.

CHAIRMAN BONACA: So your intent is one of expounding it?

DR. WOOD: That's right.

CHAIRMAN BONACA: So you don't have an issue there.

DR. WOOD: Exactly. And regarding EMI/RFI, there was no intent to identify EMI/RFI in general as an aging stressor. But EMI/RFI, and all the electromagnetic conditions in a plant, are part of the environment of the plant, and this is a position that is consistent with the IEC standard, and it is treated as a condition.

It is also a position that is being adopted by the United States because the revision of IEEE 323 includes EMI/RFI as a listed service condition.

MEMBER SIEBER: Well, there is a reg guide for that already.

DR. WOOD: That's right.

MEMBER SIEBER: 1.180.

DR. WOOD: It's inclusion in this reg guide is to reflect consistency between the IEC and the IEEE standard, and to remind people not to forget EMI/RFI, and not to provide full guidance on EMI/RFI.

The position provides a pointer to Reg Guide 1.180, and also a pointer to EPRI 102323, as both providing guidance on how to address this specific issue.

CHAIRMAN BONACA: So you don't feel that even on this issue that you do have a conflict?

DR. WOOD: That's true.

MEMBER WALLIS: If this is a harsh environment, it seems to me that harsh is defined, or a harsh environment is defined by what it does to a particular thing and in a particular context.

And if you simply look at an environment which has a significant effect on the behavior of a microprocessor, that by definition is a harsh environment for a microprocessor.

It may not be harsh for other things, but I don't see why you need to make this distinction.

If it affects the function of that device, then it is a harsh environment.

CHAIRMAN BONACA: I think it is more than that. It is the practice of how the harsh environment is (inaudible) --

DR. WOOD: Yes, there is a lot of semantics involved in it, and part of the fuzziness of the semantics is the semantics are the reasons that we went to the location categories.

MEMBER SIEBER: Right.

DR. WOOD: And I think the public comments illustrated that we were not effective in conveying that. So hence the revision with additional information.

MEMBER SIEBER: Well, you defined Category A and Category C, and Category B as everything else.

DR. WOOD: Everything in between. Now, to be fair to the commenters, there was much more conservatism in the boundaries between the representative conditions in the version that went out, and there was great value in the public comments and highlighting that we needed to give consideration to what would make this practical to implement without adding a burden, rather than reducing a burden.

So we tried to do that. This is an illustration of environmental qualifications. Some of the comments, or many of the comments that we received dealing with the need for guidance illustrated a great deal of diversity in understanding what environment qualification is, and when does it apply. When do you have to do it, and what do you have to do.

These are two views of environmental qualification. One is looking at the environment in the plant, and so you have all environments, and the rule that requires environmental qualification are given in 10 CFR 50-55(a)(h), and then demonstrating that you have accomplished the design criterion in GDC04, General Design Criterion-4, and that you accommodate the effects of, and are compatible with, the environment.

Normal operation all the way through. Harsh environments are a subset of that, and as I said earlier, there is not an explicit definition of harsh environments in the Code of Federal

Regulations. There is a definition of mild environments.

MEMBER WALLIS: Well, you could expand to fill the whole space available.

DR. WOOD: That's right. But 10 CFR 50.49 specifically addresses harsh environments. It notes that mild environments, qualification for mild environments are beyond its scope, and it doesn't say that you have to qualify for mild environments. It says that it is beyond its scope.

So that is the plant environment viewpoint. Now, where do microprocessors fit into this right now? They are in that larger bubble outside the harsh environments, but they are moving toward the inner-bubble, and part of the vision for this guide is to anticipate that, and have the guidance in place, rather than reacting.

MEMBER WALLIS: Is there likely to be an environment that will affect their performance?

DR. WOOD: Yes.

MEMBER WALLIS: I'm really just playing with words about whether it is harsh or not.

DR. WOOD: That's right.

MEMBER WALLIS: As they are not very important to me.

DR. WOOD: The harsh and mild really are in sort of standard and regulatory space. If it has an effect, it is a significant environment.

MEMBER WALLIS: Right.

DR. WOOD: And then looking at it from the equipment point of view, the Class 1E equipment point of view, you have got all the electrical equipment which are within the scope of 10 CFR 50.49, and then you have got microprocessor-based equipment which are a subset of that.

But all electrical equipment -- I'm sorry, the all electrical equipment expand beyond the scope of 50.49, because there are Class 1E electrical equipment that are not implemented in harsh environments.

So the next viewgraph is intended to sort of illustrate what is the role of DG-1077. You have the electrical equipment and harsh environments, which is the regime of Reg Guide 1.189, and you have the microprocessor-based equipment in all environments, which is the regime of BG-1077.

And then you have got this small overlap that right now is almost non-existent, but eventually it will become populated, where you have microprocessor-based equipment in harsh environments.

And then in that case you have DG-1077 and you have the conditions in Reg Guide 1.189. If you don't have DG-1077, you don't have explicit guidance about all of the blue part of the small bubble.

And also you don't have added to Reg Guide 1.189 the specific considerations for microprocessor-based equipment.

MEMBER WALLIS: So Reg Guide 1.189 wouldn't really handle this cross-hatched region is what you are saying?

DR. WOOD: Not absolutely. We think that there are some considerations that need to be addressed that are in the various sources of guidance, but you have to go ferret them out.

MEMBER WALLIS: And so it is a question of difficult to find rather than they aren't there?

DR. WOOD: I think that the reviews of the vendor topical reports on the various systems indicate that the major vendors know where those things are, but the concern is there are some subtleties, and you want to make sure that all vendors can be aware of what they need to do.