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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMITTEE

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479TH MEETING

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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THURSDAY

FEBRUARY 1, 2001

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ROCKVILLE, MARYLAND

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. George Apostolakis, Chairman, presiding.

COMMITTEE MEMBERS:

GEORGE APOSTOLAKIS, Chairman

MARIO V. BONACA, Vice Chairman

DR. THOMAS S. KRESS, Member

GRAHAM S. LEITCH, Member

DR. DANA A. POWERS, Member

DR. ROBERT L. SEARLE, Member

DR. WILLIAM J. SHACK, Member

JOHN D. SIEBER, Member

COMMITTEE MEMBERS: (CONT.)

ROBERT E. UHRIG, Member

GRAHAM B. WALLIS, Member

ACRS STAFF PRESENT:

JOHN T. LARKINS, Executive Director

ALSO PRESENT:

RALPH CARUSO

F. CHERRY

N. CHOKSHI

F. ELTAWILA

JOHN FLACH

WILLIAM JONES

MARK KIRK

RALPH LANDRY

SHAH MALIK

JOCELYN MITCHELL

GARETH PARY

NATHAN SIU

MOHAMMED SHUCIRI

ERIC THORNSBURY

EDWARD THRON

JARED WERMIEL

HUGH WOODS

A-G-E-N-D-A

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

(8:30 a.m.)

CHAIRMAN APOSTOLAKIS: The meeting will now come to order. This is the first day of the 479th meeting of the Advisory Committee on Reactor Safeguards. At today's meeting the committee will consider the following.

Treatment of uncertainties in the elements of the PTS technical basis reevaluation project; Siemens S-RELAP5, Appendix K Small Break LOCA code; proposed ANS standard on external events PRA; repriortization, proposed resolution of genetic safety issue 152; design basis for valves that might be subjected to significant blowdown loads; and proposed ACRS reports.

The portion of the session associated with the Siemens code may be closed to discuss Siemens' Power Corporation's proprietary information. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

Dr. John P. Larkins is the designated Federal Official for the initial portion of the meeting.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. A transcript of portions of the meeting is being kept, and it is requested that the speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so that it can be readily heard.

While this is my first day as the Committee's Chairman, and I think the first thing we should do is thank Dr. Powers, who is just joining us --

(Laughter.)

CHAIRMAN APOSTOLAKIS: -- for the superb job that he did the last two years leading this committee. Thank you very much, Dana.

(Applause.)

CHAIRMAN APOSTOLAKIS: I would also like to thank my colleagues for electing me chairman of this committee. I have been a member for about 5-1/2 years now, and I have served under three chairmen -- Professor Searle, Dr. Kress, and Dr. Powers.

And although their managerial styles were somewhat different, they all had one common objective, namely to make sure that this committee provided sound technical advice to the commission in a timely manner, and I can only promise to try to do the same.

Sadly, today, and this week, happens to be the last meeting of Professor Searle. He is completing his second term on the committee. Bob, I'm sure I am speaking on behalf of all of the members for the committee when I say that we will really miss you and your wise advice.

And finally also during the 5-1/2 years that I have been a member, I must say that I have been very impressed by the professionalism of the ACRS staff under the able leadership of Dr. Larkins. I believe it should be on the record that this committee could not function without the support that we are getting from the ACRS staff.

DR. SEARLE: I may have some remarks to make, but I imagine that there will be a more appropriate time a little later to do that.

CHAIRMAN APOSTOLAKIS: Whenever you want, Bob. Are there any comments or statements that other members would like to make?

DR. SEARLE: I would like to make a brief statement. For the benefit of the members of the commission staff that are so intimately involved with the ACRS, but whom we have interacted with from time to time, I would like to express my own personal appreciation and admiration for the way in which pretty much across the board they have conducted themselves and interacted with the committee.

I have always received the utmost cooperation when I was in a position where I had to work with them, and I guess the thing is that I believe we have a climate of mutual respect, and so that does the process well.

We talk about issues and we don't talk about personalities, and I am so very pleased to have had the opportunity to work with all of them. And since some of them are here, in here anyway right now, I just would like to say thank you very much.

CHAIRMAN APOSTOLAKIS: Very good. Thank you, Bob.

DR. KRESS: I think it is a shame that Bob is leaving, just when I am hearing how to speak Texan.

(Laughter.)

DR. POWERS: And need to learn how to speak Texas.

(Laughter.)

DR. SEARLE: Well, I am still working on Tennessee.

(Laughter.)

CHAIRMAN APOSTOLAKIS: Our first topic today is treatment of uncertainties in the elements of the PTS technical basis reevaluation project. Dr. Shack, I believe you will lead us through this.

DR. SHACK: Okay. We had a subcommittee meeting on January 18th, where we had a fairly detailed discussion of the treatment of uncertainties in the PTS project, and as Tom Kress has pointed out before, not only is PTS important in its own right, but this we think is sort of a prototype example of the kind of detailed treatment of uncertainties one may need in other situations.

What is unique about this is the attempt to integrate the treatment of uncertainties in all aspects of the problem in the framework that uncertainties have been treated typically in PRAs. And I think we saw substantial progress at the subcommittee meeting in the treatment of how they were handling the aleatory and epistemic uncertainties in the fracture toughness.

We saw some work towards the treatment of uncertainties in the thermal hydraulics, and I believe we are going to get an overview of the approach to the treatment of uncertainties from pressurized thermal shock in the presentation before the full committee today.

But again this is a work in progress. We are not really expecting to write a letter at the moment. This is a chance to sort of see how they are working their way through it, because they are breaking new ground here and it is not a conventional treatment of uncertainties that we are looking at. With that, I will turn it over to Mike Mayfield, I guess, to start, and Nathan Siu.

MR. MAYFIELD: Good morning. We appreciate the opportunity to come to the main committee again, or the full committee, and talk about the progress that we are making on this project.

As Dr. Shack pointed out, this is -- we are starting to move back some of the frontiers, at least in our traditional treatment of probablistic fracture mechanics as it relates to the structural integrity of major components.

I just wanted to set the stage a little bit, and then turn it over to Nathan Siu to work through the rest of the presentation. We just wanted to remind you first of all that the objective of the program is to develop a technical basis for the potential revision to the PTS rule.

We are not telling you or the public that we necessarily will revise the PTS rule. Our activity at this point is to look at the technical basis to see if there is a justification for revising the rule, and what that might look like, and then to make recommendations to the Office of Nuclear Reactor Regulation, since the rule making function is their responsibility.

DR. POWERS: Could you remind me what prompted you to undertake this daunting task?

DR. SHACK: Sure. We have seen some improvements -- well, the PTS rule itself is based on technologies from the late '70s and early '80s. We have made some major improvements in a number of areas.

For instance, in my business in particularly, the probablistic fracture mechanics area, our understanding of embrittlement and fracture toughness, and the flaw distributions that were a major source, and in fact the major source of uncertainty in the original analyses.

So we felt like based on some very limited scoping analyses, we felt like there was a strong basis to undertake a more rigorous treatment or reevaluation of PTS.

The notion at that point was that -- and I think going back to the Yankee Row evaluation, that there was significant conservatism embedded in the PTS rule, and the analyses that are in Reg Guide 1.154.

So we thought that the technology had improved to the point or matured to the point that we should take that up and revisit those technical underpinnings.

This is the first major application of risk informed methodology to what has been characterized as an adequate protection rule, and while I don't want to get into a debate on what we mean by adequate protection, the rule has been characterized as such, and it has to do with backfitting -- the regulatory piece of it has to do with backfitting requirements.

It was originally promulgated as an adequate protection rule when we revised it in the mid-1980s and early '90s, and it was again treated as an adequate protection rule.

But the fact that we are now taking a look at it in a risk informed approach is causing us to examine what we really mean and how we go about dealing with that.

So that will create for us some additional dialogue with the committee, I expect, as we go along. We are evaluating four plants in an effort to develop a generic approach that will cover the fleet of PWRs.

We recognize going in that evaluating only four plants and trying to use that as a surrogate for some 80ish PWRs brings with it certain stretches of faith, and that is something that we are taking on as a specific activity somewhat later in the program.

But it is an uncertainty in what we are doing. The four plants we are looking at are Oconee, which is a BMW design, Calvert Cliffs, and Palisades are CE designs, and Beaver Valley, Unit 1, is a Westinghouse design.

We do not intend to do plant specific evaluations for the entire PWR fleet. That is well beyond our resource capability, and it is not something that is credible for us to undertake.

We do feel that by looking at this small sample that we can at least improve on the basis for the rule that is out there today, which is based totally on stylized transients, and no plant specific features.

We are looking to use the best available tools for the analysis, the tools that exist today. We are making some advances in some of the tools, but we are not looking to make major improvements in some of the underlying technologies.

And that we felt like the improvements

that have been made since the time the PTS rule was originally promulgated that improvements in the technologies up to this point provide a sufficient justification, and it is not a practical thing for us to undertake major revisions to thermal hydraulics neutron transport calculations, and that sort of thing.

So we are using the state of the technology by and large as it exists today. As Dr. Shack pointed out, this is one of the continuing series of briefings that we feel have been very useful in bringing the committee and keeping the committee up to date on what we are doing.

And we are soliciting your feedback as we go along. We didn't want to get into this project, which is a major resource investment for us, and spend a couple of years working on it, and get to the end only to have the committee say, well, you have missed these key issues.

So we wanted to try and solicit your input along the way, not so much in an effort to get your preendorsement of the program, but to help solicit your input, and if you identify something that we are missing, then we can get that fixed as we go along, so that at the end of the day we have a complete package that the committee can review.

DR. SHACK: Just to come back to Dana's question a little bit. If you were convinced that the analysis was conservative, what would be the impetus for revising the rule? Only Palisades is going to hit the screen criteria, at least under current projections, right, for 40 years?

MR. MAYFIELD: Well, that's true. For Palisades, it was a bit over 40, and there are a small number of plants that would be approaching the screening criterion out to 60. There is, however, a fair bit of uncertainty within the licensees, or at least it has been expressed to us, about what else would the staff do with new embrittlement correlations.

As the committee probably knows, very small changes in chemical composition for the wells, and our understanding of the chemical composition for the wells can make very large changes in the estimates of embrittlement of the vessel.

So that is something that the -- I think the licensees would like to see a little more stability in what we are doing, and to remove the unnecessary conservatism.

So we originally took this on with the

notion that there was a fair bit of unnecessary conservatism embedded in the rule, and to try and bring it back, and base it on credible technology, as opposed to conservative estimates made out of ignorance. So that was motivating it initially, okay?

DR. SHACK: Okay.

DR. SEARLE: Mike, you made the comment earlier that this is based on existing technology.

MR. MAYFIELD: Yes.

DR. SEARLE: At the same time, I think it is worthwhile to recall that there were places where specifically focused activities took place and addressed what were at the time to be considered to be concerns. Namely, as I understand it, the ENDF(b)(6).

MR. MAYFIELD: Yes, sir.

DR. SEARLE: And the cross-section rendering grew out of a concern for the way in which iron was -- and some other things in that neighborhood, were being treated in the attenuation calculation.

MR. MAYFIELD: That's correct.

DR. SEARLE: So there have been some rather focused efforts in various areas to address and identify issues of that sort?

MR. MAYFIELD: That's exactly right. The

cross-section libraries was one effort. We have published -- and in fact the committee reviewed in December a regulatory guide on neutron transport calculations and improvements in the way that we go at that, and the way that uncertainties are handled in those calculations.

We have made improvements in the way we do the fracture mechanics analyses, and some of the underlying models there. We have got a much better handle on embriddlement trends today, and the flaw distribution work.

So there have been over the last 10 years a number of major undertakings to improvement the state of the technology.

DR. SEARLE: If there are -- I have become aware of a problem. Some people who are involved with the ASTM code group, apparently had questions concerning the attenuation calculation and submitted a large number of questions, which were apparently not addressed, at least not to their satisfaction, in the draft or in what is -- well, 1065, or whatever that number is.

Anyway, the Red Guide that was published just recently, and I believe that George received a communication on this concern, and I thought it had been passed along to other people in the commission.

MR. MAYFIELD: I'm sorry, but you are catching me cold today here.

CHAIRMAN APOSTOLAKIS: I'm not sure I follow.

DR. SEARLE: The thing that we talked about here last week. I'm sorry, but --

MR. MAYFIELD: Historically, there has been some disagreement over how attenuation is handled, and it is an issue that we agree that the technical basis needs to be revisited.

I think it would be fair to say that the basis for arguing for a change is not a lot stronger than the basis arguing against the change.

DR. SEARLE: I'm sorry for blindsiding you.

MR. MAYFIELD: Well, I will be happy to talk to you about it, and see where we can go. The key issue that we are here to talk about today, and that we met with the subcommittee on a couple of weeks ago, is the treatment of uncertainties and treatment of uncertainties in the major areas of the analysis.

We are also going to, in addition to giving you an overview, we are going to try and deal with some of the questions and comments that were raised during the January 18th meeting.

And with that, Mr. Chairman, I would like to turn it over to Nathan Siu to do the bulk of the briefing.

MR. SIU: Good morning. I will first give you an outline of what I am going to talk about. You have quite a few slides in your packet, and a number of those are backup slides. So don't worry about the length of it. We can obviously tailor the presentation to the time that we have got.

But what I would like to talk about first of all are the objectives and the conceptual approach regarding the treatment of uncertainties. Sometimes we are going to get a little -- well, not muddled, but we can't avoid the general issue of how is the integrated analysis proceeding, because the uncertainty analysis is an integral part of the overall analysis.

And there will be times when we are talking about in general how is the overall computation and how we will proceed. But I would really like to emphasize the treatment of uncertainty is within that computational flow.

I will give you an overview of how we are proceeding with the analysis, and we will try and provide some of the details that were or the information that was perhaps lacking in the subcommittee presentation, where we provided a high level framework, and then some of the bits and pieces, and didn't talk to well to how those two link together.

We will talk about the status of the major discipline activities in PRA from a hydraulics and probablistic fashion mechanics, and time permitting, we will get into each of these areas in a little bit more detail.

And in particular we have developed some draft results from the Oconee study. We have talked these results with Duke Energy a week or so ago and received comments on that.

These are highly draft results, but we wanted to give you a sense of how things are proceeding. In the case of the TH analysis, again, obviously we have TH results. Runs have been performed, and we have time temperature traces, pressure time traces.

The approach refers again to the treatment of uncertainties, and how we are going to deal with uncertainties in those computations, and similarly we are going to talk about the approach we are using for fracture mechanics.

And some of these things we are getting draft results, but that we are not ready to present at this point in time. At the end, I will then have a discussion of key issues and summarize where I think we are.

Okay. Again, this part of the analysis, we are assessing uncertainties in the estimates of PTS risks. That means that we are going to quantify the uncertainties, and also of course try to identify the driving sources of those uncertainties.

And the reason that we are doing this is to support the technical basis for a potential rule change. So, for example, we would be looking at potentially new screening criteria, and potentially new guidance for how you do a plant specific analysis if the screen criteria are not met.

This just illustrates a conceptual diagram of how the screen criterium might be developed, and shows the roles of uncertainties here, where this is the RT-PTS, and the RT and DT of the license, and this is the through wall crack frequency.

You might have estimates of the through wall crack frequency for a given plant, and the uncertainty bins about that estimate, and somehow we need to develop a line that relates the two, and develop a screening value for RT-PTS based on some notion of what is an acceptable through wall crack frequency.

That is conceptually how we might approach it. Again, please don't read too much into that diagram, because we have not put a lot of work into figuring out we are really going to proceed. But this shows how the uncertainties will play into that kind of process.

DR. POWERS: Could you explain to me better what the significance of the lines on either side of the square are?

MR. SIU: The dashed lines?

DR. POWERS: No, the --

MR. SIU: Oh, this would be the through wall crack frequency, or let's say the mean estimate, and then maybe you would have a 95th percentile, and a 5th percentile. So it indicates the range.

DR. POWERS: How do you decide to use 95 rather than 99, or 80, or 2, or --

MR. SIU: Well, that's part of my problem. We have not really gone through the work of figuring out exactly how we are going to use these estimates.

DR. POWERS: How do people in other

contexts decide what to use?

MR. SIU: In other contexts?

DR. POWERS: Yes. I mean, you haven't done it here, but do we know -- I mean, some people put like standard errors are the length of those bars, and they calculate a variance, and they put the square root of the variance on either side for a bunch of measurements.

It escapes me exactly what the probability is on that, but maybe it is like 82 percent or something like that, and other people would use 95. I mean, how do you decide? Is that a subjective decision entirely, or --

MR. SIU: I imagine that it would be because one of the things that we will get to is that these error bars are going to include the computed uncertainty.

There will be uncertainties that we don't think that we can calculate very well given the current state of technology, and in particular model uncertainties associated with some of the codes that we are using.

So I think what you are going to get realistically is an estimate of the computed uncertainty, plus a description of uncertainties associated with other issues -- maybe sensitivity calculations, but some indication of what the full range of uncertainty might be.

So to -- I don't see necessarily -- and again we have not worked this out, but I don't see just coming up with a simple rule that says pick a 99 and you are done.

DR. POWERS: But I think you have really answered my question.

CHAIRMAN APOSTOLAKIS: Have we used percentiles in any other situation? My impression is that we are using mean wise -- is that true -- when we allocate or when we decide that the contribution from this particular accident is such and such, why --

MR. SIU: Excuse me, George, but this is a screening criterion. This is the first step.

CHAIRMAN APOSTOLAKIS: So it is as screening guide then?

MR. SIU: Yes. They are trying to just say that if you meet a certain embrittlement level -- and that is the current rule right now, but if you meet a certain level, and if you don't come up to that level of embrittlement, you don't have to do anything more.

CHAIRMAN APOSTOLAKIS: I see. Okay.

MR. LEITCH: The three data points there represent three different vessels, or is that at a different time and --

MR. SIU: Yes, as the embrittlement increases, RT-PTS would increase. Again, how we are going to match up with the four plants, which can have very different results, and how we are going to generalize to the larger population, these are big questions.

The analysis -- to do the uncertainty analysis now, we have to categorize the sources of uncertainty, because that is built into notions of which kind of matrix we will be using.

We have to construct an aleatory model, and I believe we briefed the committee about the basic notion of aleatory and epistemic uncertainties in the PTS analysis.

And we then have to propagate epistemic uncertainties through the aleatory model, and I will try to walk you through that in a fairly high level manner.

Conceptually, how we might approach this is that we would develop event sequences, using a PRA event sequence model to identify what are the potential challenges to the vessel, or scenarios that could challenge the vessel.

And measure certain frequency, and let's call that lambda, and there is uncertainty about that frequency. That is the epistemic uncertainty about the perimeter lambda, and lambda is the measure of the aleatory uncertainty.

That result -- and again this is conceptually. We get fed into a thermal hydraulics analysis, where for each PRA scenario we identify a number of thermal hydraulic subscenarios, a different variance on that PRA scenario; and perhaps differences in timing of actions.

We would have to develop distributions for the probabilities of each of these variance, as well as distributions about the thermal hydraulic characteristic variances that we care about. For example, the pressure and temperature over time, the temperature in the down comer.

Using that information, we would feed into a stress strength analysis, where you look at the stress on the vessel, which is a function of these perimeters, the temperature and the pressure. Therefore, that of course would be uncertain as well.

And you compare that against the strength, which has its own uncertainties, and develop a distribution for the conditional probability of vessel failure given this scenario, and subscenario, and integrate the results together, and get a through wall crack frequency with probability distribution.

And without getting into the details too much, there is something obvious here. This could be a common for an explosion here as you develop more thermal hydraulic subscenarios to append on to the PR scenarios.

And that would require a lot of thermal hydraulic analyses, and then you would have to feed those into the stress strength analysis, which also has its own variance.

We are not doing it that way for obvious reasons. It's just that we can't do the computations to this level. So let me talk a bit about some of the simplifications we are employing.

CHAIRMAN APOSTOLAKIS: How much of this was done in the original analysis?

MR. SIU: Not formally. My understanding is that there were sensitivity analyses, but there were no -- for example, even PRA uncertainties in the event sequence frequencies were not computed. This was back in the early '80s.

MR. MAYFIELD: The original rule had none of the things that Nathan just talked about. They took some stylized transients and did what amounted to deterministic calculations.

Then there were some on the side probablistic fracture mechanics calculations done, where they tried to include some of the Monte Carlo scheme, including variations or distributions on flaws, on chemical compositions, some of the more obvious variables to include.

But it was nothing as elegant as what is being talked about here. There were subsequent analyses, called the integrated pressurized thermal shock analyses that Oak Ridge performed, and looked at three plants.

And those analyses looked more like what we are doing today. But the treatment of uncertainties was not as rigorous as what we are trying to do today.

MR. SIU: And that is an important point. Again, just because there were resource constraints, the time that it actually takes to run a thermal hydraulic calculation, maybe on the scale of hours, but also the pre-and-post processing requirements -- you get a result and you have to look at it and make sure it makes sense before you go forward with it.

DR. POWERS: Why is RELAP5 and not the consolidated NRC, the hydraulic code, being used for these analyses?

MR. ELTAWILA: This is Farouk Eltawila from research. We actually are doing the analysis using both codes, but we have not finished the consolidation completely right now.

So once we complete the consolidation -- so we are doing it, but we are relying on the lab because it has gone through a lot of assessments, and the consolidated code has not gone through this rigorous assessment at this time.

So eventually once we finish all the calculations, we are going to run with the final version of the consolidated codes.

DR. POWERS: So at some point in time, we will get a comparison between the two?

MR. ELTAWILA: Absolutely.

DR. POWERS: It may not be part of the GS effort, but at some time we will get to see how well the --

MR. ELTAWILA: Well, actually the analysis is done also at this time with the consolidated code, but we are focusing for the purpose of the rule making change, we are going to rely on the RELAP5 calculations.

DR. POWERS: I mean, that's fine, but we will get to see it sometime?

MR. ELTAWILA: Yes.

DR. POWERS: That's good. That's good.

MR. SIU: Correct me if I am wrong, Farouk, but even with the consolidated code, I imagine I can get significant resource requirements for a particular run?

MR. ELTAWILA: There is no doubt about it.

MR. SIU: So for that reason, we need to obviously use the standard strategy of being similar sequences to represent the results of the PRA analysis with a very limited set, a relatively limited set of thermal hydraulic times.

DR. POWERS: Can you tell me how you decide a sequence is similar?

MR. SIU: We have rules for doing that. I wasn't prepared to get into the details of the rules, and we can chat about that as we -- a little bit later perhaps, or if I haven't answered that by the end of the presentation, I will make sure that we come back to it.

We did present that at the subcommittee, and we had provided samples of the rules that we were using. Another issue with how we are approaching this, in terms of that conceptual model -- remember I showed you bins for the uncertainties about temperature and pressure over time.

Part of the uncertainties in those bins of course comes from model uncertainties in principle. We don't yet have well established techniques for dealing model uncertainties. There are a number of proposed approaches.

We have done some initial work in that area, and so I think at this point it is fair to say that the formal methods are under development, and we can chat about that a lot.

But again for the purpose of what we are trying to do -- and that gets back to Mike's point about using available technology. This is one case where we are not trying to push the envelope very hard. We are trying to use what we have got.

One of the reasons, of course, is that we have limited data now to really apply the methods that we have got if you want to use, for example, a basing approach to estimate the -- to quantify the model uncertainties, we would like to have some data to use as part of that quantification process. And the amount of data relevant for these sequences is highly limited.

CHAIRMAN APOSTOLAKIS: So you will come back to this?

MR. SIU: I wasn't planning to, but we can talk about it now if you would like. This is just a limitation, and so because of this limitation, this is how we are approaching the problem.

We are certainly going to quantify perimeter uncertainties. We are dealing with boundary conditions. Again, things like -- you can call them perimeters and a time when at which an action occurs, and variations in that.

Submodels to some extent -- for example, if you are talking about flow through an opening, we can deal with that. But talking about -- let's say RELAP5 is an assemblage of submodels and of course uncertainty is associated with that assemblage.

There is uncertainties with the nodding, and uncertainties with the application. These ere things that we are not addressing in the quantitative analysis at this point.

We are not planning to, and we are of course going to supplement whatever information we have with the results of experiments to address issues that were raised in the subcommittee, for example, about the possibility of a thermal plume.

And we are also going to perform selective sensitivity studies. So we are not going to just accept things directly as is, and in fact a comparison with the consolidated code probably would be another case of providing some benchmarking.

But again I think this is where we are going to have the qualitative discussion of uncertainties, as well as the quantitative discussion.

CHAIRMAN APOSTOLAKIS: But when you say submodel, I remember in one of the earlier presentations there was a diagram that said here we are using the correlation and we are not so sure. You are going to have an uncertainty about the correlation itself?

MR. SIU: Yes, at that level, because we can translate that relatively simply into a boundary condition kind of representation, and I think that there is enough information on that particular submodel that this issue is perhaps of less interest, the issue of limited data.

CHAIRMAN APOSTOLAKIS: But we have never really heard how you are going to do that, right? I mean, you never presented that, right?

MR. SIU: This is still frankly under discussion.

CHAIRMAN APOSTOLAKIS: Okay.

MR. SIU: Given those simplifications, this is a variance on a diagram that can be seen before. I have tried to put -- there is a lot of information here that we don't have to get into at this point, but again it shows the PRA event sequence analysis, the thermal hydraulic analysis, and the probablistic fracture mechanics analysis.

The key point here is just simply the banding idea, that we are taking sequences, and we are banding them into a small number of thermal hydraulic bins, and then possibly reexpanding those bins to account for a variance in the -- let's say the boundary conditions just as a simple example.

There are uncertainties in all of the perimeters and that's why I have the little pi there to represent the epistemic uncertainties. These are being propagated through the analysis, and that gets fed into a stress strength analysis, where the stress now is a function of the deterministic temperature and pressure traces here.

CHAIRMAN APOSTOLAKIS: And this is a generic pi, right?

MR. SIU: This is a generic pi, yes. It is not the same pi.

CHAIRMAN APOSTOLAKIS: It is not the same pi? Okay.

MR. SIU: But again the point is that for each of these thermal hydraulic subscenarios, we have a defined trace here. We don't have the bands anymore, and we try to accommodate the bands through the definition of these subscenarios, but this is a limitation in the approach that we are taken.

CHAIRMAN APOSTOLAKIS: I'm sorry, but I didn't follow that. What is it --

MR. SIU: In the conceptual model, we have the uncertainty bands, and let's say about temperature.

CHAIRMAN APOSTOLAKIS: Right.

MR. SIU: What you have here instead is a single trace that is dependent on your definition of the scenario. Let's say that instead of 10 minutes for the operator to throttle HPI, it is 9 minutes. It won't be to that fine level of detail, but that is the kind of idea.

Conceptually, you could have, of course, different bands, and we are trying to accommodate those variance through a discreet number of subscenarios. And a consequence of that is that we get basically a stress calculation for the deterministic pressure, temperature, and of course the heat transfer coefficient, and --

CHAIRMAN APOSTOLAKIS: And so for the same thing --

MR. SIU: Let's say P-1.

CHAIRMAN APOSTOLAKIS: -- where you take scenarios 1 and 3, and that's one bin?

MR. SIU: This is one bin, that's right.

CHAIRMAN APOSTOLAKIS: According to the previous conceptual model, you would run the thermal hydraulic analysis, right?

MR. SIU: In the conceptual model --

CHAIRMAN APOSTOLAKIS: And you would have an uncertainly around P and D. Now, instead of doing that, you are running three cases, right; is that what this means?

MR. SIU: Yes. Don't take the three literally, but it is a small number.

CHAIRMAN APOSTOLAKIS: And what is different from the first to the second?

MR. SIU: Well, the first one just simply said in general I could run separate -- I could do this expansion if you will for one, two, three, four, however many. So we have to bin down, and the binning is a major modeling step.

CHAIRMAN APOSTOLAKIS: Right. But in the thermal hydraulic analysis, how do you decide to have a number of -- what is different between these three runs in the same bin?

MR. SIU: Consider perhaps that this was the action at 8 minutes, and this is 10 minutes, and this is 12 minutes. It could be.

CHAIRMAN APOSTOLAKIS: Okay.

MR. SIU: Now, we have a method for identifying what are the important variables to look at, and we are trying out methods to identify the subscenario.

CHAIRMAN APOSTOLAKIS: So what you said earlier was that, yes, this uncertainty and the boundary conditions would be handled, but the uncertainty in the T/H analysis itself, at this point at least you are not handling it?

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: Okay.

DR. POWERS: So in other words, if I'm agitated over the quality of some heat transfer correlation, that it is embedded in RELAP?

CHAIRMAN APOSTOLAKIS: You will remain agitated.

DR. POWERS: Does that mean that that will affect the security of the free world, the security of the free world remains in threat?

MR. SIU: We are, of course, not doing this entirely arbitrarily. We have reasons, and that is explained in a fairly lengthy report why we are concentrating on certain issues and not on others.

And in the case of the PTS analysis, part of the point is that the time constance associated with the reactor pressure vessel, the wall, the thermal response to a transient, is relatively long.

And that means that some of the details that you might worry about for other situations may or may not have a great effect on the through wall crack frequency.

CHAIRMAN APOSTOLAKIS: And I don't think it is part of your charge to protect the free world is it?

MR. SIU: That wasn't my stated objective.

DR. POWERS: I guess maybe you need to point to me the heart in this lengthy document where that is stated, because it seems to me that taking a thermal response time of the wall to decide whether I work with heat transfer correlations or not is precisely the wrong thing to do.

MR. SIU: Okay.

DR. KRESS: If you are concluding that this heat transfer coefficient that Dana might be agitated about was very important to your final answer, you would include it in these variations, in that middle box, perhaps? That might be the thing that you are changing?

MR. SIU: You certainly could. You could. You know, at this point, I guess we -- and this is part of where we are getting feedback from the committee, of course.

We have identified certain things that we think are important and that we do need to address, and if the committee gives us a feedback that we have not considered some important things, that would be important for us to know.

CHAIRMAN APOSTOLAKIS: Yes, because a number of calculations will multiply tremendously if you are not careful here. So instead of the three subscenarios, you compare an extra 10 to describe these uncertainties.

DR. POWERS: George, let me ask you this question. If I set out and do some sort of a Monte Carlo approach on this thing, which -- and in which some respects they may be doing here, how many samples do I have to take in order to get an understanding of what the uncertainty is?

CHAIRMAN APOSTOLAKIS: If you do a traditional -- you know, a straight sampling, I think it would be into the thousands. But they would probably do some latin hypercube sampling to determine the number of runs.

DR. POWERS: Well, even if I go to such a stretch of the imagination as using limited latin hypercube sampling --

CHAIRMAN APOSTOLAKIS: I think in the waste business where they have monster codes, the number of runs as I recall is not very high, maybe 70 or 80.

DR. KRESS: Yes, that is what I recall.

CHAIRMAN APOSTOLAKIS: Which is not really too large when considering the goals that you are using.

DR. KRESS: I think you can get by with that few. I think Dana's point is going to be how can we trust this particular uncertainty, which looks like maybe 5 or 6 cases, when you really need about 70 to do it right.

MR. SIU: Well, actually, again, we were expanding on one particular thermal hydraulic bin. There are many thermal hydraulic bins. And the actual number of runs -- take a wild guess -- in the end they might be on the order of a hundred.

DR. KRESS: So you may be covering enough there to --

DR. POWERS: Excuse me, but if I just drew a circle around this and said that everything that goes in here is basically a Monte Carlo analysis -- and it's not, but let's say that it is. And I say I would like to know this uncertainty.

I would like to know that I sampled 95 percent of the possible range of outcomes with a 95 percent confidence. That is not an unplausible kind of expectation, and I think you are up around 90 calculations.

And the fact is that I could do that calculation. Can I come back after you are all over and answer that question? Let's see. At what confidence level did you sample what fraction of the possible response base here.

MR. SIU: I guess we haven't been thinking along those lines, partly because we weren't sure how to deal with again this issue of the integrated model uncertainty.

And to work the -- to overwork perhaps, and maybe that is an unfair term, but to work the perimeters side too hard given that you have got this other part that you haven't quantified -- I guess we just simply weren't thinking in those terms.

DR. POWERS: Well, I think that is a question that I would expect this committee to come back and ask you, is okay, you have a response base so big.

How much of it did you sample, and at what confidence level? Tom will ask you about the confidence level, and Bill will ask you about the fraction of the space issues.

MR. MAYFIELD: If I could, because we have had -- as we were first getting into this project, actually several years ago, some discussions about what is the level of rigor, and is it practical to put RELAP, or the consolidated code into a Monte Carlo scheme.

What level of resource are we going to invest in it. That question started being outweighed by plant to plant variability. We are doing four. So I think the qualitative opinion of those others that we are talking about is at some point -- that at some point the level of rigor in any individual transient analysis, or any individual plant analysis, is going to be swamped, or that the level of uncertainty in those analyses is going to be swamped by the plant-to-plant variability.

And we were starting to struggle with counting angels on heads of pins for one plant, and then losing that sense --

DR. POWERS: You came to that conclusion for some reason, and I guess it really surprised me, because it is not the intuition that I would come to. Can you explain?

Maybe not here, but at some point can you explain why you would think that the plant-to-plant variability would be so large compared to the phenomenological uncertainties?

MR. MAYFIELD: We can, and today is probably not the best time, but in general, if you just look back at the old IPTS studies, Oconee is probably not a good example, because that is the first one that they did, and there were a lot of assumptions made.

But if you just look at the difference in the calculated probability of failure between Robinson and Calvert, and that's a CE versus a Westinghouse design. They are about two orders of magnitude apart if I remember my numbers correctly, and yet for similar levels of embrittlement.

So we were struggling with why, what is the big deal between them, and it got down to specific sequences and what drives it. In the BMW plants, you find that steam generator tube failures is the dominating sequence, or I'm sorry, the main steam line breaks the dominating sequence.

Westinghouse tends to be small break LOCA. I think that is the dominating sequence for CE also. So it is that kind of stuff we felt like was going to swamp uncertainties in the specific calculations. But this was a judgment, as opposed to based on hard calculation.

CHAIRMAN APOSTOLAKIS: But I am not sure that you should be trying to develop a methodology for plant-to-plant uncertainty. I mean, you are developing it for a particular type.

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: And then if you do it for several plants. So the uncertainty from plant-to-plant really shouldn't play much of a role here.

MR. MAYFIELD: Well, until we go back to Nathan's first chart, where we were trying to establish --

CHAIRMAN APOSTOLAKIS: Yes, for the criteria, but not here.

MR. MAYFIELD: So the notion was -- yes, not here. The notion was what is the level of cut-off in rigor for a specific plant analysis.

CHAIRMAN APOSTOLAKIS: That's correct. I understand that.

MR. MAYFIELD: And so it was a judgment call as to what level we had to go to.

CHAIRMAN APOSTOLAKIS: But what I am getting out of all of this discussion -- and I realize that this is still a work in progress, but eventually it would be useful to try to see whether you can use a limited latin hypercube sampling scheme to demonstrate that you have picked the whole range of values.

That's essentially what it does. And also it limits significantly the number of runs that you have to make.

DR. POWERS: I will argue that the way that George did this -- the number of runs with a straightforward Monte Carlo is not larger.

CHAIRMAN APOSTOLAKIS: Well, all of the studies have seem to show that there is in orders of magnitude --

DR. POWERS: Well, having lived right down the hall from them, from them who developed the limited latin hypercube sampling for a lot of the reactor accident codes, I am fairly confident in my position.

MR. MAYFIELD: I think Ali Mosleh might have a few comments.

MR. MOSLEH: We started with that as an approach to take, where it would remove some of the uncertainties that would inevitably be encountered in the process of reduction, and in the process that Nathan showed earlier, we have to go through binning, districtizing the continuous universe, and that introduces uncertainty.

We looked at as a potential problem with reducing the problem into smaller pieces, but at the same time the complexity of running a full Monte Carlo, even with latin hypercube, in a fully integrated model, going from the PRA oriented model and all the way to the PFM, was just in terms of size and resources, and capabilities, was just too much to handle in the scope of the analysis that we were doing.

CHAIRMAN APOSTOLAKIS: Well, I would bring again the work that has been done in the performance assessment of high level waste depositories, which cannot be simpler than what you guys are doing now.

It is really huge.

So maybe what you can do is pick up some of their reports and see how they handle that, because they certainly have had the same problem.

MR. SIU: We will take a look at that, I think.

CHAIRMAN APOSTOLAKIS: That's all.

MR. SIU: What I am showing here on this diagram, just drilling down one lower level of detail than that three box diagram that you had on the previous figure, just to show you again the different analysis tracks; basically the PRA analysis track, and the thermal hydraulics analysis track, and probablistic fracture mechanics analysis track.

Part of the point of this diagram is to point out that as the way the project is really being done, as opposed to how you might conceptualize it, these are indeed being done in parallel.

Some of the thermal hydraulics analysis is done before we really had significant interactions with the event sequence analysis. So there are some runs, for example, that we are using in our analysis, and some others are just indications of what might be interesting, but aren't really folded into the final analysis.

And similarly there are currently in the results that you are going to be seeing, there isn't full feedback yet from thermal hydraulics into, for example, the PRA success criteria that we have used.

We have made some assumptions based on our understanding of the progression of the accident, and that understanding will be improved after we explore the detailed results of the thermal hydraulic calculations.

So there is a lot of interactions here that are taking place. Of course, the results of the PRA analysis will be eventually the frequencies of the various bins identified, and that gets fed into the probablistic fracture mechanics analysis when we quantify through wall crack frequency.

Similar to thermal hydraulics analysis, it develops the subscenario histories that get fed into the wall crack frequency. One of the other things that I wanted to point out, some of the discussion that we had at the subcommittee meeting was really on this issue here, what are the potentially uncertainty important scenarios.

How do we justify narrowing down the problem to a limited set of issues, and so that was the point of that discussion. Okay. Where are we now.

DR. SHACK: And just coming back to that, I mean, that is where you sort of addressed Dana's question of how important for example a heat transfer coefficient might be.

MR. SIU: We really did look at that particular one. Now, that was the specific issue of the heat transfer coefficient in the downcomer, and explored if you will through a sensitivity fashion, t he variance and the results is not very great compared to the variance that you would get from other sorts of issues.

Now, whether there are other concerns that were not addressed, we obviously did not do an exhaustive list, and it was based on the high level model of what is important and what isn't important, and again we would welcome feedback on that.

Where are we now. We have developed an aleatory model and that's what you saw. That is the event sequence model, the T/H subscenarios for different bins; and then there is a aleatory treatment of the K1C term in this probablistic fracture mechanics analysis for fracture toughness.

So at least conceptually we have the pieces, and we know how they are going to fit together. We have categorized the different model perimeters both in the white paper that the committee saw several months ago, we categorized -- at least in the preliminary fashion -- the probablistic fracture mechanics analysis perimeters.

And that has been revised a little bit, and Mark Kirk talked about that at the subcommittee, but we have also categorized the thermal hydraulic perimeters, and as was pointed out at the subcommittee meeting, the PRA analysis is conventional.

We are treating the uncertainties in the perimeters as being epistemic, and that is no big surprise.

In the PRA event sequence analysis, we do have draft distributions for Oconee. Again, we have received a lot of comments on them. We have our own comments as we reviewed the results in detail, but we will -- and we expect to revise those distributions as part of the iteration process.

Nevertheless, we thought it would be useful to bring it in front of the committee to give you an indication of what are the things that seemed to be important, and what sorts of uncertainties do we have in the results of the calculations to date.

We have in the thermal hydraulic

analysis, as I indicated in a previous slide, we have identified classes of scenarios where the boundary condition uncertainties appear to dominate the model structure.

And I am talking about the model as an assemblage, rather than individual submodels, because the submodels, where they affect the boundary conditions, we are treating through the boundary conditions.

CHAIRMAN APOSTOLAKIS: A boundary condition means at the time of operator action?

MR. SIU: For example, the size of a hole, discharged through the hole, and that sort of thing. Things are basically --

CHAIRMAN APOSTOLAKIS: And these would be handled as epistemic variances?

MR. SIU: Actually, these are aleatory. Again, you think of the variation in the operator actions. This is a level below which we are modeling. So you are saying that -- you see, the PRA defines success and failure in very global terms.

Let's say that success is throttling before 10 minutes. Well, there are variance on success, but there are also variance on failure. If I don't throttle in 10 minutes, or if I throttle in 15 minutes, what is the difference.

CHAIRMAN APOSTOLAKIS: And how about the size of the hole?

MR. SIU: The size of the hole also is -- I mean, we have got a big category that is called small LOCA , and that accommodate a wide variety of break sizes and locations. So again there is a variation there that is all lumped into that category.

CHAIRMAN APOSTOLAKIS: So you think that is an aleatory issue?

MR. SIU: That is an aleatory issue. It is different than saying if I have a particular sized hole, would I know about it. Then we have identified the potentially important perimeters, and that was a table which we will clean up, and which the subcommittee has seen in the report.

And we need to clarify a few things there, but again we feel comfortable, and at least as a first shot, we know which perimeters we need to focus on, and we are developing a process for quantifying those subscenario probabilities.

And that question came up in the subcommittee as well. Clearly, we are not taking 5th percentiles of variables and combining them and saying that is a 5th percentile of the outcome.

So basically we are looking at a DPD or dispute probability distribution kind of approach to identifying subscenarios. So it would be a discretized approach.

CHAIRMAN APOSTOLAKIS: And that is what those guys did on the performance assessment and it may be useful to you.

MR. SIU: Yes.

CHAIRMAN APOSTOLAKIS: And to see what they did.

MR. SIU: Okay.

DR. KRESS: The important perimeters identification, was that a PIRT process?

MR. SIU: Marilyn, who did the work, started with PIRT, and looked at the approach, but basically had to extend it. And frankly through the use of modeling arguments, physical modeling arguments, concluded that a very limited set of issues was important.

Again, a review of the committee would be helpful to say whether those arguments are convincing. We will demonstrate this process as part of the Oconee analysis, and obviously we intend to use this for the other plants as well.

The probablistic fracture mechanics. We

do have distributions for most of the model perimeters. For example, we have distributions for the flaw characteristics, and I think the committee was presented with that material, or was it the subcommittee. I don't remember.

We have distributions for fluence, for chemistry, the copper content and nickel content, and phosphorous. The current work is focusing on treating uncertainties and fracture toughness, and that is the K1C, and then the RT/NDT, or actually the radiation --

DR. POWERS: I have examined a document that I cannot recall exactly, discussing the need for continued research in the enburtelment of reactor vessels that has the phrase in it that the correlations that have been developed are only -- I say I believe, but it goes something like this.

Semi-empirical in nature and only include the effects of copper, nickel, product form, and fluence. It does not go on and tell me what else ought to be in there. But it looks like a lot to me.

I mean, nickel, copper, product form, and fluence, and it was a little hard for me to come up with what else there ought to be. But I am not an expert in that fashion.

My point is that that seemed to suggest

that this was an inadequate understanding here, that there was something missing, something better ought to be available.

Does that mean that we have something here through the implausible unknown that just bars progress here or something?

MR. MAYFIELD: The model that is going to be used in these calculations is the latest thing that we have put together, and that I think we have briefed the committee on, but I am not sure.

It is based on a statistical analysis of the existing embrittlement date, and coupled with a fair bit of work from Professor Odette, and some of the other radiation damage mechanists that have been looking at this.

The work does go beyond just sort of the traditional product form, copper, nickel, composition. It has looked at factors that pop up, such as long term thermal embrittlement. So there is a time at temperature factor that gets rolled in.

We have been looking at what factors show up in the statistical analysis, and do they have a physical basis. Conversely, is there something from the physical metallurgy that should be in the data, and we have gone looking for that.

And in some cases there has been some extensive dialogue between the mechanists and the statisticians. We think that the model that we have today embraces the physical understanding of embrittlement, down at a fairly basic level.

And it embraces that, as well as statistical trends in the data, and so that's as good as I get, I guess is the point.

DR. POWERS: As good as you can get now, or as good as can ever be gotten?

MR. MAYFIELD: Well, that's why we continue to work on this. We are not convinced that we have the answer. However, today, and at the level of fluence that the vessels are expected to see through 60 years, we think we have a model that captures those trends.

DR. POWERS: The phrase that I am imperfectly reproducing here has this only term in there, as though there was some heat factor, a very important factor, missing. It didn't say what it was unfortunately. It just said we only have this stuff.

Now, you have suggested as one the time and temperature factor there, but is there some great imponderable that just constitutes a barrier that we have to put in some fudge factor here to say, well, it can be no bigger effect than this?

MR. MAYFIELD: I don't think so, but Mark Kirk has come up and perhaps he has --

MR. KIRK: I think, of course, that future knowledge is never perfect, but I think the answer is that we have beaten that one pretty well. We have looked at a lot of model -- at radiation experiments on model materials that are designed to bring out certain forms of radiation damage.

And those data have been considered in the development of the model, and I think that helps to screen out some of the imponderables that one might otherwise be worried about.

But as Mike said, the form of the correlation that we are now using, much of it has a very firm physical basis, and we feel that it is important to combine both the physical and the statistical understandings.

And not so much for fitting the data, because of course you can do that without any physical understanding whatsoever, but to provide -- and I don't think this is a word in Websters. Well, I won't use it then. But the ability to extrapolate, which is of course what we are always doing here.

But we could certainly go into this in

more detail like Mike said. I think or I know that we have briefed at least the materials subcommittee on the embrittlement correlation.

That is something that we could do in the future. Certainly this is an area, along with what was brought up earlier about through all attenuation, in which there has been a lot of interest, both within the NRC, and the industry, and the international nuclear community, and continues to be -- and in fact at the ASTM E-1002 meeting last week on radiation damage mechanisms, there was discussion of this issue yet again.

And I spoke earlier this week with Stan Rosinski, who is a program manager at EPRI, and he indicated that he was going to initiate a small project under their materials reliability project, using funding from their materials reliability project to do in the short term a review of what technical basis there exists through all attenuation functions to provide the NRC some assistance in that regard.

So that information will be coming in, and if it comes in during an appropriate time frame, and I think it will, it would be considered. And just to also mention so that there is not the perception that the NRC is working in a vacuum on this.

We are currently in the process of developing a technical basis document for the embrittlement correlation. We have got a deadline on that later this year to have a draft new reg.

Equally again, EPRI is also working on a tech basis document concerning embrittlement correlations, and that is due out in February, and EPRI has agreed to provide that to the NRC so we can have the value of that information as well.

DR. POWERS: So I get the impression that what you are telling me is that I should not worry about this only. That you have put in here enough description of this embrittlement process for the regulatory decisions that you are looking to make here?

MR. MAYFIELD: I believe that is a true statement.

MR. SIU: And from the standpoint of the uncertainty analysis again, those things that are not specifically in the models are treated as contributing towards aleatory uncertainties. This was basically the reason why we decided that the K1C term needed to be treated as an aleatory issue.

DR. SEARLE: Don't worry any more. Just get nervous.

DR. POWERS: Well, I will quit the subterfuge here. It shows me that for both research programs on vessel embrittlement are no longer needed for making regulatory decisions.

MR. SIU: I will show you -- these are pretty hot off the press -- draft PRA results overview for Oconee-1, and what is new about this is not only the scenario frequencies, which se came up with a few weeks ago, and again had some review with Duke Energy to talk about specifics relative to how we characterize the plant and operations.

But also the characterization of importance with respect to probablistic fracture mechanics. We have been conducting a scoping study for Oconee just to get an idea of where the numbers are coming out.

So we have a current version of the FAVOR code that is being used to propagate the thermal hydraulics traces, the PRAs events frequencies, through to the end to develop some notion of small crack frequency.

We are not confident yet enough about the probablistic fracture mechanics material to give you a conditional probability of through wall crack given a scenario, because again this is really new stuff.

But at least I think we can indicate that these were the kinds of scenarios that were turning out to contribute to the results, and I am going to give you a caveat about these descriptions here in a second.

So, please, again don't take down literally as their are described. But we have basically -- if you focus in on these numbers here, these refer to specific thermal hydraulic runs, and certain assumptions are made, and the analysis is done.

Frequencies are assigned to these runs, and they are run through the probablistic fracture mechanics. These are the kinds of scenarios that turned out to be relatively important, and the one that I put in gray right now appears to be the most important one.

Again, all of these things are subject to change as we dig into these results and identify what is really driving them, and whether we have got it right or not.

CHAIRMAN APOSTOLAKIS: If you go to the conceptual model, can you tell us at which point these frequencies are calculated?

MR. SIU: Sure. This is the output of the event tree analysis basically. I'm sorry. Let me go to the framework, as I think that would be better.

CHAIRMAN APOSTOLAKIS: Yes, whichever.

MR. SIU: What you are seeing is that we have binned the scenarios into about 40 thermal hydraulic runs. We didn't use all 40 as it turned out, but that was the universe which we were considering.

CHAIRMAN APOSTOLAKIS: For one scenario?

MR. SIU: No, no. All the possible PTS thermal hydraulic scenarios. We ran 40 cases of RELAP, and we are binning the thousands of sequences that we got into one of those 40 cases.

CHAIRMAN APOSTOLAKIS: Okay.

MR. SIU: So when you see a specific number, like Run Number 3, that is a specific one run out of the set of, let's say, 40 roughly. And we have got the probability distributions about those frequencies.

We have not done this part here. The fractionation into subscenarios, which we talked about, we haven't approached, but we have not applied it yet to Oconee.

So we are taking a particular if you will, and all of these are collapsed into one. There is only one trace associated with that bin, and that trace gets fed into the fracture mechanics analysis with this distribution, obviously the convolution of the --

CHAIRMAN APOSTOLAKIS: So the frequencies on slide 11 are between the first two boxes?

MR. SIU: That's correct. After you have been --

CHAIRMAN APOSTOLAKIS: Over there?

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: But what you show as description is one of the scenarios that goes into that.

MR. SIU: That is the scenario that characterized that particular bin.

CHAIRMAN APOSTOLAKIS: But that bin may include other scenarios as well.

MR. SIU: Exactly.

CHAIRMAN APOSTOLAKIS: And the one you showed here is what, is a representative, or just one of them?

MR. SIU: The one I showed on this chart here?

CHAIRMAN APOSTOLAKIS: On 11.

MR. SIU: Okay. Let me get back to Slide

11, because this is worth talking about.

CHAIRMAN APOSTOLAKIS: So when you say the one that you have shaded there, the large MSLB is medium?

MR. SIU: Yes, lots and lots of scenarios feed into this bin. The total number of sequences was around 14,500 I believe.

CHAIRMAN APOSTOLAKIS: So lots of them are going into 25?

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: And why are you showing the large main stream line break?

MR. SIU: This is the description of this particular run. So this is the -- to run RELAP, of course, you have to provide the initial conditions, the boundary conditions, and certain things that occur over time.

This is a description in very loose terms of what that run did, the T/H run.

CHAIRMAN APOSTOLAKIS: But there are other scenarios that lead into --

MR. SIU: That's right. We have been many scenarios into this, which some of them may not follow this description very closely.

CHAIRMAN APOSTOLAKIS: Okay. So the

reason why you have not is because it is kind of representative of that?

MR. SIU: In a sense. I mean, if I gave you a number, it wouldn't mean anything either. So I have to give some idea of what kind of scenario this run represents.

But, yes, there are lots and lots of scenarios feeding into these, and we are examining -- now that we have got some sense of priorities here, to see if this is really right.

Are we feeding the right stuff into this bin, and do we need actually another run because the contributions from this are so large, but they are not really well represented by that run. That is the question that we have to raise after we get a chance to get some results.

CHAIRMAN APOSTOLAKIS: So this includes now aleatory stuff and everything?

MR. SIU: Again, we do not have the subscenario fractionation.

CHAIRMAN APOSTOLAKIS: We don't have it?

MR. SIU: We do not. This is simply the PRA results.

CHAIRMAN APOSTOLAKIS: So does the operator intervene here anywhere?

MR. SIU: Well, he fails to. For example, he fails to throttle the HPI.

CHAIRMAN APOSTOLAKIS: So you are just using a representative time for that?

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: Which later on would be refined.

MR. SIU: Which has to be refined based on now a more careful look at that particular scenario.

CHAIRMAN APOSTOLAKIS: So up until this point, you really don't have any model uncertainty do you?

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: So this is a traditional PRA.

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: But the new thing is that you have these bins that you are showing?

MR. SIU: Yes.

CHAIRMAN APOSTOLAKIS: Okay. Fine.

MR. SIU: And now to say what is new or not, but simply this is how we are progressing through the analysis.

CHAIRMAN APOSTOLAKIS: So this will go into 25, but 25 has not been run yet?

MR. SIU: No, 25 has been run. That's why -- look, 25 -- I can show you. In your viewgraph, on the back of the viewgraphs, this is Run 25, the thermal hydraulic trace. This -- and I don't know if it is smoothed out or not, but it gets fed into FAVOR, with a frequency and uncertainty about that frequency.

And based on the combined results of all of those things, including the PRA frequencies, we have some sense of priority, and this is what you are seeing.

DR. POWERS: Now, when you formulate the RELAP model for this number 25 calculation, which of the myriad of conditions do you tell it about? Do you tell it about the mean condition or the 95th percentile condition, or the 5th percentile condition?

MR. SIU: I will give you a high level description, but I think Dave -- well, Dave is here. Can you answer to that?

MR. BASETTE: Let me see if I caught the question correctly. This is David Basette. Of course, RELAP gives you a median or a nominal best estimate calculation for a given, or however you fix the initial boundary conditions, it gives you a best estimate calculation.

DR. POWERS: He has described this grade

line as main steam line break, with full high pressure injection. He has told us, however, that there are scenarios within that bin that can deviate to some amount.

He has provided a synoptic description of a distribution for that bin that includes a mean, a 95th, and a 5th percentile. Now, you have to formulate a run with RELAP.

You cannot put those distributions in. You have to say it is this plant, and at this time this operator does this successfully or unsuccessfully. Which one of those did you tell RELAP about?

MR. SIU: Let me respond to that, as I think I can take that. This is a somewhat more careful description of that particular scenario. So, for example, you see high pressure injection 21 seconds into the transient based on the control logic for HPI.

Now, there are variance on this. You could say the operator doesn't throttle in 5 minutes. The operator doesn't throttle in 10 minutes. The operator doesn't throttle in 15 minutes. That's not here.

This is literally what they have. So the

PRA at this point was not telling the thermal hydraulics this is the variant that you need to look at.

When we talk about subscenarios, which again we have not applied the Oconee yet. We have to start investigating those variance, and say what are the possible variance that we want to model, and then identify are there RELAP runs that will represent those variance reasonably well, or do we need new runs. We have not done that yet.

DR. POWERS: For this particular case, is the scenario you told about RELAP indicative of a frequency equal to mean, the 95th, the 5th, or some other one? Yes is not a suitable answer.

MR. SIU: No, there is no frequency associated.

DR. POWERS: There is a frequency associated with whatever calculation you told RELAP about.

MR. SIU: We have not -- let me give you an example. Let's talk main steam line break. This is a large break, and we have defined large to be greater than 8 inches here, because the size, I believe, of the TBBs.

We have not said that we have a frequency for breaks in the range of 8 to 9 inches, from 9 to 10 inches, 10 to 11. Yeah, you could -- we would have to give you -- and I don't know this off the top of my head -- what was the size of the particular hole, and what was the shape of the hole, and what discharge coefficients are associated with that.

So I don't have that information there. You could come up with, if you will, density functions for these characteristics like the break size. We haven't done that.

So I guess you could say, well, what is the frequency that you have of a break between or larger than 8 inches. Yes, we do have that. That is the PRA frequency that we have used, and that is the .0 -- well, I shouldn't give you a number off the top of my head.

We do have that based on an empirical dataset. I mean, we had one failure in 600 odd reactor years.

MR. MAYFIELD: I think the answer is that it doesn't necessary represent any of the frequencies here. It is a descripter of a class of transients that fits in the bin, and at this stage, I don't think we can tell you that they have gone back to the PRA, and we have taken up a bunch of things through some rules that have been developed, and have taken a bunch of transients that fit that set of rules, and put them in this bin.

And he is telling you about the distribution of frequency on those transients, and I don't think we can tell you today that the particular RELAP run that was made, called Number 25, where that fits in this frequency, and it is my guess that it is probably closer to the mean than any. But I don't think we can pin that down.

DR. POWERS: I think that would be an inadequate answer for me, to say, well, it is roughly the mean, or maybe the appropriate answer is it is none of these particular ones, but its going to be kind of representative in a sense that it will be carefully explained.

MR. SIU: Yes. I think as we define really what those subscenarios are -- I mean, right now you have a cartoon. It says we will develop subscenarios, but you have to develop those subscenarios based on the underlying principles.

And the principles would be, for example, what are the key variables, and what variations are you going to consider, and what probability are you going to assign to each of these variations.

And then I think at that point, I think we can give you a more meaningful answer, because now by definition when you have these discreet scenarios, you have binned things. It is either in this bin, that bin, or that bin. We haven't done that yet.

MR. MAYFIELD: Does that answer your question?

DR. POWERS: Well, the underlying question is we are going to have a bunch of thermal hydraulics, and they believe they have got these huge uncertainties in their codes that need to be resolved.

And they are going to come in and say, oh, this is giving me an unfair answer, and that the thermal hydraulics don't make any difference, because had you done this thing out here at either 95th or 5th percentile, and I am not sure which one.

You would have seen it, and it would have made all the difference in the world, this strange coefficient in an equally strange empirical correlation that does not include anything in it, except maybe copper, nickel, or fluence.

And that is what I have to listen to on why this is an unfair characterization of the uncertainly of the thermal hydraulics.

MR. ELTAWILA: I have tried to resist

getting into this, but when it comes to thermal hydraulic uncertainly, I think you raised that issue several times. When we are dealing with single faced flow, which is the case for this particular application, the uncertainty in the heat transfer coefficient -- and we are going to give you a paper.

We have done specific studies which shows that is not important. The main important perimeter would be the pressure and the temperature, and have confidence that the code can calculate these very accurately or reasonably accurately.

I think your question has the right issue, that the particular thermal hydraulic calculation that we presented, it is a representative of one particular scenario, which will give you a mean answer for that particular scenario.

We have not gone back now to look at all the other scenarios and redefining process. Is there much variation if I change the break size, or I change operator action. What will be the effect on the pressure and the temperature, and that's when we will be able to at that point to give you the 95 percent and 5 percent uncertainty.

But the model uncertainty itself is not going to be the driver in this case. In this case, it is going to be the boundary condition as everybody said here.

DR. SEARLE: I must express some curiosity about the difference between runs 3 and 4 on page 11. There is a factor of 2 in the flow area, and a factor of two orders of magnitude in the mean probability.

MR. SIU: I was afraid that you were going to ask that.

DR. SEARLE: Clearly, there has got to be a snake in the grass somewhere.

MR. SIU: There is a reason for that, and now it actually gets back to George's question, and that's why I wanted to characterize these as not literally what the PRA sequences are, but what the thermal hydraulic run is.

Again, there was a binning choice to say out of the myriad of sequences that we have, we have some of them going to this one, and some of them going to this one, and so on and so forth.

What you are seeing here in the PRA space, we don't have a fine distinction between 2 inch LOCAs and 2.8 inch LOCAs, and 1.4 inch LOCAs. We have LOCAs, small LOCAs with a certain frequency.

What you are seeing here in this particular scenario, and what this particular scenario is really representing LOCAs where, even though the HPI is on, the pressurization is sufficient that you are going down to a lower pressure.

So again I don't want to take these labels literally. This is the RELAP run that was done, and we binned a bunch of stuff into that. Again, we are going to reexamine that as we go further.

DR. SEARLE: My only comment is that the way in which you differentiate between the runs is probably woefully inadequate at this point if you are going to really examine the differences in the probabilities.

MR. SIU: We know, of course, exactly what the input decks are, and we know what scenarios feed into those. That's correct.

CHAIRMAN APOSTOLAKIS: So you said earlier that the size of the break within the class of small breaks is aleatory.

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: So can you elaborate a little bit on that? I mean, what kind of distribution did you assume here, and so what fraction of --

MR. SIU: Again, this is part of the problem with flashing results at a summary level. The scenarios that got fed into this particular bin are LOCAs, with either the HPI throttled or the break was large enough that the system depressurized quickly. Most of the contributing ones I think were just small break LOCAs, where HPI was throttled.

That doesn't match exactly the description you have here. In the physical world, when you have a break that is large enough, you do depressurize the system.

CHAIRMAN APOSTOLAKIS: But then it is not a small break anymore is it?

MR. SIU: Well, remember now that the small break refers to the diameter for which we have event statistics.

CHAIRMAN APOSTOLAKIS: Right.

MR. SIU: And small, actually believe extends beyond 2.8.

CHAIRMAN APOSTOLAKIS: Well, how do you decide? I mean, what is the aleatory probability that I would have a 2 inch small break, or a 2.8?

MR. SIU: That has not been addressed. This is the PRA sequences within which, and we still have to fractionate, and when we fractionate, we will actually find that maybe there is some bifurcation at some critical value, and we can argue if we know that critical value very well.

But the pressure is going to go along one path, and the other one is going to drop rapidly.

CHAIRMAN APOSTOLAKIS: I don't understand that. I mean, the frequencies you show there on the table include the frequency of the initiating event.

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: So the initiating event in one case is a 2 inch break, and --

MR. SIU: No.

CHAIRMAN APOSTOLAKIS: It's not?

MR. SIU: No. The initiating event is a small break LOCA, which includes a whole range of sizes. So that's why this is so anonymous. You say, oh, my goodness. How do I know really that a 2.8 inch break is two orders of magnitude less likely than the 2 inch break, because everything else looks the same.

It is the binning that we assign the sequences to this particular thermal hydrologic scenario.

CHAIRMAN APOSTOLAKIS: So for this calculation, the frequency of the 2 inch and the 2.8 inch break is the same?

MR. SIU: Exactly. It is a small LOCA.

CHAIRMAN APOSTOLAKIS: It's a small LOCA.

MR. SIU: And this one we don't throttle, and this one we do.

DR. SHACK: So what we are looking at is the difference between pressurized and depressurized?

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: But later on you will have some fraction?

MR. SIU: Oh, yes. Again, that is one of the important perimeters obviously as you go through this, because you have qualitatively different behaviors.

CHAIRMAN APOSTOLAKIS: And that's why it is --

MR. SIU: That's right. We have 15 minutes?

CHAIRMAN APOSTOLAKIS: We have 15 minutes, yes. Now, at some point, at some subcommittee meeting -- and I don't know if you did it last night, but I really would like to follow one sequence from beginning to end.

CHAIRMAN APOSTOLAKIS: Right. With all the uncertainties, have you discretized how you did it? Did it happen with some epistemic uncertainty with you, Mike?

MR. MAYFIELD: Well, it is our intent --

and we were talking about it before the session started, that probably in the May-June time frame, we will be far enough along with our calculations that we can come to -- I don't like coming in -- we felt like we needed to do something.

CHAIRMAN APOSTOLAKIS: No, I am not complaining.

MR. MAYFIELD: What we would like to do is have gotten far enough through this so that we are not giving you real time results; that we have had a chance to look at it and make sure that it is holding together. So it is probably in the May-June time frame.

CHAIRMAN APOSTOLAKIS: But we will take one sequence and beat it to death all the way?

MR. MAYFIELD: We will take one sequence and walk you right through it. That's the intent.

MR. SIU: That's right.

CHAIRMAN APOSTOLAKIS: One question I had, since I am beginning to understand this, but as you go to your Slide Number 12, before you do that, could you put up your --

MR. SIU: This is the backup slide --

CHAIRMAN APOSTOLAKIS: Which is the same as this?

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: So that is the scenario that you are calling Number 25?

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: And the Number 27 would be the one where you succeed --

MR. SIU: Well, whatever scenario. There is a mapping of it.

CHAIRMAN APOSTOLAKIS: But you are listing those two, 25 and 27, and runs as you call them.

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: And those two are the ones in red, and the one --

MR. SIU: This would feed into 27, correct.

CHAIRMAN APOSTOLAKIS: What happens with all the other scenarios now? You are throwing them out into different bins?

MR. SIU: We are throwing them in different bins.

CHAIRMAN APOSTOLAKIS: And some of them may not be steam line break bin?

MR. SIU: That's correct.

CHAIRMAN APOSTOLAKIS: Okay.

MR. SIU: And some that feed into the

steam line break bin may not be steam line breaks.

VICE CHAIRMAN BONACA: So you are looking at the pressure temperature behavior, and the fluid behavior, and --

MR. SIU: That's right. That's the binning and the mapping between scenarios, and that's where we discussed with the subcommittee some of the subjective judgment is right now in going from all these sequences to a somewhat more detailed description, which we feel pretty comfortable with.

But then jumping from that to the limited set of thermal hydraulic bins that we do have.

MR. MAYFIELD: And part of the work, yes, is subjective, but to try and bring in a rule based scheme, where it is not just tossing coins, but there is actually some technical basis for the judgment.

VICE CHAIRMAN BONACA: So it is very plan dependent?

MR. SIU: Yes.

VICE CHAIRMAN BONACA: And so I understand much more than I did before.

CHAIRMAN APOSTOLAKIS: Okay. So what else would you like to tell us?

MR. SIU: Okay. Let me just talk in summary about the draft PRA results. We do obviously have issues and we have talked about these. The binning of the sequences, and the time frame for the operator actions, which we now have the thermal hydraulic runs to get a better sense of that.

And dependencies. This particular scenario involves three operator actions or failures. Failure to isolate the break, and failure to isolate the flow, and failure to throttle HPI flow.

We need to make sure that we are handling the dependencies not only for the dominant scenarios, but obviously the scenarios that might have dropped off the map because we didn't address those in detail.

CHAIRMAN APOSTOLAKIS: Where do the operator --

MR. SIU: This is the Atheana Team. This is a subjective assessment process based on a description of context. At the Duke Energy meeting, we actually got very positive responses on our descriptions of the context, and actually there was some discussion about the numbers that were assigned.

But we didn't seem to be way off is my notion of that. Again, these are things that we will continue to refine. We had put intentionally conservative numbers in many places just to make sure that we didn't lose anything as part of this process, and now we are reexamining what we have got.

Thermal hydraulics analysis. I think we have talked about this already. This just illustrates more of a process rather than results, because we don't have results at this point on the uncertainty part of the analysis.

We have identified the key sources of uncertainty, and we talked about boundary conditions on models, and of classified scenarios regarding in a simplistic fashion whether they involve single-faced flow, or two-faced flow.

And for single-faced flows, we are going to follow the approach that we have basically described already. We are going to look at representative boundary condition variations to define subscenarios, and we are going to develop distributions for the subscenario probabilities.

And then either identify an existing T/H run to map to, or perform an additional run, and that's just the approach that we envision at this point.

CHAIRMAN APOSTOLAKIS: Are you going to identify -- and not necessarily only here in the thermal hydraulic analysis, but the overall analysis, the important perimeters or models that seem to drive the risk?

MR. SIU: That's right. That's part of the assessment process. It's not only what is the number, but what is driving that number.

CHAIRMAN APOSTOLAKIS: And how are you going to do that? I mean, you have perimeters all over the place.

MR. SIU: Yes. I imagine that there will be some sense of decomposing the results, because of course one of the results of a risk assessment is that the dominant scenarios also typically dominate the uncertainties.

That's just the way that the math works out. You can have a very unlikely scenario that is very uncertain, but it doesn't really contribute then to the final result. So I think we will be able to concentrate on a few scenarios, and that's the hope.

CHAIRMAN APOSTOLAKIS: But then you are going to the P/H analysis and FAVOR, and --

MR. SIU: But again the point of what Professor Almenas showed was that there is a rationale for identifying what are the important perimeters, and so that would at least be our starting point for talking about what seems to be driving this.

CHAIRMAN APOSTOLAKIS: My understanding is that in the waste area that they have been struggling with this issue now for 2 or 3 years, and I have seen a paper or two where they have proposed something to Hizenberg, who used to be a member of the staff.

I am not saying that is the way to do it, but since those guys have attempted, it would be worthwhile looking.

MR. SIU: Thank you, yes.

CHAIRMAN APOSTOLAKIS: Actually, your problem has a lot of similarities with that problem, because it involves complex computer programs and uncertainty propagation, and so on, and so you can benefit a lot from what those guys have done.

MR. SIU: Yes.

CHAIRMAN APOSTOLAKIS: Of course, they cannot use the traditional importance measures that we use in level one and PRAs.

MR. SIU: Right.

CHAIRMAN APOSTOLAKIS: Because you have computer programs with physical phenomena. That's why it may be worthwhile to look at what they have.

MR. SIU: This is a Mark Kirk slide obviously. It is 21st Century stuff. I am way behind. But this is just an indication of --

MR. KIRK: What is wrong with this?

MR. SIU: Nothing is wrong. This is great. Let's walk through it.

DR. POWERS: The thing that jumps out immediately is that the embrittlement model only has fluence, copper, nickel, and product form on it. All this other stuff that you told me that yo were going to put into it apparently doesn't make this viewgraph here.

MR. SIU: Well, my understanding of what are the important perimeters, yes, are here. And the point is to show that these are the major uncertain elements feeding into the shift model, which I understand work is still ongoing as to the shift model itself.

It has a blue band around it, and I don't know if that is an indicator, but this is one place where work is going on, and that's one of the issues that I indicate, and where we are still doing things.

But the point is to show that there are perimeters feeding in, and there are epistemic uncertainties associated with these perimeters, and they get fed into the process, to the resistance side if you will of the stress strength equation.

And on this side, on the driving force, you have uncertainties in the flaw density and flaw size. Again, we have characterized these distributions already.

Of course, you have the thermal hydraulic input, pressure and temperature, and you have the vessel dimensions that get fed into the stress intensity factor calculation, and determine if the applied stress is greater than the strength.

And this box here shows again the recognition that because of the things that are not in this model explicitly, you have chosen a model at a certain level, and the strength is an aleatory issue, and that gets fed into eventually an aleatory description of the vessel response to the thermal hydraulic scenario, which is the applied stress.

In the interest of time, I think I will move on. There is a similar diagram for arrest toughness. Okay. Only two slides to go. Key issues. These issues again become apparent as we dig into the results. We finally have a prioritization of results that tells us which things to focus on.

The success criteria, and how much time is available for the operators to perform their actions is something that we need to look at very carefully. And more generally how do we quantify the human error probabilities, which is -- obviously a consideration of uncertainties is an important part of that quantification process.

And in the thermal hydraulics analysis, there is the question of how we are going to deal with model uncertainties, especially for the two-phase scenarios, and we still have to develop just as a mechanical matter the perimeter of distributions, and what are the uncertainties for the boundary conditions.

Probablistic fracture mechanics analysis. We have uncertainties in the fracture toughness and the radiation shift. Again, that is that embrittlement model that I talked about, and there are significant uncertainties in crack arrest and how you model that, that still need to be addressed.

I separated the integrated analysis out from these three because in some fashion we have been focusing so much on the three boxes, and we have not talked enough about the integration of those boxes, and I am talking about our project, as well as this presentation.

And this binning is obviously really, really important. It drives a lot of the results. We have to look at that very carefully. This is one where again I suspect we wouldn't be quantifying the uncertainties in our binning process, but we would have to recognize it is a source of uncertainty.

We believe that we are consistently treating uncertainties across the different disciplines, and we are trying very hard to be consistent.

We think we are quantifying most of the potentially important source of uncertainties, and again we have a rationale for saying that. So the model perimeters, boundary conditions, and submodels. We are addressing those explicitly.

Model structure uncertainties associated with the system codes, for example. And as was pointed out, maybe these are not important for many of the scenarios that we care about, but they are likely to be important for some of the scenarios.

And I believe at this point that we can only treat them qualitatively, but we will see. We recognize that we may need to refine our models, depending on the results of experiment sensitivity and perhaps the integrated code word.

We will document the approach and we will update to the white paper the committee saw earlier. And this was mentioned already, but work is in progress, and we are iterating on the initial results.

So later when spring comes by, hopefully we will have something. But we indeed can walk through a scenario, and what we tried to do today, of course, was give you some sense of at least the beginning parts of that scenario.

We think that the approach for treating uncertainties may be useful, and another risk informed applications, and certainly it is a model that we are going to try out as we start approaching other issues. And with that --

MR. LEITCH: Might this work lead to relaxation of some conservatism that is in the pressure temperature curves that are in the tech specs now?

MR. MAYFIELD: That's another possible application of this. We have backed off quite a ways there, but that is another possible application of this, as well as using this kind of scheme to look at relief for the boilers.

As the embrittlement trends tend to go up, the boilers are being pinched more and more on their hydro test temperatures, and the time that it takes them to get to those temperatures. So we think this structure may be a good way to look at the underpinnings for those pressure temperature and hydro test temperature requirements.

MR. LEITCH: I think that some licensees have already applied for some relaxation in those curves to give them greater operating flexibility. Is the basis for that some of this work?

MR. MAYFIELD: No, it is simply a change in the fracture toughness curve.

MR. LEITCH: Okay.

MR. MAYFIELD: We went way from the very conservative reference fracture toughness curve, and are permitting them to use the initiation fracture toughness curve, and that's the big change that was made in the ASME code.

MR. LEITCH: Okay. Thank you.

MR. MAYFIELD: Mr. Chairman, there are two points that I would like to make as we close. We have named four plants here, and I would like to emphasize with the committee and on the record that we are using them because they have kindly volunteered to support this effort, and not because we are concerned about their integrity from a pressurized thermal shock standpoint.

So they have stepped forward and volunteered to help us in this activity. And finally I would note that we welcome the opportunity to come before the committee and discuss the pressure vessel embrittlement research, and the need for it, and to explain to you why Dr. Powers is so completely wrong in his assessment.

(Laughter.)

MR. MAYFIELD: And unless there are any other questions, we thank you.

DR. POWERS: I would hope that Dr. Powers would get a chance to rebut.

CHAIRMAN APOSTOLAKIS: Dr. Shack, are we done with this?

DR. SHACK: We are done with this.

CHAIRMAN APOSTOLAKIS: Well, we finished a minute-and-a-half early, which pleases me to no end. Thank you very much, Nathan and Mike. We will recess until 10:35.

(Whereupon, a recess was taken at 10:14 a.m., and the Committee meeting was resumed at 10:34 a.m.)

CHAIRMAN APOSTOLAKIS: We are back in session. The next topic is the Siemens S-RELAP5 Appendix Case, Small Break LOCA Code. Dr. Wallace as I understand it could not get here on time, but Dr. Kress has kindly agreed to lead us through this. Dr. Kress.

DR. KRESS: Thank you. Dr. Wallace is having airplane delay problems, and that's why he is not here. I am sure that he would have wanted to be here.

The purpose of this meeting today is for the full committee to review the NRC staff safety evaluation report on the Siemens Power Corporation S-RELAP5, which is a thermal hydraulic code.

The application for its use is for Appendix K Small Break LOCA analysis only. You want to keep that in mind, because our review should focus on the Appendix K requirements, and not best estimate, or those things.

We will get a chance later when we come back to us for application to have this code be used for best estimate for large break LOCA, but that's not part of today's meeting.

We did have a couple of subcommittee meetings, one back in August, and the latest one on January 16th and 17th. We had a real turnout of committee members to that. I think the people there were me and Graham Wallis.

So what you hear today is -- and we did have our consultants there, too, our usual suspects. But what you will hear today is a very abbreviated summary of what went on in the subcommittee meeting.

So with that -- and we are expected to have a letter on this.

CHAIRMAN APOSTOLAKIS: In fact, we have a draft.

DR. KRESS: I think there is a draft.

CHAIRMAN APOSTOLAKIS: There is a draft in there.

DR. KRESS: So with that, I will turn the floor over to Ralph Landry.

MR. LANDRY: Thank you, Dr. Kress. As Dr. Kress said, my name is Ralph Landry. I was the lead on the review of the Siemens S-RELAP5 code, and what we would like to do today is present to you the results of our review of S-RELAP5.

And as Dr. Kress said, S-RELAP5 has been submitted by Siemens Power Corporation for application to small break LOCA in PWRs, specifically Westinghouse and Combustion Engineering Design PWRs, under the guidelines of 10 CFR, Part 50, Appendix K.

So that a lot of what we did in the review is supposed to be along the guidelines of Appendix K and the requirements that came out post-TMI-2 accident. But we looked at a lot of depth in this code,, a lot more depth than has typically been done in small break LOCA analyses code reviews, because we knew that the code was coming in again for a large break LOCA for a best estimate application.

So while we were looking at the code, we looked at a lot of depth in it to make sure that we understand this thoroughly before we even start the next phase of the review.

What I would like to do today is cover some of the milestones in the application which we received and talk about very briefly some of the code modifications that have been made.

This code is a combination of a group of codes that have been approved individually, some additional modifications. This combines the A&F RELAP code, which was submitted and approved for small break LOCA under Appendix K, which combines that with the TOODEE2 HROD model code; the RODEX2 fuel model code, and the ICECON containment model code.

So that the code that is now running under the name of S-RELAP5 is a combination of the codes to run as an integrated unit, rather than individual codes from which data must be taken and put into the next code, that code run, and you can iterate back and forth.

But now the codes can talk to each other and transfer information at specific time intervals without having to manually take data from one code to another.

I would also like to spend a little time talking about t he assessment which is done for this code. The assessment has been done more extensively than is required under the guidelines of Appendix K and the requirements of NUREG 0737.

We would like to talk about some of the regulatory requirements and how the regulatory requirements for a small break LOCA have been satisfied in the code, and the conclusions of the staff.

We received the code just a little over a year ago. Now, when the application came in, Siemens understood that the manner in which we conduct code reviews today is that we have to have not only the documentation for the application, documentation for the code, but the code itself.

The applicant submitted to us the code in a source code form and in a bindery form so that we could install both on the computer. We could build the code ourselves and make sure that the code builds the same as the code that is being used by the applicant.

We have test cases that we can run on the code, and we have of course all documentation for the code. We requested or sent out a request for additional information in December, and we have now received the formal response to those requests for additional information.

That sounds like there is not much time in which to review the REIs. In reality, the way we have been conducting code reviews has been to communicate to the applicant as we perform the review the concerns and issues that we have in our examination of the code.

So that we have communicated our REIs to the applicant throughout the past year. Then when we had all the REIs together, we then sent the REIs through the normal signature process, and formally asked them in December.

We have received draft copies of their responses along the way from the applicant, and now the applicant has formalized and gone through their QA procedure, and is sending their formal response to the REIs.

So it sounds like there is a big time lag before the REIs, and then suddenly everything comes at the end. In reality, it is not a big time lag, because we ask the questions and get answers as we go through the review.

And we have found in conducting these reviews that this is a very efficient way for us to conduct a review. We have prepared a draft safety evaluation report that was submitted to the Thermal Hydraulic Subcommittee for their review.

We have discussed that with the Thermal Hydraulic Subcommittee as Dr. Kress pointed out. We have had meetings with the subcommittee, and we talked very briefly with them back in March in the context of other code reviews.

That, yes, we had received the code, and yes, we were accepting it for review. There seemed to be sufficient material to allow us to do a formal review.

We met with them in August to go through the review plans, and to talk in detail about the contents of the code, and then we met with the subcommittee again in January, at which point we reviewed with them the safety evaluation report, which the staff had prepared.

And we are meeting today with the full committee, and we plan on finalizing the SER after this meeting.

We will go through and make sure that we have covered every concern that we have raised, and that we have covered every concern that the subcommittee has raised, and the concerns that you may raise today. So that when we issue a final SER, we can have all the issues properly closed.

Very briefly, some of the modifications that have been made to the code. The code started at A&F RELAP, which is a version of RELAP5 MOD2. You are probably all aware that the version that research has is RELAP5 MOD3.

Siemens started with a MOD2 code, and made some changes, such as making the code multidimensional, TOODEE2 capable, in the hydraulic components.

This is used primarily in the areas such as the downcomer, where we have been seeing that 1D modeling does not seem to be the best way. That there are TOODEE hydraulic effects, and so the applicant has modified the code to make it 2D hydraulic capable, especially in those areas where 2D effects become important.

There have been changes made in the energy equations. They have been reformulated to get rid of some of the problems that we have seen with

RELAP5 in the past.

One of the problems that we had several years ago was a misapplication of the code for containment analysis, and that came back to us, and we looked at what was being done.

And we said, wait a minute, you can't do this with RELAP5 because if you go from a very high pressure node to a very low pressure node, with a very large change in area and volume, the code doesn't conserve energy properly, and this is not the intent of the code.

Well, changes have been made in the way the energy equation is formulated in the code so that some of those problems are alleviated in this version of S-RELAP5.

We looked a lot at the numerical solution scheme that has been installed in the code. The numerical solution has been changed, and the approach to the S-RELAP5 code over the other RELAP5 codes to correct some of the numerical problems that create numerical instabilities, numerical diffusion, and other problems.

Those have long been a problem with the code. Sometimes they are created because the code developer's intent is to make the code fast running.

Well, they make it fast running, but they make the use of the code a real art form because if you don't use the code exactly right, you can create numerical instabilities.

Some of that has been taken out to make the code more robust, and with the recognition that you can still have a fast running code today without having to use the numerical schemes that make it unstable.

DR. POWERS: Instability tends to be a self-revealing thing.

MR. LANDRY: Right.

DR. POWERS: I mean, you get a bunch of spikes with RELAP.

MR. LANDRY: Right.

DR. POWERS: The other issue of numerical diffusion is a little more subtle isn't it?

MR. LANDRY: Yes.

DR. POWERS: Is it possible to tell just by routine examination of the results if you are getting a numerical diffusion?

MR. LANDRY: A knowledgeable user can.

DR. POWERS: All right.

MR. LANDRY: What Siemens has done is improved the numerics and the solution techniques, so that it reduces the amount of numerical diffusion, and it makes it less of an art so that the user, while they still are using knowledgeable users, it becomes less of an art, and less sensitive to the user.

They have improved the aspect of numerical diffusion.

DR. POWERS: What I am struggling with is when people talk about uncertainty analysis -- and I am dealing with an issue somewhat tangential to this particular SER -- they sometimes raise the issue of the numerical solution itself being a source of uncertainty.

And I am wondering is that a major uncertainty here?

MR. LANDRY: We don't think it is. So we are going to get into that more when we look at the code for the large break application, because that is based on an uncertainty analysis.

But in the discussions which we have had with Siemens' personnel at this stage, it appears to us that they have done a lot to take that numerical uncertainty out of, or reduce it, in the code.

DR. POWERS: Now, are there things that one should worry about other than the numerical diffusion and instabilities in these codes as far as the solution algorithm itself goes?

MR. LANDRY: There could be if the code is used by an unknowledgeable user, because you have to make sure that you are not making all the standard mistakes that a user would make, violating courant limits, and things of that nature.

DR. POWERS: This code lets you know about violating courant limits?

MR. LANDRY: Well, codes don't always come right out and tell you that. You have to be knowledgeable enough to recognize what the code is doing.

DR. SEARLE: That is where some of the instabilities come in.

MR. LANDRY: That is where some of the instabilities come in, but a lot of this is in the hands of the user also to recognize when the code is not behaving --

DR. POWERS: Noding schemes area also a problem.

MR. LANDRY: -- numerically correct, and when the result that the code is giving is wrong for numerical reasons, and not because of a phenomenological reasons.

Let's see. One of the points that we

looked at was with the heat transfer model. While the vast majority of the correlations that are used in the code are directly out of the RELAP5 set of codes, a change has been made to incorporate, instead of Dittus-Boeter for field boiling and for gas heat transfer, another correlation, the Shiralkar-Rouse correlation, which has a better representation of data.

It is a newer correlation, and represents data that has been checked against FLECHT SEASET data, and appears to be a better correlation to use. A very close correlation to Dittus-Boeter, but the uncertainty in the data seems to be much better.

So we feel like that is the kind of attitude that we want to see in an applicant that they will not just use a correlation because it has been used for 35 years, but look at it and say there is a better correlation today.

And let's try it out, and if it works right, and it gives very good answers, and it is stable, and it represents data better, let's go to a better correlation.

DR. SHACK: Just for my information, where does the virtual mass term arise from in here? Why do

I get a virtual mass term in the momentum equation?

MR. LANDRY: Gee, I wish Graham Wallace was here so he could go into that one. Let me ask Joe Kelly from Siemens if he could respond to that.

MR. KELLY: Joe Kelly from Siemens Power. It comes out in the toofuwood (phonetic) model, and so if you have an equation for the relative velocity, it comes in the time rate of change and in the relative velocity in the phases.

So it is the idea of like if you have a ball of liquid, or excuse me, a bubble trying to be accelerated in a liquid, it has to also accelerate some of the liquid around it.

MR. LANDRY: Thanks, Joe. Joe has been dealing a great deal in this discussion with the concerns that Dr. Wallace has raised on momentum, and so I appreciate his response.

Some of the models that have been changed to be consistent with the requirements of Appendix K include adding the Moody choke flow model. Power current flow limit has been upgraded.

DR. KRESS: We had some discussion about that in the subcommittee. Why do you have a code that is configured in such a way that it is basically incompatible with something like a Moody model, in the sense that the code itself calculates the things that create the critical flow as things progress down the pipe to the hole?

But the Moody model takes boundary conditions and calculates the same thing in a different way. Do you recall how that discussion turned out?

MR. LANDRY: That discussion came out that if all of the conditions were being calculated and fed directly into the Moody model, there could be a problem. But the code calculates fluid conditions, which then become the boundary conditions for a hard line Moody model.

And once those conditions are input into the Moody model, the Moody model will calculate correctly as it is supposed to calculate.

DR. KRESS: So you just calculate the boundary conditions?

MR. LANDRY: Right.

DR. KRESS: And where do you stop the calculation to decide where the boundary is?

MR. LANDRY: That is in the nodalizing of the pipe or --

DR. KRESS: It is the hole in it, the nodes of the pipe with the hole in it, you stop there?

MR. LANDRY: Yes, but that has to be in the user guideline specifications; that part of the sensitivity studies are where to come with the final mode before you nodalize for the break itself to see that you are getting the right conditions into the node for the break.

Okay. They have added to the code EPRA pump data. They have added, as I said earlier, the ICECON code, RODEX2, TOODEE2, and they have changed the code architecture, so that even though it is based on the RELAP5 MOD2 code, the architecture now matches the RELAP5 MOD3 series of codes, the more modern architecture, and it is based on FORTRAN 77.
So they are upgrading into a more modern structure.

DR. SEARLE: I have a couple of questions. Well, one basically. Is there somewhere in all of this that tells us what the limits are on the application of this code?

MR. LANDRY: In the submittal, yes. Th is application is for a small break LOCA.

DR. SEARLE: No, no, I am talking about in terms of the models that are being used to define specific physical phenomena, are there any cautions about trying to apply this code in cases where clearly you don't have that situation?

DR. KRESS: Are you thinking maybe about upgrades? Is there something --

DR. SEARLE: Well, for example, we know that there are a bunch of people running now talking about increasing the burn up on fuels. I think there are probably problems with Baker-Just when you go above 40K maybe.

DR. KRESS: Yes, I think you're right.

DR. SEARLE: And is there anything that cautions you that you may be walking the plank if you try to use this in the wrong region?

MR. LANDRY: Well, the documentation provides the perimeter range over which the different models are reviewed, assessed, and acceptable. We have to rely on user guidelines that they will not use the code outside those ranges.

And then we do have the option or we have the requirement when a calculation comes in to review the application of the code to see that it was applied and used within the proper range of perimeters for each model.

DR. SEARLE: You mentioned user guidelines.

MR. LANDRY: Yes.

DR. SEARLE: Have you looked at the user

guidelines to convince yourself that a reasonably sensitized user would be able to pick up on any problems by looking at those guidelines and thinking about what it is that he is trying to apply to them?

MR. LANDRY: Well, the manuals that we have seen, we would in our judgment say, yes, the reasonable user would understand where the code is to be used and where it's not.

This code -- I think what you may be referring to is other codes which are given out, or sold, or distributed throughout the world, and throughout the industry, and you don't have the control over the user.

This code is used solely within the corporate structure of Siemens Power. So that they do have through their quality assurance program have the control to ensure that the code is used properly, and it is not used outside acceptable ranges or applicable areas for even things like Baker-Just equation.

DR. KRESS: And we had a concern in the subcommittee about default values built in, and they might not properly be used. But Ralph's answer was what set our mind to ease on that, that it is within the corporation, and when they get specific applications, that is one of the things that they look at.

MR. LANDRY: Right. It is not a pure black box where the code is used, and just an answer is given to us. We have the responsibility to review the way it has been applied also.

MR. LEITCH: I seem to recall some earlier versions of RELAP5, when you put in various sizes of small break LOCAs, a prediction of peak fuel temperatures had some fairly significant discontinuities in it, and gave rise to questions about the validity of the code. Does this have that same problem?

MR. LANDRY: This I don't believe does, because the RODEX2 model has been incorporated in the code.

MR. LEITCH: Say again? RODEX2?

MR. LANDRY: Which is the fuel model. The fuel model, which Siemens is using in this code, is a fuel model which we have reviewed and approved for use in the Siemens fuel design work. We have reviewed that pretty heavily, and that is not using the RELAP5 fuel model now.

MR. LEITCH: Okay.

MR. LANDRY: In talking briefly about the code assessment that has been done, the small break

LOCA assessment cases are pretty well defined for applications. Post-TMI, the requirement came out in NUREG 0737, Section II.K.3.30, of what was required for assessment of a small break LOCA code.

And there the position of the staff is very short, and says that appropriate LOFT and semi-scale tests are to be used for assessment of small break LOCA.

If you go down in the text, it then suggests two specific tests; a specific LOFT test, and a specific semi-scale test, should be used for the assessment purposes.

DR. KRESS: That wa a subject discussed also at the subcommittee.

MR. LANDRY: Right.

DR. KRESS: And I remember the flavor of the discussion was why is it that we believe that just two tests provide sufficient validation for a code for Appendix K purposes. And I don't recall what the answer to that was.

MR. LANDRY: Well, those two tests looked at two specific problems that came out from the calculations that were done for the TMI-2 accident.

DR. KRESS: As I remember, one of them, I believe, was the LOFT test. Basically you could match it with just some energy balance, which almost any company could do.

MR. LANDRY: Right. But in assessing the S-RELAP5 code, Siemens has gone beyond those two tests that were required. In fact, they looked at all the test data that were available and said these two tests are superseded by other tests at a later time.

DR. KRESS: That was the answer. I remember it now.

MR. LANDRY: And would be better tests, and tests that would give a more thorough examination of the capability of the code. In fact, the assessment was done against a different semi-scale test, and a different LOFT test, against the --

DR. KRESS: And that leads me to another question. Do we have a bad rule when we specify just those two tests are sufficient to validate a code?

You know, it has nothing to do with this Siemens application. But is this a bad rule that we have?

MR. LANDRY: Well, I would rather say that at the time, and with the data that were available, we felt that this was --

DR. KRESS: Well, at the time, that may be just about all it was.

MR. LANDRY: -- the best information we had for assessing the phenomena that we saw occurring in CMI, and that the codes had to predict in particular -- well, there are other tests or other assessments that have to be done.

DR. KRESS: Does the rule read -- well, it may not be in the rules. It is in the NUREG.

MR. LANDRY: The NUREG says --

DR. KRESS: Does it suggest at least these tests?

MR. LANDRY: Well, I quoted the position of the staff in the SER verbatim, and the position simply says or concludes with that they have to assess against appropriate LOFT and semi-scale tests.

In the descriptive material that follows that position, it suggests that these two tests are the tests that must be used, L3-1, and S-07-10B.

DR. KRESS: So Siemens could have stopped with just those two?

MR. LANDRY: According to those requirements, they could have, but they didn't.

MR. BOEHUERT: But it's really up to you guys though, isn't it, Ralph?

MR. LANDRY: Yeah. But they didn't stop there. They went into two different LOFT and semi-scale tests, plus 2D flow tests, and UPTF tests, and a very recent BETHSY test.

But then in looking at the assessment that was done, Siemens put together what they called an informal PIRT, because Appendix K doesn't require a PIRT.

But they put together an informal PIRT that looked at different locations in the reactor cooling system, and different phenomena that would occur, and how they rank those phenomena, and then what test data, what test facilities, what test data would best represent the phenomena that they are trying to examine.

That was then used and the total assessment was based on that informal PERT. So the assessment that was performed was not just based on the one semi-scale and LOFT test, but it was based on these tests, plus all the tests that were done in response to their informal PERT.

So our conclusion was that they examined significant perimeters throughout the range that could occur in different components of the system, and throughout the different aspects of the small break LOCA.

They have substituted newer tests, which are supposedly and should have better data, better qualified data, for the older tests. So they have used good tests, better qualified data, and a more expanded assessment that is required.

DR. POWERS: A NUREG is not a rule. It is a recommendation.

MR. LANDRY: That is correct. The only caveat that we make is that following the issuance of the NUREG and the bulletins and orders, some plants may have had put into their licensing basis the requirement that they have to analyze these two tests.

So that when we get the applications and using this code that we would have to make sure that if it is in the licensing basis of a particular plant that they use S-07-10B and L3-1.

That those cases would be analyzed or there would be a change made to their licensing basis to use these assessment cases instead.

DR. POWERS: An interesting point.

MR. LANDRY: Now, we have already touched on some of the regulatory requirements in a previous discussion. In looking at the application which we have received, the modeling requirements of 10 CFR, Part 50, Appendix K, which such is Moody critical flow, have been incorporated in the code.

We believe that the assessment not only meets the intent of II.K.3.30, but goes beyond the requirements of II.K.3.30. A full assessment has been done, a very good assessment.

Instead of calling it an informal PERT, this is a step beyond the requirements of Appendix K.

Many sensitivity studies have been put into the application, and these are required of all licensing basis LOCA codes. They have looked at a range of break size once they determine the worst break.

Then they vary the effect of time step, loop seal model, and pump model, radial flow foreign coefficient, nodalization, and what they found in all of these sensitivity studies after they determined the worst break size is that each of these effects is less than five degrees on peak clad temperature.

So that then comes to the conclusion that, yes, they have a converged solution, and the code is functioning properly.

DR. KRESS: It also says that peak clad temperature is not very sensitive to those things.

DR. POWERS: Why does it show that they have got a converged solution?

MR. LANDRY: I'm sorry?

DR. POWERS: Why does it show that they have got a converged solution?

MR. LANDRY: Well, in addition to looking at the numeric response, it shows that there isn't a big variation for any of these perimeters. Altogether, it is not just those perimeter variations.

DR. POWERS: You can't tell from the fact that it is only 5 degrees. You can only tell how the iteration approaches that 5 degrees.

MR. LANDRY: Correct. Correct. It's not just that. It is everything combined that indicates that they are converging.

DR. POWERS: All right.

MR. LANDRY: Conclusions that the staff has arrived at is that the ANF RELAP code which was approved, the RODEX2, TOODEE2, ICECON codes, all of which were approved individually by the staff, have been combined into an integrated code, an integrated package that can perform the entire calculation without transferring data manually from code to code.

We believe that the code documentation supports the modifications made to the ANF RELAP code. We accept the modifications.

DR. KRESS: Let me ask you a question about that. I think in our subcommittee meeting, we put it this way. We expressed some disappointment in the status of documentation, in the sense that the equations and models that we were presented were different than the ones that are in the documentation we had.

And that some errors in the previous equations were still in the documentation. Is that going to be fixed over some time period, or is the situation going to be different when they submit for the best estimate application, or maybe I should be asking this to the Siemens people. I don't know.

MR. LANDRY: Well, I think Jerry Holm from Siemens will address -- would you rather address that now or later, Jerry?

MR. HOLM: I can address it right now. There were a number of what were characterized in the subcommittee meetings as typos identified in two of the documents that we submitted, the models and correlations document in the programmer's manual.

And in conjunction with supplying the response to the request for additional information, we went through and tried to identify all the typos in those two documents, and we have provided revised documents, along with the RAI responses.

DR. KRESS: And those will be the

documents, plus any MODs that you make, and that you will submit for the best estimate analysis?

MR. HOLM: They will be the starting point for the best estimate. There have been some small number of additional model changes made for the best estimate program, and we will describe those and modify those documents.

DR. KRESS: You will get rid of the Moody model, for example?

MR. HOLM: It won't be used for the large break LOCA, but it will still be in there, because we have to use it for small breaks.

DR. KRESS: So it would be an option, I guess?

MR. HOLM: Yes.

MR. LANDRY: Okay. As was just discussed, we point out errors in the course of the review and in documentation. One thing that we would like to emphasize is that this has been a very fast review.

If you look at the history of reviewing computer codes, one year is a fairly quick turnaround on a review, and we feel that is primarily because Siemens Power Corporation has been very responsive and very cooperative during the conduct of this review.

When we asked questions, they were very

quick to work together with us to arrive at an acceptable answer. We feel that their cooperation and their willingness to work through any problems that we discovered in this review was instrumental in being able to conduct a review in such a relatively short period of time.

DR. KRESS: I would like to second that comment, Ralph. We found in the subcommittee meeting that their ability to answer our questions, and their candidness with their responses was actually refreshing. So I agree with you.

DR. SEARLE: Are we going to see some actual run results later?

DR. KRESS: Probably not. Did you plan on presenting some results still?

MR. LANDRY: Yes. In my presentation I will show one data --

DR. SEARLE: Very good. Thank you.

MR. LANDRY: So the conclusion of the staff's review is that we find the S-RELAP5 code acceptable for use in satisfying the requirements for analysis of the small break LOCA under the requirements of 10 CFR, Part 50, Appendix K.

DR. KRESS: That is the major finding right there.

MR. LANDRY: That is the import that you have to get to.

DR. KRESS: You have to get there, otherwise --

MR. LANDRY: Otherwise, we go back and start over.

DR. KRESS: -- you go back and start over, yes. Okay. I guess now we turn the thing over to Jerry Holm, of Siemens.

DR. SEARLE: Jerry, I've got to say this logo you have on here with a PWR bird cage, and a BWR box, gives me the cold shivers.

MR. HOLM: The topic today will be the Siemens PWR Appendix K small break LOCA analysis, and this is going to be based on the code S-RELAP5, and my name is Jerry Holm, and I am the manager of product licensing for Siemens.

And I will give a short introduction, and then Joe Kelly will give some more detailed information about the code and the methodology. But of course we have to keep it to something of an overview since we have only got about 45 minutes or less.

Again, I am just going to give about three slides for an introduction, and then Joe Kelly will talk about the S-RELAP5 code, and the first thing he will show is the relationship to the RELAP5 family of codes, since RELAP5 itself is extensively used in the industry.

We will give a summary of Siemens' enhancements, and only a selected few of those that Ralph Landry talked about, the ones that we thought were most important. We will give a summary of the methodology for the Appendix K LOCA, analysis, and then a summary of validation.

And we have chosen one of the benchmark cases to show some plots from so you can see the technical comparisons. Then I will get up at the end and just make a quick conclusion.

Okay. Ralph Landry alluded to the fact that we are going to be presenting or submitting to the staff a large break LOCA methodology, and what we call our realistic large break LOCA methodology using S-RELAP5, and that submittal will be later this year.

Right now what we have submitted to the NRC is the use of S-RELAP5 for small break LOCA, and we have also submitted it for non-LOCA methodology. Our future plans are to extend this code to BWR LOCA analysis, and long LOCA analysis.

And in fact the R&D program for the

conversion to a BWR LOCA will start later this year after we submit the realistic LOCA methodology and the development staff free up to do that work.

Our motivation for this is primarily that the cost of benchmarking and doing maintenance on codes is increasing, and our desire is to choose one code to try to maximize the results of our benchmarking work, and also to maximize the expertise of our staff.

It is a lot cheaper for us to do work and become experts in one code, rather than six, and that is the main purpose. We have been working on S-RELAP5 for realistic LOCA methodology for close to 15 years now, and it is that extensive effort that led us to choose this as the base code.

We provided, we think, an extensive amount of information to support the staff's review, and we have a topical report which describes the methodology in the benchmarking.

And then in addition to that, we provided a significant amount of supporting documentation; our models and correlation manual, a programmers guide, an input requirements manual. We provided on a CD-ROM the code source and an executable version, and sample cases so that the staff could actually run the code.

And we have made a presentation to the NRC in March of last year, and two presentations to the ACRS Thermal-Hydraulic Subcommittee. And then we provided a formal response to the RAIs which we sent last Friday.

And the main point is that we have tried to provide sufficient information to support the use of the code for small break LOCA. With that, I will turn it over to Joe Kelly.

MR. KELLY: Okay. This is the same outline slide that you saw just a minute ago with Jerry, and I am going to give a very brief overview of the history of S-RELAP5 thermal hydraulics code, and then talk about the Appendix K methodology for small break LOCA.

And show one example of the validation, and that is the BETHSY test, and it is the International Standard Problem Number 27. Actually, Ralph Landry covered this, but I had it in pictorial form, and it is the relationship of the S-RELAP5 code to the other flavors of RELAP, and also the other codes that we have incorporated in it.

We started with MOD-2 of the RELAP5 code which was developed at the IENL, and we made changes to it to perform non-LOCA transients, main stream line break, and small break LOCA Appendix K analysis.

And this resulted in the ANF-RELAP code which had been submitted and approved in several different topicals between the years of 1983 and '89. And so this is the code that we have been currently been using in our licensing applications.

Since that time, RELAP5 Mod 3 was developed, and from that we have primarily taken upgrades to the code architecture to make it more portable. Again, as Ralph said.

Also, there are three stand alone codes; RODEX2, which is fuel rod performance, TOODEE2, which is a hot rod model accounting for diversion flow due to flow blockage; and ICECON, which is a containment analysis code.

Again, these are stand alone, and they had all been submitted and approved individually, and they were used in concert with ANF RELAP, and that required manual transfer of data from one code to the other.

You know, the output of one is input to the other, and sometimes it would require an iterative process in those two. So what we have done now is build these three codes into what is now history lot 5, and so the data transfers happen automatically so you don't have a staff intervention there.

And also so that if it is something like the effect of containment pressure on a large break LOCA, that is an integral part of the analysis, and not something that has to be done off-line, iterating between the results of two codes.

DR. KRESS: This may be a little off the subject, but how do you validate a hot rod model like TOODEE2? Don't you have to have a full bundle test, with an actual and a radial power distribution? How is something like that actually validated?

MR. KELLY: Well, unfortunately, you have the wrong person up here to answer that, because my experience is more in realistic, and this is more Appendix K, and from what little I know of it, what it does is that it implements NUREG 0630, and the regulations to do with that.

DR. KRESS: Okay. I understand that. Okay. But you may need something more when you get to the realistic.

MR. KELLY: If on realistic we were going to try and take credit for the enhancement in heat transfer that you see when you have blockages is a drop with shattering, et cetera.

And that would be a much longer assessment program to validate that, using something like the

FLECHT-SEASET 163 rod test, but we are not planning to try and take credit for that.

And then finally there are a number of enhancements that Siemens developed, which I will briefly show in the next slide. There are a number of enhancements, but they mainly fall into four different areas.

They are mass conservation, energy conservation, momentum conservation, and the constitutive models.

In mass conservation, the numerics have been improved to minimize mass error during long term transients, and I will show something on that in the next slide.

Energy conservation, again, Ralph mentioned this. We reformulated the energy equation to eliminate the problem that would occur when you have a flow going across a large pressure drop.

It is not important for small break, but it is important for large break, having to do with energy deposition into the containment.

For momentum conservation, traditional RELAP5 uses cross-flow junctions, and in a way to try to emulate multi-dimensional, or multi-regional flows might be a better way of saying it.

What we did instead was implement a 2-D component, and is primarily used in either the core or the downcomer. And what had been seen in the past with cross-flow junctions was anomalous flow recirculations. The 2-D component eliminates those.

Of course, there are hundreds of constitutive models in the codes, and a number of those have been upgraded, primarily to increase accuracy in the large break LOCA application.

But there are also modifications to what is called a vertical stratification model, which helps improve the loop seal clearing prediction.

And in talking about long term mass conservation, when system thermal hydraulic codes, such as TRAC and RELAP5 were first applied to small break LOCA back shortly after the TMI, one of the primary challenges was long term mass conservation.

When you start running these transients out to a million time steps, what would happen is that the errors in solving the mass equation would accumulate over time, such that in effect the code would be either creating or destroying mass.

And if that fraction became appreciable relative to the inventory in the vessel, then there is no validity in the calculation whatsoever. So this is an area that Siemens paid some attention to.

And so what I am going to show is from results from integral assessments and the small break LOCA sample problem, which were part of the submittal in the topical report.

So the three integral tests and the PWR sample problem, the transient time for each of the tests, the number of time steps, and the mass error, and again, this is the error in conserving mass for the entire system, normalized with the initial mass and expressed in percent.

So, for example, if you go to the BETHSY test, there were over a million time steps, and the cumulative mass error is less that 2/1000ths of 1 percent. So that is very good. So we do not have a problem anymore with long term mass conservation.

And that is all that I am going to say about the code, unless I get questions, and we are going to switch to an overview of the methodology.

And the first thing to realize is that we define a methodology as basically two things. It is the codes that we use, but it is also how we use those codes.

And once a topical report has been approved that methodology is then encapsulated into an analysis guideline. And then that analysis guideline, together with the quality assurance procedure -- because as Ralph said, all our users are in-house, and they are subject to the analysis guideline and a QA procedure.

So consequently you have the plant model nodelization specified, and you ensure that the Appendix K conservatisms are correctly applied. Also, there are sometimes additional conservatisms that Siemens prescribes.

For example, the way that we do loop seal modeling for the small break LOCA; and also things like the delays in diesel start times. And these are all specified as exactly what you are going to do in the analysis guideline.

And then because of the QA procedure, the analysts are constrained to adhere to those guidelines. So that gets rid of things like the user effects that you hear about a lot these days.

When you looking at performing small break LOCA analysis, you can do a PERT and come up with many, many phenomena that appear to be important. But there are really four major factors.

The first is, if you will, the transient that you are running, for determining the limiting single failure; and for most of our plants, this is usually a loss of one diesel generator set.

So consequently we are going under the assumption of only one high head safety injection system being available. That's what makes the small break LOCA something interesting.

The next is where you are in the fuel cycle, and the limiting condition is normally the end of cycle, and the reason for that is that gives you a top-skew power profile, so that your high power part of the core is in the part of the core that will become uncovered.

The next is break size, and so we perform a break spectrum to determine the limiting condition, and that is really looking for a window. And what the window is bounded by are very small breaks, where the break flow would be smaller than the capability of the safety injection system to make it up. So those cases don't even uncover.

It is bounded on the other side by breaks that start getting large enough so that you get a fairly rapid depressurization to the accumulator set point, which then recovers the core.

So what you need is a break that is small enough that the flow is -- excuse me, large enough so that the flow is greater than the safety injection makeup, but small enough that you get a gradual depressurization rate so that you have a prolonged transient with significant core uncover.

And then finally there is how you treat loop seal clearing. The peak clad temperature is affected by both which loop is clear and the number of loops.

So we have come up with a proposal and a methodology for this in order to remove the variability that you see between calculations. I did bring backup slides on both the break spectrum and the loop seal clearing. So if there are questions about that, I can provide more details.

Now, looking at the validation matrix. Actually, there is four of them. The first is called the general matrix, and it is a set of separate effects and integral effects tests, and those are performed and documented for every code version.

Then there is the small break LOCA matrix, and again it is both integral and separate effects tests, which is what Ralph showed. And that was part of the small break LOCA submittal.

Similarly, there is a non-LOCA assessment matrix, and those are a set of integral tests which were part of the non-LOCA submittal, and for the realistic large break LOCA, there is a PIRT based matrix that is much more extensive. You know, in the order of more than a hundred tests.

And so when we come in with the realistic, you will be seeing that, and what that does is that it shows not only code applicability of the transient, but also how we determine the uncertainties in the models so that it can then get propagated through the uncertainty analysis.

And the small break validation matrix you have already seen. It is one BETHSY test, International Standard Problem 27, two-inch LOCA break, and this one goes through pretty much all of the expected phenomena that you want to see.

You know, the natural circulation phrase, loop seal clearing, core boil-off, and also recovery.

Semi-scale S-UT-8 has core uncover before loop seal clearing. So it is a different kind of test. LOFT LP-SB-03 is basically a core boil-off and uncover.

UPTF loop seal clearing, this is a proprietary test that was run at KWU, and so it is a full-scale model of the loop seal. And so there is a separate effects test to examine the clearing process.

And then 2-D flow test, and the purpose of those was to provide some assessment for our 2-D component.

I am going to show the results of one, and this is the BETHSY ISP-27, and it is probably being shown for two reasons. One of those is that it is the most comprehensive test, in the sense of going through all of the phenomena. The other one is that it is also the best data comparison.

BETHSY is a full-height, 1/100 square model of a 3-loop RWR. So as test facilities go, it is pretty big. For example, it is 17 times larger than semi-scale.

Test 9.1b is a 2 inch break with no high head safety injection. It results in deep core uncover and rod heat-up.

In the S-RELAP5 assessment, the input model follows our small break LOCA modeling guidelines, with a few small changes. Obviously if you are doing an experiment and you want a realistic prediction, you don't use ANS, plus 20 percent, for the power.

You use the actual power that was used in the test. Also, we note from the test results that one of the intact loops, and so loop number 2 clears.

And so what we have done is apply our loop seal modeling methodology, where we bias the broken loop and one intact loop to plug. So then in our calculation, we clear loop number 2 just as it was done in the test.

And that loop seal clearing is something that we can talk more about if you would like. It is something that in reality is more statistical, and you can't really do it deterministically.

And so what we have done is put in a biasing methodology to limit the variability and ensure a conservative result. Also, for the critical flow model, getting the break flow correctly in a small break test, if you want to do a prediction of the test, it is very important.

And so you can't use Moody here. So we use the more realistic critical flaw model in the code. Similarly, even though I said BETHSY is pretty large, it is only about 420 rods, and so it is just slightly larger than one 17-by-17 assembly.

So using a 2-D component for the core didn't make a lot of sense, and we used a 1-D core model. And as I said, we get an excellent comparison of both the core collapsed liquid level, and the maximum rod temperature. And that's what I am going to show.

So this is the core collapsed liquid level comparison. The black line is the data, and the red line is the S-RELAP5 prediction. It says core collapsed level in meters. It is actually core, plus a good chunk of the lower plenum, okay?

And it's versus time, and the data is done by delta-P cell, and so what you are really seeing is a delta-P measurement. And that is what we have plotted also for S-RELAP5.

This is not a sum of wood fractions, but rather it is a pressure difference between replacing the lower plenum and the top of the core. So what you are seeing here in the initial part is the front coast down, and that is the frictional pressure drop having to do with the flow coasting down.

Once you get to this point, the two-phase mixture level is in the upper plenum, and the core, although two-phase, is completely covered. That's why the collapsed liquid level just sits here constant for a while.

The little blips in both the data and the calculation is the depression and recovery of the core level did a loop seal clearing. Immediately after that, the liquid level in the upper plenum has receded into the core, and you begin the boil off part of the transient.

It is at this point that you have reached a pressure, such that the accumulators begin to inject. You recover the core inventory, and the PCT location would occur about this point in time.

And for the maximum clad temperature, it is temperature versus time, and again the black curve is the data, and the red curve is S-RELAP5, and as you can see, there is a very good prediction of the dryout time, and also the peak temperature and the recovery.

And there is about a 20 to 25 degree K overprediction in the S-RELAP5 calculation, but this is considered to be an excellent comparison.

DR. KRESS: Could I see your previous curse a minute? Although it doesn't matter to the temperature, what causes the bouncing around to 4,000 seconds?

MR. HOLM: Well, that is a good question. At this point, the core is two-phase. It is a liquid solid at about this point. So you have a two-phase level in the core, and remember I said that there are delta-P measurements, and not just measurements, but it is the delta-P in the core.

So you are not seeing void fraction

changes, because this looks like a two meter change in level, which would be catastrophic. But actually it is an instantaneous delta-P difference between two computational volumes.

And what you are seeing is a liquid level crossing a cell boundary, and the thing that we discussed in the subcommittee about having to accelerate the liquid, and it gives you a little bump in the momentum equation.

And so there is an instantaneous pressure spike associated with the level crossing. So what you are probably seeing, because you see so many of them, is the level doing this, going back and forth across the cell boundary.

But the indication of the collapsed level is artificial, in the sense that we are not taking two meters of water in and out. So, in summary, the proposed Siemens SBLOCA methodology replaces the combination of ANF-RELAP and the TOODEE2 code with S-RELAP5, thereby streamlining the analysis.

And that is good for us from the standpoint of being able to concentrate more effectively, but it also makes the reviews easier. And also we have done some work to improve the loop seal clearing behavior, and that is the biasing methodology.

And now I did not show this, but Ralph alluded to it as well, and it is in the topical report, but the results from the PWR sample problem and the sensitivity calculations show that this methodology is both convergent and robust.

The assessment shows that S-RELAP5 is capable of capturing the important phenomena for SDLOCA, specifically loop seal clearing, core boil-off, and recovery, with an acceptable level of accuracy.

And therefore the proposed methodology, or the proposed use of S-RELAP5 for an Appendix K SDLOCA is suitable for licensing. And then I will give the floor back to Jerry Holm.

MR. HOLM: Since I only have one slide, I will use this mike if that is okay. The bottom line from our perspective is that the SER provides Siemens with the ability to reference the topical report in future licensing submittals without further NRC review, and that's why we submit topical reports.

And the draft SER to be seen has no additional conditions or restrictions over and above what we have put on the methodology ourselves inside the topical report. So we consider that a very successful review from our perspective.

Our goal in this meeting is to hopefully come out of here with concurrence from the committee that the NRC can issue this SER by the end of February, and that is our presentation, unless you have questions.

DR. KRESS: Do the members have any burning questions that they want to ask? I remind the committee that there is under Tab 3 that there is a report on the subcommittee meeting, and provided by the cognizant engineer.

And we also have one of our consultant's reports under PINK3, and then we have a draft letter that we can look at.

DR. SEARLE: Is the consolidation of these cases the aspirations of doing BWR with the same codes as you do PWRs with? Does this suggest that these predictions are going to become once again the realm of the physics, rather than the realm of the programmer?

DR. KRESS: I am not sure I know what you mean.

DR. SEARLE: I am being facetious. I am very happy to see that it does appear that physics is being to reemerge in the storm tossed waters of this whole process.

CHAIRMAN APOSTOLAKIS: Are we done?

DR. KRESS: Yes, I can refer it back to you.

CHAIRMAN APOSTOLAKIS: Well, it seems that we are doing great today, right? We are 20 minutes ahead of time.

DR. KRESS: We have a skillful chairman.

CHAIRMAN APOSTOLAKIS: Wow, I'm impressed. Yes, sir?

DR. POWERS: Let me point out that I had passed out to members a section of the research report which involves a pretty categorical disagreement between two members in assessing three research programs.

And one of those members is being vigorously and heavily lobbied, but has not wavered one iota in his position. We will need to have an ACRS position.

We need to look at that and be prepared at least to interrogate the two people on their positions, or to establish a run.

CHAIRMAN APOSTOLAKIS: Okay. By the way, are we going to receive the full research report?

DR. POWERS: Undoubtedly at some time.

Like since I don't have at least three of the inputs, and I am struggling with at least three others.

CHAIRMAN APOSTOLAKIS: But sometime during this meeting you mean?

DR. POWERS: I didn't say that.

CHAIRMAN APOSTOLAKIS: That's why I asked you. If you had said it, I would not have asked.

CHAIRMAN APOSTOLAKIS: Okay. So we will recess and reconvene at one o'clock.

(Whereupon, the committee recessed at 11:40 a.m.)

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

(1:01 p.m.)

CHAIRMAN APOSTOLAKIS: Okay. This afternoon the first subject is the proposed American Nuclear Society Standard on Eternal Events PRA, and we have three members of the group that developed the standard here, Bob Budnitz, Ravi Ravindra, and Nilesh Chokshi, right?

Ravi Ravindra is with EQE, and Dr. Budnitz is with Dr. Budnitz. We all have the draft, and I understand that it is not out for public comment yet is it?

And there will be no transparencies, but we will have a short introduction by Dr. Budnitz, and then perhaps we can discuss the standard. So, Bob.

MR. BUDNITZ: I am going to spend what I think is less than 10 minutes with an introduction, and what I am going to do, because I think it is the right thing to do, is to outline for you a half-a-dozen technical issues that we face, and some only a minute or so about them, so that you will know what we think are the important issues that confronted us.

And there was some stuff that we did that wasn't or we don't think was controversial, although you might, and that's fine. But we are going to at least outline to you what we think is the central technical challenge that we faced going in and how we resolved it.

And then you can ask questions, and we will be happy to discuss with you whatever. And just to be sure that you understand, there was a five member writing group, and the others were Tori Ye from Southern California Edison, and Bill Henries from M-Yankee.

But in fact the writers are in front of you. The three of us wrote everything that you see. Those others didn't write anything, although they were very important in review. Nilesh had principal responsibility for the seismic hazard part, and Ravi Ravindra wrote the seismic peculiars part, the part on seismic margins, the part on wind.

I wrote all the rest -- the flooding. Ravi and I together wrote the part on how you screen other events, and Nilesh was in there working on all that stuff, too. So it was a three-person effort.

CHAIRMAN APOSTOLAKIS: Does ANS have a procedure in which --

MR. BUDNITZ: Yes, I am going to say that next. The procedure is as follows. The American Nuclear Society has a committee, a risk committee, a

Risk Informs Standards Consensus Committee, and Paul Miko chairs it.

It has 24 members on it, and it has been in existence a couple of years, and that committee in the ANS is the balloting committee. That committee appointed us, and we report to them.

The ANS committee recently balloted to release this standard for public comment. There is two ballots. There is a ballot to release public comment, and then a few months from now, or a few centuries from now, depending on how it goes, they will ballot to accept the standard, and then it goes out.

So it was released for public comment on January 26th, which was just the other day, 60 days, and anybody can get it. We sent it to you in advance. It was publicly available about the first of the year, and we sent it out to a lot of people in the first year, but the comment period started on the 26th.

And that process will run its course as we get public comments and we respond to them. We started this process in the summer of '99. It was about a year-and-a-half, but all of the serious writing was done between about the 1st of January a year ago and August.

We pretty much had this thing wrapped up in that 7 or 8 month period, and from August until we released it, we held it up for 4 or 5 months because we were waiting to watch what happened to the ASME standard which were coordinated, and I will say something about that next.

As I hope you know, the ASME, American Society of Mechanical Engineers, outfit has a committee on which I serve, which I spent three years trying to put together a standard for PRA methodology for internal events, accidents that initiate from transients and LOCAs, and that's what we mean by internal events.

And that process after 3 years isn't quite done, although it is converging very rapidly now. I am on that committee, and we hope that in another couple of months we will have that done, because we are now responding to public comments from the draft that was issued for public comment in August.

We think in another couple of months that will be done, and we were waiting to release this for that period because the ASME standard had not settled down.

There were some very important questions that were being discussed, which we will come to in a minute, which we were hostage to, in the sense that we rely on, and we waited.

It turned out that that came out in a way that didn't effect very much of anything that we had written, and so then we released it for public comment.

So I am just going to make it real quick and short so that we have time for discussion. I am going to talk about the scope. The scope of our thing is earthquakes, wind, flooding.

And then a section that I am going to call other external events. They are external to the plan. Fires is not part of this, unless it is a forest fire. But aircraft crash, industrial facilities, and so on.

Earthquakes have a separate chapter, and winds, and flooding, they have separate chapters. And we have another one on other. Now, the other has two sections. One is screening, so you can screen something like hail storms if you are in Arizona.

But if you can't screen it, there is also a section on how you analyze it if you can't screen it. And then we also have a separate chapter on seismic margins, separate from seismic PRA, and I will describe that in a minute. So that is the scope.

Now, a crucial piece of this is that the scope also includes important sections that are this long, just a few lines, because we referenced the ASME standard by reference. We say do ASME, and I will just describe what they are.

For example, the whole system's analysis part of PRA, we reference ASME directly. I mean, we weren't going to rewrite how you do a Bayesian update of generic data. ASME does that. Although we have some places where we supplement ASME because we need to.

Like, for example, in HRA, human reliability analysis, if you have to do something a little different for seismic, we tell them that, but the rest of it we reference directly.

We reference directly the peer review requirements of the ASME, and how you put together a peer review team, but we have supplementary guidance in there, some requirements about peer review. For example, emphasizing lockdowns, because that is something important for us that was not quite so emphasized in ASME.

We reference the documentation section in ASME directly, but we have supplemental requirements and documentation, and how you document seismic margins, or when, or whatever.

And we also reference the application section, the crucial section on applications in ASME which tells you how you go about doing application once you have got a PRA.

We also in order to make it seamless with ASME, we use the same format. The ASME has high level requirements in a broad area, and then what we call sporting requirements, which are the ones that you have really got to meet, that are below that and we did the same thing.

The idea was so that a person who is using it together would see the same sort of thing. But crucially we don't have three columns of capability, and I will come to that in a minute.

We don't have that. We have one, which was intended to be what ASME's column 2 was in the first round before they got to three, which is what we will call a good quality, state-of-the-art PRA today, and that's what we have. I will talk about that in a minute.

And also crucially, for almost every requirement, you will see that we wrote a commentary. Sometimes short, and sometimes long. Sometimes longer than the requirement. ASME doesn't have any of that. We think that is a valuable addition, and you can quiz us about that if you want.

I mentioned already peer review, and I will come back to the three columns in a minute. We wrote a special set of requirements on peer review, and Ravi actually wrote them, emphasizing the need for walk-downs to make sure that for external events that you captured the plant, because plant specificity is crucial for these events which damage things in the plant.

PSHA. As you have observed, I'm sure, we exclusively endorse the Livermore and EPRI hazard studies of the late '80s by saying that if you did one of those, you met the standard for probablistic seismic hazard analysis.

But what we mean by she made the standard for 1988, you still have to do an update to make sure that no earthquake information has come along since then that would invalidate what they did.

But we explicitly endorse that, but we also have a whole lot of requirements, which if you are doing it over, or if you didn't do that or whatever, that you have to meet.

And those requirements are pretty much tailored to the well-known -- and I will say this because I was an author, as was our chairman, George

Apostolakis, of the well-known SSHAC process, the senior seismic hazard analysis committee that I served on and George did. I chaired it a few years ago.

And which has a process for seismic hazard analysis, and we explicitly have based our thing on that. If you are doing it over, or if you hadn't done it in IPEEE. I am just going on with issues, and then you can talk later.

Fragilities. Ravi wrote this piece. The seismic fragilities for PRA are the well-known standard methods that have been used for some years with the fragility curves.

But for seismic margins, we explicitly reference -- and if you do it, your okay, the CDFM, and if it was used in seismic margins, and if you do that, that's the acceptable method.

Uncertainties. This is a crucial point. We explicitly incorporate treatment of uncertainties in the standard, in the requirements, in the things that you have to do. You can't meet this standard if you have not considered uncertainties.

The reason that I am saying that is because if you don't know, I will tell you, but the word uncertainty appears almost nowhere in the ASME standard for internal events.

I am on that committee, and have been for 3 years, and I am unhappy with that, but that's the way it is. I am a minority there. We have done that. We don't see how you can do an external events PRA without that.

One last topic, and then I will turn it over to you, and you can ask questions, or if my colleagues want to add something that I went by in this introduction, they will tell me.

Seismic margins. As you know, more or less half of the nuclear plants in the United States did a seismic margin review than a seismic PRA when they were satisfying the IPEEE around 6 or 8, or 10 years ago.

And if they did a seismic margin review well, they ought to be able to meet the standard that we wrote for seismic margin. And the reason that we did that is because if you have done that rather than the other, and you meet it, we want to give them the benefit of that.

Because there are some applications for which seismic margin is very well tuned, and they ought to be able to say we met it and we can do those applications.

By the way, there are many applications

where seismic margins is not well tuned, and I can mention those if you want, or I can them here, or you can ask me, and for those, those limitations you can't do it.

But at least if you did that, then you ought to get the benefit of that. So we have written a whole separate section of the standard outlining the method of seismic margins, and these are IPEEE margins.

And it is well know that those that did it, mostly we think did quite well there, and they ought to be able to meet the standard, and then there are some applications that they can do. The crucial limitations of seismic margins are as follows.

Well, I will just say what it is good for, for sure. If I have got some SSC that is very, very stout against earthquakes, and the seismic margin review has revealed that through the analysis, why that information is just as valid as if you did a seismic review.

And it turns out that there are a lot of applications like that, and that is very valuable, and they can do that. On the other hand, if in your application you have got some accident sequence in which a seismic failure combines with a human error, or with some non-seismic unavailability or something, seismic margins doesn't do that very well at all.

It doesn't capture it well, and the analysis isn't structured to do that, and it wasn't intended to. And certainly seismic margins can't produce for you a core damage frequency, at least not as structured.

So those sorts of applications are unavailable to some plant that has only a seismic margin review. However, it is our opinion, and when we issued this now just for comment, it is our opinion that nevertheless those that have a good one that can meet it ought to be able to get the benefits of what they can do, and that is our motivation.

I just have one other thing to say about that, and then I am done. For at least a decade, the community of people that play around in this sandbox, of which the three of us are among them, have struggled with what we could do to provide additional guidance, so that if someone with a seismic margin review could get more out of it.

And the proposal has been around for a long time, including a very thoughtful proposal that Ravindra came up with with Bob Murray about 10 years ago.

Your own staffer, Rich Cherry, ACRS staff, five years ago wrote a very useful paper about that. A couple of years ago, Bob Kennedy wrote another one, and we finally decided that it was time to do something to explore how to do better.

And this is relevant to this directly, but about a year ago Ravi and I went to the NRC and ANS -- and Nilesh has got a conflict with this hat on, but NRC gave a grant to ANS to fund Ravi and me to do that study.

And Ravi and I have just within the last few weeks completed a study and published it, and I have it in front of me -- but you don't have it yet, although I will send it to you -- whose scope is the following.

We have explored how you could take a good quality seismic margin review, and extract more from it than you would think. For example, more risk type information, or more CDF type information, by doing in some cases directly, or by doing some additional work.

And we have written a paper that explores the things that you can do with it, and what the limitations are. And that has been published, and although it was reviewed by a few people, we are going to send it around the community of people, people like you, but also the seismic margin side of the community and get some feedback from them.

Because if what we propose makes sense to them, then sometime in the next 6, 8, or 12 months, we are going to propose an amendment to this standard, a supplement, and in which requirements for that will be in there.

So that if you can do that, then you can use the seismic margin review that you have to get more out of it than you now can. And in order to push that along, our final report, which I have in front of me, actually has in it proposed draft standard language, just like in the requirements that you see.

And it is Ravi's and my cut as to what the requirements would be, and that if you did them, then you could get more out of it, but there are some limitations.

And I will just end with that, and ask my colleagues, Chokshi and Ravindra, whether they want to add something that perhaps I went over too quickly, and then you get to ask us. Anything that I didn't cover?

Oh, wait, I have left something out. Very important. When we had what we thought was a satisfactory draft, about six months ago, we sent it around to 6 or 8 of our peers; peers meaning they could have been people like us and been on the committee, but the committee was just small.

And we got review comments back from most of them, which were very helpful for us, in terms that we made some changes. But this was an informal review.

And because mostly what we got back was, yeah, you are on the right track, we have a lot of confidence that what we are doing here is congruent with the larger community.

Crucially, we got comments back from Bob Kenney, from Leon Reiter, from Alan Cornell, from Bob Murray. EPRI actually funded Greg Hardy to do some work, and Bob Kassawara participated in that.

So we have before the final, we have those comments and feelings, and so we have a feeling like I said that that's okay, and then also crucially, John Stevenson, another important member of this community, was originally going to be a member of the writing team.

And then just as we were starting, he dropped off, but has remained sort of an associate member right along. We have sent him everything, and he has made comments. His stuff is here, too. Okay.

I just wanted to be sure that I put that in.

And then finally, this work was partially supported by the Nuclear Regulatory Commission, who, besides that special project that Ravi and I did, the NRC gave a grant to ANS to support this, and the support by the way paid for staff and so on.

But it also paid for things like our travel and some administrative expenses. So I want to recognize that Nuclear Regulatory Commission support for this effort.

CHAIRMAN APOSTOLAKIS: Thank you, Bob.

MR. LEITCH: My first question I think relates to exactly the issue that you were talking about, and perhaps it is my lack of understanding concerning exactly what a seismic margin assessment is.

I am referring to page 5, the third paragraph. I guess these are all paginated the same way.

MR. BUDNITZ: I hope so.

CHAIRMAN APOSTOLAKIS: Maybe you can use the page numbers at the top.

MR. LEITCH: Yes, that is page 5.

MR. BUDNITZ: By the way, if you could also point to the section, like 1.3.2, it will help us.

CHAIRMAN APOSTOLAKIS: Page 5.

MR. LEITCH: It is the paragraph that immediately precedes 1.3.3, and it says throughout the standard the phrase, PRA, is used in a generic sense.

And then the intent is to include SMA methods, as well as PRA methods within the scope of the phrase, PRA. So when we see PRA in this document then, as I understand it, it may mean either what we normally understand by PRA, or it may mean SMA.

And I'm just not sure that I understand the distinction between those two. Could you held me with that a little?

MR. BUDNITZ: Sure. Well, what I meant by that paragraph, which I wrote or I suppose I'll say we, is that, for example, it says 1.4 to 1.10, and when you are talking about peer review or that sort of stuff, and we just didn't say PRA or SMA everywhere.

But the sections that are explicitly SMA, differ. You pretty much have to sort it out. Are you unclear about what an SMA is and what it does?

MR. LEITCH: Yes.

MR. BUDNITZ: There was an expert panel that I chaired in 1984, '85, and '86, although these fellows were involved, which invented that method.

The purpose of a seismic margin review is to evaluate the plant, and ascertain its seismic capacity, defined in terms of the way we define fragilities; the fragility curve or fragilities.

But if you have, say, four components, then you have to combine them in the right way, for example, and they might even have various other things.

So the purpose of a seismic margin review is that you go to your plant and you evaluate its capacity. You don't pay any attention to the hazard. Now, the way it was structured was a little different than that.

You pick what we call a review level earthquake. In the east, we suggested it, and almost everybody picked .3G. The idea was that it has to be higher by a factor than your design basis, which is 3.1 or .15.

But by the way, if you are in Arizona, in Palo Verde, with a .25G design, you pick .5 as your review in other words. And you review the plant to the review level earthquake, and it has guidance in there that tells you how you can screen out using the guidance a whole lot of SSCs that clearly are stronger than that, and then you have to evaluate the ones that aren't.

So, for example, you might end up in typical power station with only 2 or 3 dozen components, SSCs, for which you have to actually do an evaluation.

And for those, it goes further. You don't work out in the seismic margin method that everybody uses. You don't work out the full seismic fragility curve. You develop what we invented and called the HCLPFC capacity, the high confidence low property failure capacity, which is the capacity which literally on a full fragility curve is the capacity in which there is a 95 percent confidence that you have less than a 5 percent probable of failure.

But really it was intended to be the capacity at which -- we have a very high confidence that this thing wouldn't fail, because you don't really believe the tales of these log normals all that well.

And the notion was -- and there is a method called the CDFM method, the conservative deterministic failure margin method, or else you can get it from your fragility curves, for working out the HCLPFC capacity of a pump, or a valve, or a wall, or a large tank.

And what the seismic review margin was or did was that it worked out the HCLPFC capacities of every one of those SSCs that wasn't screened out, and then it combined them to work out the HCLPFC capacity of the station by choosing two success paths -- they are supposed to be different if they can be -- that the operators would use.

Say you need this component, and you need this component, and you need this component, and you need that system, and you need this thing.

And then we will imagine that they are called success paths A and B.

And you would work out the HCLPFC capacity of the success path A, and the HCLPFC capacity of success path B. And then the stronger of those was the HCLPFC capacity of the plant, because if you used that success path, you had high confidence that with that HCLPFC capacity that you could shut down, and that's how it is done.

That is a real tour. There is a little more to it, but that's a quick tour.

DR. KRESS: Somehow on page 18, your discussion on the definition of seismic margin doesn't reflect very much of what you just said, but still it is not a very satisfactory definition.

MR. BUDNITZ: That's fair.

DR. SHACK: There is nothing about margin in it.

MR. BUDNITZ: Probably what we should have done was just said go to the SMA literature.

DR. KRESS: Or something maybe.

MR. RAVINDRA: Initially, we were thinking of writing an appendix, like what we have for the seismic PRA, and we were thinking of writing an appendix that describes the salient features of the SMA method.

And we were kind of debating whether such an appendix would help the user or not.

DR. KRESS: Well, would that show up in your proposed addendum to the thing?

MR. BUDNITZ: No, but we could do that. There is a lot out there that we could probably pull together. We ended up not doing it. There are 5 or 6 reports, which taken together, tell you everything you want to know about it.

CHAIRMAN APOSTOLAKIS: There is a danger here of jumping back and forth. Why don't we start with questions on Chapter 1, called the introduction. Do the members have any questions, which is the first 10 pages. Which numbering scheme are we following?

Let's go with the printed numbers at the--

MR. BUDNITZ: Well, we can do it by section. If you go to Section 1.9 --

CHAIRMAN APOSTOLAKIS: Well, let's do it by chapter, the first 10 pages. Any questions? Well, I have some. On page 5 and 6, Section 1.3.3, there is a very interesting discussion of LRF, and maybe it is just an opportunity to comment on it.

You state that some accident sequences that are externally seismic initiated, which do not contribute to LRF in the internal event analysis, in fact now contribute to LRF because you assume there is no evacuation, right?

MR. BUDNITZ: Yes.

CHAIRMAN APOSTOLAKIS: And the evacuation was the criterion for whether something is early or not.

MR. BUDNITZ: Or it is impaired or something.

CHAIRMAN APOSTOLAKIS: And you assume there is no evacuation at all?

MR. BUDNITZ: No, we told the analyst to figure out what they assume.

CHAIRMAN APOSTOLAKIS: To do what?

MR. BUDNITZ: To work it out. If

evacuation is impaired, then you do count for that. We don't say assume no evacuation.

CHAIRMAN APOSTOLAKIS: But that is a judgment call isn't it?

MR. BUDNITZ: Absolutely, but that is the analyst's job.

CHAIRMAN APOSTOLAKIS: They are not going to do any analysis for that and see what buildings collapse, and bridges fall down. They will just make a judgment, that the evacuation is really not effective against LRF, right?

DR. POWERS: George, I think you could -- you know, your consequence code, typically you could have quite a range of inputs to the code.

CHAIRMAN APOSTOLAKIS: But they don't do that though I don't think. It is just a judgment on the part of the analyst. You don't run any codes to see the impact of evacuation, right? It is just a judgment on your part.

MR. BUDNITZ: Well, just to describe what we said. If this isn't clear, we have to clarify it.

CHAIRMAN APOSTOLAKIS: No, it is clear. I just wanted to --

MR. BUDNITZ: To meet the standard, the analyst is not to take the LRF from the internal events and swallow it whole. He may observe that in fact protective actions are impeded, and perhaps not as effective, or whatever, for a big tornado, let's say.

And the analyst is explicitly told that the analyst shall account for that before he categorizes alert sequence for LRF or not, depending on the event. Nothing more, but nothing less.

CHAIRMAN APOSTOLAKIS: Very good.

MR. RAVINDRA: I think that after the core damage, if the containment systems don't function, then you have a question of release; am I right?

MR. BUDNITZ: Sometimes.

MR. RAVINDRA: So because of that, in the seismic event, the containment systems are not any stronger than the actual core itself. Therefore, the assumption that the containment systems would remain after the core damage has occurred may be not really true for seismic events.

MR. BUDNITZ: So that is the reason that you have to look at that.

MR. RAVINDRA: Yes, that you have to look at that.

MR. BUDNITZ: Sure.

DR. KRESS: That is accounted for in the

PRA.

VICE CHAIRMAN BONACA: Yes, the PRA accounts for that.

CHAIRMAN APOSTOLAKIS: That is just a definition of LRF. Look, there was no question. It's just that I like it.

DR. KRESS: We are glad that you pointed that out actually.

CHAIRMAN APOSTOLAKIS: On page 7, you state what you also told us, that this category was consistent during the spirit of category 2 of ASME standard. And I am wondering why the forces that forced ASME to have a category one did not apply to you?

MR. RAVINDRA: Currently, they are at work.

CHAIRMAN APOSTOLAKIS: They are at work now?

MR. BUDNITZ: Yes.

DR. KRESS: They are working on it.

CHAIRMAN APOSTOLAKIS: Are they doing any work?

DR. SHACK: You have a seismic margins analysis, which to a certain extent is a category one.

MR. BUDNITZ: There is actually a

technical thing here. We don't believe, the three of us, that we know how to write a capability category three just to start for seismic, or for tornados, or for the external hazards.

And the reason is because there are certain technical things that you have to do that we don't do, and we don't know how to do. A category three --

CHAIRMAN APOSTOLAKIS: One is the question, not three.

MR. BUDNITZ: How about three? So we don't know how to write three, okay? We don't know, for example, how to capture in a way that you would actually like to capture the correlations. We don't know how to do HRA well for post-seismic or post-tornado human actions.

Those are major things that are pointed out there. There are a whole lot of things. We don't know how to do three. If somebody came in and said they put down a category and claimed it was a category three, I would have real trouble trying to agree with that unless we had done something that no one has ever done.

And by the way the notion of category three in ASME was somebody at least had to have done it sometime so we would have an example. It can't be something that is a dream.

Now, category one is different, capability category one. The idea of capability category one is that you have compromised your peer rate compared to the middle column, which is the -- by one of several things.

For example, you might use generic rather than plant specific things. You might be conservative rather than realistic. I don't mean conservative for screening, but actually conservative in your analysis. Or you might not have much level of detail.

VICE CHAIRMAN BONACA: But you are allowing this in the standard, too.

MR. BUDNITZ: Right. So we could have candidly written another category for that, but instead we have allowed it here. If you do less, why is there some stuff that you can't do. But we didn't write a whole column for it. We could have, but we didn't.

VICE CHAIRMAN BONACA: I would just like to express a personal opinion. I was very impressed by this standard, for the simple reason that it looked like a standard, and the ASME standard, I don't feel like it looks like one.

And yet you are allowing the kind of latitude in the approach in the context of the standard, which should be there. But I felt that it was well developed, and by the way, the commentary that you have is a key issue, because it allows you to pull out all the discussion that I saw in the ASME rev. 10, which I think was the one impeding somewhat the document.

And here it is separated that way, but I think that one of the great strengths of this thing is that you don't have category one, two, or three.

DR. KRESS: With respect to your one category, in your Chapter 6, you strictly endorse the ASME, which is Chapter 3 in the ASME.

MR. BUDNITZ: We don't endorse it. We incorporate it by reference.

DR. KRESS: Incorporate it by reference. I'm sorry.

MR. BUDNITZ: But that is an important way to say it.

DR. KRESS: That is. But my question is that chapter in the ASME document is cast in terms of the three categories. Is there any conflict between yours and that, or can one just assume that you are looking at the category two parts?

MR. BUDNITZ: I'm glad you asked that.

DR. KRESS: Okay.

MR. BUDNITZ: I am sure that after the ASME process has run its course, but before we finalize this finally, because that is probably a few months away, we are going to have to rewrite some of this to clarify that.

But the problem is that the ASME process -- I'm part of it and I'm on the committee -- is still in the works, and so we are trying to get this thing out and gone, and go through the process and get the technical stuff right.

And later on we are going to have to clarify exactly what by reference means, because the thing that we referred to when we wrote this -- that Chapter 3 is different. It changed last week, and we have another meeting in two weeks and it is going to change again. And sometimes the change is significant.

DR. KRESS: I had that problem trying to relate the two, because I don't have the latest one either.

MR. BUDNITZ: If you think about the middle category as a good quality pairing, that's --

CHAIRMAN APOSTOLAKIS: But maybe what Dr.

Shack said is really the reason why you don't have pressure. You have a screening, you have the SMA, which the main idea behind category one was to use a quick calculation to rank systems, structures, and components, and to do certain things very quickly for which those people felt that you didn't need a detailed uncertainty analysis.

MR. BUDNITZ: Actually, I don't agree with you. That is only one of the motivations behind the capability of one in ASME. Unfortunately -- and I say this now as a member of that team -- some people think they can do more with it than that, and that is partially true.

But I don't think there is an awful lot more -- some people think there is a lot more that they can do than I can think of.

CHAIRMAN APOSTOLAKIS: Well, that is the essence of it. The idea was to be able to screen quickly and identify contributors --

MR. BUDNITZ: Well, some people think the idea of it was more than that. Some people think that you can do everything you can do in PRA with that thing.

CHAIRMAN APOSTOLAKIS: No, no, no. That's not reality.

MR. BUDNITZ: I am just exaggerating. Nobody really thinks that.

CHAIRMAN APOSTOLAKIS: Ravi.

MR. RAVINDRA: Now, the chapter on the external event screening, if the analyst wants to use it, he can treat that as any other external event, and decide based on the frequency of the break itself, he can screen it out, and based on some bounding calculations, he can screen it out.

Or he can use the seismic margin approach to go a little further to screen out initial components and systems. So this is a continuous screening process. So the method is already there. Now just to reformat it into category one or category two.

We were also waiting for the ASME to complete his work, and for the dust to settle down, and only then can we do it.

CHAIRMAN APOSTOLAKIS: Okay. On page 7, you say that the ASME, the bottom paragraph -- well, first of all, you say that a well executed SMA represents a good fit to many of the applications contemplated for ASME category one.

MR. BUDNITZ: Yes.

CHAIRMAN APOSTOLAKIS: And then you go on and say especially insofar as an SMA generally is well suited to the categorization of SSCs according to their size and capacity, and to the screening of SSCs according to their safety significance.

This refers to what you said earlier, Bob, with the two parts and so on?

MR. BUDNITZ: Yes.

CHAIRMAN APOSTOLAKIS: Okay.

MR. BUDNITZ: But there are limitations.

CHAIRMAN APOSTOLAKIS: But the categorization, you see, that word means something about internal event PRA. You don't mean categorization according to some importance measure and so on do you?

MR. BUDNITZ: I think it says categorization of SSCs according to their capacity.

CHAIRMAN APOSTOLAKIS: And what does that mean?

MR. BUDNITZ: HCLPF above .3G.

CHAIRMAN APOSTOLAKIS: Okay. That kind of thing.

MR. BUDNITZ: The sort of things that SMA does for you.

DR. SHACK: Just coming back in my absolutely simple-minded view of these things, as I don't know anything about it, it seemed to me that in a seismic margin analysis, you have a simple amount of -- I mean, you have identified two of the ways that you can succeed. So you have sort of set a bound on things.

MR. BUDNITZ: Correct.

DR. SHACK: And so to that extent, you actually have some PRA like information, but what you don't have is a complete set of event trees. But you have picked your two success paths, and so to that extent you do have some bounding information.

MR. BUDNITZ: Correct.

DR. SHACK: For example, suppose you stupidly forgot about the strongest success path, where you had a whole bunch of SSCs that were extremely stout earthquakes, but had no human intervention, and it was all automatic, and you knew it and so on.

You might completely misunderstand your seismic capacity. You might think it is smaller, when it is really very strong.

MR. BUDNITZ: Correct.

CHAIRMAN APOSTOLAKIS: Anything else on chapter one?

(No audible response.)

CHAIRMAN APOSTOLAKIS: Okay. Chapter 2 is definitions. Any comments?

MR. WALLIS: Well, the seismic margin one didn't help me, and then when I looked at Chapter 3.5, you launch into methodology without saying what it is that you are doing.

In seismic fragility, there is a very nice definition of Seismic fragility.

MR. BUDNITZ: Point taken.

MR. WALLIS: It is not there for seismic margin.

MR. BUDNITZ: Perhaps Ravi and Nilesh, we have to go back and rewrite that appendix on seismic margin that we wrote for --

VICE CHAIRMAN BONACA: On the positive side, I didn't see any glaring error or mistake.

CHAIRMAN APOSTOLAKIS: Oh, you mean on chapter two?

VICE CHAIRMAN BONACA: Yes.

CHAIRMAN APOSTOLAKIS: I have a couple of comments on chapter two. Anybody else wants to go ahead of me? The definitions?

DR. KRESS: The definition of core damage.

MR. BUDNITZ: Oh, wait. We took that straight from ASME. All the systems stuff, and just so you understand, but we were constrained and so we decided to make it seamless with the ASME standard that we are going to use, and if their definition is changing, and it has changed a little bit, we are going to incorporate it.

Perhaps I need to say that up front. The idea was --

DR. KRESS: That might be helpful.

MR. BUDNITZ: We are doing a low power shutdown standard by the way, too. If all the definitions aren't the same, you sure won't be able to use them together for applications. Let me make a point to be sure to say that.

CHAIRMAN APOSTOLAKIS: On the same page, page 13, the discussion of epistemic uncertainty focuses on model uncertainty, but part of epistemic is perimeter uncertainty as well.

And maybe if you can make that a little bit clearer, because the definition seems to focus almost exclusively on the modeling assumptions.

MR. BUDNITZ: I don't see that.

CHAIRMAN APOSTOLAKIS: I see it.

MR. BUDNITZ: Okay. Thank you.

CHAIRMAN APOSTOLAKIS: Anything from the members on definitions? That's it. Now, Chapter 3 is very big. So maybe we can break it up. Maybe go and include 3.1 and 3.2, and 3.3, first; is that reasonable? Technical requirements, general. Now, which pages are these?

It starts from page 20. Does anybody have any comments on that?

(No audible response.)

CHAIRMAN APOSTOLAKIS: No? Okay. 3.4, technical requirements. Yes, let's do the whole 3.4.

MR. BUDNITZ: Oh, by the way, I want to make a comment about context here that I think will help you. Having just spent 3 years with the ASME team, one of the complexities that the ASME team faced and faces in writing its standard is that there are a hundred plants out there that have had a PRA, and because of twins, there are 60 or 70 PRAs.

For many of the sections of internal PSHA, the plants use different methods, very different HRA methods, very different methods for success criteria, and very different methods for this and that.

And to try and write a standard that captures and enables those with good quality to still meet it turned out to be a difficult trick. And it was a big struggle.

It is important for you to understand that it is our opinion here as writers of this that there is far less variability in the way the size of PRAs that were done were done and were accomplished.

They are mostly similar; the fragilities part, the hazard part. So it was simpler for us. We didn't have to struggle in very many places. We are trying to write a requirement and that we know someone had done quality work different ways. That was good luck for us.

CHAIRMAN APOSTOLAKIS: Let's go to page 24. I mean, it may be an unfair question, but I think we have to see if we can resolve it. The seismic hazard analysis high level requirement, (a), says that the frequency of earthquakes shall be based on a site specific PSHA that reflects the composite distribution of the informed technical community.

Now I know where that comes from, but somebody who takes this and is innocent in the ways of life, how does he make sure that this is a composite distribution of the informed technical community? I mean, are you imposing an impossible requirement?

MR. CHOKSHI: I think if you go, in terms of how you meet the requirement, it basically says the SSHAC approach is one --

CHAIRMAN APOSTOLAKIS: But the whole thing rests on experts you choose, right?

MR. CHOKSHI: Yes.

CHAIRMAN APOSTOLAKIS: And how you define the community.

MR. CHOKSHI: Yes, but the chart lays out the selection of experts, and the process of how you go about doing that.

CHAIRMAN APOSTOLAKIS: So that is really the intent?

MR. CHOKSHI: Yes.

MR. BUDNITZ: And in fact if you turn to the detail a few pages later, it goes to that directly.

CHAIRMAN APOSTOLAKIS: And the same thing on the same page, page 24, there are words like credible and I wonder. I mean, that becomes clearer --

DR. SHACK: The big difference here is their commentary gets a lot of that in, and the high level requirements become very concrete --

CHAIRMAN APOSTOLAKIS: When you go to their comments.

DR. SHACK: Yes, and their commentary is a very strong suggestion, like do it.

MR. CHOKSHI: We were struggling how to

get some of these ideas across and the commentary was a good vehicle to do it.

VICE CHAIRMAN BONACA: It is a good commentary, and I agree, but it gives you the written path, which is the standard way to do it.

DR. SHACK: It gave you guidance.

VICE CHAIRMAN BONACA: It doesn't say you can't do it otherwise.

CHAIRMAN APOSTOLAKIS: I am a little confused though. If you go to page 26, where the commentary says existing LNEL and EPRI hazard studies and many hazard studies conducted for plant PSHAs also meet this overall requirement.

Now, are these two studies, studies that differ by a factor of 10?

MR. BUDNITZ: It is Livermore '93.

CHAIRMAN APOSTOLAKIS: So it is the updated Livermore?

MR. BUDNITZ: Yes.

CHAIRMAN APOSTOLAKIS: So they don't differ that much anymore.

MR. BUDNITZ: Except for details.

CHAIRMAN APOSTOLAKIS: All right. On page 31, the last note at the bottom of the page, HA-D3, somewhere in the middle it says that the characterization of ground motion includes an epistemic uncertainty in the ground motion model.

Have people done that? Have people developed an epistemic uncertainly in the ground motion model?

MR. RAVINDRA: For each ground motion -- well, there are many ground motion models, and so the collection of that represents the distribution.

CHAIRMAN APOSTOLAKIS: But who developed the distribution? I mean, I can have five models with uncertainties, and as you know, people have been arguing about a particular model from Southern California and so on.

But if I pick one, how do I develop the epistemic uncertainty in the model itself given that I have the other six models floating around? Is there a methodology that tells me how to do that, or do I have to --

MR. BUDNITZ: I understand your point, George. Suppose the word said, "an epistemic uncertainty amongst the several ground motion models." Suppose it said that.

CHAIRMAN APOSTOLAKIS: Yes, we need a better word. I am not sure that is the best, because that is related to another question that I have regarding sensitivity studies.

MR. BUDNITZ: But that is not a small point. In fact --

CHAIRMAN APOSTOLAKIS: It is not a small point, no.

MR. BUDNITZ: In fact, if you go with -- let's say you go with Dave Boore's model.

CHAIRMAN APOSTOLAKIS: Yes, good fellow.

MR. BUDNITZ: Then if you are ignorant that Abramson has done a different model, then you may not capture this model epistemic uncertainty.

CHAIRMAN APOSTOLAKIS: Exactly. But if I am aware though that Abramson has another model, I still don't know how to meet the standard. You know, how do I develop my epistemic uncertainly now, and I think that is something that needs elaboration, because I don't think we should ask the user of the standard to do research.

By the way, I am focusing on things that I thought required discussion. I think this is a very good standard.

MR. BUDNITZ: Well, George, let's go on then. Just keeping reading, because --

MR. RAVINDRA: In terms of the person writing the commentary, I think the civil engineering professionals are the first one that came up with that concept. Most of the civil engineering standards and building codes come with commentary so that the user knows the basis, and not just the requirements.

CHAIRMAN APOSTOLAKIS: Wonderful. It is about time we learned something from you guys.

MR. BUDNITZ: George, let's keep going. The next sentence --

CHAIRMAN APOSTOLAKIS: I read the next sentence.

MR. BUDNITZ: But it says that SSHAC gives guidance on an acceptable process to be used for determination of -- and in fact you and I were authors, and that guidance isn't really enough.

CHAIRMAN APOSTOLAKIS: It is not. I think you need to soften a little bit what you are saying here, and find a way around it.

Now, on page 33 -- and this is something that is not unique to the standard, but something that bothers me in general, but look at the requirement HA-F2, which I think is a reasonable thing to say, but I will voice my concern.

The PSHA shall include the appropriate sensitivity studies, and then you have a commentary, which is fine. It says examples of useful sensitivity studies include an evaluation of alternate schemes used to assign weights to experts, and so on, and so on.

My problem with sensitivity studies is that I don't know what to do with them. What if some combination of these things shows a core damage frequency that is way out of this world, or it is above the goal? Now what do I do?

I did the sensitivity study, and I am above the goal, and everything else that I have done shows that I am below the goal. What good is it? I mean, shouldn't we be through Bayesians assign probabilities to all of these things, and include them? I mean, Bob, I don't know what to do with that.

MR. BUDNITZ: Read the sentence.

CHAIRMAN APOSTOLAKIS: I read the sentence.

MR. BUDNITZ: It tells you why. The PSHA shall include appropriate sensitivity studies and intermediate results. Why? To identify factors that are important to the site hazard, and that make the analysis traceable and reviewable.

Now, here is the point. If you do a sensitivity study and find out that Factor 44 is not important, then you have learned something. If you do a sensitivity study and find out that Factor 44 is important and you didn't include it, you have actually erred. So that is how it is used.

CHAIRMAN APOSTOLAKIS: No, that is not the way that I read it. If I assign different weights to individual expert models, or an evaluation of the way different experts make different assignments, and I find a result that is an order of magnitude greater than what I believe is a realistic estimate, I don't know what to do with it.

What do I do? Do I report it to the regulator, for example? And what is the regulator going to do? Because you give a realistic distribution and they say, well, if I do this gain here, I am a factor of 10 higher.

MR. CHOKSHI: I think within the sensitivity studies you still have to be realistic. You still have to use realistic assumptions and values.

CHAIRMAN APOSTOLAKIS: I think this is a relic of traditional engineering, where they were not doing uncertainly analysis, and let's play with the variables a little bit to see what happens.

When you do a rigorous uncertainty analysis the way you guys demand it, I think you have to be very careful with what kinds of sensitivity studies you are asking.

I mean, I can see saying, you know, maybe the distribution has a higher this or that, but it has to be constrained. Otherwise, I can see it getting out of hand, and that is not the intent for sure.

MR. BUDNITZ: Look at the note. Sensitivity studies in the intermediate results provide important information to reviewers. And by the way, you might also say the analysts, of course.

CHAIRMAN APOSTOLAKIS: Yes.

MR. BUDNITZ: About how some of the key assumptions affect the final results of this complex process.

CHAIRMAN APOSTOLAKIS: Right.

MR. BUDNITZ: It is no more, but it is no less.

CHAIRMAN APOSTOLAKIS: Bob, let's say I do find that I have two key assumptions, and then I change things. I assume something else and the thing jumps up. Am I under a requirement here that says, no, you are not going to play that game.

If you want this factor to become six, you also have to tell me what is the probability that it will be come six. That's where I am going. Otherwise, I don't know what to do with it.

MR. BUDNITZ: George, this is a deep intellectual challenge. Let me give you an example, all right?

CHAIRMAN APOSTOLAKIS: All you have to do is say do I look --

MR. BUDNITZ: No, let me just give you an example straight from seismic hazard. Suppose there were five of us at the table, and --

CHAIRMAN APOSTOLAKIS: When in fact you are only three. But go ahead.

MR. BUDNITZ: But suppose there were five of us at the table who were ground motion experts, and who had different ground motion models, and even though they are all different, A, B, C, and D, whether you used A, B, C, or D models didn't make much difference to the results.

But you went to E, and you used hers, or his -- it doesn't make a difference -- and it made a big difference. Now you know something. What you know is -- first of all, you know what I just said, but you also know that there is the possibility that the other four might be wrong, and so then you have got to go and inquire.

So what you do with it depends on what you learn, but we don't tell you to incorporate it in the analysis. It is nothing more than important information about how some of the assumptions effect the result.

CHAIRMAN APOSTOLAKIS: But you know that the paralysis that this --

MR. BUDNITZ: Are you suggesting that we should not have done that? Are you suggesting not to do sensitivity analysis? Are you suggesting not to publish intermediate results?

CHAIRMAN APOSTOLAKIS: No, I want you to define them better, and tell me what to do if I find a situation like that.

MR. BUDNITZ: I can't tell you what a decision maker would do.

DR. KRESS: If you are requiring a good vigorous uncertainty analysis, what do you need with a sensitivity analysis?

CHAIRMAN APOSTOLAKIS: Exactly.

DR. KRESS: I think that is the point.

CHAIRMAN APOSTOLAKIS: And you guys do require a vigorous uncertainly analysis.

MR. CHOKSHI: Even if you do a vigorous uncertainty analysis in something like hazard, you will be making some --

CHAIRMAN APOSTOLAKIS: You can say identify what is important.

MR. CHOKSHI: Exactly.

CHAIRMAN APOSTOLAKIS: I think it is time that we abandon this.

MR. BUDNITZ: George, you and I both understand how difficult it is to deal with the inside that you got from this assumption that you made that you know is wrong. I mean, sometimes you can assume something that is physically incorrect, and couldn't happen.

You say, gee, let's suppose the water has density, too, or something.

CHAIRMAN APOSTOLAKIS: It's not always easy.

MR. BUDNITZ: And it is not going to make any difference, and if it doesn't make any difference, it doesn't.

CHAIRMAN APOSTOLAKIS: But this is related, Bob, to the issue of assigning equal weights to the experts, I think. All of these things go together.

MR. BUDNITZ: It is all related.

CHAIRMAN APOSTOLAKIS: We have to finally say, look, this is the probability that I am assigning to this, okay? Whatever that is. You don't disagree with that do you?

MR. BUDNITZ: I am not going to argue that. Let me describe. In the end, George, there is an analyst, a person, or perhaps it is a team.

CHAIRMAN APOSTOLAKIS: Yes.

MR. BUDNITZ: And if they sign the thing and say I, we, take professional responsibility for what we did, and the sensitivity study showed something cockeyed, and we don't believe it.

CHAIRMAN APOSTOLAKIS: Oh, they say we don't believe it.

MR. BUDNITZ: No, they might say. Let's assume that, or else they might say, gee, maybe we should believe it. In other words, it comes down to professional responsibility doesn't it?

CHAIRMAN APOSTOLAKIS: And eventually maybe --

MR. BUDNITZ: Well, he assigns the probabilities after it. If he finds out that it doesn't make any difference, then he is not going to worry a priori about assigning probabilities to these things. I mean, I look at it as a way of narrowing down --

DR. KRESS: You can't really do that

because in order to do a sensitivity analysis, you have to put ranges on these things. And you are not going to just arbitrarily choose those. You are going to choose something that is within the range of probability.

CHAIRMAN APOSTOLAKIS: Exactly.

DR. KRESS: So you do assign some sort of probability to it.

MR. BUDNITZ: There is assigned probabilities, and there is assigned probabilities.

CHAIRMAN APOSTOLAKIS: I would like to see a discussion or part of the commentary here that reflects what we just said.

MR. BUDNITZ: That is very helpful.

CHAIRMAN APOSTOLAKIS: That's all I am saying.

MR. BUDNITZ: That is very helpful.

CHAIRMAN APOSTOLAKIS: Page 43. You already talked about it, the HRA thing, and you recognize that this aspect can represent an important source of uncertainty in the numerical results.

You are silent regarding references here, where I see in other places that you are more than willing to provide references.

DR. KRESS: Does George have a lot of

references on this?

CHAIRMAN APOSTOLAKIS: No, but for example, if --

MR. BUDNITZ: Are you looking at SAB2?

CHAIRMAN APOSTOLAKIS: I am looking at SAB2, yes, the very last sentence.

MR. BUDNITZ: The point is well taken. It seems to me that we could and should provide some citations.

CHAIRMAN APOSTOLAKIS: Especially if there some studies that are particularly related.

MR. BUDNITZ: Absolutely. It is an omission.

CHAIRMAN APOSTOLAKIS: Any other comments on 3.1, .2, .3, from my colleagues?

MR. LEITCH: I have a question about 37.

CHAIRMAN APOSTOLAKIS: Page 37?

MR. LEITCH: Page 37, yes.

MR. BUDNITZ: Can you cite the requirement, like SM-A1 or something?

CHAIRMAN APOSTOLAKIS: Yes, we can do that.

MR. BUDNITZ: It is somehow different from yours because of the printer.

MR. LEITCH: This is 3.4.2.1,

Introduction.

CHAIRMAN APOSTOLAKIS: Introduction to the seismic PRA technical requirements.

MR. LEITCH: And it speaks about the trimming of certain events and the adding of certain events. And it gives some examples of trimming. Could you help me with an example of adding?

MR. BUDNITZ: Of course, and perhaps we can add that. The internal events PRA model basically has in their basic events no structures. Walls don't fail. But there can be a basic event of wall fails, and then of course harm is pump, or piping, for example.

That is an example of where one must expand the horizon of the SSCs concerned. There are others, but that's an obvious one.

MR. LEITCH: That helps my understanding of it. Thank you.

CHAIRMAN APOSTOLAKIS: Any other comments on technical requirements for systems analysis, seismic fragility analysis? I don't have any, except that it seems to me that it would require a specialist to do this analysis. It is not like the internal events.

MR. BUDNITZ: I would argue that you

require a specialist to do internal events, too.

DR. KRESS: I was kind of shocked to hear that.

CHAIRMAN APOSTOLAKIS: Well, what I mean is that you can have a systems engineer spending some time learning what the fault trees and the event tree, and he can develop those and do a decent job.

I don't think you can take a systems engineer, train them a little bit, and have him do this. This is really a specialist's job. That is what I mean.

VICE CHAIRMAN BONACA: The hazard analysis has to be done by specialists.

CHAIRMAN APOSTOLAKIS: Yes, because there are so many disciplines that have to come today. Bob, you have been with it for too long, and you think it is trivial.

MR. BUDNITZ: Obviously, unless you know how buildings respond to ground motion, you can't do the response analysis. That's a specialty.

CHAIRMAN APOSTOLAKIS: Well, even understanding the fragility curve. So, shall we move on? I don't see -- well, Jack?

MR. SIEBER: I think that one of the problems here is that because a lot of confluence have fragility associated with them that the event trees change. You end up blocking off success paths as you go through. That has to be by a person more knowledgeable than system engineers that I know.

MR. BUDNITZ: The appendix on seismatary explicitly tells you that this must be done by a team of systems fragilities and so on people interacting, and short of that, it won't be successful, and it tells you that in plain English.

CHAIRMAN APOSTOLAKIS: Okay. So we will move on to -- I'm sorry.

MR. LEITCH: page 45, and it is requirement SAE8. There is a sentence there that puzzles me a little bit. It says that while this standard does not require the analyst to assume an unrecoverable loss of off-site power after a large A earthquake, the general practice in seismic PRAs has been to make such an assumption.

That seems a little confusing to me. Why doesn't this standard require that?

MR. BUDNITZ: We permit the analyst to argue if a basis can be established for the recovery of off-site power after the earthquake. They have to have basis. So I would just say that it does not require the analyst to assume that loss of off-site power is unrecoverable.

MR. WALLIS: Isn't this where you need one of your little notes that peer review will look over this assumption real closely?

MR. BUDNITZ: Well, just to give an example, there are some exit sequences that run up to 120 hours and one might successfully argue that at my plant I will recover one of those through some -- well, we just -- we didn't want to require that conservatism if there was a basis, and so we explicitly permitted it.

MR. LEITCH: Okay. I understand.

MR. BUDNITZ: And I am quite sure that is the right thing. You don't want to require something that they could argue for.

MR. RAVINDRA: Also, it is a function of the size of the earthquake. If it is a small earthquake, you make be able to quickly record some off-site power.

MR. LEITCH: This specifically says a large earthquake. But I understand.

CHAIRMAN APOSTOLAKIS: All right. 3.5, seismic margining assessment. We already have a comment from Dr. Wallis that he hasn't seen a beautiful description of what it is. Can you guys provide a beautiful description of what it is?

MR. BUDNITZ: We said we were going to write that appendix that we sort of didn't do yet.

CHAIRMAN APOSTOLAKIS: All right.

MR. UHRIG: I am a little bit confused here. 3.5.1 has the feed high level requirements. If you go to the definition of success paths, it talks about bringing the plant to a stable hot or cold shutdown condition, and maintain it in this condition for 72 hours.

And then seismic requirement B here is the minimum of two diverse success paths, and so two of those methods, shall be developed consistent with structures and equipment that can be used to bring the plant to a safe stable shutdown, and maintain this condition for a period of 72 hours following an earthquake larger than the RLE, which is the review level earthquake.

Whereas, it doesn't talk about the review level earthquake in the definition of the success paths.

MR. BUDNITZ: Correct. The success path -- you are looking at the definition section, back in the definition section?

MR. UHRIG: Yes.

DR. POWERS: Page 65.

MR. UHRIG: Well, that's where the requirements are. The definitions are back about 10 pages.

MR. BUDNITZ: No, page 18 says a success path is a set of components that can be used to bring the path to a stable condition in 72 conditions.

DR. POWERS: Right.

MR. BUDNITZ: Now, this says -- oh, you are talking about the hot or cold? Maybe we need to add that.

MR. UHRIG: No, no.

MR. BUDNITZ: This says --

MR. UHRIG: You want two sets of components.

MR. BUDNITZ: -- this requires. So that defines or requires that you shall develop two of them that can do it after an earthquake larger than the RLE. So that is more restrictive, except for the --

MR. UHRIG: It really doesn't define the level of earthquake.

MR. BUDNITZ: Correct. It just tells what the path is.

MR. WALLIS: And what is this review level earthquake? It seems to have a pretty wishy-washy --

MR. UHRIG: Well, it is about a factor of two greater than your safe shutdown isn't it?

CHAIRMAN APOSTOLAKIS: Let's answer this question.

MR. BUDNITZ: Nilesh, do you want to answer that?

CHAIRMAN APOSTOLAKIS: Let's finish this question first

MR. UHRIG: No, I think it is pertinent here, but the way I interpret this is that roughly a factor of two greater than these safe shutdown earthquake is what you are defining as the review level earthquake.

Certainly at least 50 percent greater; .3 versus .5, and you have a .5 for the review level, and the .3 is your SSE. Or if you have a .5 as a safe shutdown, then what would you say, a .8?

MR. BUDNITZ: Well, of course, we don't use it up there, but that's right. It is specifically instructed that the margin method doesn't apply for places where the design basis of this earthquake would be way above high-G. It just doesn't. Go ahead. You are looking at requirement SM-A1 is where it tells you about that.

CHAIRMAN APOSTOLAKIS: Page what?

MR. BUDNITZ: SM-A1. It is sort of page 66 in my version. The requirement is that it just has to be larger, and then the guidance says more.

MR. WALLIS: But how much larger? Larger by a fraction, or by a factor of two?

CHAIRMAN APOSTOLAKIS: The note tells you more.

MR. CHOKSHI: I think the background of the matter, and based on similar experiences used in nuclear power plants, there has been two level earthquakes that have been established, 0.3G, and 0.5G, and basically they look at those two, because that provides a very good level for screening.

You can screen a number of margins at 0.3G, and you can screen fewer at .5G. So primarily in the margin matter it is 0.3G or 0.5G are used if anyone wants to know what your design basis was.

So if you are at 0.2G, you can still use 0.3G, but if your design basis was much greater than 0.3G, most likely you will have to use 0.5G. So the practical is 0.3G and 0.5G dealing with earthquakes.

MR. WALLIS: Is your standard saying that you shall use 0.3G and 0.5G?

CHAIRMAN APOSTOLAKIS: No.

MR. CHOKSHI: Well, by reference,

referencing the matters. You know, if you go to the definition, and if you look at page 17, and in the note it refers to that point; that the majority of plans in the eastern and midwestern United States held reviews of 0.3G, because their design basis is generally lower than 0.3G.

And then if you go to the seismic margin methods, which are referenced here in the EPRI reports, they explicitly talk about 0.3G and 0.5G.

MR. BUDNITZ: But the requirement is only that the hourly shall be selected greater than the SSE. That is the only thing that is required.

Now, if you select a review level earthquake that is 20 percent above your SSE, you don't get as much information.

MR. WALLIS: So don't you need more guidance about how to select?

CHAIRMAN APOSTOLAKIS: There is a whole NUREG.

MR. WALLIS: So there is a whole NUREG, which I don't have the benefit of.

CHAIRMAN APOSTOLAKIS: There is a whole NUREG.

MR. UHRIG: The other issue that was confusing me here on page 66, and this issue is that you have a high level requirement E, which says the seismic margin calculations shall be performed for critical failure modes in structures, systems, and components, such as structure failure modes, et cetera, and failure modes again.

And then down to requirement G, the seismic margin shall be reported based on margins calculated for the success paths. And I am confused by the shift in emphasis here.

MR. BUDNITZ: Oh, let me -- let's go to that.

MR. UHRIG: Require E versus Require G.

MR. RAVINDRA: Do you want me to answer that?

MR. BUDNITZ: Go ahead.

MR. RAVINDRA: For every component that is on the success path, we either screen the component out because it has a high capacity, or we make a calculation as to the seismic capacity of the component.

Now, the success path is a chain of a series of components, and so when you calculate the success path capacity, generally you take the lowest of the capacities of the components that appear on the success path.

MR. BUDNITZ: The weakest of them.

MR. RAVINDRA: The weakest.

DR. SHACK: The ones looking at a component margin is looking at the plant seismic margin.

MR. BUDNITZ: So, you see, G says the plant seismic margin shall be reported based on the margins calculated for the success paths. I mean, if it is four components -- A, B, C, and D -- and let's say that three of them have a HCLPF capacity of 0.1G, and one of them has a HCLPF capacity of 0.2G, then 0.2G is the capacity of the success path because that is the weakest link.

MR. UHRIG: Yes.

MR. BUDNITZ: I mean, it is a little more complicated than that. If you do and's and or's, you have to take the strongest of the or's, and the weakest of the and's. Maybe I said that backwards.

MR. UHRIG: Is there anything magic about 72 hours? Is that when all the after shocks have gone?

MR. BUDNITZ: No.

MR. UHRIG: So is that just an arbitrary number?

MR. BUDNITZ: No. It is what the systems

people have always used. Nothing more than that. It comes straight from the systems, and not from the after shocks.

MR. SIEBER: That's right.

CHAIRMAN APOSTOLAKIS: Anything else?

(No audible response.)

CHAIRMAN APOSTOLAKIS: Where are we now? Oh, 3.6 and 3.7., other external events. Comments on this?

(No audible response.)

CHAIRMAN APOSTOLAKIS: And 3.8, high winds.

VICE CHAIRMAN BONACA: I thought those were very good sections.

CHAIRMAN APOSTOLAKIS: I thought so, too.

VICE CHAIRMAN BONACA: And particularly the commentary. It is so helpful because it gives you a lot of reference. It is almost like hands-on, and it is succinct enough. The other thing is that it provides a clear understanding of how you are looking missiles and how you are looking for targets. So it is well done.

CHAIRMAN APOSTOLAKIS: Have there been any PRAs with high winds?

MR. RAVINDRA: The example is Indian

Point.

CHAIRMAN APOSTOLAKIS: High winds?

MR. RAVINDRA: For high winds, yes, because there were some structures that were not designed for the missile and for the loading, and they had the potential to fade and collapse on other structures.

And so the high wind was considered as an important external event for Indian Point. There was a partial look into some systems that are affected by the high winds.

But the experience is somewhat limited compared to the seismic. And when it comes to the external flooding, the experience is much more limited.

MR. BUDNITZ: There are 3 or 4 external flooding PRAs that I happen to know about.

CHAIRMAN APOSTOLAKIS: That dominate?

MR. BUDNITZ: That are important enough that they actually carried it through.

MR. UHRIG: Quad Cities?

CHAIRMAN APOSTOLAKIS: No, that was internal.

MR. BUDNITZ: The one I know is the Westinghouse plant in Kishko, in Slovenia. But by the way, it is a perfectly good Westinghouse plant. It just happens to be on a river that floods every hundred years.

And although the dike is big enough, they had to do the whole analysis because it wasn't all that big.

VICE CHAIRMAN BONACA: And winds with the early plants, they really had no screening, and so they were very vulnerable. Adam Neck was a perfect example. It had plenty of missiles and plenty of targets. So it was really a dominant contributor.

MR. BUDNITZ: And ANO did a complete flooding analysis right down to the end, and then found that it wasn't important, and so it didn't matter much. But they actually did this some years ago.

MR. UHRIG: George, can I go back to one quick question here. In 3.5, you talk about generic data. What is the source of this generic data? Page 66. It says that it must be justified if you use it.

There is two or three places in here where it refers to generic data, and I just wondered.

MR. RAVINDRA: Over the years, there has been a collection of data from sources, either the qualification test data, which has gone beyond the qualification level for components, and --

MR. UHRIG: Is this coming out of the reg guides?

MR. RAVINDRA: No, this is the sanction qualification data. The industry has collected data on the seismic qualification of different kinds of components, and that part of the database.

Then we have also collected the data on the earthquake experience, looking at how the nuclear plant type equipment were found in the large real earthquakes.

And then there have also been some tests conducted by Lawrence Livermore Lab and Sandia, and Brookhaven, sponsored by NRC, to do the fragility testing. All that information forms a database that is generic, and not specific to any particular component in the plant.

So if someone wants to use generic data, he has to certify that it is really applicable to the particular component.

MR. UHRIG: So he has to show that the numerical values in his plant are comparable to those that are being used there?

MR. RAVINDRA: Yes.

MR. BUDNITZ: Which comes around to saying that my compact valve is similar enough to those that were tested or observed.

CHAIRMAN APOSTOLAKIS: All right.

MR. UHRIG: Thank you.

MR. BUDNITZ: I mean, that's what it comes down to in terms of the engineering.

CHAIRMAN APOSTOLAKIS: All right. 3.9, external flooding.

DR. KRESS: Just a general question, and not on 3.9, but when you incorporate references to acceptable methodologies -- for example, in the high winds, you have three or four.

Now, the NRC, I don't know how they will use this standard, but if they say we want you to use this standard for the quality of your PRA, are they going to have to go in and study all these references, and decide whether or not they really think they are acceptable?

What was the criteria for deciding that they were acceptable methodologies? Was it just the expert judgment of you three, which I figure it was. That's probably good enough for me, but I don't know if it is good enough for NRC or not.

MR. BUDNITZ: We decided that a particular methodology or in some cases an application, go there and see what they did, would be acceptable. And what we are seeking is a review of our peer community to make sure that they also agree.

CHAIRMAN APOSTOLAKIS: But eventually the staff will have to decide whether to adopt this, right?

MR. BUDNITZ: Whether they also agree.

CHAIRMAN APOSTOLAKIS: And that's when this question will come up.

DR. KRESS: I would hate to have to go to every one of these and review every method on them.

CHAIRMAN APOSTOLAKIS: They have already members who know that. There is some knowledge within the staff. PRA configuration control. Fine?

DR. SEARLE: Yes.

CHAIRMAN APOSTOLAKIS: Risk assessment application process.

DR. KRESS: That's fine. They didn't reference. They incorporated by reference the --

MR. BUDNITZ: You skipped right over peer review.

CHAIRMAN APOSTOLAKIS: I skipped what?

MR. BUDNITZ: The peer review.

CHAIRMAN APOSTOLAKIS: Because it is unimportant. Peer review.

VICE CHAIRMAN BONACA: Here I think you are making a reference to the ASME description of that, and that is somewhat of a contested issue here.

You know, what do you mean by -- I mean, you seem to impose additional requirements here just because I expect the expertise that you need in seismicity, and special exception events is somewhat different than the one that you use for the level one.

MR. BUDNITZ: Right, but --

VICE CHAIRMAN BONACA: And so maybe that is a moot issue here.

MR. BUDNITZ: But the general requirements are taken from ASME by reference. For example, ASME has a section that describes how you pick two of your that don't have a conflict of interest, or that type of requirement. That requirement, we just are not going to do it over.

CHAIRMAN APOSTOLAKIS: So, application process and documentation. I don't know --

DR. KRESS: You skipped over my section again.

CHAIRMAN APOSTOLAKIS: No, risk assessment and application process?

DR. KRESS: I wanted him to reiterate this is incorporated by reference to Chapter 3 of the ASME, and go back to it to see if there was any incapabilities or any inconsistencies. I don't know what Chapter 3 now looks like in the ASME.

And the version that I had, there did seem to be some inconsistencies, and so I don't know if they will stay or not.

MR. BUDNITZ: The only person around this table that knows is I, because I am on the team.

DR. KRESS: Yes.

MR. BUDNITZ: But it is not a secret, and I can tell you. It is very important that you should understand that there has been a change in the words, which may or may not represent a change in the philosophy, but let me describe.

When the three columns first came out a year-and-a-half ago, they were described as application categories.

DR. KRESS: Right.

MR. BUDNITZ: Like somebody thought that ISI would be in category one, and core damage frequency application is in category two. Over the last 18 months, it has become transparent that that is not the right way to think about it, and those three columns are now capability categories for the PRA, or for elements of the PRA.

Now, what that means is that you grade your PRA once, just once. You go find out your capability one for this, or capability two for that. Or by the way, in our case, you either meet it or you don't.

If you don't meet a piece of this, you can still the thing if that piece you don't meet doesn't matter.

Now, what Section 3 in ASME does is it says, okay, you have an application. You go to the application and you decide which pieces of the PRA you need for that application.

For example, you may not need the HRA piece, or maybe it is at the center of your application. So you decide which piece, and then you decide whether or not for your application that you need capability two or capability one, or capability three, although I kind of think it will always be two, but let's not argue, except for screening.

And then you go to the PRA, and see what you have got. If you need capability two for the application that you have got, and everything that needs it is two, then you are home.

If you need capability two, HRA, and you have a capability one, then you can't do it. You have to either upgrade it or do something else. So that's exactly what it is, and this is just the same.

DR. KRESS: Okay. It sounds like they are consistent now.

MR. BUDNITZ: Now, here, what you do is that since we don't have three categories, you are going to decide whether you need -- for example, suppose in the application you don't need the hazard, because the only thing you are worrying about is the capacity of a large pump.

Then if you meet the standard for the fragility's part, then you can use it, even if you don't meet the standard for the hazard part. It is just as simple as that, and I think it is pretty straightforward.

CHAIRMAN APOSTOLAKIS: Does the industry certification process include external events?

MR. BUDNITZ: No.

CHAIRMAN APOSTOLAKIS: And do they plan to use this?

MR. BUDNITZ: No, they have made an informal commitment, and it is not in writing, but they have said the words; that they will add to the certification process review requirements that cover this topic, and also low power shutdown when it comes along, and also fire when it comes along, so that they would have the same scope in the end.

CHAIRMAN APOSTOLAKIS: Okay.

MR. WALLIS: And what about the Section 7 documentation? I found the small print part, the note, useful, and it deserves bigger print. And we might even borrow some of your remarks, speaking about documentation for other purposes, such as thermal hydraulics.

CHAIRMAN APOSTOLAKIS: Are you allowing us to do this?

MR. BUDNITZ: I don't run anything, but it is my view that if you cite any American National Standard, as is in anything else, you can do anything that you want with it. It is a public document, and you just have to reference where it came from.

MR. WALLIS: We have a bit of a struggle with documentation requirements in other fields, and not just in this one, and we find that a surprising reluctance on the part of the originators of documents to make sure that they are right, and it is surprising.

DR. SEARLE: One is moved to wonder whether or not the clientele that will use this standard is any more competent in reading these words than these other people have been decoding similar remarks.

DR. KRESS: I don't think this needs much decoding. It is pretty clear.

CHAIRMAN APOSTOLAKIS: Okay. We have two minutes. Does anyone have a comment that is of great significance?

DR. KRESS: I think they did a good job.

MR. WALLIS: They did a good job.

CHAIRMAN APOSTOLAKIS: It is a good job, but that is not of great significance.

(Laughter.)

DR. KRESS: For this committee, that is.

DR. SEARLE: It is, and actually, George, I was surprised.

CHAIRMAN APOSTOLAKIS: Okay. We don't even have two minutes because NEI wants to say a few words. Bob, real quick.

MR. BUDNITZ: I need 10 seconds. I just turned to page 109 as I was turning through.

CHAIRMAN APOSTOLAKIS: And you have a question.

MR. BUDNITZ: And in the middle of the page is two references to Bernard, et al.

CHAIRMAN APOSTOLAKIS: Two references to

what?

MR. BUDNITZ: Bernard, et al, and I want to tell you that Don Bernard died a month ago, and I miss him, and I just want to say that I miss him. He was a terrific guy, and this field we are in is richer for his work, and I just wanted to say that for 10 seconds, okay?

CHAIRMAN APOSTOLAKIS: Thank you. Okay. Mr. Heymer. Do you want to come sit up front, or --

MR. HEYMER: I will just make comments here, George, and it will be very quick. My name is Adrian Heymer, and I am project manager at NEI with the Reg Reform Group.

The reason why I am here and some of the other people aren't is because they are out of town. The standard has only been out for a couple of days, and we have got some preliminary feedback. We did call some people when it came out.

We have had some feedback from EPRI, preliminary feedback, and preliminary feedback from a couple of the other groups.

And that feedback which came in this morning by a telephone call -- and as I sat here listening to the presentation, I just wondered if we were looking at the same documents.

And it may be because when you do a quick read, you read from the dark side and think the worst, and have not had time to digest it. But the gut feel, or at least the initial feel from the industry that have looked at it is that for reasons best known to themselves, I guess, judging by the discussions that have gone on, they feel they are precluded from using seismic margins approach.

DR. SEARLE: By this?

MR. HEYMER: Yes.

CHAIRMAN APOSTOLAKIS: Why?

MR. HEYMER: They just -- the comment I got back is that we have invested a lot of time and effort in seismic margins, and we failed to use this in a risk informed approach, and we would have to go to a seismic PRA.

So that is -- and I think that may be a process of the way that they have read it, and how they think they might have to apply it. But I think that might need some interaction, and we will provide you some comments on that as we will, and there will be some interaction on that as we go.

CHAIRMAN APOSTOLAKIS: Thank you.

MR. BUDNITZ: Just to say, about 50 plants did a seismic margin review using the EPRI method. We wrote these requirements to track the EPRI method. It is our judgment without knowing in detail that most of the plants that use the EPRI method will be able to show that they meet the standard.

Now, you don't go any further than that. If they have a competent margin review, it is our opinion that we have written the standards so that they will meet it.

Now, once they have met the standard, if they can't use it, that's not a fault of our having written the standard to tell them what they did, and to check it right, I think. In other words, I don't quite understand the match here.

MR. HEYMER: Well, you will have to take the comment in the sense of people are reading it for a couple of days, and they need to think about it, and sit down, and produce some comments, and there is going to be some industry iteration.

Because it was also interesting to note that the same people that made that comment said now what would really be good in this standard is if we had some additional guidance to take the seismic margins approach further.

And that's what I heard you were going to do anyway. So I encourage you to work on that and incorporate in the standard if you can.

MR. RAVINDRA: Can I add one thing to that?

MR. HEYMER: Yes.

MR. RAVINDRA: This committee has a subcommittee that endorsed our earlier draft of the standard.

CHAIRMAN APOSTOLAKIS: I think Adrian made the point. Thank you.

MR. HEYMER: There was a comment on the uniform hazards spectra, and there was a feeling that you are asking us to reevaluate that, and verify it, and there was significant effort and resources expended in doing that some time ago.

And it wasn't clear to the people who were reading it why we have to go back and reassess that.

MR. CHOKSHI: I also got an informal feedback on that point, and all it needs is a little bit more guidance and explanation.

MR. HEYMER: I think some of the other points that have been mentioned here have been good. I think on the plus side, I think the commentary section, I think if you expand on that, a lot of people found that very useful and a very good addition.

And there was a lot of positive comment in that regard. And I guess if we are saying that we are going to allow seismic margins, or at least not to cover seismic margins in the standard. And there is also going to be a section in there on the seismic PRA.

Perhaps we need some insights or some screening criteria of when one would be appropriate, and when you should move to a seismic PRA. And that's about it with regards to the extent of the comments.

CHAIRMAN APOSTOLAKIS: Thank you, Adrian.

MR. BUDNITZ: Thank you.

CHAIRMAN APOSTOLAKIS: Thank you, gentlemen, very much. This has been very enlightening and useful, and very friendly. You did a great job.

MR. BUDNITZ: Can I ask one further question?

CHAIRMAN APOSTOLAKIS: Yes.

MR. BUDNITZ: I have no idea what to expect. Are you going to consider writing a letter?

CHAIRMAN APOSTOLAKIS: Yes, we will consider writing a letter.

MR. BUDNITZ: Thank you. I just didn't know.

CHAIRMAN APOSTOLAKIS: We will recess

until 2:50.

(Whereupon, the committee hearing recessed at 2:33 p.m., and was resumed at 2:50 p.m.)

CHAIRMAN APOSTOLAKIS: Okay. The next issue is Reprioritization of Generic Safety Issue 152, Design Basis for Valves that Might be Subjected to Significant Blowdown Loads. Mr. Leitch is our leader on this. Graham.

MR. LEITCH: Dr. Apostolakis, the purpose of this session is to hear a presentation from the NRC staff regarding the proposed resolution of generic safety issue 152.

And that issue is the design basis for valves that might be subjected to significant blowdown loads. It is of particular interest for HPCI and RCIC, and reactor water cleanout valves on boiling water reactors.

And the concern was that while the valves might meet the NRC approved design basis, the design basis might not address the need for the valves to close against the differential pressure resulting from a large sized high energy pipe break.

So with those words of introduction, I will turn it over to Mr. Michael Mayfield, who will introduce the staff's presentation on this topic.

MR. MAYFIELD: Thank you. I am here this afternoon, and Ken Karwoski, who has recently joined my division, is going to make the presentation.

He is supported this afternoon by Sher Bhatar, the Chief of the Engineering Research Applications Branch, and Tom Scarborough from NRR.

So we are here to talk about the closeout, and not just reprioritization of this generic safety issue. So with that, Ken, why don't you go ahead.

MR. KARWOSKI: Good afternoon. My name is Ken Karwoski, and I will be discussing the staff's basis for proposing the closeout of generic safety issue 152 and seek ACRS endorsement on this proposal.

Generic safety issue 152 was raised by the ACRS back in the 1989 time frame, and as a result of its review of the staff activities related to generic safety issue 87, which had to do with the failure of the high pressure coolant injection isolation valves to close following a postulated pipe break.

GSI-87 is closed and it was closed in-part as a result of industry activities in response to Generic Letter 89-10 and its supplements, and in particular Supplement 3 to Generic Letter 89-10.

Generic Letter 89-10 focused on the ability of valves to function as designed. What the

ACRS was concerned about though was the adequacy of that design, were those valves capable of closing following a postulated high energy line break.

In order to understand the staff's basis for closing out generic safety issue 152, I would like to spend a few minutes on Generic Letter 89-10. In the mid-to-late '80s, the Office of Research did some testing on motor operated valves and identified a number of valve performance weaknesses.

As a result of that, they issued Generic Letter 89-10, and once again focusing on the ability of the valves to function as designed.

However, as part of that, licensees had to resurrect what the design basis for these valves were, and how to dig out the information to say what are these valves, or how are these valves supposed to operate, and under what conditions.

After the research testing results became available, the industry also did some additional testings on motor operated valves. They confirmed a lot of the problems that were identified in the research sponsored tests, and as a result of that, they started to develop working groups and users groups.

And there currently is still a joint

owners group addressing valve issues, not only motor operated valves, but air operated valves. Generic Letter 89-10 had seven supplements, and those supplements were -- the first one was issued in '89, and the last one in 1996.

Although Generic Letter 89-10 focused on the ability of the valves to operate as designed, the capabilities of the valves, that is, the actual design basis of the valves, was captured as a result of industry activities.

And the adequacy of the design was confirmed in part based on NRC inspections performed in response to 89-10, and confirmed through review of various documents, including the inspection reports, FSARs, and other licensee and NRC documents.

The NRC inspections did evaluate the reasonableness of the design pressures. If there looked like there was an indication where the valves were not designed to a full differential pressure, some of those issues were flagged to ONRR.

One of the examples that we provided in our write-up was Big Rock Point, where the valves were not designed for a full differential pressure, and ONRR subsequently evaluated those exceptions on a case-by-case basis and determined that in the case of

Big Rock Point that even though the valves were not designed for that condition, it was acceptable from a safety standpoint.

Priority focus of many of the early inspections in response to Generic Letter 89-10 were the more risk significant valves of HPCI, RCIC and reactive water cleanup.

Although the inspections focused on those, the lessons learned from the inspections applied to all motor operated valves, and in some cases applied to other valve types.

The staff briefed the ARCS numerous times in the 1990s regarding motor operated valves. In particular, in October of '93, the staff briefed the ARCS subcommittee on mechanical components, and at that time the chairman of the subcommittee, who happened to be the individual that raised the concern, indicated that he believed that the issue had been addressed and would recommend closure to that.

Subsequent to that, research confirmed many of the results and analysis presented to the ACRS at that time, and we confirmed basically that the actions taken by the licensees and by the industry in general, that we believed that there was sufficient evidence to close Generic Safety Issue 152.

And that concludes my presentation. If there are any questions, I will be glad to try to address them.

MR. LEITCH: So the reason for our confidence then is that Supplement 3 to Generic Letter 89-10 basically focused the industry's attention in this area. The industry did get the message, and investigated these valves and corrected them, if necessary.

And that was all backed up by NRC inspection activities?

MR. KARWOSKI: Yes. Basically, although 89-10 focused on the adequacy or the capability of the valves to function as designed, the industry took the initiative on their own, and in some cases upgraded the design of some of these valves.

So we are confident that those valves are capable of operating under a postulated pipe break event. And the industry has and continues to take an initiative in MOVs.

There is a periodic valve verification program currently underway. So they have taken those lessons, and they continue to apply them, and as they identify weaknesses, they improve their programs.

MR. WALLIS: Can I ask you about the

distinction between design and performance? I mean, you have used the word design a lot. And they may well be designed to do something. Do they actually do it?

I mean, if they were tested against the full differential pressure several times, did they still work?

MR. KARWOSKI: In the early days, I think the early testing indicated that, no, they wouldn't work under those conditions. As a result of 89-10 and the work done in response to that, that is where the licensee said, okay, here is the pressure that I need to operate against. Do they operate.

And that's where -- and so that is the performance aspect, and that is what the whole purpose of the 89-10 program was; is do they function as they were designed.

MR. SIEBER: Yeah, but they were relied on testable prototypes, as opposed to valves in a plant, in order to establish the relationship between design and actual performance; is that not correct?

For example, there was an industry program, and they did it in some steam plant someplace, a coal plant, where they tested prototype valves of various types in order to see whether the valve would lock up under these high DPs and high flows, or how much force it took in order to move the stem.

And a lot of utilities found that the motors were too small, or the gear trains were wrong. And then when they changed the gear train, it was too slow to perform the isolation in the time frame called for by the safety analysis.

Or if they changed the motor and didn't change the valve, the motor was so strong that it would drive the valve disk through the bottom of the valve.

Or you would overheat the wiring to it, and so this was not without a lot of problems. I presume that in the inspection process that every BWR was evaluated as to whether they did in fact determine what the design conditions were, and did it have an appropriate prototype test to say that their valve was good or not good.

And did either leave things as is, or change gear trains, motor operators, or the valves themselves. But that's what I gathered from the inspection material that I reviewed. Is that correct?

MR. KARWOSKI: Tom Scarborough may be able to add more, but the inspection did look at the reasonableness of the design and focused on valve factors and whether or not the licensees were implementing the latest lessons learned.

With respect to the actual testing of the valves, I know that there were some concerns expressed by licensees regarding the reasonableness to test all the motor operated valves under postulated pipe break events.

And so in some cases there may be groupings of valves, where they tried to group valves in order to limit the amount of testing based on the limitations in the plant. But Tom may be able to add more.

MR. SCARBOROUGH: Yes. This is Tom Scarborough. One of the things that you mentioned, that earlier program on the prototypes. Once they got 89-10, they realized that they needed a better way of learning more about blowdown flow conditions.

And the Electric Power Research Institute established that multi-million dollar program to do a number of blowdown tests and develop a first principle's model to look for blowdown performance.

And they found that there were critical perimeters of the sharpness of the edges, internal edges, and the clearances, in part in running the models.

And so what happened, especially for the HPCI, RCIC, a lot of licensees ended up running the EPRI model, and determining if they had any concerns regarding performance under blowdown conditions.

And then if they did, they would go in and adjust the internal clearances, or round off the internal edges to the valve. So that is how they were able to address performance, because of the definite concerns of trying to run a test on those type valves.

And during the inspections which I participated in, a large number of them, we did look at the difference of pressures that they were assuming, and how they came up with the thrust requirements for the used valve factors in the EPRI model, and then what actions did they take to address those. So those are the types of things that we looked at.

MR. WALLIS: So the assurance that they will work is based on the fact that they conform with an EPRI model?

MR. SCARBOROUGH: That's part of the basis. They would run the model, and we would prepare a safety evaluation on the model, and we evaluated it, and the licensees would use that as part of their determination.

Some licensees --Comanche Peak, for example -- actually did run some blowdown tests on their unit two when they were starting up to get direct information that they could apply to unit one.

So there was some actual test data that people had, but in a large number of cases it was using the EPRI model for the blowdown conditions.

MR. SIEBER: I guess in an operating plant you just can't create the conditions necessary to test the valves without taking a saw and sawing them off.

MR. SCARBOROUGH: Right.

MR. WALLIS: It is a basic problem, but it is very difficult to be sure a valve will work without testing. I mean, you can't just compute, and you're not always sure it will always do exactly what you thought it would do.

MR. SIEBER: Well, the EPRI models is an empirical model, and it is based on tests of prototype valves under a variety of conditions. So it is probably the best thing that you can do.

DR. POWERS: If I recall the SDR on that model properly, and I may not, my recollection is that there were questions about the length of upstream and downstream piping around the valve. Did those get resolved?

MR. SCARBOROUGH: Yes. As part of the evaluation of the model itself, and adjustments to the model, this was like a 2 or 3 year process when we reviewed it, and we were able to resolve those.

There were some changes to the model that were made, and the model, as it turned out, seems to be reasonable, and it seems to be tracking pretty well.

DR. POWERS: And I further recall that there were questions about whether the valves being tested had experienced the kind of aging and degradation that valves in the plants would have experienced. Did that issue get resolved?

MR. SCARBOROUGH: Right. And in the case of the Board Warner valves, we were concerned that there wasn't enough, and so EPRI did add an additional 5 percent margin any time that you are using a Board Warner valve with the EPRI model.

But we did evaluate and thought there was enough preconditioning of those valves as part of that, and that was part of our evaluation of what the model was predicting, and what the actual thrust requirements were.

So we went back and looked at all of

those, and in the final analysis, whether or not we accepted the model was based on having enough margin to account for any preconditioning that the valves had not achieved as part of the test process.

DR. POWERS: I guess the question always arises on how you decide how much margin to ascribe the phenomena that are of aging and degradation kind of nature.

MR. SCARBOROUGH: And that is part of what the joint owners group program that Ken mentioned are doing. Right now they are doing testing of valves in the plants under flow conditions -- not blowdown, but flow conditions -- and looking for changes in the thrust requirements.

And at the end of October of 2002, their five year testing program will be complete, and they will be preparing an updated report to establish a long term periodic verification program, with some potential need for testing either static or dynamic, but with diagnostics to evaluate that.

And one of the things that they are finding so far is if you open the valve up and do any maintenance, internal maintenance on the valve, the thrust requirements drop dramatically immediately.

But then they rise back up, and so that is something that had been found during the EPRI testing, and they are confirming it through the JOG program, and that will probably be part of their long term program when they come in in 2002.

DR. KRESS: Since the subject of your report is adequacy of the design basis, I guess your basic conclusion is that the design basis was inadequate?

MR. KARWOSKI: No, the conclusion is that the licensees, as a result of 89-10, and the emphasis on MOVs during the 1990s, that we confirmed that licensees did design the valves, or the valves are capable of operating under blowdown conditions.

DR. KRESS: With the improvements?

MR. KARWOSKI: With the improvements.

DR. KRESS: But those improvements didn't come about because of the design basis?

MR. KARWOSKI: Well, you see, that is where the concern has broken up into two phases; the adequacy of the design, which is GSI-152, and then the capability of the valves to function as designed, which was the focus of 89-10. It is hard to separate the two, but that's the distinction between the two points.

DR. KRESS: But my conclusion would have

been that the design basis was inadequate.

MR. SIEBER: In some plants.

MR. MAYFIELD: This is Mike Mayfield. That was the question that was put on the table, and as I understand from looking and reading that I have done, and in the briefings that I have had, what the 89-10 determined was that in general things were okay. And where there were some difficulties, the licensees had taken action to correct those.

DR. KRESS: But they weren't required to?

MR. MAYFIELD: They weren't required to. And as it turns out, the resolution to this generic safety issue doesn't require any subsequent action on the part of the staff because the licensees had already taken that action.

DR. KRESS: That is what I was going to get to; do we need to change the design basis.

MR. MAYFIELD: And I think the answer to that is that when you say design basis, as I understand it, these were -- did they correctly estimate how big the opening would be, and had they fully expected the full break, the full opening break, downstream in the valve.

And in some cases -- and I understand that the answer to that was no, and they have gone back and fixed that.

MR. KARWOSKI: Or like I mentioned at Big Rock Point, where they analyzed and determined that even though they weren't designed for that, that it was not a safety factor.

DR. KRESS: I guess in another world, where we might be getting new reactors every 3 or 4 months, or something, you would be constrained to go back and change the rule, or change the design basis. And under the situation now, you don't have that.

MR. MAYFIELD: I don't think it is so much that they -- I don't know that there is anything that we would go change other than we would look a lot harder perhaps at specifics that were included in the design.

MR. KARWOSKI: And I think the concern, the original concern was for older plants rather than the newer, because in the newer plants, they are frequently analyzed for pipe breaks.

MR. SIEBER: And the actual requirement comes from the ASME code does it not, and which says that under certain conditions you classify this as a high energy line, and if you need to be able to isolate it, as opposed to someplace in a rule or a reg guide saying that.

So if you endorse the code, and you have a code book plant, the requirement is embedded in your license, in your FSAR.

MR. KARWOSKI: But also from a practical standpoint, if a licensee says they are going to operate this system in this fashion, and it calls for the valve to close -- and this was part of the 89-10 review --

MR. SIEBER: Right.

MR. KARWOSKI: -- they have to show that the valve is in fact capable of doing that.

MR. SIEBER: Of closing.

MR. KARWOSKI: Because they were supposed to review the procedures and determine under what conditions the valves were expected to operate.

MR. WALLIS: It is a little tricky, because your are asked to demonstrate that a valve will do something which it never does, and so you never have a realistic test really in the plant.

So it must be rather difficult to give such conclusive proof when this thing has been sitting there all this time, and it is always going to work when it is called upon to work when it never does it routinely.

MR. KARWOSKI: That's correct, but that's the purpose for the testing and the monitoring, to provide you added assurance. And there is in most cases redundancy.

MR. WALLIS: But then you don't test something that has been sitting in the plant for 10 years.

MR. SIEBER: I think that 89-10 requires licensees to commit to periodic testing.

MR. WALLIS: At full pressure.

MR. SIEBER: Well, to test the torque requirement and the stem factor, and so on, and that's what MOVATS and MOVs, and all those are required to do.

MR. SCARBOROUGH: The new Generic Letter 96-05, which is sort of the follow-on of 89-10, is the periodic verification, and that is part of the joint owners group program; is that now that they have established the design basis capability for these valves, how do we monitor them and make sure that we don't have degradation.

And those programs have in place where they use diagnostic testing, and there is a dynamic diagnostic testing program going on to look for areas of degradation.

And then they are going to have an ongoing static diagnostic, with possible some dynamic diagnostic testing in the future. So they have a program established to look for that type degradation.

MR. LEITCH: Any further questions?

MR. WALLIS: Why do valves stick?

MR. SIEBER: Packing glands dry out and operators pull up on the nuts. They rust.

MR. KARWOSKI: And pressure locking from a binding.

DR. KRESS: The perversity of nature.

MR. WALLIS: Maybe small leaks that build up oil or something?

MR. SIEBER: No, that's the BWR.

MR. WALLIS: So if you knew why they deteriorated, you could specifically look for those things?

MR. KARWOSKI: That is correct, whether it be the grease deteriorating or whatever, correct.

MR. UHRIG: Isn't most of that done with signature analysis in the testing?

MR. SCARBOROUGH: Yes. Now a lot of the plants use stem mounted string gages for a direct measure of the torque and thrust. But in the future, especially for the low risk valves, they are looking for a motor control center improvements in that area that have been made in the last 2 or 3 years, which are quite dramatic.

And which they can actually get a good impression of what the thrusts are that are coming out of the motor, and so there are a lot of improvements in that area that they are looking for as well.

MR. LEITCH: Okay. Any other comments or questions?

(No audible response.)

MR. LEITCH: Thank you.

MR. KARWOSKI: Thank you.

MR. LEITCH: Dr. Apostolakis is away from us for a few minutes. I think the next thing on the agenda is writing reports.

DR. POWERS: Fortunately, FACA prevents you from starting anything until it's time.

MR. LEITCH: So let's adjourn until four o'clock then.

(Whereupon, the meeting was concluded at 3:15 p.m.)

CERTIFICATE

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

Name of Proceeding: Advisory Committee on Reactor Safeguards 479th Meeting

Docket Number: (not applicable)

Location: Rockville, Maryland

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

John Mongoven

John Mongoven
Official Reporter
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