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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

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516th MEETING

+ + + + +

THURSDAY

OCTOBER 7, 2004

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ROCKVILLE, MARYLAND

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The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Mario V. Bonaca, Chairman, presiding.

MEMBERS PRESENT:

MARIO V. BONACA	Chairman
GRAHAM WALLIS	Vice Chairman
F. PETER FORD	ACRS Member
RICHARD S. DENNING	ACRS Member
THOMAS S. KRESS	ACRS Member
GEORGE E. APOSTOLAKIS	ACRS Member
GRAHAM M. LEITCH	ACRS Member
DANA A. POWERS	ACRS Member

MEMBERS PRESENT: (CONT.)

VICTOR H. RANSOM	ACRS Member
STEPHEN L. ROSEN	ACRS Member-at-Large
WILLIAM J. SHACK	ACRS Member
JOHN D. SIEBER	ACRS Member

NRC STAFF PRESENT:

JOHN T. LARKINS	Executive Director, ACRS/ACNW
SAM DURAISWAMY	Technical Assistant, ACRS/ACNW, Designated Federal Official

AGENDA ITEMS	PAGE

P R O C E E D I N G S

(8:31 a.m.)

CHAIRMAN BONACA: Good morning. This meeting now will come to order.

This is the first day of the 516th meeting of the Advisory Committee on Reactor Safeguards. During today's meeting, the Committee will consider the following:

Safety Evaluation of the Industry Guidelines Related to Pressurized Water Reactor Sump Performance, Pre-Application Safety Assessment Report for the Advanced CANDU 700 design, Proposed Recommendations for Resolving GSI-185 "Control of Recriticality Following Small-Break LOCAs in PWRs", Mitigation System Performance Index Program, and Preparation of ACRS Reports.

The first session is going to be transmitted in the broadband TV throughout the building.

This meeting is being conducted in accordance with the Federal Advisory Committee Act.

Dr. John 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 a member 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 they can be readily heard.

I will begin now with some items of current interest.

First of all, Dr. Richard Denning has joined us an official member of the ACRS. I welcome you on board.

(Applause.)

CHAIRMAN BONACA: Secondly, you have in front of you a package of items of interest. I would like to point out on the second page, you will see there is the dates of the Nuclear Safety Research Conference. It's being held from October 25th to 27th at the Marriott at Metro Center. For those of you who are interested in attending, there is information in related to the conference here.

With that, I think we can move from the introduction to the first item on the agenda. That's the safety evaluation of the industry guidelines related to the pressurizer water reactor sump performance. And Dr. Wallis is going to lead us through this presentation.

VICE CHAIRMAN WALLIS: I surely thank you, Mr. Chairman, I will.

Good morning.

We're going to hear about the latest in a series of steps currently undertaken by the staff to resolve GSI-191 concerning the potential for sump screen blockage during water recirculation following a LOCA.

I remind you that the staff issued Reg Guide 1.82, Ref 3, describing a set of requirements and necessary calculations. In our letter, we commented that it gave little guidance about how to perform these calculations.

The staff recently issued a Generic Letter asking for information on the evaluations by licensees. We reviewed various versions of this letter and commented that the calculations depended on guidance that was being prepared by NEI. NEI has now supplied this guidance and we are here today to hear the staff's response in the form of a safety evaluation report or SER.

I think it would be useful, both to us and to the staff, to bear in mind several matters which came up at the subcommittee meeting, which some of the members didn't attend because it is only a subcommittee. I think this will help to put the guidance and SER in perspective.

I invite the staff to correct me if anything that I say is wrong.

Item 1, calculations using the NEI baseline method for a large break near a steam generator covered with insulation in a particular PWR leads to generation of 14,000 cubic feet of debris, of which 5,100 cubic feet gets to the sump screen.

This corresponds to 50 feet thickness of debris on a 100 square foot screen, which is larger than is installed in some plants.

The staff's modification of the guidance using this factor of 40 percent to change the damage pressure would increase the amount of debris.

Item 2, an effect which has been called the thin bed effect appears to really be the effect of any layer of pure cal-sil or of fibers more or less saturated with cal-sil or perhaps with some other particular matter. It can occur anywhere in the layers on the screen.

There was a single repeated Los Alamos test in which a thickness of about 18 mils, or less than half a millimeter of cal-sil mixed with a small amount of fibers produced this effect.

Now 18 mils of cal-sil on a 100 square foot screen is a little over a gallon which is a volume of two inch thick insulation on a two inch pipe one foot long. It's about three times as much as we have here.

Item 3, many of the calculation procedures appear to be based on physics which may be unrealistic. There are about a dozen unresolved technical issues which were raised by the subcommittee.

Item 4, several parameters in the procedures and calculations appear to be based on a sparse database, sometimes a single experiment or even a single data point. For some materials like coating debris, there may be no database at all.

Item 5, the available database for some parameters does not encompass LOCA conditions. There are many uncertainties about how to apply a limited range of lab tests to realistic LOCA conditions.

If the staff restricts use to the range that has been validated, which appears to be its intent, the methods may be unusable without further extensive testing.

Item 6, there is no guidance for some

effects such as chemical or downstream effects. Plant specific methods appear to be required.

Now I said all this because I think we need to put it in perspective and the staff needs to respond to these issues which came up at the subcommittee meeting sometime today if they can do so.

Thank you very much.

MR. JOHNSON: Thank you. My name is Michael Johnson. I'm from the Office of the Nuclear Reactor Regulation. And I'm joined by staff from NRR and also staff from Research and also support from LANL.

We certainly appreciated the opportunity to meet with the subcommittee last month. And we appreciate the opportunity to meet with the full committee today.

And, Dr. Wallis, we are certainly aware of the issues that you have raised. And we look to be able to talk to those issues as we go through the presentation.

I want to open with some high level overall comments. And then we'll move out throughout the presentation.

As was pointed out, and the committee is well aware, GSI-191 is an important safety issue. And was pointed out, we issued a bulletin following our briefing. In August we issued a Generic Letter. A central part of that Generic Letter is to have licensees doing evaluation. And, of course, that the --

VICE CHAIRMAN WALLIS: I'm sorry to interrupt. We don't have any handouts from you?

MR. JOHNSON: You don't have handouts from me. That's correct.

VICE CHAIRMAN WALLIS: Are there going to be no handouts? So we don't know what you're going to say?

MR. JOHNSON: That's right.

VICE CHAIRMAN WALLIS: Oh, that's very interesting. Thank you.

MR. JOHNSON: Of course a central part of this Generic Letter -- we talk to the Generic Letter as the industry's evaluation guideline. And we are here today, of course, to talk about the staff's evaluation, our safety evaluation of the industry guidelines related to GSI-191.

I wanted to just make a point before we get started and that is that I think the staff has done a tremendous amount of work in terms of dealing with this issue. And we're very proud of what the staff has done.

And that tremendous amount of work, I think, includes interacting with the industry and external stakeholders at almost every stage of development of the industry's guidance report. It includes carefully reviewing the various final industry submittals that we had that formed the basis of the guidance report.

And so, again, the staff has done a tremendous amount of work on this activity. In addition, we worked very hard to consider the subcommittee's comments that were provided last month.

And we're going to try again in today's presentation to be able to focus in on what we heard as the major comments and what we've done in terms of revising the SE, where possible, to incorporate improvements.

In our presentation, what we plan to do is provide first of all a brief overall description of the approach at a very high level. Following that, Tom Hafera, who is going to come and is going to talk very briefly -- I think one of the things the subcommittee was interested in when we met with you was trying to get a practical feel for what actually happens when you apply the application methodology.

In fact, some of the numbers that you talked about, the 14,000 and 5,000, we've taken a look at that and Tom is going to be able to talk about the LOCA accident and, in fact, where we think the evaluation methodology takes you from our perspective.

In addition to that, we're going to touch on each of the major aspects of the safety evaluation. And for that, we're going to talk about what the guidance report provides, we're going to talk about what areas in which we found that there were additional constraints that were necessary or additions that were necessary in the guidance.

We're going to touch on issues raised by the subcommittee again. And we're going to highlight the changes that we made as a result in the staff safety evaluation.

Before I begin -- and so we're going to move into that -- but before we begin, I actually wanted to make three points.

The first was we look at the evaluation methodology, which is really the guidance report and the additional constraints that are captured and discussed in the safety evaluation as a package.

It's possible -- we talked about this last month, we'll talk about it again today -- to identify specific issues in individuals areas where limitations in testing or analysis or experience as a result of those limitations, there are uncertainties.

But we believe that when you consider the issues, those issues in the context of this overall approach, this overall package, that this package provides reasonable assurance of adequate protection and, in fact, will result in real safety improvements to the plants once that evaluation is done --

VICE CHAIRMAN WALLIS: Oh, I'm going to ask you about --

MR. JOHNSON: -- and plants put it --

VICE CHAIRMAN WALLIS: -- that. This package provides reasonable assurance for protection. There's no assessment in any of this about the consequences of using the guidance. Are you going to do all that today? We haven't seen any of that.

MR. JOHNSON: We're going to, again, talk about where this evaluation package takes you. We have, in terms of the approach that we've used, looked -- again, our primary focusing in reviewing what was proposed by the industry was to step back and ask ourselves in these various areas, in pulling together this package, does the package provide the ability for the staff to have reasonable assurance. And yes, we are comfortable that it does.

MEMBER KRESS: How do you separate the package from individual issues? I mean the package is made up of these individual issues of which there are problems with. How do you reconcile that?

MR. JOHNSON: In fact, that's a very good question. And certainly what we did in terms of going through the package was to look at the individual pieces.

And, in fact, one of the criticisms that we've had from the industry, in fact, was that, you know, these areas, some of these areas are, you know, there was already, I guess, a perspective about how conservative the package was and that we looked in individual areas. And, perhaps, the way we ended up was a package that is stepping back, overall conservative.

Well, we were very mindful when we went through the individual issues to look at those individual issues. And we couldn't arbitrarily -- we could not blindly -- couldn't blindly rely on conservatisms in certain areas of the package to account for areas in other areas of the assessment where we don't have enough information. The coatings, Dr. Wallis pointed out, the coatings issue is one.

So what we did was in those areas, we looked where we couldn't provide -- couldn't find justification, adequately justification of what was provided to us, we looked at stepping back, taking an approach that was conservative for that particular area.

And then as you step back, that's what gives us confidence that across the spectrum, this package does, in fact, this package, in fact, is sufficient for us to have adequate assurance that these plants will operate in a manner that is more safe once they've done the evaluation and once they've made the fixes.

VICE CHAIRMAN WALLIS: I don't see how an evaluation makes any difference to the safety. It's still the same plant. You've just evaluated it. Now you have to figure out what to do.

MR. JOHNSON: Absolutely -- well --

VICE CHAIRMAN WALLIS: Until you've done something, you haven't changed anything.

MR. JOHNSON: I agree with that. The end of my sentence was the evaluation, they do the evaluation and make fixes that are necessary.

VICE CHAIRMAN WALLIS: Well, let's separate the quality of the evaluation from the actions that might be taken to assure this safety. It seems to me those are two different issues unless you can somehow -- maybe you can craft a couple of them in a convincing way. I'd love to see it but --

MR. JOHNSON: Let me come back to that point, if I can, because that actually touches on a point that I want to make.

The -- you know, the staff's primary focus, and I wanted to make this very clear, our primary focus was to look at the evaluation. We want to have clear criteria about what is needed in terms of the approach for the evaluation but what is acceptable to the staff in terms of that evaluation because no matter what fix gets implemented by the industry, we have to go back and be able to assure ourselves that, again, we have reasonable assurance of adequate protection, these plants are safer.

So we've been very focused on the evaluation. We've not been focused, it's not been the staff's responsibility to design, to identify the fixes, to design those fixes. And, in fact, we've not talked about that.

The industry will talk about that perhaps in the presentation that they make. Again, our focus has been on the evaluation methodology.

However, having said that, the industry guidance report and the staff's SE package provide for licensee consideration of a range of solutions from housekeeping and FME programs, for insulation change out or modification, for improving coatings and the coatings program, or modifying the sump design.

The approach also has in it the ability for licensees to implement creative fixes, including backwash designs and active strainers.

There's a risk-informed piece of the alternative method that we'll talk about in the approach that provides the ability for licensees to rely on more realistic assumptions in the analysis of breaks -- for the analysis of breaks that are greater than the debris generation break size.

And for modifications --

MEMBER APOSTOLAKIS: Excuse me, Mike. I'm a little -- as I was reading the document, I was trying to understand what the risk-informed approach means. And I came to the conclusion that what the document meant was looking at systems, right, trying to cool the core essentially without maybe alternate ways of doing it.

And, again, reading the report and various comments from the subcommittee, especially the subcommittee chairman, I saw the word uncertainty all over the place. Yet there was no effort to quantify this uncertainty. And then it occurred to me that this Agency really has pioneered the quantification of uncertainty in such difficult circumstances when it issued NUREG 1150.

So I'm wondering why this -- an approach that would try to quantify the uncertainties in the models and the assumptions about 40 percent, 15 percent, and so on, why this project did not attempt to do something like what NUREG 1150 did in quantifying uncertainties in severe accidents that were not smaller than this.

And yet they did it. They assembled experts. Is that because it's too expensive? Or different people are doing it? And why isn't that the kind of approach part of what we call risk-informed? Risk-informed is not just bringing other systems into the picture. It's quantifying the uncertainties that you have. And these uncertainties may be mechanistic models and mechanistic assumptions as it is here.

But when I read the section on risk-informed, they didn't say anything about that. It talked about cooling the core.

So am I off base here? Or should you

start doing something like 1150?

MR. JOHNSON: You've never been off base Dr. Apostolakis.

MEMBER APOSTOLAKIS: Oh, thank you very much, Mike. You can go on.

(Laughter.)

VICE CHAIRMAN WALLIS: Well, I'd like --

MEMBER APOSTOLAKIS: I'm sorry?

VICE CHAIRMAN WALLIS: The risk-informed part only refers to the accident sequence and the effect on safe temperatures in the containment. There's no effect whatever on any of the material in this document about transported debris and sump blockage.

MEMBER APOSTOLAKIS: Yes, but that was part of my question.

VICE CHAIRMAN WALLIS: That's absolutely right. I think, Mike, isn't that true? That risk-informed is not being applied to any of those parts of the problem.

MR. JOHNSON: Yes, when we thought risk-informed, and I do want to come back to the point I was trying to make and go through that point, but -- and we are going to talk about the alternative that you talked about --

VICE CHAIRMAN WALLIS: Yes, we've got to move on. I'm sure you have a lot to say.

MR. JOHNSON: Let me -- if I can just go through and we'll touch on that --

VICE CHAIRMAN WALLIS: Okay, I'm afraid --

MR. JOHNSON: -- maybe and get to your question.

VICE CHAIRMAN WALLIS: -- that you're going to get questions, I'm sure, at some time.

MR. JOHNSON: Absolutely, absolutely.

The point I wanted to make was we're not focused on the evaluation -- we focused on the evaluation. We've not been focused on the fixes. There's flexibility throughout this guideline for creative fixes. There's flexibility in terms of the risk-informed alternative.

We want licensees to avail themselves of those but certainly the responsibility for the fix, the responsibility for the fix rests with the industry.

And the last point I wanted to make is, you know, the staff has said and the Commission agrees -- has agreed that it's time to move forward with resolving GSI-191, which means placing the responsibility, again, on the industry for beginning the evaluation and making changes to the sumps if those changes are needed.

This issue --

VICE CHAIRMAN WALLIS: I'm sorry. You resolve an issue by placing the responsibility on somebody else? Is that how you resolve an issue? How do you say -- isn't an issue resolved when sort of an risk implications have been reduced or changed back to an acceptable level or something?

Isn't it -- it's not resolved until some action is taken. You don't just resolve it by studying it, do you?

MR. JOHNSON: Absolutely.

VICE CHAIRMAN WALLIS: So if you're smart you can't say it's resolved by your studying some evaluation method until something has been done.

MR. JOHNSON: No, my point is that we've evaluated the issue to a point where we're ready to transfer this issue over to the industry. We're ready for licensees to begin the evaluation an to ultimately make the fixes -- make fixes to their plants if those fixes are indicated by the results of the evaluation.

VICE CHAIRMAN WALLIS: Could we say this is a step on the way to resolving the issue?

MR. JOHNSON: Yes, absolutely.

VICE CHAIRMAN WALLIS: Okay. Thank you.

MEMBER KRESS: Let me paraphrase what I think I've heard. You're convinced that if you go through this methodology and follow it properly, that you will end up with a conservative assessment of the effect of blockage on the net positive suction head so that --

MR. JOHNSON: Correct, that's correct.

MEMBER KRESS: Okay. So that's what we need to look for is whether or not -- how you make this judgment of the conservative.

MR. JOHNSON: Right, that's right.

MEMBER KRESS: Okay.

MR. JOHNSON: This issue has been on our plate for 25 years. We were counting last night and we came up with 25 years as the number.

VICE CHAIRMAN WALLIS: Well, that doesn't resolve anything yet does it?

MR. JOHNSON: There are already vendors we've spoken with who are out performing the evaluation using the baseline, using the baseline and the draft SE for plants, working on the evaluation and engineering fixes to resolve the issue.

We heard at the subcommittee meeting that there's at least one licensee who is anxious to move forward with an active solution. And anxious for not having further delays in our efforts.

So, again, Dr. Kress I think you're right. My summary would be the evaluation that we have is a good evaluation and it will provide for reasonable assurance of adequate protection once the evaluation is done and fixes are made. The guidance is adequate to support that. And I hope that we get a letter from the ACRS following this presentation and the rest that you hear on this issue --

MEMBER KRESS: Were you able to do anything to accommodate the licensee who wanted to pursue an active thing? Or does he have to wait for all this stuff to get resolved?

MR. JOHNSON: We have, in fact, one of the pleas of that individual who spoke at the subcommittee meeting was to enable the active solution.

We believe that the alternative we already have in the SE, as proposed by the industry in the guidance report, the ability for licensees to employ active solutions.

MEMBER KRESS: So he could go ahead and proceed with that with assurance?

MR. JOHNSON: Absolutely.

MEMBER KRESS: Okay.

MEMBER APOSTOLAKIS: So did you answer my question, Mike, and I missed it or --

MR. JOHNSON: We will answer --

MEMBER APOSTOLAKIS: Oh, you will? Okay.

MR. JOHNSON: -- your question later.

If there are no other questions, I would -- is Dave Solorio -- Dave? Dave is going to talk about the overall approach.

MR. SOLORIO: Thanks, Mike. Good morning. My name is Dave Solorio and I work in the Office of Nuclear Reactor Regulation. I've been before a number of you to talk about license renewal in the past.

To provide an overall perspective for the sump evaluation approach and lend perspective to the presentations that will follow, my intention is to provide a quick summary of the major elements of the staff's safety evaluation report to illustrate the process a pressurized water reactor licensee would use to go through should it choose to use the NEI guidance report and the staff's SER to perform a mechanistic evaluation of sump performance to respond to Generic Letter 2004-02, Potential Impact of Debris Blockage on Emergency Recirculation During Design Basis Event Pressurized Water Reactors.

My remarks will focus on the staff's SER.

And this slide that we've shown up here provides a process flow chart I'll use to illustrate the evaluation steps a licensee would go through.

Following my presentation, Mr. Tom Hafera will go over an example to illustrate how the SER could be used in evaluating sump performance.

The top half of the slide is a basic illustration of how we envision the industry's guidance report plus the staff's SER to be one vehicle by which a licensee could perform an evaluation of sump performance. I want to stress it is one way. Licensees are free to propose alternatives that the staff would be willing to review.

The end result of applying the guidance in these two documents would be a determination of whether the as-built sump design was sufficient or plant configuration changes were needed. Plant configuration changes could be resizing the sump or activities directed at limiting critical debris sources.

The bottom half of this slide illustrates the major evaluation areas in the staff's SER. I have designated by a small yellow circle, numbered one through seven, these steps. And obviously we would expect that the guidance report would be used in parallel.

I did not show Chapter 5, Physical Refinements, which discusses ways to reduce debris sources mainly because there was not as many questions on that section at the subcommittee meeting.

Staff presentations will follow mine and they will be brief on several of these sections and they are geared towards highlighting what we did to respond to the questions received at the 922 subcommittee review of this topic.

A major concept to recognize in the guidance report is that there is a baseline method, or first step method, which is intended to be a quick and easy way to reach a conclusion. But there are costs in terms of fidelity. If the results show the margins are not acceptable, refinements have been offered in some areas, but not all, to obtain a more realistic estimate.

Item 1, Section 3.3, first off, a licensee needs to determine the break size and location that generates the maximum debris insulation source term.

Item 2, Section 3.4 --

MEMBER APOSTOLAKIS: So what's the frequency of that?

MR. SOLORIO: The frequency?

MEMBER APOSTOLAKIS: Yes.

MEMBER APOSTOLAKIS: I'm trying to understand. I mean all this is conditional on a break, right?

MR. SOLORIO: Yes. But what --

MEMBER APOSTOLAKIS: Because somewhere there in the report, you guys say this is a low probability event.

MR. SOLORIO: Yes.

MEMBER APOSTOLAKIS: Therefore we can use risk-informed approaches, which struck me as a very strange statement.

MR. SOLORIO: Well, in --

MEMBER APOSTOLAKIS: You can't use risk-informed approaches if the probabilities are higher than ten to the minus four or five? Anyway, that's an editorial comment. But --

mR. SOLORIO: Okay. Well, the guidance in the NUREG 1(a)(2), Rev 3, and also what's transmitted in the -- or its application in the guidance report by the industry put together, is really to go off and look in terms of what break sizes could you generate the maximum debris --

MEMBER APOSTOLAKIS: I under --

MR. SOLORIO: -- not to factor in the

frequency of that break size.

MEMBER APOSTOLAKIS: I understand that. But what is the frequency?

MR. SOLORIO: The frequency --

MEMBER APOSTOLAKIS: Is the frequency of a large LOCA?

MR. SOLORIO: Let me ask Donnie Harrison that.

MR. JOHNSON: I think again you're asking about an aspect -- we're going to get to the question that you have about the alternative method and it's in that method, the alternative method, where we look at, for example, we establish the debris generation break size based on work coming out of 50.46. That's what we were sort of referring to as the risk-informed approach.

MEMBER APOSTOLAKIS: But it should be down --

VICE CHAIRMAN WALLIS: George, can we move on? I think --

MR. SOLORIO: If we can hold it until then.

VICE CHAIRMAN WALLIS: -- we have to move on until we get to the risk-informed part.

MR. SOLORIO: Yes, I think that's --

MEMBER APOSTOLAKIS: It's not the risk-informed part.

VICE CHAIRMAN WALLIS: But we haven't got to that discussion yet.

MEMBER APOSTOLAKIS: I understand but what I'm asking is not risk informed.

VICE CHAIRMAN WALLIS: But we've got about ten technical items to discuss first.

MEMBER APOSTOLAKIS: Well, Michael used the expression adequate protection several times earlier.

VICE CHAIRMAN WALLIS: I think you have a very good point. But I'm just saying that --

MEMBER APOSTOLAKIS: Okay.

VICE CHAIRMAN WALLIS: -- I think he's going to get to it.

MEMBER APOSTOLAKIS: Okay.

VICE CHAIRMAN WALLIS: If he doesn't get to it, you can ask it all again.

MEMBER APOSTOLAKIS: In a subtle way, you are telling me to shut up.

(Laughter.)

MEMBER APOSTOLAKIS: Message received, Graham.

MR. SOLORIO: Item 2, section 34, next the break needs to be translated in terms of debris generation. We get to debris generation through the construction volumes or zone of influence as we refer to it.

Special considerations are called out for coatings due to their ability to represent an additional volume of material that could, under optimal conditions, transport to the sump screen.

Lastly, refinements are available in this area if necessary.

Item 3 deals with section 35, highlights that not only must we be concerned with generated debris, but there are also debris sources already lying around containment or easily washed off by a break that can possibly be transported to the sump.

Item 4, section 36, highlights the transport mechanisms that can be assumed in terms of how much of the generated debris can be expected to make it to the sump. Should the licensee determine that using the rough approximation methods of the baseline yields large transport percentages, there are refined methods that can be used to gain a more realistic estimate.

Item 5 --

VICE CHAIRMAN WALLIS: Now wait a minute.

There's a feedback loop. It says no thin fiber layer.

MR. SOLORIO: Oh.

VICE CHAIRMAN WALLIS: Do you understand what that means?

MR. SOLORIO: Well, in response to your comments from last subcommittee meeting, we've added an attachment.

VICE CHAIRMAN WALLIS: Yes, but it says thin particulate layer. There's nothing about a thin fiber layer. You can have a fiber layer ten foot thick and have a particulate layer of one mil. And I understand that is the effect that we're talking about.

MR. SOLORIO: Well, this --

VICE CHAIRMAN WALLIS: Do you understand that?

MR. SOLORIO: -- this triangle that you're asking me about, I believe --

VICE CHAIRMAN WALLIS: Well, the guidance is very, very unequivocal about this thin bed effect.

MR. SOLORIO: Well --

VICE CHAIRMAN WALLIS: And I'm just asking you if you understand what is meant by this -- how do they evaluate --

MR. SOLORIO: Yes, we do Dr. Wallace. And we are going to actually present a brief description --

VICE CHAIRMAN WALLIS: Oh, you are going to present that, okay.

MR. SOLORIO: -- of that in one of the slides that --

VICE CHAIRMAN WALLIS: Thank you.

MR. SOLORIO: Let's see where was I. Let's see, Item 4 --

MEMBER ROSEN: No, I don't understand right there. If you answer the question yes, I understand if you have a thin fiber layer, you go right back to the beginning to step 2.

MR. SOLORIO: Well, what I meant to say, and maybe it's confusing, if the licensee would say I don't have a thin fiber layer --

MEMBER ROSEN: Yes.

MR. SOLORIO: -- then there still -- there needs to be -- you need to go back and look at whatever debris source might equivalently create some kind of a mat against your screen and lead to a head loss.

VICE CHAIRMAN WALLIS: You keep going around forever until you find a fiber layer?

MR. SOLORIO: No.

VICE CHAIRMAN WALLIS: That's what it looks like.

MR. SOLORIO: It's just meant as a feedback loop that is the licensee would ask that -- would conclude that they don't have it, then they would have to go back and assess it for other debris sources.

MEMBER ROSEN: Well, diagrammatically it's not very clear.

VICE CHAIRMAN WALLIS: So this is one of the technical questions I guess. Shall we move on to the --

MR. SOLORIO: Sure.

MR. JOHNSON: Actually we added this. This is not -- you won't see this diagram in the SE. We simply put it up to talk about the various blocks.

VICE CHAIRMAN WALLIS: But it's supposed to explain things to us. So --

MR. JOHNSON: Yes.

VICE CHAIRMAN WALLIS: -- we can ask questions about it?

MR. JOHNSON: Yes, absolutely.

MR. SOLORIO: Sure, sure.

Item 4, section 36, highlights the transport mechanisms.

Let's see, item 5, section 37, is the kind of what we've been waiting for step, the determination of the head loss across the sump screen which ultimately tells you if you're done, for the most part or if you have more work to do, which item 7 is meant to illustrate.

Item 6, section 7, is the kind of hold the horses step. Before you can make your final decision if you're done or redesign as necessary, you have to consider for the effects of debris making it through the sump screen and their effects on emergency core cooling system components and the operation of them.

VICE CHAIRMAN WALLIS: Why is additional consideration of chemical effects in a feedback loop?

MR. SOLORIO: Yes, sir. If -- well, you are aware that we're running tests, the Office of Research are funning tests to determine the impact of the chemical effects. Licensees are -- we're going to share that information with licensees.

The idea here is that if, in fact, this testing shows there is an issue that needs to be addressed, then you would have to go back and determine or consider those chemical effects in your debris, the regeneration step.

VICE CHAIRMAN WALLIS: That same is true

of downstream effects. If further research shows that metal pieces go through the screen and have some downstream effect, then you have to do something about that, too. I don't know if it's a feedback loop. But it should be a box that is somewhere in the diagram.

MR. JOHNSON: That's right. And we actually -- in box 7, in fact, upstream and downstream is here in box 6.

MR. SOLORIO: And we already know that there are concerns because we've seen testing in certain plants where we've seen the effects of downstream effects so we know it's a real issue.

VICE CHAIRMAN WALLIS: Are you going to talk about that later? Are you going to talk about each of these boxes later? Is this the outline of your presentation?

MR. SOLORIO: We're going to talk about the majority of them and --

VICE CHAIRMAN WALLIS: Okay.

MR. SOLORIO: -- our decision and which ones we talked about really stem from the questions that the subcommittee asked.

VICE CHAIRMAN WALLIS: So I guess if you don't visit one box --

MR. SOLORIO: And we were going to talk --

VICE CHAIRMAN WALLIS: -- we can ask you to visit it?

MR. SOLORIO: -- about this one.

MR. JOHNSON: We are going to talk about downstream effects.

MR. SOLORIO: We are going to talk --

VICE CHAIRMAN WALLIS: This is sort of an --

MR. SOLORIO: -- downstream effects.

VICE CHAIRMAN WALLIS: -- outline of your presentation.

MR. SOLORIO: Yes, sir.

MEMBER APOSTOLAKIS: Now every year, Mike, said that, you know, maybe individual pieces of this are not too satisfactory but the overall approach is acceptable between the NEI guidance report and the staff's consideration.

So I assume then that Box No. 6, where you are formulating possible additional design changes, will be done at that level? That you will look at the whole thing and say well, gee, you know, maybe they ought to consider this design change. Is that correct?

MR. SOLORIO: Yes.

MEMBER APOSTOLAKIS: You will not look at individual boxes. How would you do that? Is it an integrated decision-making process in a deterministic world? Is that what it is?

MR. JOHNSON: Well, let me be -- I'm not quite sure that I understand your question. We -- the Generic Letter requires that licensees provide the results of their evaluation to us --

MEMBER APOSTOLAKIS: Yes.

MR. JOHNSON: -- and their plans to make any corrective action that they would make. So the licensee would have gone through this exercise, figure out whether or not --

MEMBER APOSTOLAKIS: Yes.

MR. JOHNSON: -- they could redesign their sump. They'll propose corrective action -- they'll plan corrective actions. And our plan then going forward is to audit some of those plants in terms of the evaluation, in terms of what they actually put in place to make sure that from our perspective, those are acceptable.

But the licensee does the evaluation. The licensee does the redesign using that evaluation to assure that at the end, they have sufficient net positive suction head so --

MEMBER APOSTOLAKIS: But you --

MR. JOHNSON: -- they can provide long-term cooling.

MEMBER APOSTOLAKIS: -- will review that?

MR. JOHNSON: We plan to audit those results --

MEMBER APOSTOLAKIS: Audit, okay.

MR. JOHNSON: -- and with the oversight process going forward, we would look to see that be a key feature or an ongoing feature, I should say, in terms of --

MEMBER APOSTOLAKIS: But my question is could there be a situation where you're maybe unhappy with Box No. 3 but then a licensee argues that we are so conservative in Box No. 4 that we really don't have to worry about Box No. 3?

And you said earlier that it's really the big picture that counts. So could that be the case? And how will the decisions be made here? What's acceptable? And what's not?

MR. JOHNSON: Well, you know, conceivably a licensee -- remember again, this is one acceptable means. And so -- and, in fact, a licensee can come in -- I would anticipate the licensees would --

VICE CHAIRMAN WALLIS: Well, Michael, you're missing the question. The question is you've got all the boxes. And you're uncertain about them. And some of them you're very uncertain about.

For instance, you know nothing, almost next to nothing about chemical effects. How can you give assurance that the entire picture is all right?

Now I'm going to give you an analogy. It occurs to me -- I take my car to the garage. And the guy says well, your brakes are not very good and your transmission is about to go and your engine is only firing on three cylinders. But the whole car is okay.

Is that an analogy that makes sense here? What are you trying to say?

MR. JOHNSON: Well, actually -- let me try to answer your question but I thought actually your question was a little bit different.

With respect to chemical precipitation effects, we recognize -- licensees -- and we told the industry, the industry recognizes that at the end, their fix is going to have to accommodate what comes out of the testing that's going to -- that's ongoing. And we'll get those insights around the end of the year.

As they are doing the evaluation and planning their fixes, they will need to accommodate what comes out of that. If the answer is nothing, then they're good. If --

VICE CHAIRMAN WALLIS: You've said that before. But, I mean, there is this basic question that George is asking. And I don't think you're addressing it.

MR. JOHNSON: Actually, I thought George's question was -- I thought your question was how is the staff going to decide these -- with respect to these various aspects of the evaluation if it's okay. And we do that all the time.

We look at staff evaluations and use our engineering judgment to decide whether the justification provided by the staff, whether the alternate means is acceptable. And we make a decision based on that. That's what --

MEMBER APOSTOLAKIS: I understand that and --

MR. JOHNSON: -- what we do.

MEMBER APOSTOLAKIS: -- I'm --

VICE CHAIRMAN WALLIS: Well, maybe we should on. Are you finished? Are you going to talk about all the boxes here?

MR. SOLORIO: I'm going to be done in about a minute or less.

Let's see. I was just going to mention

that item 4A, section 6, is adjunct approach. It begins with brake selection for performance, sump performance evaluations. It allows for more realistic assumptions and use of risk insights. And, Dr. Apostolakis, we have a presentation on that later.

Item 7, while there may be areas where additional study can help reduce conservatism, the approach in totality provides a comprehensive process for evaluating sump performance.

And now I'll turn it over to Mr. Hafera if there are no more questions.

MEMBER FORD: Could I ask -- are you going to discuss item 7 at all?

MR. SOLORIO: Well, actually through the example that Mr. Hafera will give, he'll give some practical consequences or ways or strategies a licensee might use to address the issue.

MEMBER FORD: Okay, since I suspect we won't have much time to do that, I just draw your attention to there could be undesired consequences. If you remove circuit-based insulation, then you will increase the danger of cracking of stainless steel components underneath that insulation as fully discussed in Reg Guide 1.36.

MR. SOLORIO: Got it.

MEMBER FORD: Thank you.

VICE CHAIRMAN WALLIS: Are you going to have some time here -- I don't know how long you're taking on these various things but it doesn't look like many slides so you're going to have to tell us if you've got a lot to come and we've got to hold our questions. I just don't know how to pace this presentation. I'm sorry.

MR. JOHNSON: We'll try to help with that, Dr. Wallis. We do want to move rather quickly.

MR. SOLORIO: Dr. Wallis, Tom has three slides and the remaining presentation is 13 slides. So --

VICE CHAIRMAN WALLIS: Okay. We know all this, don't we? Do we need to look at these slides? Well, maybe we do? I don't know. The guidance doesn't address --

MR. SOLORIO: Operator problem.

MR. HAFERA: My name is Tom Hafera. I work in Plant Systems Branch. And we seem to be getting a lot of questions that are kind of expanded on the sort of -- that are maybe not as well founded in what actually happens during a LOCA.

So I want to go over that real quickly with everybody. What I have here is a slide that shows basically a plan view of a pressurized water reactor. It shows a LOCA in progress. There's the zone of influence there. There shows debris and how it's going to be transported.

This is the sump --

MEMBER APOSTOLAKIS: Where is the break?

MR. HAFERA: The break? The break would be approximately right here.

MEMBER APOSTOLAKIS: Okay.

MR. HAFERA: Okay? There is the sump, the little red box, okay?

VICE CHAIRMAN WALLIS: It's a tiny thing.

MR. HAFERA: It is a little tiny thing. Here is your containment basement. There's a plan of the containment basement. Containment basements are typically about 130 feet in diameter. So it's about the size of One White Flint North, okay?

Here's the sump. There's --

VICE CHAIRMAN WALLIS: How big is the sump?

MR. HAFERA: -- the sump right there. The sump itself is typically around about 10 to 12 feet square so it's about a 10 feet by 10 feet --

VICE CHAIRMAN WALLIS: So that strainer --

MR. HAFERA: -- sump.

VICE CHAIRMAN WALLIS: -- is a tiny thing.

MR. HAFERA: So it's a tiny thing, that's correct.

VICE CHAIRMAN WALLIS: And it's going to have --

MR. HAFERA: So it is --

VICE CHAIRMAN WALLIS: -- 53 pickup --

MR. HAFERA: -- well, see this is --

VICE CHAIRMAN WALLIS: -- loads of fiberglass in it?

MR. HAFERA: -- in deference to Mr. Andreycheck, I think he used --

VICE CHAIRMAN WALLIS: I don't think --

MR. HAFERA: -- his words were a little bit exaggerated to achieve the shock effect that he wanted. So -- and that's what we're going to try to address, okay?

VICE CHAIRMAN WALLIS: So you're -- but he's from industry and he's from Westinghouse. He ought to know what he's talking about.

MR. HAFERA: That's correct. And I'm an ex-operator and I should know what I'm talking about, okay?

So there is the sump. And it's about 10 or 12 feet in diameter -- or 10 or 12 feet square. It shows a water level. That's a standard sunken sump. There are also sump designs that are not sunk --

VICE CHAIRMAN WALLIS: It's 10 foot square?

MR. HAFERA: Basically.

VICE CHAIRMAN WALLIS: It's 100 square foot on a floor level so 5,000 cubic feet of debris would be 50 feet high in that box?

MR. HAFERA: Well, obviously you can't get 50 foot high in the box. The --

VICE CHAIRMAN WALLIS: That's what --

MR. HAFERA: -- box is only 10 feet deep.

VICE CHAIRMAN WALLIS: -- but, you see, that's the sort --

MR. HAFERA: Okay?

VICE CHAIRMAN WALLIS: -- of thing we're up against it seems to me.

MR. HAFERA: Okay, so --

VICE CHAIRMAN WALLIS: Okay, so you're going to explain?

MR. HAFERA: I just want to explain so everybody understand, you know, this is the basic layout.

And this is the fundamental things that we're looking for, things that we are going to look for is how debris is moved around the containment and transported, how it gets generated, how it ends up getting in the little sump.

Notice how the arrows show the tortuous path and there's many hold up places -- opportunities to hold up debris, particularly large debris. And notice that the sumps typically have multiple layers.

The one that we're really worried about is this strainer here. So that's why you'll hear a lot of us we talk about mainly we talk about small finds and not so much large debris because large debris typically gets caught up in these obstructions or it gets caught up in trash racks.

MEMBER KRESS: But what is that arrow that bypasses the strainer?

MR. HAFERA: The arrow that what?

MEMBER KRESS: Bypasses the strainer. No, over to the right?

MR. HAFERA: This one?

MEMBER KRESS: Yes, no that one, yes.

MR. HAFERA: Well, this is just showing an alternative design. A lot of other plants --

MEMBER KRESS: Oh, I see.

MR. HAFERA: -- have them come out the side.

MEMBER KRESS: I see, okay.

VICE CHAIRMAN WALLIS: So you're giving us the impression that not much of the fibrous debris gets to the sump?

MR. HAFERA: Hang on. Let me go forward.

VICE CHAIRMAN WALLIS: Okay, let's --

MR. HAFERA: My second slide, let's talk about large break LOCAs and just how a large break LOCA progresses and what the fundamental numbers are.

I want you -- first of all, I have to say, this is from a MELCOR code. MELCOR is a realistic code. It's not a design-based code. So therefore each plant is going to have different numbers than these from a design basis standpoints. And this will not match.

The other thing is these are bulk average conditions. This is not plant specific. This doesn't model any specific plant.

Okay, we have three phases. In a pressurized water reactor, there are three phases to a LOCA. There's a blowdown phase, an injection phase, and a recirculation phase. Boiling water reactors don't have an injection phase. They go straight from a blowdown phase to a recirc phase.

You have to understand that our initial

conditions, the reactor coolant system pressure is 2,250, 530 degrees. Containment is basically zero pounds. And approximately about 110 degrees. That's our starting point.

Our LOCA is a very short term but a very violent event. It occurs in about 45 seconds, okay? Typically, your containment -- but the other thing to recognize is it's also a cool down event. Your reactor coolant system cools down rapidly. Your pressure goes down rapidly.

Within 45 seconds, you are well below high-pressure injection. You are below low-pressure injection system capacity. You're also within -- shortly after that 45 seconds, you're going to get to cold shutdown conditions where you have the opportunity from an operational perspective to maybe start throttling back on flows or doing some operational things that could help mitigate this problem.

You also recognize approximately 45 seconds in, your break flow slows significantly. You no longer have large expanded jets. You have very short, very slow, low flows. And the quality is basically single phase 45 seconds into the event. Very short.

Containment pressure peak at about -- for this example, 36 pounds in 20 second. Shortly after that, it starts to come back down fairly rapidly. And a lot of that has to do with whatever the plant's containment spray system set point is, what their ideal generator start time is because those are all sequenced as part of their safety injection operation.

Containment temperature, you don't get to 500 degrees in containment --

VICE CHAIRMAN WALLIS: Are you going to give --

MR. HAFERA: -- it's gets to --

VICE CHAIRMAN WALLIS: -- us a lecture --

MR. HAFERA: -- about 300 degrees --

VICE CHAIRMAN WALLIS: -- I'm sorry, I'm sorry, are you going to give us a lecture on LOCA or are you going to talk about the issues of --

MR. HAFERA: I'm going to tie this in on my next slide.

VICE CHAIRMAN WALLIS: I'm sorry. Okay.

MR. HAFERA: My next slide, okay?

So you only get to about 300 -- 300 peak -- 300 degrees peak. And then it begins to slow.

So the other thing a lot of that temperature transient is so fast, a lot of that heat doesn't get to translate and conduct to a lot of the structural materials in the large components in containment. They end up equalizing at a fairly low temperature, fairly rapidly.

Okay, after the violent event, the injection phase begins. Now this, again, this, for a pressurized water reactor, they are pumping cold, clean water from a refueling water storage tank that is typically very large and its design basis typically to make sure you get enough water on the containment basement to make sure you have adequate NPSH.

So -- and notice this injection phase lasts a fairly significant amount of time. Twenty-seven minutes -- that gives a lot of opportunity as this containment basement -- go back to my previous slide -- basically -- so this little sump fills almost instantly, it's so rapidly filled.

And once that fills, after -- even while the LOCA is going on, there's no velocity towards the sump. The velocity is random, randomly distributed throughout the 130 foot containment basement so --

VICE CHAIRMAN WALLIS: Where does --

MR. HAFERA: -- debris gets --

VICE CHAIRMAN WALLIS: -- where does the water go? It all goes --

MR. HAFERA: It goes to the basement.

VICE CHAIRMAN WALLIS: It makes a pool in the basement.

MR. HAFERA: It goes to the pool in the basement. It ends up -- it goes up and then it comes down and it goes to the basement.

And then it's just randomly going around the basement. There's --

VICE CHAIRMAN WALLIS: So you --

MR. HAFERA: -- random --

VICE CHAIRMAN WALLIS: -- said the --

MR. HAFERA: -- turbulence.

VICE CHAIRMAN WALLIS: -- sump fills almost at once.

MR. HAFERA: Right.

VICE CHAIRMAN WALLIS: And the question might be with what?

MR. HAFERA: With water. With water. Okay, back to my -- so where was I -- so that just goes to show, this is what you're pumping in. Safety injection, spray flow -- again, it's all clean, chemically-treated water, cold water. So that's your initial source, your initial source.

And as I mentioned, containment pressure, by the time now, as we go through the injection phase and we start to get close to the recirculation phase, these are the important parameters that come up down here.

VICE CHAIRMAN WALLIS: The pool is the pool on the floor of the building, not the pool in the sump. The sump is full. And that's the pool on the floor.

MR. HAFERA: Exactly, exactly. So there are some key parameters there. When you go to initiate recirculation, this example shows seven pounds, seven pounds in containment. Saturation temperature for seven pounds is about 230 degrees. So, again, the pool temperature at 187 is significantly sub-cooled at that point. That's a key point to remember.

The other key point to remember is, again, pool depth. Now what we have heard is some plants may not necessarily be meeting their pool depth. And that is going to be a big concern.

VICE CHAIRMAN WALLIS: Could you tell me at 187 degrees what the NPSH has to be?

MR. HAFERA: Yes, I will.

VICE CHAIRMAN WALLIS: How many feet of water?

MR. HAFERA: My next slide --

VICE CHAIRMAN WALLIS: Thank you.

MR. HAFERA: -- my next slide, okay?

MR. JOHNSON: Why don't you --

MR. HAFERA: Well, let me just finish this last point, okay? Now I forgot what my last point was.

(Laughter.)

MR. HAFERA: Oh, three and a half feet. Pool depth is very important because pool depth translates directly to turbulence or laminar flow. The deeper the pool, the more laminar and quiescent the flow is --

VICE CHAIRMAN WALLIS: Can we avoid --

MR. HAFERA: -- particularly near the --

VICE CHAIRMAN WALLIS: -- that? I'm --

MR. HAFERA: -- floor.

VICE CHAIRMAN WALLIS: -- sorry, could we avoid qualitative statements please because the guidance gives quantitative methods.

MR. HAFERA: Well --

VICE CHAIRMAN WALLIS: And just talking about things doesn't really help address these --

MR. HAFERA: Okay.

VICE CHAIRMAN WALLIS: -- these methods. So, you know, I like what you -- you're helping us get a perspective but --

MR. HAFERA: That's correct.

VICE CHAIRMAN WALLIS: -- vague statements about there's a lot of turbulence doesn't mean anything unless it's quantified.

MR. HAFERA: And now we're going to tie it to how it effects the sump. My next slide please.

Now, again, this is an example exercising our methodology in the safety evaluation --

VICE CHAIRMAN WALLIS: How did you assume 10,000 square feet -- cubic feet where Mr. Andreycheck gets 14,000 from one steam generator?

MR. HAFERA: I can't speak for Mr. Andreycheck. All I can speak of -- for is our data came from our parametric study for a typical Westinghouse four-loop plant.

VICE CHAIRMAN WALLIS: Okay. And he --

MR. HAFERA: Those were the -- that's --

VICE CHAIRMAN WALLIS: -- from Westinghouse?

VICE CHAIRMAN WALLIS: -- well, that's the data that we got from Westinghouse, okay? From four-loop dry -- as I mentioned we've got our data. There's Westinghouse four loops, three loops, two loops, there's ice condensers, there's sub-atmospheric containments, there's BMW, there's CE plants.

We came up -- the Westinghouse four-loop plant is what we feel is the limiting plant on a large, dry containment, 10,000 cubic feet of -- and we're assuming all the insulation on the steam generator is fiber.

MEMBER SIEBER: Yes, and that's just one loop because the loops are compartmentalized.

MR. HAFERA: Yes. Well, we're figuring 10,000 total.

MEMBER SIEBER: Yes, but you're only -- the zone of influence only effects on loop.

MR. HAFERA: Exactly. Well, what I assumed here, okay, is I assumed -- and if you look at my first slide, everybody has that picture --

MEMBER SIEBER: Don't go back.

MR. HAFERA: All right. Everybody has the picture. I assumed at the first slide, it shows the zone of influence encompassed 90 percent of the steam generator --

MEMBER SIEBER: Right.

MR. HAFERA: -- and one-quarter of the remainder of containment because the containment is compartmentalized.

MEMBER SIEBER: Right.

MR. HAFERA: So that's how I came up with .9 and .25.

VICE CHAIRMAN WALLIS: The .9 times --

MEMBER SIEBER: So this isn't that much different than the Westinghouse --

VICE CHAIRMAN WALLIS: Oh, it's very different --

MEMBER SIEBER: -- statement of --

VICE CHAIRMAN WALLIS: -- the .9 times --

MR. HAFERA: Okay --

VICE CHAIRMAN WALLIS: -- 1,300 is 1,170.

MR. HAFERA: -- .9 times 1,300 is 1,170, .25 --

VICE CHAIRMAN WALLIS: That's about less than a tenth --

MR. HAFERA: -- so --

VICE CHAIRMAN WALLIS: -- of what he said.

MR. HAFERA: -- again, so what I'm coming up with is about 1,720 cubic feet.

VICE CHAIRMAN WALLIS: But he said he got 14,000 from one steam generator --

MR. HAFERA: I can't --

VICE CHAIRMAN WALLIS: -- with using the zone of influence in the guidance. So you're off by a factor of 10 from him. That's all I can say. I don't know who is right.

MR. HAFERA: Exactly.

VICE CHAIRMAN WALLIS: It seems --

MR. HAFERA: Exactly.

VICE CHAIRMAN WALLIS: -- very strange to me that the guy who runs the plants or knows about the plants comes up with a number that's a factor of 10 different from you. That says something about uncertainty.

MR. HAFERA: Well, if you also recall during that subcommittee meeting, Bruce Latellier attempted to challenge in Mr. Andreychek and --

VICE CHAIRMAN WALLIS: Yes, and Mr. Andreycheck --

MR. HAFERA: -- we ran out of time.

VICE CHAIRMAN WALLIS: -- asked -- because I brought him right in front of me here.

MR. JOHNSON: But not to put too high a hat on these differences, we're not showing this because we want --

VICE CHAIRMAN WALLIS: You're showing me this because you want to --

MR. JOHNSON: -- discredit --

VICE CHAIRMAN WALLIS: -- convince of something. And don't make excuses for it.

MR. JOHNSON: We want to give you a practical perspective about how we think the evaluation comes out.

VICE CHAIRMAN WALLIS: I understand.

MR. JOHNSON: That's all that is.

VICE CHAIRMAN WALLIS: I understand, Mike. But just you've got to be straightforward. And if your numbers are very different from somebody else, that creates a quandary for us, doesn't it?

MR. JOHNSON: Yes, it does. We can back our numbers up.

CHAIRMAN BONACA: Graham?

VICE CHAIRMAN WALLIS: Yes.

MR. LATELLIER: If I may, this is Bruce Latellier from Los Alamos National Lab. The value of 1,700 cubic feet was represented about the 95th percentile of many thousands of random break locations placed around the volunteer plant piping system.

And that number of 2,000 to 2,500 cubic feet is corroborated by a number of studies for large break LOCA done earlier for the BWR study and done primarily in a manual fashion using engineering judgment.

VICE CHAIRMAN WALLIS: Thank you. So you have some support there?

MR. LATELLIER: Yes.

VICE CHAIRMAN WALLIS: Thank you.

MR. HAFERA: If our use our methodology, I'm going to go through this fairly quickly because we do have a lot more to go through, this basically shows fractional values of what the 1,720 -- what happens to it, how much of it becomes small finds, how much of it becomes large pieces, how they're transported up into containment, washed back down, transported to active pools, inactive pools, and eventually end up on the sump screen.

VICE CHAIRMAN WALLIS: Excuse me. You're only talking about the fiberglass insulation on the steam generator? You're not talking about coatings?

MR. HAFERA: That's correct. Because this is just a simplified approach to show how our method works.

Using this, I come out with -- and assuming a 100 square foot screen, which is a representative number --

VICE CHAIRMAN WALLIS: How uncertain are things like these 90 percent goes to the upper level and 10 percent goes to the lower level? Are these just somebody's estimate?

MR. HAFERA: Those are approximate number that are in -- there are actual more accurate numbers in the SE, but I used the approximate value --

MEMBER SIEBER: Are these numbers --

VICE CHAIRMAN WALLIS: Okay.

MR. LATELLIER: Bruce Latellier, once again, those branching fractions, those transport fractions are based on containment blowdown calculations. And we've made the engineering approximation that the debris follows the proportion of the fluid flow primarily.

We've done these calcs to confirm that the velocities are high enough to actually effectively transport debris of this size. And it is, where necessary, where possible I should say, it is supported by experimental evidence generated during the BWR resolution for the entrapment on gradings, washdown through gradings due to containment spray.

So we tried at every opportunity to use defensible data for the branching fractions for transport analysis. Where that is not available, we use conservative estimates.

VICE CHAIRMAN WALLIS: So about a third of it gets to the screen? Something like that? These estimates. And if there is uncertainty, it could be a half or something like that? So it's a significant amount is the message I got --

MR. HAFERA: So it's a significant --

VICE CHAIRMAN WALLIS: -- from all this.

MR. HAFERA: -- amount. The bottom line is it is a significant amount. We show 60 depth --

VICE CHAIRMAN WALLIS: Right.

MR. HAFERA: -- which, again, would not quite fill the sump but pretty close to filling that sump back on my first slide.

VICE CHAIRMAN WALLIS: Right.

MR. HAFERA: If we use our correlation and, again, there's a lot of assumptions and I'm using ballpark numbers, I get a head loss of about 10 to 17 --

VICE CHAIRMAN WALLIS: I'm going to challenge that. I think it's a very important issue. In the guidance, you accept that homogeneously mixing the product caused them the fibrous is conservative.

And yet I read Los Alamos' report, and I listen to Bruce, and I'm told that a thin layer of the particles depositing on top of the fiberglass can create a far bigger pressure drop.

So, you know, if you get a thin layer of fiberglass which then filters out the particles like a filter in a chemical plant, and you get a filter cake of the particles, you have an entirely different problem than if you're going to distribute these particles uniformly through this great mass of fiber.

MR. HAFERA: That's correct.

VICE CHAIRMAN WALLIS: Isn't that true?

MR. HAFERA: And we did say that we're going to talk about --

VICE CHAIRMAN WALLIS: And if I take that 300 pounds, it's a lot more than what I was waving around earlier --

MR. HAFERA: But again --

VICE CHAIRMAN WALLIS: -- this cal-sil.

MR. HAFERA: -- what that boils down to as we get down here to our bottom line --

VICE CHAIRMAN WALLIS: But you don't convince me at all with this 10 to 17 feet. You've put assumptions in there which seem to be incompatible with what I'm learning about thin bed effects. And I learn more every day as I read more about it. It doesn't -- you know, it's not convincing to me.

MR. HAFERA: Okay. Well, again, we're going to discuss thin bed effects later. And that's just something --

MEMBER ROSEN: Well, could I --

MR. HAFERA: -- that has to be considered.

MEMBER ROSEN: -- ask a specific question about the row that says with 100 square foot screen, small finds only --

MR. HAFERA: Yes.

MEMBER ROSEN: -- yields an approximate depth of six feet. Why do you think only small finds will get in there when a fairly significant fraction of the large pieces are transported? Are they not?

MR. HAFERA: Well, this shows -- and basically, again, I rounded off the value that was in the SE, but about 35 percent of the large find -- of large pieces will get there.

MEMBER ROSEN: So why don't you think --

MR. HAFERA: I didn't include them just for the sake of this example. I didn't include coatings, I didn't include concrete dust, I didn't include a lot of things. This is just a representative --

MEMBER ROSEN: Well, this --

MR. HAFERA: -- example.

MEMBER ROSEN: -- is a very unchallenging example is what you've chosen.

MR. HAFERA: Okay.

MEMBER ROSEN: It's an example where --

MR. HAFERA: But this is basically what

Mr. Andreycheck presented at the subcommittee.

MR. SOLORIO: And I think, Tom, what you started by saying when you started your presentation, we're just trying to show that it can be exercised, I guess.

MR. HAFERA: Right.

VICE CHAIRMAN WALLIS: Well, it think it's very revealing. You've got six feet of debris. Now as I understand it, the tests that have been made have involved an eighth of an inch of debris, and an inch, and so focus on very thin layers of debris. And we're going to take that knowledge base and extrapolate to the six feet thick of debris.

We'd better be damn sure that we understand what's going on if we're going to extrapolate it like that.

MEMBER KRESS: Well, I think your result tells you that that's not an acceptable result.

MR. SOLORIO: Right.

MR. HAFERA: That's right.

MEMBER KRESS: So you're not really going to use that --

MR. HAFERA: The bottom line is --

MEMBER KRESS: -- number. It's something that has to be done.

MR. HAFERA: We didn't get there.

MEMBER KRESS: Right.

MR. HAFERA: The bottom line is most plants, you know, a head loss of 10 to 17 feet, most plants only have a margin of two to five --

VICE CHAIRMAN WALLIS: Yes, even with --

MR. HAFERA: -- so they can't live with this. So what's that telling them? That tells them that they have to go do some type of design change. And they're going to have to do some type of remediation of that --

VICE CHAIRMAN WALLIS: Now isn't this true --

MR. HAFERA: -- concern.

VICE CHAIRMAN WALLIS: -- that almost all plants are going to reach this conclusion?

MR. HAFERA: What we've determined is most likely most of them will.

VICE CHAIRMAN WALLIS: Yes, and so --

MR. HAFERA: Most of them will.

VICE CHAIRMAN WALLIS: -- what's important is to work on the fix --

MR. HAFERA: Exactly.

VICE CHAIRMAN WALLIS: -- obviously. Not all this analytical material.

MR. HAFERA: Right. But as a condition, we can't go fix people's sump.

MEMBER SIEBER: You need to know the --

VICE CHAIRMAN WALLIS: You need to know things ---

MEMBER SIEBER: -- overall results --

VICE CHAIRMAN WALLIS: -- but you --

MEMBER SIEBER: -- to know whether --

VICE CHAIRMAN WALLIS: -- already know an awful lot. You already know an awful lot about the problem.

MR. HAFERA: Okay. That --

VICE CHAIRMAN WALLIS: It would seem clear to me that people have got to be working hard on the fix.

MR. JOHNSON: That was my opening -- that was one of my opening points. That was number two of my opening points.

MR. HAFERA: Exactly.

MEMBER SIEBER: Was that the analysis? You don't know whether the fix is any good or not?

MR. JOHNSON: That was Number One of my opening.

CHAIRMAN BONACA: Now you're going to take us through some of the refinements, right?

VICE CHAIRMAN WALLIS: Yes, you're going to take us through some --

MR. HAFERA: Okay, so --

CHAIRMAN BONACA: So we're going to see how you're going to work on the baseline to take down to the refinements? Okay.

MR. HAFERA: Right. Again, and I don't think I'll even go over this too much. There are plants out there that are all RMI plants so, therefore, they don't have Nukon. And they wouldn't get this large volume.

But basically what it shows is latent debris, of and by itself, can produce a thin layer. And the thin bed effect.

VICE CHAIRMAN WALLIS: Even with the RMI?

MEMBER SIEBER: Without any --

MR. HAFERA: Without any --

MEMBER SIEBER: -- without any insulation --

MR. HAFERA: -- without any insulation whatsoever.

MEMBER SIEBER: -- contribution.

VICE CHAIRMAN WALLIS: That's a very plant-specific thing. The plants have to --

MR. HAFERA: That's a very -- right.

VICE CHAIRMAN WALLIS: Right. Now this RMI, that's the point that we had in the subcommittee was RMI is very good for this point of view. But if some of it gets through the screen, what does it do to the pump we were asking? Maybe the pump just eats it up. But we didn't seem to know in the subcommittee meeting.

MR. HAFERA: Well, the --

VICE CHAIRMAN WALLIS: What is the downstream effect going to be?

MR. HAFERA: -- downstream effects, and again we have a presentation on that later --

VICE CHAIRMAN WALLIS: You're going to get to that, okay.

MR. HAFERA: -- but, again, that's an engineer -- that can be engineered out.

MEMBER ROSEN: Did you just leave off by mistake the head loss for the RMI latent fiber only case? I don't see it.

MR. HAFERA: Yes, I didn't go so far as to go to head loss with these two cases just to show basically what the debris bed --

MEMBER ROSEN: Well, you --

MR. HAFERA: -- thickness is.

MEMBER ROSEN: -- told us it's 10 to 17

feet for the top case.

MR. HAFERA: Right.

MEMBER ROSEN: For the RMI latent fiber, is it 10 to 17 feet also? Is it approximately the same? Or -- I mean give me some feel for it quantitatively what you would expect.

MR. HAFERA: Well --

MR. SOLORIO: Wouldn't it be less, Tom, because we're --

MR. HAFERA: Yes.

MR. SOLORIO: -- dealing with less fiber?

MR. HAFERA: It would be significantly less.

MEMBER KRESS: Unless you assume a thin bed correlation.

MR. HAFERA: Yes, sir, Dr. Kress.

CHAIRMAN BONACA: What are the operator actions you are referring to down there?

MR. HAFERA: Okay, yes, thank you. I'll get to that real quick.

VICE CHAIRMAN WALLIS: Can we have a thin bed with this latent fiber?

MEMBER KRESS: Well, that's undetermined because we haven't characterized latent fiber.

VICE CHAIRMAN WALLIS: But you say you've got 1.7 inches and we were getting thin beds with an eighth of an inch all through the document that you reviewed.

MEMBER KRESS: It certainly seems possible you could get it.

MR. HAFERA: It's possible.

VICE CHAIRMAN WALLIS: It's possible but you don't know. So the plants have to do it all -- they have to brush up all their stuff in the plant, do all their testing to find out if they can get a thin bed. Is that what you expect them to do?

MR. HAFERA: Yes. They have to evaluate --

VICE CHAIRMAN WALLIS: You want them to --

MR. HAFERA: -- their containment.

VICE CHAIRMAN WALLIS: -- you're putting an awful lot on these plants.

MR. LATELLIER: If I may add, Dr. Wallis, Bruce Latellier, we are assuming that latent fiber is capable of forming a thin bed. And that's the reason for Tom's example to show that based on a rough estimate of total latent debris inventory and the fibrous fraction that was characterized in the recent LANL study, that there is potentially a substantial amount of fiber present.

And they must assess their plant cleanliness for that --

VICE CHAIRMAN WALLIS: Thank you, Bruce.

MR. LATELLIER: -- contribution.

VICE CHAIRMAN WALLIS: That's a very good point. So it means the staff is going to get it, if they go through with all this, a whole lot of submittals from plants explaining how they use the vacuum cleaner and how they picked up all this stuff. And all the tests they did. And they'll all be different.

And you're going to somehow assess whether or not there is a thin bed when we don't quite know what a thin bed is and what causes it?

MR. HAFERA: Well, we know what a thin bed is and what causes it. And we're going --

VICE CHAIRMAN WALLIS: You don't.

MR. HAFERA: -- to present that later.

VICE CHAIRMAN WALLIS: Well, okay, maybe you can convince me.

MR. HAFERA: Okay.

MEMBER ROSEN: Now I'm going away from this chart with the idea that an RMI latent fiber only bed is significantly less than 10 to 17 feet. And to me significantly less it's a third of that or five feet or something like that, which is still very important.

MR. HAFERA: That's correct.

MEMBER ROSEN: Okay.

MR. HAFERA: That's correct. Very good point.

So, again, just practical solutions here. Practical solutions that plants could do. Double jacketing their insulation. There's a low cost, low tech solution that would really produce a large effect. It really reduces the ZOI and it will reduce that number quite significantly.

MEMBER ROSEN: Do you have a test that shows that?

MR. HAFERA: Yes, we have tests that show double jacketed insulation --

MEMBER ROSEN: Ralph, could you speak to that?

MR. HAFERA: -- that are not nearly as susceptible to damage.

MR. ARCHITZEL: We showed the subcommittee the OPG tests were done and that upped the cal-sil from around 24 pounds to like around -- somewhere around 250 or 300 pounds in offset seams on the double coverage. So it was a tremendously significant increase in destruction pressure.

MR. SOLORIO: That was Ralph Architzel.

MR. HAFERA: So that's a quick low tech method that can have a big impact.

Modifying sump screens. We heard somebody might want to use an active sump screen. We also know that there are sump screen designs that aren't susceptible to thin bed effects, stacked disks and what have you.

And there are a number of other things that can be done. Refining the zone of influence model --

VICE CHAIRMAN WALLIS: That might lead you to getting bigger --

MR. HAFERA: We're seeing that --

VICE CHAIRMAN WALLIS: -- if you refine it, it might get bigger.

MR. HAFERA: Well, the zone of influence model is not necessarily real -- it doesn't correlate real well at low pressures. So that could produce a --

VICE CHAIRMAN WALLIS: But it might --

MR. HAFERA: -- significant impact.

VICE CHAIRMAN WALLIS: -- it might grow. There's only inference if you learn more about it, it might get bigger.

MR. HAFERA: It's possible. You could add trash racks in barriers along the floors of containment.

Operator actions, operators can take high pressure injection systems out earlier, cool down the plant faster, go to shut down cooling-type recirculation faster, a lot of operator actions or --

VICE CHAIRMAN WALLIS: Now these are all the things --

MR. HAFERA: -- potentially --

VICE CHAIRMAN WALLIS: -- you think might be done? These are things you think might be done? Right? They're conjecture? These things that look like reasonable candidates for thinking about?

MR. SOLORIO: Well, we know, Dr. Wallis, from a conversation we've had with industry that they're looking at increasing their sump screen sizes, at least some contractors --

VICE CHAIRMAN WALLIS: They're looking at -- but I don't see any kind of design that says we've made all the calculations and it looks as if this thing will work. You're way a long way away from that, right?

MR. JOHNSON: That's right. Again --

VICE CHAIRMAN WALLIS: A long way from --

MR. JOHNSON: -- we've --

VICE CHAIRMAN WALLIS: -- anything --

MR. JOHNSON: -- not --

VICE CHAIRMAN WALLIS: -- that will work.

MR. JOHNSON: -- seen designs, right. That's right.

MEMBER ROSEN: You're aware of the difficulty of crediting operator actions during a LOCA like this which is very different than things that have -- operators that have been typically trained to do.

CHAIRMAN BONACA: So they are not --

MEMBER ROSEN: This is not a simple --

MR. JOHNSON: Well, I think, again --

MEMBER ROSEN: -- approach.

MR. JOHNSON: -- if you go back to the LOCA does and how it progresses and when you're on recirc and when your sump screen actually starts to show degradation, you're talking long-term into the event where you have time to plan it ahead of time.

And you have a -- your plant is already cooled down. Your containment is already depressurized. So you have a significant response time.

MEMBER ROSEN: Well, I grant that. I grant that.

VICE CHAIRMAN WALLIS: I agree with that.

MEMBER ROSEN: But I also ask you to grant the fact that the plant has just had a LOCA. This is not normal.

MR. JOHNSON: Oh, absolutely.

MEMBER ROSEN: This is not your normal day at the plant.

MR. SOLORIO: No, you're right.

MR. JOHNSON: No, it's a bad day in the control room.

VICE CHAIRMAN WALLIS: Can you give me --

MR. JOHNSON: And I've had a few.

VICE CHAIRMAN WALLIS: -- estimate, now we've been through some of these in the past, historical events where the Agency has decided that action should be taken on some major issue. And then there are various designs and they have to be approved and all.

How long does it take to implement? To go from now to doing all these calculations in the plants, to designing things, and to actually implement something, getting approval from the Agency, how long does it typically take to do something like that?

MR. JOHNSON: Well, the schedule that we have -- this is Mike Johnson, the schedule that we have published and requested in the Generic Letter has licensees completing their evaluation --

VICE CHAIRMAN WALLIS: No, no, I'm not really asking about that. I'm asking about say post-TMI, there were some changes because lessons were learned. Didn't it take quite a few years before anything substantial happened in the plant? So I'm just trying to put it in perspective.

MEMBER APOSTOLAKIS: He's not talking about just the study.

MR. JOHNSON: You mean how long does it take them to implement their changes?

VICE CHAIRMAN WALLIS: I'm looking for the solution.

MEMBER APOSTOLAKIS: The solution itself.

VICE CHAIRMAN WALLIS: If you look down the road about what steps if I were an engineer I would have to take to get to a solution, how long it would take. And I'm guessing it's something like ten years. Am I wrong?

MR. SOLORIO: Well, I don't know if I can answer TMI but, Rob Elliot, I mean how long did we take to -- or did the industry take to implement the fix for the BWRs?

MR. ARCHITZEL: We issued the Bulletin in May of `96 and all the licensees completed their modifications by the spring of `99.

VICE CHAIRMAN WALLIS: So it's three years. So there's hope.

MR. ARCHITZEL: We gave them a year to --

MEMBER SIEBER: Think about it.

VICE CHAIRMAN WALLIS: Okay.

MR. ARCHITZEL: -- do the evaluation and then told them that plants starting in the spring of the following year had to start completely hardware modifications --

VICE CHAIRMAN WALLIS: That's good. That's --

MR. ARCHITZEL: -- in their first outage.

VICE CHAIRMAN WALLIS: -- a historical precedent and we can maybe extrapolate it to this case.

MR. ARCHITZEL: And I suspect the vendors that helped with the BWRs are probably going to try and jump in on the PWRs, too. So there's probably a lot of experience there.

VICE CHAIRMAN WALLIS: Thank you.

MR. JOHNSON: 2007 is our expectation in

this case.

VICE CHAIRMAN WALLIS: Well, I don't know about what your expectation is. I'm just looking for evidence that it has happened before.

MR. HAFERA: Okay. Well, that concludes my high-level presentation. There's people who follow me to provide more details in the specific areas and hopefully get to some of the other more detailed questions.

MEMBER APOSTOLAKIS: Why did it take 25 years, Mike? You say this has been around for 25 years?

MR. JOHNSON: There's a real good history in front of the SE --

MEMBER APOSTOLAKIS: Yes, I saw that.

MR. JOHNSON: -- that talks about it.

MEMBER APOSTOLAKIS: That's too long.

MR. JOHNSON: Right. Well, we learned things at various stages. We took on a problem with the boilers. We, at that time, recognized that 50 blockage wasn't going to be good for the peaks. What we did -- and at that time, thought that we need to have this mechanistic evaluation.

We had some events that caused us to recognize that --

MEMBER APOSTOLAKIS: So it was the evidence?

MR. JOHNSON: -- that it was more of a problem. So we've learned things over that time. But we've ultimately dealt with the issues.

CHAIRMAN BONACA: Well, I mean, that's right. I mean it's 25 years of inadequate improvements. So we hope that this will be an adequate improvement. And that's the thrust of our comments, I believe.

MEMBER APOSTOLAKIS: How raised the issue, do you remember?

MR. JOHNSON: I'm sorry?

MEMBER APOSTOLAKIS: Who raised the issue? Who raised it?

MR. JOHNSON: Who raised the issue?

MEMBER APOSTOLAKIS: Yes.

MR. JOHNSON: Who raised the sump blockage issue?

MEMBER APOSTOLAKIS: Twenty-five years ago.

MR. JOHNSON: I honestly don't know the answer to that.

MEMBER APOSTOLAKIS: Okay.

MR. ARCHITZEL: The sump blockage -- Ralph

Architzel -- the sump blockage issue was raised around 1979, right around TMI time initially.

VICE CHAIRMAN WALLIS: So this was a post-TMI issue?

MR. ARCHITZEL: No, it wasn't. It was actually before TMI. It was studied for about four or five years until the `84 time frame because we have to go back to USIA 43 but it was about five years before it was resolved in `85. It might have been --

VICE CHAIRMAN WALLIS: So that's more historical information about how long it took to do something. Okay.

MR. KOWALL: Good morning. My name is Mark Kowall. I'm a reactor systems engineer in the Plant Systems Branch.

This morning I'm going to discuss section 3.3 and 4.21 of the SER. These sections deal with the break selection. And the overall process for identifying the limiting break location.

MEMBER APOSTOLAKIS: Why are you doing that? The subcommittee requested it?

MR. KOWALL: This is one of the major areas. This was one of the blocks that Dave Solorio had on his slide. I'll go through it very quickly.

Basically this section provides the

guidance and consideration for identifying the limiting break location. The criteria used to identify this location is the estimated head loss across the sump screen.

There are really two key attributes that I emphasize and those are the maximum amount of debris transported to the sump and the worst combinations of debris mixes transported to the sump. So -- and to identify this limiting break location, you are really looking at what gets to the sump.

MEMBER KRESS: And do we know how to determine what the worst combination is? Does that relate to the thin bed effect?

MR. KOWALL: That relates to the thin bed, that's right.

VICE CHAIRMAN WALLIS: And it relates to when it is transported, or how the stuff builds up, or whether you get a thin bed on top of fiberglass, or inside it, or on the bottom of it, or how well mixed they are, and all that sort of stuff?

I don't see anything in the guidance that tells you how to calculate those things.

MR. LATELLIER: Bruce Latellier. You're correct in noting that there's very little time-dependent advice on time-dependent debris bed formation given in the guidance. The limits of our ability to model transport do not -- just simply don't -- do not warrant a detailed effort in that regard.

However, there are important phases of the accident sequence that can be considered, that being the high velocities during pool fill up, the spray washdown, and finally the low velocity recirculation phase.

And if you think about those effects, the first opportunity for accumulating very large quantities of large debris only occurs in the initial phase. And depending on your sump screen configuration, for example, a horizontal arrangement below grade, that's a very credible event where you'd have a large, bulky homogenized bed.

VICE CHAIRMAN WALLIS: So you've got the large debris first.

MR. LATELLIER: That is one possibility. Alternatively, if that large bed does not form, the small suspended finds can continue to accumulate indefinitely to form the thin bed behavior that we're most concerned about.

And so there's some important separations in the accident sequence that allow us to think about what are reasonable bed configurations.

VICE CHAIRMAN WALLIS: That was very helpful, Bruce. And as I'm sitting here, I'm thinking about how a beaver builds a dam, he puts the twigs in first, he puts the large debris in first. And gets a structure, which is your fiberglass.

And then he puts the mud on which is your cal-sil or whatever. He builds himself a thin layer. And he stops the water going through.

MEMBER ROSEN: Do we have a contract with him?

(Laughter.)

VICE CHAIRMAN WALLIS: I believe they have beavers at MIT.

But, you see, this is the kind of thing that occurs to me. And I don't see anything in the guidance that tells you how to calculate those things.

These are all sort of the beginnings of understanding of these things. And you're doing a great job. You guys are working very hard. It's just a question of whether or not you're ready. Okay.

MR. KOWALL: The section also provides considerations on the piping systems that need to be considered, and break size. Basically all RCS piping and attached piping.

And also secondary side breaks if they're part of the licensing basis and rely on recirculation must be considered.

All phases of the accident scenario are considered. This is an overall process. It's a number of iterations for identifying the limiting break location.

Then section 4.2.1 provided or proposed the application of Branch Technical Position MEB 3-1 for break locations to consider.

Next slide.

VICE CHAIRMAN WALLIS: Can I ask you about this? This I have a real problem with. And I asked at the subcommittee.

It says no guidance for plants that can substantiate no thin fiber layer. So if they don't have a thin bed effect, there's no guidance for them.

MR. KOWALL: Well --

VICE CHAIRMAN WALLIS: So they're finished and they can't use the guidance.

If there is a thin bed effect, they're likely to be finished because they can't get the water through. So how do they escape from this Catch 22?

MR. KOWALL: One of the -- I guess we talked about this at the subcommittee meeting. One of the examples of this was in the coatings area with the assumptions on the particulate size for the coatings, working toward -- or with thin bed, if a plant can substantiate they do not have a thin bed, the staff has enhanced the --

VICE CHAIRMAN WALLIS: Well, how can they substantiate they don't have a thin bed? Thin beds sort of occur by luck. When you do an experiment -- you do a lot of experiments and then gee whiz, we've got a thin bed here. And it explains some anomalous results. It's not something which is part of the technical knowledge.

So how on Earth are these folks going --

MR. KOWALL: They may not --

VICE CHAIRMAN WALLIS: -- to establish --

MR. KOWALL: -- that's true --

VICE CHAIRMAN WALLIS: -- that they don't have a thin bed?

MR. JOHNSON: We talked about it at the subcommittee -- Mike Johnson.

VICE CHAIRMAN WALLIS: Yes, but we're still talking about it because you haven't resolved it.

MR. JOHNSON: Well, what we said was we really don't believe that there are going to be licensees who substantiate no thin bed. What we were doing was looking at the guidance

VICE CHAIRMAN WALLIS: You don't --

MR. JOHNSON: -- to make --

VICE CHAIRMAN WALLIS: -- believe --

MR. JOHNSON: -- sure that in --

VICE CHAIRMAN WALLIS: -- that they will?

MR. JOHNSON: What we were doing is making sure the guidance would handle that eventuality should a plant come in an try to substantiate no thin bed, how then would they implement the guidance? And so that's what we're taking care of in this case.

MEMBER SIEBER: How would they substantiate no thin bed?

MR. ARCHITZEL: Let me just point out something here. It's not necessarily on the existing designs but an all RMI plant, the idea was an all RMI plant, perhaps with a modified design, with no fiber in the plant except for the latent, with a modified screen size, using the criteria we had in the guidance report of the one-eighth inch could distribute that over the one-eighths inch and demonstrate that --

VICE CHAIRMAN WALLIS: But we know one-eighth doesn't mean anything any more. We know from Bruce Latellier's very clear explanation a thin bed can occur anywhere on any layer. It doesn't have to be an eighth of an inch.

MR. ARCHITZEL: No, no. I'm saying the total fiber that is existing. So it doesn't matter.

VICE CHAIRMAN WALLIS: Well, if they don't have as much as an eighth of an inch?

MR. ARCHITZEL: Over the modified square --

VICE CHAIRMAN WALLIS: Well, there is another statement in your guidance that says cal-sil can form a layer with no fibers at all.

MR. ARCHITZEL: Well, the plant may not have cal-sil but the point is there are plants that could do that calculation and demonstrate they don't have a thin bed.

The other point is the plants could put in modified strainer designs that are not susceptible to the thin bed effect. There are two ways you get that.

MEMBER SIEBER: That's the only choice as I see it because you can get a thin bed out of latent fiber with no --

MR. ARCHITZEL: But not necessarily --

MEMBER SIEBER: -- RMI unless the screen is huge in size.

MR. ARCHITZEL: Well, that's the point. If the screen is 500 square feet, depending on your latent, you might get -- or 800 or 1,000 --

MEMBER SIEBER: That's pretty tough --

MR. ARCHITZEL: -- developing -- depending on the geometry --

MEMBER SIEBER: That's pretty tough --

MR. ARCHITZEL: -- of the screen design also.

MEMBER SIEBER: -- to do that in some of these containers.

MR. ARCHITZEL: It depends on the latent debris term if you're an all RMI plant. There is a possibility that the condition exists is the only point we're making so we have a provision for that.

The reason for that comment is if you have that condition, where you have the modified -- the real reason, the additional one, if you had a design fix that is not susceptible to thin bed --

VICE CHAIRMAN WALLIS: Guidance only applies to plants that do have a thin bed effect. So now you're saying that almost all plants are going to have this thin bed effect.

MEMBER SIEBER: I think so.

VICE CHAIRMAN WALLIS: You say almost none are going to substantiate they don't have it.

MR. ARCHITZEL: There would be a lot that would substantiate they don't have thin bed because --

VICE CHAIRMAN WALLIS: There would be?

MR. ARCHITZEL: -- because of the fix.

VICE CHAIRMAN WALLIS: After the fix?

MR. JOHNSON: After the fix.

VICE CHAIRMAN WALLIS: But now, you're asking to assess now what's the state of it now?

MR. ARCHITZEL: Probably most of them couldn't justify now --

VICE CHAIRMAN WALLIS: We will find that they all have thin beds now. Is that what we're going to find?

MEMBER KRESS: Is there a substantial database to back up your statement that some screens are not so susceptible to thin bed effects? And I presume these are the corrugated screens?

MEMBER SIEBER: Or vertical screens.

MEMBER KRESS: Vertical corrugated?

MR. ARCHITZEL: Disk strainers, et cetera, and the testing was done. I mean that's the testing that was used for the BWRs in those propriety screens.

MEMBER KRESS: That testing exists?

MR. ARCHITZEL: Yes.

MEMBER KRESS: Okay.

MR. KOWALL: And as a result of this

discussion from the subcommittee meeting, the staff did add Appendix 8 to the Safety Evaluation Report that discusses the thin bed.

VICE CHAIRMAN WALLIS: Yes, I have read Appendix 8. And it describes some effects.

MR. KOWALL: Yes, it gives examples --

VICE CHAIRMAN WALLIS: It describes some effects.

MR. KOWALL: -- of thin bed. It gives examples of where this has occurred, events --

VICE CHAIRMAN WALLIS: It doesn't give me a clear recipe for predicting things. It describes all of the effects. It's very useful for saying this is the state of knowledge.

But if I were to try to use it to develop design criteria and to evaluate my plant, I think I'd have a lot of trouble.

MR. KOWALL: The second exception the staff took to section 3.3 was with respect to the secondary break locations. The guidance report proposed that secondary side break locations be analyzed consistent with the current licensing basis.

The staff's position on this is that the secondary side breaks should be analyzed consistent with RCS piping, LOCA piping. And the basis for this is that the current licensing basis does not consider all the issues and concerns associated with this GSI-191.

Even though the secondary side analyses are not performed in accordance with 50.46, to demonstrate acceptance criteria of 50.46, if the sump is relied on to mitigate the consequences of secondary side breaks, then licensees should identify limiting locations and ensure that their sump will perform its intended function.

And this is consistent with the staff's position in Reg Guide 1.82. It doesn't specifically distinguish between -- okay.

Additionally, the staff concluded that it's not appropriate to evaluate only locations consistent with Branch Technical Position MEB 3-1.

We concluded this for a number of reasons. It's not consistent with the requirements of 50.46. The staff previously rejected this for the BWRs. Not consistent with Reg Guide 1.82 considerations. And this would also apply to secondary side breaks.

VICE CHAIRMAN WALLIS: Okay. Move on.

MR. ARCHITZEL: My name is Ralph Architzel. And I'll discuss the debris generation section. I'll try to do it shortly. I'd like to make one additional discussion on the other areas.

The guidance report uses the zone of influence approach. This is what the industry has proposed founded in ANSI 58.2, Free Jet Expansion Model.

VICE CHAIRMAN WALLIS: And you are perfectly happy with the model that's in ANSI?

MR. ARCHITZEL: We have written Appendix I. We've modified Appendix I. It was proposed by the industry. And we feel there are deficiencies associated with that. There are theory deficiencies. Overall when you take that model, we consider it conservative from a regulatory perspective.

VICE CHAIRMAN WALLIS: Did you look -- did anyone look at the original document on which it is based, the ANSI model? Did they find that the conical pressure distribution is simply assumed?

MR. ARCHITZEL: I'm not sure. We went --

VICE CHAIRMAN WALLIS: Things like that? I mean --

MR. ARCHITZEL: -- back to --

VICE CHAIRMAN WALLIS: -- did anyone critical examine the basis of this model? Did anyone critical examine knowledge about what happens in supersonic flows? Or you just accept it? You accept it as -- I mean I could see accepting a standard. I mean it sounds authoritative.

MR. ARCHITZEL: We did do a critical look at that standard. And that is Appendix 1. And we'll move on to that.

VICE CHAIRMAN WALLIS: I have to read that again because I think it's changed some more since --

MR. ARCHITZEL: We did -- we last night sent you another revision --

VICE CHAIRMAN WALLIS: -- that's another revision --

MR. ARCHITZEL: -- of three --

VICE CHAIRMAN WALLIS: -- last night, fine.

MR. ARCHITZEL: -- pages additional --

VICE CHAIRMAN WALLIS: That makes it difficult for me to assess it.

MR. ARCHITZEL: I'm sorry?

VICE CHAIRMAN WALLIS: It makes me difficult to assess something you sent me last night.

MR. ARCHITZEL: That wasn't the thought behind -- I mean we tried to address the comments --

(Laughter.)

MR. ARCHITZEL: -- you made earlier trying to clarify what we feel are deficiencies relative to the physics for that model. But if we step back from it and ignore -- yes --

VICE CHAIRMAN WALLIS: So you've now gone to what we suggested you do some time ago. You've actually gone to examine whether the model is good and what its deficiencies might be. You're beginning to do that? Is that so?

MR. ARCHITZEL: Well, we're accepting the use of the model still.

VICE CHAIRMAN WALLIS: But now you're examining its deficiencies, having accepted it?

MR. ARCHITZEL: For the application with the precision we're talking about.

VICE CHAIRMAN WALLIS: So you bought the car and now you're looking at what's wrong with it?

MR. LATELLIER: If I may add, the use of the ANSI model was proposed by the industry.

VICE CHAIRMAN WALLIS: Yes.

MR. LATELLIER: Based somewhat on the recommendation of Reg Guide 1.82, that the staff found it to be an acceptable method. So, therefore, it is incumbent on the staff to be totally comfortable and convinced that it is appropriate for the use.

VICE CHAIRMAN WALLIS: Let's say -- this clearly -- in all of this, a lot of education going on. You guys are learning. Every time I hear anything from you, there's new knowledge, new appreciation. That is the way it's going. I appreciate that. That's very good.

But, you know, the question is whether, since you're in this great learning process about these phenomena, you can make decisions based on things which you may learn tomorrow are not appropriate quite the way you thought they were.

That's what I'm concerned about. You're in that learning process now and yet you are trying to make decisions based on things which you have trouble coming to grips with.

MR. ARCHITZEL: Well, I guess I'd phrase the zone of influence situation as so conservative in terms of what has been proposed --

VICE CHAIRMAN WALLIS: How do you know it's conservative?

MR. ARCHITZEL: Conservative relative to the CFD examples that were proposed by the BWRs and it's modeled on the destruction pressures as they are measured. And then the assumption of anything with an equivalent sphere being totally destroyed is where the conservatism

VICE CHAIRMAN WALLIS: I agree. You

certainly have put some conservatisms in it, yes.

MR. ARCHITZEL: So that aspect seems to cover anything --

VICE CHAIRMAN WALLIS: And you have added --

MR. ARCHITZEL: -- from shock wave versus being due to pressure and it's not really pressure, it's really shock, the way we treat it and the way it's transformed into an equivalent volume sphere throws a tremendous conservatism into --

VICE CHAIRMAN WALLIS: So the --

MR. ARCHITZEL: -- this analysis.

VICE CHAIRMAN WALLIS: -- initial shock wave that no one had analyzed, is that --

MR. ARCHITZEL: Well, we saw on the shock wave, we saw how it is a near term effect. And it can go far. But basically we've never really resolved whether the damage is caused by shock or caused by the pressure or the mass flow into the damaged targets. But we accept it.

VICE CHAIRMAN WALLIS: You have never really resolved those issues? But you've made some judgment about what's an acceptable damage pressure?

MR. ARCHITZEL: Yes.

MR. LATELLIER: It was never the intent of the staff's experimental programs to develop a first principles model of the damage mechanism. It's based on empirical evidence of damage at given spacial locations within the jet as correlated by various metrics that can be modeled.

For example, the stagnation pressure or in the case of the ANSI model, an impingement pressure that's arrived at by averaging the mass flux on a large target.

VICE CHAIRMAN WALLIS: Well, again, we don't have time to go into all that.

MEMBER RANSOM: Has there ever been any agreement in what they even mean by impingement pressure?

MR. ARCHITZEL: Well, we heard you. And we put some additional words in destruction pressure. We're talking the same thing. It's that measured pressure at that face of that --

VICE CHAIRMAN WALLIS: But with the state of the art where you send me letters the night before explaining things better, it seems to me this could happen tomorrow, too, because it's a learning process. And I appreciate that. You're doing a good job there.

I just wonder if you don't have more things to learn.

MR. ARCHITZEL: Well, I'd like to get through this section as quickly --

VICE CHAIRMAN WALLIS: Okay. I'm sorry, I'm sorry, Ralph. You have to do it.

MR. ARCHITZEL: Yes, as I mentioned before, we did use the -- transformed those freely expanded jets into sphere. And that's a significant conservatism of the approach, equivalent volume spheres.

VICE CHAIRMAN WALLIS: Isn't that -- oh, I'm sorry. Every time you say conservative, I'm going to say what. But I'm sorry. We don't have time.

MR. ARCHITZEL: Yes, there could be long distances --

VICE CHAIRMAN WALLIS: There could be long distance --

MR. ARCHITZEL: Okay.

VICE CHAIRMAN WALLIS: -- you know, it does --

MR. ARCHITZEL: Under refinements proposed by industry, I'd like to say that they did offer the direct impingement refinement where the models don't resize the jets. And we're accepting that. So they can use those jets.

They used the debris-specific destruction zones, which is probably what most plants will do anyway, as opposed to the lowest damage pressure.

They allowed -- they proposed simplification to an entire compartment. And we're accepting that.

The other section we have on debris generation is the characteristics, which have been provided for construction, pressure, density, size, and size distribution.

Next slide please. Regarding the safety evaluation, we considered the guidance report approach acceptable. And we have noted some modifications.

VICE CHAIRMAN WALLIS: Now the presentation that your colleague made where you had a number that was a tenth of what the Westinghouse men had, was that based on this 40 percent? Or based on the NEI guidance before you had modified it?

MR. ARCHITZEL: I think the example we intended to use was using the destruction pressure that would be six pounds. I don't know how complicated --

VICE CHAIRMAN WALLIS: Was it with your modification? Or was it the NEI guidance?

MR. ARCHITZEL: It should have been the six pounds.

VICE CHAIRMAN WALLIS: Was the ten pounds used or the six pounds?

MR. ARCHITZEL: It was just a gross estimate because we got --

VICE CHAIRMAN WALLIS: A gross estimate really isn't very good on this problem is it?

MR. ARCHITZEL: It was the compartment use. What we did was essentially use the compartment model.

VICE CHAIRMAN WALLIS: Did you use your modified destruction pressure or the NEI guidance destruction pressure in arriving at 1,720 pounds of cubic feet of debris?

MR. LATELLIER: It corresponds roughly to the modified damage pressure.

VICE CHAIRMAN WALLIS: Because Mr. Andreychek or whatever, it's garbled in the transcript. And a much bigger zone of influence, you emphasized that if you used your modified 40 percent. That's why I'm bringing it up here.

MR. ARCHITZEL: Well, I think if you had an open containment --

VICE CHAIRMAN WALLIS: You're making the zone of influence bigger with this 40 percent.

MR. ARCHITZEL: The 40 percent increases

the zone of influence. As the industry pointed out, it can triple it, okay, because --

VICE CHAIRMAN WALLIS: It can triple it?

MR. ARCHITZEL: Triple the zone of influence.

VICE CHAIRMAN WALLIS: Triple the zone of influence.

MR. ARCHITZEL: Depending on the way you're going through the model. And the reason is --

VICE CHAIRMAN WALLIS: Is that three times the amount of debris? Or not?

MR. ARCHITZEL: It should because those are the assumptions if there's that much when you go out to that additional volume, you're limited by the compartment. That's what I was saying earlier.

VICE CHAIRMAN WALLIS: So this factor of three, which could be said to be sort of an uncertainty that's you're compensating for is bigger than these uncertainties about how much stuff gets -- or about the same as the fraction that gets to the screen of all the debris you created? So --

MR. ARCHITZEL: Certainly big than the jet -- well, and some of that can be done away by industry can test for that effect. And they can also get more robust material. Now one --

VICE CHAIRMAN WALLIS: Well, these factors of three and all that are all right if you're dealing with a problem where you can show that you get 0.1 feet of water on the screen. But you've got five feet of water head loss on the screen and it's just about to begin to challenge your NPSH, factors of three begin to make a big difference. I mean it could be 15 or it could be two. It makes all the difference in the world to whether or not your pump --

MR. ARCHITZEL: The input to an analysis the licensees don't --

VICE CHAIRMAN WALLIS: George was right about uncertainties. Of course, George is right on many things. But he was certainly right to focus on uncertainties in this issue.

MEMBER APOSTOLAKIS: I see you are in a good mood today.

(Laughter.)

MR. ARCHITZEL: Well, one thing we did along those lines will be, as Dr. Apostolakis mentioned, in the Appendix R we offer alternatives like with this non-physical aspect of the NC model it grows unbounded ways as you go to low pressures. We do have a discussion where you can use empirical data to drop that down. So it's available if industry wants it. But we haven't approved that. It's very conservative.

You can increase the destruction pressure the conservatisms harness. There are ways to address that factor of three you can test. So there are things you can do about it. But it's usable now. And well it's approvable, I guess, is the way to put it.

I think perhaps I'm done.

VICE CHAIRMAN WALLIS: Coatings, coatings, coatings, please.

Now you have 10,000 square feet typically of coatings in a plant, several mils thick.

MR. ARCHITZEL: I think it's 300,000 square feet.

VICE CHAIRMAN WALLIS: Three hundred thousand?

MR. ARCHITZEL: Somewhere in there.

VICE CHAIRMAN WALLIS: Three hundred thousand square feet? Thank you. I thought it was 10,000. Where did I get 10,000.

MR. ARCHITZEL: I think that was --

PARTICIPANT: That's the debris.

VICE CHAIRMAN WALLIS: That's the ZOI.

MR. MURPHY: Excuse me. This is Mark Murphy. Ten thousand square feet was the amount of unqualified coatings that was volunteered by industry.

VICE CHAIRMAN WALLIS: So there are 300,000 in the plant.

MR. MURPHY: That's an approximate number.

VICE CHAIRMAN WALLIS: And you're going to have a zone of -- so you're asking them to increase the zone of influence because of uncertainties to 10D, ten times down to the pipe. That's about as big as the zone of influence for these other destructions, right?

MR. ARCHITZEL: Say it's one-fourth the containment of --

VICE CHAIRMAN WALLIS: Now we're talking about tens of thousands of square foot of coatings which is several mils thick.

MS. LAURETTA: This is Angie Lauretta from Plant Systems Branch, NRR. We're not asking them to use the 10D. That is a default value. There is a lack of data in this area.

VICE CHAIRMAN WALLIS: That's right. So you're --

MS. LAURETTA: We have no basis for --

VICE CHAIRMAN WALLIS: -- with no basis, they have to use 10D.

MS. LAURETTA: No, that is not --

VICE CHAIRMAN WALLIS: What do --

MS. LAURETTA: -- what the SER says.

VICE CHAIRMAN WALLIS: -- they use?

MS. LAURETTA: They need to come in with the justification for whatever value they use.

VICE CHAIRMAN WALLIS: Oh, I thought you had 10D. Where did 10D go?

MS. LAURETTA: That is an option.

VICE CHAIRMAN WALLIS: It is a default value. Well, okay. If it's a default value, it's essentially what you would accept. And they have to justify anything else.

MS. LAURETTA: That is the only value we have --

VICE CHAIRMAN WALLIS: Okay, it's the only value I have to go on, too, because I haven't done any calculations, okay? So 10D is a big thing.

MS. LAURETTA: That is a --

VICE CHAIRMAN WALLIS: It's like the --

MS. LAURETTA: -- conservative --

VICE CHAIRMAN WALLIS: -- zone of influence we saw for the other debris. You've got 300,000 square feet of debris, several mils thick. It doesn't take much mass to show that you can build up a thin bed on almost anything, any size screen out there.

Coatings I understand you're asking them to assume are broken up to the grain size of the individual stuff that went into the paint.

MR. ARCHITZEL: Actually I'd like to correct that.

VICE CHAIRMAN WALLIS: They're very small.

MR. ARCHITZEL: We're not asking that at all or proposed that.

VICE CHAIRMAN WALLIS: Where did that come from?

MR. ARCHITZEL: And we're accepting that.

VICE CHAIRMAN WALLIS: Oh, you're accepting that? Okay, we'll accepting or asking for, it's the same thing to me.

MR. ARCHITZEL: Well, we might ask for something different. And we could have a distribution that was used.

VICE CHAIRMAN WALLIS: Well, don't prevaricate on me. I'm sorry.

MR. ARCHITZEL: Okay.

VICE CHAIRMAN WALLIS: But look, you've got 300,000, you've got 10,000, you've got very simple math to show that if there is a thin bed of this stuff, which is fine particulate, you're going to have trouble.

And your decision to say you've now got to use 10D or something like that rather than the 1,000 psi which they recommended has a profound effect on this problem, this part of the problem. And there seems to be no basis of understanding about what coatings do to put filtration in on the bed.

So you're taking, well, I know Petrangelo said that it's a big step into the dark in another context but this seems to be like another one of those, isn't it? I'm trying to help you to clarify where you are. I'm not trying to criticize you guys.

I just want to bring out where I think you are in this problem.

MR. ARCHITZEL: I believe the staff does recognize that there is data here we simply don't -- we don't have a defensible basis for either 1,000 psi damage pressure or a 10 psi damage pressure.

I think it's rather misleading for the industry to say that the staff has increased the size by three orders of magnitude when, in fact, it should be 100 percent based on the knowledge we have today.

VICE CHAIRMAN WALLIS: We should assume all the coatings go to the screen?

MR. LATELLIER: There is no evidence to

support otherwise. That's why the staff is not --

VICE CHAIRMAN WALLIS: Well, why does the --

MR. LATELLIER: -- endorsing --

VICE CHAIRMAN WALLIS: -- staff not --

MR. LATELLIER: -- the --

VICE CHAIRMAN WALLIS: -- say that?

MR. LATELLIER: -- 1,000 psi damage contour.

VICE CHAIRMAN WALLIS: Well, why does the staff not then say that we're going to be conservative since we know nothing and it all goes to the screen.

MR. ARCHITZEL: I'd like to correct that. I mean we have a tremendously conservative alternate position so we're not --

VICE CHAIRMAN WALLIS: What can be more conservative --

MR. ARCHITZEL: -- going to --

VICE CHAIRMAN WALLIS: -- than saying they --

MR. ARCHITZEL: -- it's not like steam blowing breaks have not never happened in plants. They've had steam blowing, they've had water breaks, we know that all the coatings don't come off.

VICE CHAIRMAN WALLIS: But he just said --

MR. ARCHITZEL: So we're not going to --

VICE CHAIRMAN WALLIS: -- said you should assume that it all comes off because we don't know enough.

MR. ARCHITZEL: I'm not agreeing with that. We have some --

VICE CHAIRMAN WALLIS: You see but okay -- so we've now established that there is internal debate among the staff and its consultants about what they know about these problems. Have we not? You disagree with him.

MR. ARCHITZEL: Angie was the reviewer in this area. Let me back out of it, okay. Angie Lauretta.

MS. LAURETTA: This is Angie Lauretta. We are very much aware that there is a lack of data in this area. That is why in the SER we have asked that licensees come in with justified values based on experimental data or have provided a default value. That is the only value we are able to justify proposing. There --

VICE CHAIRMAN WALLIS: So let me put that in perspective. You guys do this on so many things. You put it on the licensees. Now is each individual licensee going to develop a technical base which is bigger than you folks have with your consultants? Each one of them? Or is it going to be some industry consortium that's going to establish all these knowledge bases which you don't have?

MS. LAURETTA: It's our responsibility to review the guidance that was proposed by the industry. It is our expectation that when they come in with a proposal that they are able to justify what they proposed.

VICE CHAIRMAN WALLIS: But if you -- I'm sorry --

MS. LAURETTA: We were not able to do that in this --

VICE CHAIRMAN WALLIS: -- but if you know --

MS. LAURETTA: -- case.

VICE CHAIRMAN WALLIS: -- nothing about it, how can you evaluate what they propose?

MS. LAURETTA: They don't know anything about it either.

VICE CHAIRMAN WALLIS: Well, that is a very profound statement. Thank you.

(Laughter.)

MR. ARCHITZEL: I think with the time we've argued about this, the next topic --

PARTICIPANT: Let's move on.

MR. ARCHITZEL: I'd like to -- at this point, generally we accepted the characteristics of debris that's in the guidance report. There were some modifications of particulates. And then as far as the ACRS questions, I think I've already talked about the -- we've revised -- we visited the destruction pressure definition. We've changed this. Dr. Wallis noticed Appendix I with its additional explanations.

VICE CHAIRMAN WALLIS: Is it going to change tomorrow night?

MR. ARCHITZEL: We're done changing Appendix I. We got them all unless we get additional comments.

We may do a clean up on the definition of destruction pressures through the document because that was the point you made. But it's not going to be anything other than editorial clean up on that -- additional cleanup.

On the paint chip size, that was a question that was raised by the ACRS for the no thin bed analysis. We decided that -- we placed a requirement that the paint chip size should be the size of the screen openings. And that's what is in the SE right now for that situation.

VICE CHAIRMAN WALLIS: That's bad. And you're assuming that paint chips come which could actually cover the screen?

MR. ARCHITZEL: We haven't done a --

VICE CHAIRMAN WALLIS: That's like leaves on the drain in a street?

MR. ARCHITZEL: No, actually what we have -- we had that discussion --

VICE CHAIRMAN WALLIS: Is that an analogy of paint chips on this screen are like leaves --

MR. ARCHITZEL: -- if you did it that way and you --

VICE CHAIRMAN WALLIS: -- like leaves on a drain in the street, you've seen what they do to a drain on the street?

MR. ARCHITZEL: Right, the point is --

VICE CHAIRMAN WALLIS: The drain has bars like a screen and a few leaves have to be dug off by somebody coming by.

MR. ARCHITZEL: There's two ways to look at it. Either you could look at it as a surface coverage-type effect like the latent debris and the placards. Or you could look at it like a correlation problem.

We have been actively revising the

treatment in the SE to say with paint chips under this condition, you could look at transport. They're heavy. They don't necessarily transport. It's not in the version you saw last time.

So we're trying to be practical because if we did take your approach, you're right with 100,000 square feet -- licensees aren't going to build 100,000 foot screens but then you can say how does it transport? How is the head loss? You have to do an intelligent look at the head loss associated with paint chips which isn't a coverage thing. It's more how does it build up, it's own particulate. There is a need to examine that.

And I guess that's end of my part of the presentation.

VICE CHAIRMAN WALLIS: All right. Thank you very much, Ralph.

MS. LAURETTA: I'd like to add something. I'd like to add that the reason we have decided to go forward with this is because the 10D we have proposed is something we have confidence in as a conservative default. That's what --

VICE CHAIRMAN WALLIS: Why do you have confidence --

MS. LAURETTA: -- enables us --

VICE CHAIRMAN WALLIS: -- why do you have confidence in it as a conservative default. Why isn't it 20D? Or 50? Or 100? Or 9.6?

MS. LAURETTA: Precedent.

VICE CHAIRMAN WALLIS: Possible?

MS. LAURETTA: Precedent.

VICE CHAIRMAN WALLIS: Precedent? You mean you've made this --

MS. LAURETTA: Well, it was done --

VICE CHAIRMAN WALLIS: -- guess before?

MS. LAURETTA: The staff has established the position with the BWRs and we are standing behind what was done --

VICE CHAIRMAN WALLIS: And do you know --

MS. LAURETTA: -- and accepted --

VICE CHAIRMAN WALLIS: -- what the basis of that decision was? Why do you have this supreme confidence that it is conservative?

MR. LATELLIER: If I can address some of the history, I believe that the industry was very proactive in offering the pressure wash data that they have provided in Appendix A of the GR. This was a high-pressure impingement environment unfortunately that did not address relevant temperature ranges.

And there is a continuing debate about the effect of both temperature and rapid temperature transients on the effects of coating and possible delamination. For that reason, the staff is not comfortable in endorsing the 1,000 psi damage contour proposed by the industry.

However, we have considered that information as approaching the relevant conditions that we're interested in. And we don't want to be -- to impose an undue penalty by assuming 100 percent failure.

However, we are also recognizing that there is very little data to provide a defensible basis for either side of this issue. And essentially we are asking for that information to be provided either by individual licensees or by an industry consortium, which has been the typical mode of practice in the past.

VICE CHAIRMAN WALLIS: Now, Bruce, you're almost writing my review for me. You've said there's very little data to provide a defensible basis. I'm tending to reach that feeling myself.

MR. LATELLIER: You would simply be emphasizing our concerns.

VICE CHAIRMAN WALLIS: But when you say that and someone else says we're sure something is conservative, I don't understand the logic.

MR. LATELLIER: As I've said, we've tried to give due consideration to the information that has been provided. That the pressure regimes are relevant, the temperatures are not.

VICE CHAIRMAN WALLIS: I understand you're doing the best you can with what you have. And that's very appropriate. There's got to be a logical thread in the argument if you just follow this by a layperson and someone who isn't as knowledgeable about it as you are.

MR. MURPHY: This is Martin Murphy. I also want to point out that qualified coatings are tested at pressures and temperatures. And, therefore, it does give us confidence that coatings outside the zone of influence will be able to stay adhered in the event of an accident.

VICE CHAIRMAN WALLIS: These are the qualified coatings which have been inspected and all that sort of thing?

MR. MURPHY: That's correct.

VICE CHAIRMAN WALLIS: Okay. Let's move on.

MR. WAGAGE: Good morning. My name is Hanry Wagage. I'm going to present to you the staff evaluation of debris transport section of the guidance.

I recognize that we are pressed for time. I'll quickly go through the presentation unless you have questions.

VICE CHAIRMAN WALLIS: Well, this conservative -- here we've got the word conservative again. And I see 60 percent here, 15 percent there, 70 percent there. Is --

MR. WAGAGE: I will --

VICE CHAIRMAN WALLIS: -- this someone's feeling that they are conservative values? Or are they -- again, Bruce said there's some sort of basis. So I guess I'll leave it alone. Let's go on.

MR. WAGAGE: If I answer that question, what we did was to use the baseline guidance and detailed analysis to calculate for the volunteer plant. Then we compared the results and then we decided by going through detailed analyses this --

VICE CHAIRMAN WALLIS: It seems to me that the staff and its management should get together and rigorously say what steps we're going to go through in order to make the statement that something is or is not conservative because this word is used so loosely that I don't know what you mean. Maybe you do but --

MR. WAGAGE: What I mean by conservative is that it gives a worse condition than the realistic conditions.

VICE CHAIRMAN WALLIS: The worst? Worse than what's realistic? Well, let's go --

MR. WAGAGE: Realistic.

VICE CHAIRMAN WALLIS: -- on. I just made my statement. Let's move on.

CHAIRMAN BONACA: This is for the baseline calculation.

MR. WAGAGE: Yes. For the -- these are the key points of the baseline guidance --

CHAIRMAN BONACA: Yes, I understand.

VICE CHAIRMAN WALLIS: -- on debris transport. This methodology is based on NUREG/CR-6762 log tree. The objective of this methodology is to calculate the conservative higher mass of debris going onto the sump screen.

We discuss different transport mechanisms given the presentations before. It's important to remember that baseline guidance assume only small fine debris would transport onto the sump screen. Large debris would stop by grading, radiological sensors, and trash facts.

In sort of going through our detailed

analysis to get the final number, the baseline guidance uses conservative fractions to quantify the logic tree. These guidance are the two analytical refinements brought through on pool debris transport. They were --

VICE CHAIRMAN WALLIS: Could we just move on to the end of this. I mean you've said they're conservative. You did actually modify their guidance by having this 15 percent value? You only allowed them to hang out 15 percent in the pools, the remote pools or something? Why did you do that?

MR. WAGAGE: Yes, that comes in the next slide under limitations, you've got the --

VICE CHAIRMAN WALLIS: Well, maybe that's what we need to discuss --

MR. WAGAGE: -- yes --

VICE CHAIRMAN WALLIS: -- otherwise we don't need to spend much time on this?

MR. WAGAGE: Yes, actually we're talking about the relocation into --

VICE CHAIRMAN WALLIS: Your limitations.

MR. WAGAGE: -- inactive pools. The baseline guidance assumed that the fraction of debris moving into inactive pools is the fact on fraction of inactive pools and the total sump pool. Inactive pool, for example, is reactor cavity when water is stagnant, which would not participate --

VICE CHAIRMAN WALLIS: Debris gets in --

MR. WAGAGE: -- and it would not come onto the sump screen.

VICE CHAIRMAN WALLIS: That's what you need is lots of inactive pools.

MR. WAGAGE: Yes, it's good but beside it's very hard to base our analysis --

VICE CHAIRMAN WALLIS: If you could divert the debris to the inactive pools, you'd be in great shape wouldn't you?

MR. WAGAGE: That's true, yes.

VICE CHAIRMAN WALLIS: And yet you're only giving them 15 percent credit so there seems to be a chance here to do something?

MR. WAGAGE: The reason of giving 15 percent limit is that this assumption of one fraction is equal to the amount of debris moving to the inactive pools has other assumptions in all, that the debris is uniformly mixed with water. But that doesn't ever happen.

So we wanted to limit that to 15 percent. However, we let licensees come up with analysis --

VICE CHAIRMAN WALLIS: What's the basis of your 15 percent? Why wasn't it 25 or seven or zero?

MR. WAGAGE: We did the debris transport for the volunteer plant.

VICE CHAIRMAN WALLIS: You ran the computer program or something? And you said that in the end with some sort of uncertainties statistically you got 15 percent? Or you ran some sort of logical --

MR. WAGAGE: Let me just finish --

VICE CHAIRMAN WALLIS: -- validation of this 15 percent or did it come from somewhere?

MR. WAGAGE: Let me first tell you what we did. What we did was --

VICE CHAIRMAN WALLIS: Did it come from somewhere? Just tell me in about six sentences the basis of the 15 percent that's believable.

MR. WAGAGE: The basis of the 15 percent is the analysis we did for the volunteer plant using the detailed analysis and the baseline guidance. The baseline guidance gave 14 percent for the volunteer plant and we came up for the volunteer plant, it's close to 15 percent. We gave a round number of 15 percent.

We had to come up with some number. That's why --

VICE CHAIRMAN WALLIS: You had to come up with some number?

MR. WAGAGE: Some number we can base on --

VICE CHAIRMAN WALLIS: That's why you --

MR. WAGAGE: -- our basis is the volunteer plant analysis.

MR. SCHAFFER: Dr. Wallis, this is Clint Schaffer of Terry Corporation. I did a lot of the transport analysis for the volunteer plant.

And our biggest concern with that model that's in the NEI guidance was that it's not based upon real physics. And we also don't have a survey on how big the inactive pools could be for the fleet of plants out there.

So our only way of judging this was to evaluate the volunteer plant in detail, apply the baseline guidance to that plant, and compare then side by side. In doing so, it was found that if we had a 15 percent inactive pool, that was okay for this one plant.

We were concerned about where to put the limit so we just based it on that gauge. Fifteen percent was okay for the one plant analyzed in detail.

And I think we have wording that says if they can justify more, then let them do so. But we had to cap it.

CHAIRMAN BONACA: Wouldn't that number -- let me finish -- the number depend on the relative position of the breaks to the screen to the sump?

MR. SCHAFFER: It depends on a lot of factors. First of all, it depends on the compartmentalization around the break itself. Obviously if it's highly compartmentalized, you might keep a lot of debris right there in the break zone.

Also a lot of the debris gets blown into the upper reaches, which comes down at a later time.

The big concern is that the inactive pool might already be filled by the time a lot of the debris comes to the sump pool. There are so many factors involved.

MEMBER ROSEN: Well, how did you deal with the fact that the volunteer plant may, in fact, have better hold up of inactive pools then all the other plants or many other plants? It seems to me that it could be next to no hold up in some plants.

MR. SCHAFFER: Well, in the volunteer plant, the analysis illustrated something like three percent of the fibrous debris made it into the inactive pool. And when it applied to baseline, there was 14 percent.

So the baseline highly over-estimated the inactive pool fraction here but see there's other places in the models where the NEI guidance is over-conservative. So we're actually trying to balance over and under conservatisms of which you can't really quantify.

But here is one case where we could quantify it as a package.

MEMBER ROSEN: Well, that's just mumbo-jumbo to me. The idea that the volunteer plant could demonstrate about 15 percent if good. And that's one stake in the ground. But it's only a stake in the ground for that plant. And going back to saying well, you know, there's a lot of conservatism in this analysis, so some plant that really only can hold up three percent if going to have to deal with the requisite amount of debris anyway really doesn't give me a lot of comfort.

MR. LATELLIER: Dr. Rosen, this is Bruce Latellier. There is one important attribute of the volunteer plant that needs to be understood.

This particular plant has an elevated steam compartment cavity so that the sump pool is not actually able to fill a significant fraction of the sump. It has an annular pool only.

Whereas most plants, the sump pool is on the same level, the same elevation as the steam generator compartments. Therefore, the level of turbulence in this annular pool is much higher than you might expect for other cases.

And that gave us some confidence that our residual hold up fraction was bounding.

MEMBER ROSEN: Well, that's helpful.

MR. LATELLIER: It was appropriately low.

MR. WAGAGE: During our presentation to the ACRS subcommittee on thermal hydraulics two weeks ago, we had a question on debris moving into the upper containment. The subcommittee asked justification for the fraction of debris moving into the upper containment.

The justification was that when they did detailed analysis for the volunteer plant, it had less -- it had significantly higher amount of debris moving into the upper containment. The reason is that once the debris moving into the upper containment, part of that would not end up on the sump screen.

So based on our volunteer plant analysis, we accept that fraction of debris moving into the upper containment in the baseline.

Thank you.

VICE CHAIRMAN WALLIS: Now, I'm just trying to see how we're pacing the presentation here. We have some details on head loss and we have some details on downstream effects, alternate evaluation. And then there's going to be some wrap up from the staff. Is that you total presentation? Or is there another --

MEMBER SIEBER: You have to do that all in three minutes.

PARTICIPANT: That's it.

VICE CHAIRMAN WALLIS: Okay, thank you.

MR. LU: This is Shanlai Lu from Plant Systems. I'm going to cover the SUS actions, head loss section. It is an important section because this issue comes from the head loss and we're hope we're ending at the head loss section because what's automated design available, what's the exact head loss if the plant makes the modification?

The question here is how you are going to calculate the head loss across the screen with a given debris bed. And ACRS questioned the NEI document and the SER in terms of the user of NUREG/CR-6224. And especially last time, the industry asked a very simple question.

And NUREG/CR-6224 correlation is am

empirical correlation. It has been validated against test data. The temperature range for the test data is between 60 to 125. Can the industry use it beyond 125? So that's one of the issues -- major issues right now we're trying to address.

And the staff did a lot of analysis during the past two weeks. We're trying to address this issue. And the research and Bill Krotiuk did a lot of work to just come up with the basis.

At this point, the staff is comfortable to expand the application range of the temperature in terms of temperature --

VICE CHAIRMAN WALLIS: You have done more experiments in the last week?

MR. LU: No, analysis.

VICE CHAIRMAN WALLIS: Why did you extend the range to 220 when --

MR. LU: Okay, that's one thing we are trying to explain that to you. And I don't know whether we can do it within three minutes.

VICE CHAIRMAN WALLIS: You did not do any more experiments?

MR. LU: No.

VICE CHAIRMAN WALLIS: And yet you extended a data range?

MR. LU: Correct.

VICE CHAIRMAN WALLIS: How did you do that?

MR. LU: Correct. The data in question, everybody believes that for the empirical correlation, you always have to stay within the test data range. That was our position before. And then, of course, if you stay within 125 degree, you cannot make it. Nobody can really use it.

Based on Tom Hafera's presentation, you can see at least the 187 core temperature. So how can you apply this correlation? If it cannot be applied any methodology? And the answer is no at this point, okay?

So what we did, we just did -- Research did a sensitivity study trying to identify -- learning what's the physical phenomenon which would stop us from using this correlation beyond 125 degree. And when we found the limiting physical phenomenon actually is the air bubble formation seen --

VICE CHAIRMAN WALLIS: Now wait a minute, this is a new phenomenon that's never been studied before. As I understand it, you get an anomalous behavior of calcium silicate in one test at Los Alamos at this temperature of 125. You don't know why it happens. It may be due to some kind of rearrangement of the particles or some kind of way in which they interact. Who knows?

And you're going to extend that to 220 with out understanding what's going on? Are you going to say it's due to air bubble formation which is a new hypothesis?

MR. LU: Yes, that's right, that's the physical -- yes.

VICE CHAIRMAN WALLIS: Am I --

MR. KROTIUK: Dr. Wallis -- may I -- just a moment please.

My name is Bill Krotiuk. I'm with the Office of Research. What I did is that I looked at -- made an assumption that the water upstream of the screen was completely saturated with dissolved air. And then using -- and the amount of that dissolved air was -- basically came out of test data that was run around 1975.

And as a result with the pressure drop through the screen, there were two considerations. One is that the pressure downstream of the screen had to remain above the saturation temperature of the water in the pool to prevent flashing --

VICE CHAIRMAN WALLIS: To prevent loss of

NPSH for one thing.

MR. KROTIUK: Right, yes. And then the second thing was to -- the assumption was made that when you drop that pressure, that the amount of air that was dissolved would come out of solution and form a void. So that was the second criteria.

And the criteria was that the void fraction on the downstream side of the screen had to remain lower than three percent --

VICE CHAIRMAN WALLIS: But Bill, Bill, I think you've done a great job. But does it have to do with the correlation, which for flow through of a bed.

MR. KROTIUK: Right.

VICE CHAIRMAN WALLIS: I mean bubbles come out. That's a different phenomenon. Bubbles came out in the Los Alamos tests. There were a whole lot of bubbles dancing --

MR. KROTIUK: That'S correct.

VICE CHAIRMAN WALLIS: -- underneath the bed in their tests. You already have bubbles. That was never analyzed by them as causing any effect whatsoever.

MR. KROTIUK: Right.

VICE CHAIRMAN WALLIS: And the NUREG correlation doesn't say anything about bubbles. I don't want to cut you off but I don't see the relevance in the production of bubbles.

MR. KROTIUK: Could I make one other comment -- is that additionally I looked at the effect of the properties of the water, meaning the viscosity plus the density. And what happens as you increase temperature, the viscosity reduces.

And it actually, the correlation then, you know, it's directly proportional to viscosities, so the pressure drop would actually decrease. So that's the other consideration.

MEMBER SHACK: Did you do any calculations to match against an observable temperature dependence over the range for which you do have data?

MR. KROTIUK: Yes.

MEMBER SHACK: And your calculations predict that dependence?

MR. KROTIUK: Yes -- basically yes.

MR. LU: Yes, I'm going to show you plot here.

VICE CHAIRMAN WALLIS: And I thought there was an uncertainty. The cal-sil specific area had to be adjusted for each data point.

MR. LU: That's really what we need. If it's within three percent, we can tolerate that. But if it's beyond that --

VICE CHAIRMAN WALLIS: I don't understand what you're doing here. You're trying to claim that you've extended the database. You have not. You have extrapolated it to 220.

MR. LU: We can extend the application range of the --

VICE CHAIRMAN WALLIS: You've extrapolated --

MR. LU: Extrapolated.

VICE CHAIRMAN WALLIS: -- using assumptions.

MR. LU: That's right, based on analysis.

VICE CHAIRMAN WALLIS: You extrapolated an extraordinarily database.

MR. LU: In terms of coming out of the water, there's actually a lot of data there.

MEMBER ROSEN: What is the need for doing all this? It seems to me we're just talking about a high temperature test.

MR. LU: Yes.

MEMBER ROSEN: Well, that's not beyond the state of the art.

MR. LU: Yes, you are correct because the major issue right now is first, of course, the viscosity. But the viscosity drops if you have higher temperature. And so that if you do drop -- and so the test was what's the upper limit.

MEMBER ROSEN: Why don't you just do this in a loop with a higher temperature? And stop all this calculation.

MR. LU: And if we knew that it would drop, if it remains in a single phase, we don't need it to. Why? Why do we need to run a test if we know what the outcome would be?

MEMBER ROSEN: Because a lot of people don't believe that the way --

MR. LU: They don't believe they need to understand as why viscosity drops the temperature goes higher.

MEMBER ROSEN: I think we understand that.

MR. LATELLIER: If I could add one clarification. There are two important issues here when we talk about the possible effects of temperature. One, which the staff has focused on recently, is simply the behavior of water properties and its association with head loss. Those phenomenon is an explicit part of the development of the correlation.

The other aspects, which I believe Dr.

Wallis is focusing on, have to do with changes in the bed morphology, how is it packed, how does it respond to long-term immersion. There are a number of issues that may be important.

We have tried to test to look for those effects in Nukon fiberglass beds. And we have not observed them over the limited test range -- admittedly limited test range that we have.

Those effects largely fall into the category of similar to those insulation types that have not been tested. There are simply some configurations that we don't -- have not fully investigated. And that will always be true. But I'd like to keep those distinctions in mind.

VICE CHAIRMAN WALLIS: Well, I think -- I like what you say, Bruce, it's always very helpful. But what I'm hearing from this presentation seems to be extraordinary.

You have -- if you look at the tests, some of these numbers like this 880,000 three to the minus one is based on one data point of one test at one flow rate with one composition of the bed and one thickness at one temperature. You're going to extrapolate that to something?

MR. LU: That's right. Right now we're

trying to extrapolate just the temperature.

VICE CHAIRMAN WALLIS: I don't think you want to dig the hole any deeper. I mean do you want to go on with this presentation?

MR. LU: Okay, well, I think we have the basis to why we can extrapolate the application range of the correlation beyond 125. But at this point, the calculation we can provide it to you.

The next item, and I understand it's also one of the major items the subcommittee raised is about a thin bed effect and also during the subcommittee presentation and Dr. Wallis you asked for at least one page of description, a physical description.

VICE CHAIRMAN WALLIS: I was very happy to see a description in Appendix E, I think it is.

MR. LU: Yes, that's --

VICE CHAIRMAN WALLIS: A boxed in description of --

MR. LU: Exactly, that's what Clint Schaffer did during the past two weeks with the staff together. And they did Appendix --

VICE CHAIRMAN WALLIS: I would say that the cause is not yet known. It's hypothetical. But it has been observed that a thin layer of a few mils or less than a millimeter of particles, not a fibrous bed, it's really the particulates the key thing, causes a high head loss. You don't quite know why.

MR. LU: Okay.

VICE CHAIRMAN WALLIS: That is the --

MR. LU: There is one thing I want to just explain --

VICE CHAIRMAN WALLIS: And this thin layer can be any anywhere in the bed.

MR. LU: Yes.

VICE CHAIRMAN WALLIS: And there's nothing magical about an eighth of an inch of fiberglass.

MR. LU: Right.

VICE CHAIRMAN WALLIS: There's nothing magical about, you know, it being particularly thin bed. It's just that a small amount of particulates, if it gets together --

MR. LU: Right.

VICE CHAIRMAN WALLIS: -- like the mud on the beaver dam, can stop water going through.

MR. LU: Okay.

VICE CHAIRMAN WALLIS: Like the clay on the --

MEMBER SIEBER: You need some fibers.

MR. LU: You need the fiber to sustain.

VICE CHAIRMAN WALLIS: That's really the --

MR. LU: To support.

MEMBER SIEBER: You need the fibers to start it.

VICE CHAIRMAN WALLIS: But there is fiber in cal-sil and cal-sil beds have been --

MR. LATELLIER: Yes, cal-sil has its own fiber. That's the reason.

VICE CHAIRMAN WALLIS: And there's fibers in the debris on the floor of the plant and --

MR. LU: That's right. So to form a thin bed, you have to at least have two parameters there. You have to have a particulate and you have the fiber.

VICE CHAIRMAN WALLIS: You have them there all the time in any plant.

MEMBER SIEBER: Yes.

MR. LU: That's right. But you may not have --

VICE CHAIRMAN WALLIS: If you vacuum the floor and take out this and take out that --

MR. LU: Right.

VICE CHAIRMAN WALLIS: You might not have them any more.

MR. LU: That's right. That's the reason

we are saying that a thin bed effect is a very important effect. It needs to be considered in head loss calculation.

VICE CHAIRMAN WALLIS: I find this thin bed thing something like religion.

MR. LU: It's --

VICE CHAIRMAN WALLIS: You invoke it. You invoke it. But -- and there is a description. At least I've got a description of it.

MR. LU: Yes.

VICE CHAIRMAN WALLIS: But not seeing any hard-nosed explanation of it, what it is, why it is, how you predict it, what its consequences are, what its limitations are, what kinds of things create it, and what things don't, you know did -- okay.

MR. LU: But in three minutes --

VICE CHAIRMAN WALLIS: That's the first step. You've described what you think it is.

MR. LU: But I'm trying to -- actually, I'm trying to do that but I don't think in three minutes I can really explain every single detail where it goes. But there are 20 pages --

VICE CHAIRMAN WALLIS: But the bottom line is --

MR. LU: Yes.

VICE CHAIRMAN WALLIS: -- it's got to be evaluated by the plants.

MR. LU: That's right.

VICE CHAIRMAN WALLIS: That's the bottom line.

MR. LU: It needs to be evaluated because it may introduce high head loss.

VICE CHAIRMAN WALLIS: It's the effect of getting all the particles together so they make an impervious layer almost an impervious layer.

MR. LU: That's right.

VICE CHAIRMAN WALLIS: It has to be evaluated by the plants.

MR. CULLISON: Can I interrupt just a second? Graham, because I hear a couple of different things here I want to clarify.

First of all, I think that when you discussed thin bed, you're thinking about it only in terms of cases where you don't have a thick bed, that is I think that Dr. Wallis is considering the possibility of inhomogeneous beds.

And I don't -- are you considering that possibility that you might require -- assuming that there is a lot of insulation that's on the bed, and it's a thick bed, are you considering the possibility of requiring consideration of inhomogeneous beds?

MR. LU: Okay, first off, if you look at debris generation as it is right now and from the break location through the transport, you have to remember the picture, the first picture we showed of the plant. And it goes through that. The first 20 or 30 seconds, you generate all the debris and the debris starts to flow around and mix together.

It's very hard, it's very, very, hard, practically to justify, you are going to have a pure inhomogeneous bed. It's very hard. And most likely what comes to the sump screen is actually well mixed is number one.

The second, and experimentally it's impossible to generate an inhomogeneous bed. If you run a test facility, you dump the fiber first. You dump the particulate later. You are going to have that one.

But in reality, it's just -- I just cannot -- from engineering judgment side, I just cannot see how come --

VICE CHAIRMAN WALLIS: Let me give you a different constrained judgment. The particles are very mobile. They go through the screen initially.

MR. LU: Right.

VICE CHAIRMAN WALLIS: And are swept out of the -- they go through the screen. It goes through the rapture.

MR. LU: Right.

VICE CHAIRMAN WALLIS: They go all around the thing. By the time they get back to the screen, the fiberglass is there.

MR. LU: Right.

VICE CHAIRMAN WALLIS: So you filter them out on the fiberglass. Is that engineering judgment?

MR. LU: Okay, hold on. Let me just give you -- to extrapolate a little bit on that phenomenon.

When it comes in, it's not just particulate itself. It's also with some other fibers. If you do not have a raw mixture of just pure particulate, your phenomenon is credible.

But if you still do have a mixture with other fibers, you mentioned that the fiber may come later, right? And then after going through the --

VICE CHAIRMAN WALLIS: I said the particles might come later.

MR. LU: Yes, particle may go on later or your fiber will mix with that, so you may have --

VICE CHAIRMAN WALLIS: So they may come later because this -- when you start the pumps, you know, the fibers go down, then you start the pumps. The fiberglass pulls up to the screen.

MR. LU: Right.

VICE CHAIRMAN WALLIS: Now the pumps spray water into the containment, which washes down the dust.

MR. LU: Right.

VICE CHAIRMAN WALLIS: Which is fine.

MR. LU: Right.

VICE CHAIRMAN WALLIS: And it filters out on the fiberglass.

MR. LU: In that case --

VICE CHAIRMAN WALLIS: I'm just suggesting there are plenty of scenarios where you don't get a homogeneous bed.

MR. HAFERA: That disagrees with the way the scenario works. The way the scenario works, again, spray starts early. The series of sprays will automatically start. And the only time they'll start as soon as your reactor -- your containment pressure gets --

VICE CHAIRMAN WALLIS: Where do they come from? Where does the water come from?

MR. HAFERA: It comes from the refueling water storage tank. It's clean water. It's clean water. It's clean water.

VICE CHAIRMAN WALLIS: Okay.

MR. HAFERA: That's right.

VICE CHAIRMAN WALLIS: Okay, so you haven't started --

MR. HAFERA: For the first 27 minutes, it's clean water.

VICE CHAIRMAN WALLIS: Okay. But it comes later.

MR. HAFERA: Right. So then later -- and as Shanlai mentioned, so later --

VICE CHAIRMAN WALLIS: Washing down is later --

MR. HAFERA: -- you wash down your containers.

VICE CHAIRMAN WALLIS: So what comes later by wash down is not the same as what came earlier from the LOCA.

MR. HAFERA: That's correct.

VICE CHAIRMAN WALLIS: So there's a chance to have a nonuniform bed.

MR. LU: But just think about it as deeper. And the particulate may just go through the reactor system.

VICE CHAIRMAN WALLIS: But we're arguing

qualitatively about whether your fantasy is more realistic than mine because we don't have anything sure to base it on.

MR. HAFERA: But it --

VICE CHAIRMAN WALLIS: I'm not going to argue about it. Well, that's not engineering.

MR. LU: Okay, then I guess we'll get to the next point and we still can't handle that. Even though it was an inhomogeneous bed and you have a layer of particulate deposited on the fiber, the current correlation can predict the same bad effect as it has right now.

VICE CHAIRMAN WALLIS: Have you checked that the correlation predicts the thin -- I thought the correlation was fixed up whenever you got a thin bed effect so that it went through the data points.

MR. LU: Yes, yes.

VICE CHAIRMAN WALLIS: It's not predicting anything.

MR. LU: Exactly. We did not -- actually once you get a thin bed effect, it's beyond, you know, the application range. You don't need to worry about it.

VICE CHAIRMAN WALLIS: Okay. Go ahead.

MR. LU: Okay, so in terms of NCR

requirement, that's the reason we want to require the licensee to perform the calculation for post thickness and consider the thin bed after licensee will remove all the debris.

So they still have to consider the latent debris deposited on the screen and the cause of thin bed which will give significant head loss. That's the requirement in SER.

And so in terms of head loss suction, and we tried to address -- actually responded to all the subcommittee comments and these are two major issues we tried to address. And based on our analysis, we believe we can extrapolate the correlation beyond the 125 degree.

And also the thin bed has been defined in Appendix 8. And we're very detailed description --

VICE CHAIRMAN WALLIS: Just one more question. I see you're finishing up here.

MR. LU: Sure.

VICE CHAIRMAN WALLIS: So you're completely satisfied for all the basis for the correlation, all the mechanical, mechanistic-type theory that went into it, all the equations are based on something sensible, and that the data range is sufficient for you to have faith in this correlation?

MR. LU: Yes.

VICE CHAIRMAN WALLIS: Is that a true statement?

MR. LU: Yes, at this point, I think -- and it's reasonably bound the test data we have. And following the correct application procedure and there is always place we can improve. We can run more test. We can do more study.

But it's empirically later and then right now if we're talking about 36 pickup truck versus one pickup truck load of debris, and this part of uncertainty is actually -- we are using a surgeon's knife to cut the notch.

VICE CHAIRMAN WALLIS: Be careful about the words you use. I'm just giving you advice here.

When you say the correlation conservatively bounds, it's not -- it doesn't conservatively bound. If you fix up the correlation to change the coefficients so that it goes through the highest point, you know, of some very limited data, that's not really saying that the correlation conservatively bounds.

It's saying that you can fix it up to go through the highest point. But you didn't make any prediction about what was the biggest possibility.

This conservatively bounding is based on either some enormous database or some mechanistic icon to how big it can be.

MR. LU: Right.

VICE CHAIRMAN WALLIS: Just sort of making it go through the highest point of small set of experiments doesn't really conservatively bound anything.

MEMBER KRESS: Let me ask you about your extrapolation.

MR. LU: Okay.

MEMBER KRESS: You, of course, know the viscosity of water as opposed to temperature.

MR. LU: Right, sure.

MEMBER KRESS: Do you correct the correlation for that viscosity change? Or do you just assume it's --

MR. LU: The viscosity, of course, is the water property once you have a higher temperature, we are going to -- yes, we are using that realistic viscosity. It depends on temperature.

MEMBER KRESS: Then you multiply the correlation by the ratio --

MR. LU: Yes --

MEMBER KRESS: -- of the viscosity?

MR. LU: Yes. So in that regard, actually the total temperature drop -- so that's the reason that we have a strong belief and the technical basis to extrapolate.

VICE CHAIRMAN WALLIS: Okay. Thank you very much.

MR. UNIKEWICZ: Good morning.

VICE CHAIRMAN WALLIS: Good morning.

MR. UNIKEWICZ: My name is Steven Unikewicz, engineer at the Division of Engineering, Mechanical Branch.

I'm going to speak very briefly about downstream effects. Tom sort of lead us off going through the whole accident scenario. We've gone through a lot of presentations that bring us through bringing water to the face of the sump screen.

What I'm going to talk about very briefly is what happens downstream to the sump screen. Up to this point in time, a lot of the discussion has been focused on what is the fluid passing through the screen. Downstream effects is the evaluation of the CCS system and the containment spray systems downstream of the sump screens.

As the fluid passes through, it's going to have a number of different properties. It's going to have an abrasiveness to it. It may have fiber in it. It may have different constituents from latent debris. It will have a certain abrasiveness to it.

As it passes through downstream components, downstream components such as pumps, values, heat exchangers, instrument tubing, things of that nature, the effect of that --

VICE CHAIRMAN WALLIS: Maybe we can skip the whole thing because you simply say licensees have to determine all this stuff.

MR. UNIKEWICZ: That's correct.

VICE CHAIRMAN WALLIS: Well, in that case, maybe we can move onto the next presentation.

MR. UNIKEWICZ: If you so desire.

VICE CHAIRMAN WALLIS: Thank you.

MR. KOWALL: My name is Mark Kowall. This is the last presentation on the Section 6, Alternate Evaluation Methodology.

This section describes an alternate approach which includes elements which are realistic and risk-informed. This was a methodology developed jointly between industry and the staff through a series of public meetings that were held in May and June of this year.

Part of the motivation for this approach is the ongoing 10 CRF 50.46 rulemaking effort, which defines a transition break size comparable for the LOCA.

A comparable approach in GSI 191 is to define a debris generation break size to distinguish between customary and more realistic design basis analysis.

And this debris generation break size is defined as all auxiliary piping attached to the RCS and in the RCS main loop piping a break size equivalent to a double-ended rupture of a 14-inch diameter pipe.

VICE CHAIRMAN WALLIS: So there's no debris if the pipe is bigger than that?

MR. KOWALL: There is. For pipes bigger than that, we still must demonstrate mitigative capability.

VICE CHAIRMAN WALLIS: So there is still debris generation for the bigger pipes?

MR. KOWALL: That's right.

VICE CHAIRMAN WALLIS: I was just surprised by your definition. I thought the bottom line was that below 14 inches, you have to use all the conservative assumptions which are in --

MR. KOWALL: That's correct.

VICE CHAIRMAN WALLIS: -- Appendix A. That is you have a bigger pipe, you can back off on some of the conservatisms?

MR. KOWALL: The next --

MEMBER SIEBER: But that's unrealistic.

VICE CHAIRMAN WALLIS: But it's not the debris generation which is effected by the break size. It's the LOCA calculations.

MEMBER SIEBER: Right, right.

VICE CHAIRMAN WALLIS: Right. So I was surprised to debris generation --

MR. KOWALL: That's just the term we're --

VICE CHAIRMAN WALLIS: -- as the qualifier of the LOCA break size.

MR. KOWALL: -- using.

VICE CHAIRMAN WALLIS: We know what --

MR. KOWALL: It's just --

VICE CHAIRMAN WALLIS: -- you mean --

MR. KOWALL: -- terminology.

VICE CHAIRMAN WALLIS: -- but it just seems odd.

MEMBER KRESS: And the next two bullets cover exactly what you're --

VICE CHAIRMAN WALLIS: Well, the bottom line here is that you haven't changed any of this debris transport creation, clogging, and stuff, none of that is changed by any of this risk-informing.

MR. KOWALL: That's correct.

VICE CHAIRMAN WALLIS: You still have to assume mitigation. The only thing that might change is perhaps the sump temperature isn't quite the same? We don't know if that's good or bad because if sump temperature is low, there's more viscosity, there's more pressure drop.

So we're not quite sure whether that's good or bad. But the only thing you're buying is some of these environmental characteristics you might call it of the LOCA and what is the temperature/pressure history.

MR. KOWALL: Right.

VICE CHAIRMAN WALLIS: You're not changing anything about how you evaluate the situation.

MR. KOWALL: That's right. The --

CHAIRMAN BONACA: Is there a change in the zone of influence maybe?

MR. KOWALL: The zone of influence, it all relies on the baseline methodology as described so the only thing impacted here would be elements of the --

VICE CHAIRMAN WALLIS: So the effect if probably --

MR. KOWALL: -- NPSH --

VICE CHAIRMAN WALLIS: -- very small.

MR. KOWALL: -- calculation.

VICE CHAIRMAN WALLIS: The effect on the conclusion is probably very small. Unless you actually risk-inform in the way George may have indicated, you might be able to later on if you can make uncertainty analysis of all these phenomena. You haven't really changed the problem by risk-informing it.

MR. KOWALL: That's right.

VICE CHAIRMAN WALLIS: We had some hopes, I think, when we wrote our letter a month or two ago, whenever it was, that if you risk-informed all these aspects of the problem, you might learn something which would be useful and might actually have some application.

But this effort to risk-inform is having very, very little effect on anything.

MR. JOHNSON: But I don't know that that's true actually. We're also, through this risk-informed effort changing -- I tried to emphasize this -- changing the ability of licensees -- what they can do in terms of mitigation for those breaks beyond that debris generation break size.

And I think that's actually where the potential benefit is. It's in the fixes where I think this provides the opportunity, single failure, safety related, realistic or more reasonable assumptions and realistic calculations.

VICE CHAIRMAN WALLIS: So the NPSH requirements might be reduced in some way, for example? Excuse me. So something would perhaps have some effect on this. But all the stuff we've been talking about today that's in the guidance isn't really influenced.

MR. JOHNSON: In terms of the analysis?

MEMBER APOSTOLAKIS: Wasn't the objective of NUREG 1150 to represent the community's views on severe accidents? Okay, at least the U.S. community, the experts on severe accidents. So they had, you know, workshops, and this and that, trying to present what the community knew at the time about the various phenomena that could take place after core damage?

Are your results what the community of experts in this field knows right now? Or is it just Los Alamos's and yours?

MR. LATELLIER: If you're referring to the break size, is that what you're referring to?

MEMBER APOSTOLAKIS: The whole thing.

Well, I mean there are uncertainties all over the place, aren't there?

MR. JOHNSON: Well, again, understanding what we mean by this alternative evaluation and its ability to be risk informed, which is that we are identifying a break size smaller than the double-ended guillotine break of the largest pipe, we're basing that on the expert elicitation and all of that work that went into the technical basis for --

MEMBER APOSTOLAKIS: For 50.46 --

MR. JOHNSON: -- 50.46.

MEMBER APOSTOLAKIS: -- I understand that, yes. But the rest of the study, you did not have plan to do that?

MR. JOHNSON: Right.

MEMBER APOSTOLAKIS: And the question is really why not. I mean it's been 25 years.

MR. JOHNSON: Well, the answer to why not is --

MEMBER APOSTOLAKIS: Is it that expensive? I mean --

MR. JOHNSON: -- the practical answer to why not is you know all of the work, for example, with respect to 50.46, we owe the Commission a proposed rule in December and then we're in the rulemaking process.

All of that work which we're not trying to get out in front of is going to take us years. And, again, the Commission has been very clear. We don't have years to deal with this issue.

MEMBER APOSTOLAKIS: No, no, no, they are two different objectives, Mike. I mean they are there trying to risk inform the cornerstone of the activities of this Agency for 40 years. You are not trying to do that.

All I'm saying is you know we saw a lot of fractions, of things happening this way and that way, uncertainties and phenomenon, and so on, how difficult would it be to try to put some uncertainty distributions in this?

Would it be too hard? That's why you're not attempting it? Or is it something -- I mean look, it's also fine to say we haven't thought of it, we haven't had time to do it.

MR. JOHNSON: Well, again, I'm just going to tell you what I told you before. As a practical matter, we didn't have time to do it.

MEMBER APOSTOLAKIS: You didn't. Okay, fine. But do you think that would be a good idea?

VICE CHAIRMAN WALLIS: There's always a

follow-on question, right.

MR. JOHNSON: On a schedule that would not impact issuance of the SE, it's a fine thing to do. I think actually this gets done in conjunction with the 50.46 rulemaking.

MEMBER APOSTOLAKIS: Again, I wouldn't want to tie this to 50.46. That's a much longer term project.

MR. JOHNSON: I understand. But we're trying --

MEMBER APOSTOLAKIS: I mean the stuff that you have done already you can use, of course. I'm not saying don't do that.

MR. JOHNSON: Right.

VICE CHAIRMAN WALLIS: Okay. Can we move on, George? Or do you want to pursue this risk-informed part any more?

MEMBER SHACK: I just want to ask a question. As I read this, there is a difference in the zone of influence in the risk-informed model, that you're using the hemisphere based on the break size?

MR. KOWALL: For the Region 1 space, that's right. The guidance proposes the use for breaks that are partial breaks inside of the main loop piping that's right. But I think that's the only limitation.

MEMBER APOSTOLAKIS: But I wish, in general though now, I wish when the staff says risk-informing something, or uses the term, it doesn't mean just looking for operator actions or alternate means of doing something.

It seems to be risk informed means addressing the uncertainties. And the uncertainties in some problems, like this one, happen to be in the models you are using, the parameters you are using, and so on. Now that would be risk-informing this issue in my mind.

MR. HARRISON: This is Don Harrison from the PRA Branch. And I would truly agree with you. I think the use of the phrase risk informed in this application is probably a misnomer. It's really more of a traditional deterministic resolution of the issue. It's where you've got uncertainties, we put on conservatisms as best as we feel that they're conservative.

The only piece of this that really even deals with the risk is in the solution, the fixes, that whatever is proposed as a solution will have to have a certain reliability, demonstrated reliability.

That's the only piece of this that's

really risk informed. The rest of it is more of a traditional approach. Even within the traditional approach, if there was time and money and resources, you could do a best estimate approach and put in the uncertainties in the calculations.

Then we'd be arguing over is it, you know, 92 percent with what kind of distribution it is but since we only have limited data -- so it would be very -- I think from a personal standpoint, it would be very --

CHAIRMAN BONACA: Yes, okay. We need to move on. We have another presentation. We're already 15 minutes late. So we have to move --

MEMBER FORD: But isn't risk informed, Mario, important? I would like --

CHAIRMAN BONACA: I understand that. I'm only saying that this presentation right now is out of control. I'm saying we need to put some more to what we have.

VICE CHAIRMAN WALLIS: The thing is we didn't know how long industry was going to take. And we've now just been told -- or I've just heard that industry actually wants to make a fairly long presentation.

MEMBER SIEBER: Let's take a break.

VICE CHAIRMAN WALLIS: I think it's an important enough topic that we should probably hear them. I think that they have a great stake in the outcome. So we ought to hear what they have to say.

If the members will be patient and listen, we'll just keep going.

PARTICIPANT: I'll be very patient.

VICE CHAIRMAN WALLIS: Did the staff want a moment to just wrap up or do you want to wrap up after industry?

MR. JOHNSON: If I can, I'd like to wrap up after industry.

VICE CHAIRMAN WALLIS: Thank you.

CHAIRMAN BONACA: Let's do one thing then. Let's take a break right now.

PARTICIPANT: Yes, that's a good idea.

CHAIRMAN BONACA: Take a break until 11:15 and then we'll come back again for the remaining part of the presentations.

(Whereupon, the foregoing matter went off the record at 11:59 a.m. and went back on the record at 11:14 a.m.)

CHAIRMAN BONACA: Okay, let's get back into session again.

Just a brief announcement regarding the agenda. This will go to noontime so we will proceed with this issue until noontime, adjourn -- I mean recess for lunch between twelve and one and at one o'clock, we will look at ACR-700, okay?

So that's the plan. So ACR-700 presentation is moved now to 1:00 p.m.

MEMBER KRESS: And reduced to an hour.

CHAIRMAN BONACA: Yes, reduced to an hour, yes, if we can, yes. And then at two o'clock, we'll take on GSI-185.

Okay, with that, Graham?

VICE CHAIRMAN WALLIS: Thank you, Mr. Chairman.

Well, this, as I think you're all aware, is an important issue. I think it's important that we hear industry's side to it. And I'm really looking forward to hearing from John Butler. So please go ahead.

MR. BUTLER: Thank you. My name is John Butler. I'm a Project Manager at NEI.

If it's possible, I'd like to take a couple minutes either now or toward the end to give Tim Andreychek a chance to clarify the basis for his values used in the subcommittee meetings. Do you wish to do that now?

VICE CHAIRMAN WALLIS: Want to do it now or at the end?

MR. BUTLER: Now might be instructive.

VICE CHAIRMAN WALLIS: Now -- since everyone is waiting for it, now we might as well have it. I just wanted -- it's sort of an anticlimax effect here.

MR. BUTLER: It's less than an order of magnitude. We don't even worry about it.

VICE CHAIRMAN WALLIS: Now this isn't the PRA.

(Laughter.)

MR. ANDREYCHEK: My name is Tim Andreychek. I work for Westinghouse Electric.

And the basis for the numbers that I came up with, the percentage for the thermal hydraulic subcommittee were walk-down data that was performed on a once-through steam generator design.

The numbers that were presented today to the full committee were based on a volunteer plant that used a U-tube steam generator.

What I would suggest this means is that each plant with different dimensions of a steam generator are likely to have different debris loadings. And I think that's the point you need to make.

It's not that one number is any more correct than the other. One number is correct for that particular plant design.

That's all I have. Thank you.

VICE CHAIRMAN WALLIS: That's helpful. And, of course, if you're going to get a perspective on the problem, you need to know the range of these numbers, not just one number. At least we have two data points now. Thank you.

MR. BUTLER: Well, actually Tim's point kind of serves as a good lead in to one of my first points that I want to make in my slides is that this issue effects all 69 PWR plants. And each plant is unique in some aspect.

There is no easy way to group plants together. They can generally be grouped together but each is going to have its own specifics, either through the insulation materials that they use, and you can have twin plants at a site that have differences in the insulation materials that they used.

Just through years of operation, those differences will come about. Differences in the latent debris that is found through sampling techniques, differences in the containment coatings that are used, both the types of coatings that are used and the surface areas that they use -- or surfaces that are coated and percentage that is qualified versus unqualified.

And certainly in the containment designs and, you know, very much the sump designs that are used. And it is carried through to the rest of the systems, the pumps that are used are very different.

So in effect, I can't stress this enough, there are 69 different solutions to this problem. So the trouble we have or the difficulty we have with coming together with evaluation methodology is you have to somehow provide some acknowledgment that there are 69 different solutions.

And it does not allow you the luxury of being real explicit in certain areas. In some cases you have to recognize that from a practical standpoint, that simplifications are necessary that will effect some plants more than others.

VICE CHAIRMAN WALLIS: Well, John, there may be some rule for NEI to evaluate promising solutions which might apply to a significant number of plants.

And it seems to me that it is very difficult to put the onus on every plant to work out a solution. That it may well be that certain kinds of engineering solutions, which can be shown to be effective, could be worked out collectively.

And NEI might have a role in doing that rather than having everyone be on their own.

MR. BUTLER: Everyone in the end is going to be on their own. The task force that NEI has used to develop the guidance in coordination with the Westinghouse owners' group has had the participation of the major vendor groups who will be providing services to the plants in resolving this problem.

They have their own ideas. We've discussed those ideas in our meetings. There are a pretty good variety of screen designs that are being offered. There are a number of other design changes in terms of insulation change out. And some of these were mentioned in the staff presentation.

But there are also some fairly inventive changes that can be incorporated. You know, I stress this again, the reality is whether or not a particular fix is appropriate for a plant is very specific to the plant situation in terms of what their requirements are, what their time schedule is, just a number of factors.

So it's up to each plant to decide based upon the information that they have, that's provided in the guidance, and provided by the vendors, what is appropriate for them.

MEMBER ROSEN: Well, I think what is being suggested here is you help the industry with some sort of users group or interchange of information. If one plant comes up with an inventive fix on one aspect of this problem, everybody should know about it.

MR. BUTLER: Yes. And we'll continue to evaluate what's most appropriate.

Our first opportunity to do that will come up at our December workshop. And we have a session planned in which the various vendors will, in effect, be making their case for their inventiveness and their solutions.

VICE CHAIRMAN WALLIS: John, how about defining the problem? I mean invention is one thing but it seems that there is no knowledge base about effective, you know, the coatings which are reduced to the particulate level, there's no basis for evaluating the effect of that on a screen or whether it makes a thin bed and all that.

So you are suggesting that each plant

conduct an experimental program to develop this? Wouldn't it be much better if industry got together and said we need this information collectively because we all have coatings?

NEI might have a role in pulling people together to do that. Or EPRI or somebody other than just all these plants left out there on their own.

MR. BUTLER: The importance of the 6224 correlation has certainly been highlighted with the staff's draft SER. We were intending to apply that for the range of conditions that we needed to apply it for without restricting ourselves to explicitly the testing conditions that were used to support the correlation.

We felt that that was appropriate. That there were sufficient understanding of the physics of that. If we need to do additional testing in order to apply the 6224 correlation, we will have to do that.

VICE CHAIRMAN WALLIS: But I don't think it's just applying it. I think the guidance and the SER indicates that for some things like paint chips or paint debris or whatever it is, for latent debris, there really isn't some way you can just plug something in to the correlation.

Experiments haven't been done to find out any information about it. You can't plug information into something when we've got no database for that stuff.

MR. BUTLER: Well, the correlation is --

VICE CHAIRMAN WALLIS: So --

MR. BUTLER: -- applicable for -- if you have a good understanding of --

VICE CHAIRMAN WALLIS: -- but you have a faith that it's applicable to materials for which it has never been tested?

MR. BUTLER: We have faith in if you have a good understanding of the characteristics of whatever your debris is, the particulate size, the surface area, that the correlation is applicable.

VICE CHAIRMAN WALLIS: Well, look at what happened in Los Alamos. They thought they had an understanding, did an experiment, and all of a sudden, here's a test which gives you seven times the pressure drop which they thought they had -- they would have had, you know?

Obviously this requires then some more data to figure out what is going on. And the same thing could happen with any of these kinds of debris. You can't just extrapolate somebody's hypothesis or correlation to all these areas where there isn't any data.

That seems to me inappropriate even if public safety isn't the question here. Even more so when you've got people looking over your shoulder who are concerned with the credibility of all of this.

MR. BUTLER: Well, you're asking very good questions perhaps to the wrong person. I'm certainly not an expert on 6224 --

VICE CHAIRMAN WALLIS: Well, would it --

MR. BUTLER: -- correlation.

VICE CHAIRMAN WALLIS: -- surprise you if a year from now you and the Agency, just like just guessing the future, found that they had to put a lot of money -- and I'm talking about billions, into some research to really get a substantial knowledge so that you know what you're doing about this issue?

Would it surprise you if that were to happen? Because it wouldn't surprise me at the moment. Now maybe I don't know enough about this but I'm getting the impression it's a very big problem. There are an awful lot of unknowns. And that you need to know what you're doing.

Therefore, you ought to be prepared to spend some money and do some work.

MR. BUTLER: Well, to answer your

question, I've gotten past being surprised by this issue.

VICE CHAIRMAN WALLIS: Okay. So you wouldn't be surprised by anything?

MR. BUTLER: No.

VICE CHAIRMAN WALLIS: Well, I think we need to do that. We need to -- I think the job of the ACRS, among its other jobs, is to try to sort of figure out how to tell it like it is. And so what I'm trying to do in all of this is to get you folks to help us to understand it like it is.

And that may well -- the conclusion of that may well be that you've got to do some more thorough work to understand what's going on. I don't know. But that may be one of the conclusions. Okay.

MR. BUTLER: Without trying to go back and describe in detail the industry guidance, we did present to the subcommittee some details on the evaluation guidance.

I want to stress the point that our intention was to provide a set of methods -- and I've use the words deliberately from the Commission SRM, meeting SRM because it did follow along what our intention was with the guidance, to have a practical and realistically conservative set of methods that plants could apply.

The baseline methodology, we still feel strongly that it is a conservative set of methods to give you a conservative result. We may not recognize what best estimate and realistic is when we see it but we can certainly recognize it when it is conservative to the point of almost being ridiculous in some of the values that it gives you.

Our intent with the conservative baseline was to help plants decide how best to resolve the problem. Whether that is to spend their time in the effort refining the analysis to be more -- to remove some of the conservatism in that approach through a CFD analysis or through some other method, or whether there is a most cost-effective approach just to remove some problematic insulation material so that they can meet the requirements with a conservative baseline analysis, or some combination.

So our intent was to use that to guide the problem and allow plants to make the best decision that they could.

VICE CHAIRMAN WALLIS: What do you think about this approach of analysis? Some engineering is done by -- well, we know a lot about things. We use computers. We predict things. We can predict now how airplanes fly and wing design very well because we understand what is going on.

There are other areas of engineering where we make things and they work. When people first made the internal combustion engine, it worked. They knew almost nothing about combustion and heat transfer and all that. But they made something that worked.

Is this an area where really we know so little about what's going on, we've got to start testing things and seeing if they work rather than trying to analyze the problem? What's your feeling about that?

MR. BUTLER: My feeling for that is I would love to have a better understanding of a realistic scenario and how that effects recirculation. But what we're not dealing in a realistic scenario. We're dealing in design basis space.

And you're starting off with a postulated break, an instantaneous double-ended guillotine break, which you could argue is either extremely low probability or impossible to occur. And from that, it just continues to pile on some very unrealistic assumptions throughout the scenario.

It would not be instructive to try to model that to have a better understanding of what that gives you. I think it would tell you it gives you some large amount of debris. And I think we've already covered that.

We need to understand that we are in design basis space. We haven't risk informed any aspect of the current regulations in how we apply that.

So we need to assure ourselves that we meet the regulatory requirements and our hope is that we can do that without being overly conservative to the point where --

MEMBER APOSTOLAKIS: Graham, let me -- I'm trying to understand your fundamental problem here. Are you saying that we don't know enough to be able to say that what we're doing is conservative? Is that your basis thesis here?

VICE CHAIRMAN WALLIS: Well, I'm looking forward to the day when the problem is solved. And it seems to me that -- well, if you were out there and not in nuclear regulatory space at all, that you are say designing a new plant to do something, you'd do your analysis. And you'd have all sorts of uncertainties.

And because you have uncertainties in the analysis, you do a lot of build and test and try it.

I mean you never go and build a chemical plant to make something -- or very rarely would you accept maybe really crisis mode like the Manhattan Project and have built something without having build pilot plants, without having tested things, without having found out the properties of the things you're going to use.

You'd have done a whole lot of things in order to make sure that when you actually built this plant, it worked. And here we seem to be in this sort of analytical world where everything is analyzed with tremendous uncertainty.

And that's not a comfortable situation for an engineer to be in.

MEMBER APOSTOLAKIS: So you are not convinced that what they are doing is conservative?

VICE CHAIRMAN WALLIS: No, I'm not saying that at all. I'm saying that to solve the -- I can't think you can analyze the problem away. It seems to me there has to be projected solutions.

There has to be very careful planning of engineering to make sure these solutions can be assumed to be effective in some way which may well involve big tests because that's the way engineering works when you don't know enough about things to analyze the problem and make secure predictions.

It has to do with uncertainty but at a very fundamental level. You have to build things and design things. And you have to evaluate things. And I don't think that -- you know, that's what the industry is eventually going to have to do.

And that's where they need help is in figuring out with this very uncertain problem with all these aspects to it, how you can come up with any sort of believable fix and make it credible and show that it's the right thing to do.

CHAIRMAN BONACA: But let me ask you a question. Does the -- I mean one of the concerns you have, if I understand it, is the knowledge base supporting this effort is sufficient? And will the effort of developing or completing the knowledge base stop at this stage as -- I mean the plan seems to be that industry will go out now and apply this process for a baseline calculation.

And I dare say that most of them will find that they cannot meet the requirements with the baseline calculations. So they'll go through a refinement process.

Now all this will take an extended period of time. It will take months. In fact, I believe you have an objective of -- I mean a year or two before you get some results out -- will the industry and the NRC continue to develop the knowledge base to fill those gaps in this period of time? Or we'll just simply say knowledge base is what it is today and that's it? I mean we're not going to go any further.

MR. BUTLER: Well, the answer to your question is a qualified yes. Certainly with the screen designs, there are modifications to the designs that need to be applied. Specific designs that various vendors are proposing, some have been tested, testing the specific designs for various debris loadings. Some additional testing may need to be performed so that the individual resolution option designs have testing requirements.

Some have been done. Some will need to be done. There may be a need for additional testing of specific debris types that are problematic and are difficult for the plant to remove. So some of that will occur.

CHAIRMAN BONACA: I'm asking because clearly --

MR. BUTLER: The reason I'm saying it's qualified is we're on a very tight schedule. So a plant is going to have to make a decision. Can he accomplish what testing he needs to accomplish on the time that's --

VICE CHAIRMAN WALLIS: Maybe I should in answer to George's question try another analogy. I mean if you look at airplanes, I used airplanes before, Boeing, as I understand it, now has a very good base using computers for predicting how an airplane will fly if they design it.

The Wright brothers did not have that. And they had to do all kinds of things by guesswork and trial and error and so on. They developed a knowledge base. And eventually they didn't fly very far but they did get something off the ground.

I don't know how far you are with this problem. Are you at the Wright brothers' level? Or are you at the Boeing of today level?

And my feeling is you are certainly not at the Boeing of today level. And I'm not quite sure how far you are ahead of the Wright brothers in terms of really coming to grips with this problem and what you need to do with it.

And so I suspect that you cannot analyze it the way you are trying to solve it by just analysis. Something else has to be done. And part of it is knowledge base but part of it is going to be sort of gutsy, down-to-earth engineering of figuring out what to do and showing how and demonstrate it works. Isn't that the case?

I mean after they've gone through all this exercise, these 69 plants, you're going to have some meetings with management and say what do we do?

MEMBER KRESS: Well, I think the likely thing they'll find out, using the methodology, they'll find out they're not -- currently have screens big enough and they'll make them bigger. And they'll do this -- they'll fix it like the BWRs, make them corrugated and enough surface area that the methodology will predict that that surface area is in the positive suction head.

And then we're going to be stuck with this question, oh, what about thin bed effects? Because it will still be there. And they're going to -- I think I hear that there are designs for which the thin bed effect can be shown not to be there. I think it's corrugated screens.

Now the question I'm going to have when that time comes is are we sure that the generation rate, using the zone of influence, is overly conservative still, and can you show me the database that backs up the statement that you have no thin bed effect?

I think that's the way things are going to play out.

VICE CHAIRMAN WALLIS: So it's still going to be at the analytical level. They analyze all this stuff and then --

MEMBER KRESS: Oh, yes, it's strictly going to be analytical.

VICE CHAIRMAN WALLIS: -- what they have to do is --

MEMBER KRESS: The question is --

VICE CHAIRMAN WALLIS: -- what they have to do is --

MEMBER KRESS: -- is this analysis conservative?

VICE CHAIRMAN WALLIS: What they have to do is satisfy the staff then?

CHAIRMAN BONACA: I was trying to understand -- my question is what is the risk of proceeding now with a guidance that is limited, okay? And it seems the biggest risk is the one of realizing a year from now, a year and a half, that we don't know enough or even worse to go through certain modification and find that we have to modify them further. That's really the biggest risk I see.

And I wouldn't mind having that risk if I knew that the knowledge base is going to be expanded over the next year or two to the point where then we'll have also closure on some of these questions.

I'm not sure that, however, if we start on this path, we will ever have closure on some of these issues because probably the work will not be done.

MEMBER APOSTOLAKIS: Are we discussing now the overall issue? Or are we still in the presentation?

(Laughter.)

CHAIRMAN BONACA: Well, we are already through half of the remaining time for this presentation.

VICE CHAIRMAN WALLIS: Then we should move back to the presentation, George. You're very appropriate. And it is a very appropriate comment.

MEMBER APOSTOLAKIS: I would suggest that maybe the speaker should show the slides that send a message or have a point rather than describing the guidance. I mean we know what it is. I mean why you develop the model, okay, yes, sure. I mean the guidance.

But is there a place where you have -- you're making a point.

PARTICIPANT: It's the SER that we're

discussing today so --

MR. BUTLER: The point of these slides is to set up the points I'm going to make in the later slides.

CHAIRMAN BONACA: I think most members are -- well, anyway, I mean it's your presentation but --

MR. BUTLER: Let me just make one point off of this slide. We've made a number of comments about conservative and we can argue how conservative. But we also have a number of simplifications in the guidance that we don't want to lose that or at least make a point before we lose it that those simplifications are there from a practical standpoint of plants applying the guidance.

And I'll make a point in a later slide about one of these simplifications that we are apparently losing.

I did want to make the point that this guidance, the baseline guidance, the industry guidance, has been applied by a number of -- or the vendor groups that have been participating within NEI on our task force. And I am aware of calculations that are either -- are fairly close to being completed or have been completed for at least six plants.

I can only characterize these results as preliminary because they have been conducted throughout the development of the guidance. And they don't necessarily follow all the guidance explicitly. And they don't address any of the changes resulting from the draft SER.

But one thing that is common in the results is that it is showing a fairly significant and consistent increase in the screen area if that's all you do is increase the screen area.

VICE CHAIRMAN WALLIS: Can you tell us what you mean by fairly significant?

MR. BUTLER: In the range of 1,000 to 2,000 square feet.

VICE CHAIRMAN WALLIS: And some are now 12 square feet?

MR. BUTLER: Pardon me?

VICE CHAIRMAN WALLIS: Someone, I think, said the smallest one in existence is 12 square feet?

MEMBER SIEBER: No, none that small.

MR. BUTLER: I think --

MR. ANDREYCHEK: That was current -- metric that you have one 12 square feet at the low end.

VICE CHAIRMAN WALLIS: And you are saying that they have to be now several thousand square feet?

For these plants anyway?

MR. BUTLER: The results so far have been performed with no other modifications but to increase the screen area. And the results are showing --

VICE CHAIRMAN WALLIS: I think that's very helpful information. It gives us some idea of the --

MEMBER APOSTOLAKIS: So this --

VICE CHAIRMAN WALLIS: -- consequences.

MEMBER APOSTOLAKIS: -- is impractical? Is that what you're saying? It's impractical to do this?

MR. BUTLER: No, no.

MEMBER APOSTOLAKIS: No?

MR. BUTLER: Again, there are 69 different plants. Some plants can accommodate -- have designs that can accommodate fairly large increases in screen areas. Others are more limited in the screen area they can accommodate.

MEMBER APOSTOLAKIS: So they will do what? They will go back to --

MR. BUTLER: They will have to make modifications to their --

MEMBER APOSTOLAKIS: Somewhere else.

MR. BUTLER: -- debris generation.

VICE CHAIRMAN WALLIS: And some of them

might have to build a different sump or something? Or build something on to the containment to handle the debris?

MEMBER SIEBER: No, I think --

VICE CHAIRMAN WALLIS: There are all --

MEMBER SIEBER: -- that would be --

VICE CHAIRMAN WALLIS: -- sorts of things you might think of.

MR. BUTLER: Again, my first point in the presentation is there are 69 different resolutions to this problem. Each plant --

VICE CHAIRMAN WALLIS: But if they can't --

MR. BUTLER: -- has its own --

VICE CHAIRMAN WALLIS: -- fit it -- if they can't fit it into the existing sump, they're going to have to do some busting of concrete or something.

MEMBER SHACK: No change out of insulation would probably be the next step.

VICE CHAIRMAN WALLIS: All right, insulation, that's the other thing.

CHAIRMAN BONACA: Or even, you know, manage debris throughout the containment with different barriers and things of that kind. Localize the debris so that --

MEMBER APOSTOLAKIS: So what do you mean by manage, Mario?

CHAIRMAN BONACA: By manage I mean is that so you don't have transport of all the debris down --

MEMBER APOSTOLAKIS: No, I understand the consequences. But what does management of the debris mean? I mean what can they do now to manage that?

CHAIRMAN BONACA: I'm talking about placing within containment probably barriers of some kind or --

MEMBER SIEBER: Insulation.

CHAIRMAN BONACA: -- screens.

MEMBER APOSTOLAKIS: So they're physical --

CHAIRMAN BONACA: Physical means --

MEMBER APOSTOLAKIS: -- modifications.

CHAIRMAN BONACA: -- so that you reduce the amount of debris that will come to the sump by block it in different locations in the containment.

MEMBER KRESS: Go from 50 percent to 30 percent? You can't get much help that way.

VICE CHAIRMAN WALLIS: So only about 40 percent of the plants have cal-sil? I understand about 40 percent PWRs have cal-sil insulation in them somewhere?

MR. BUTLER: I have not heard that figure.

VICE CHAIRMAN WALLIS: That's the number that we found out at one of our meetings, I think. Do these six plants have cal-sil insulation in them?

MR. BUTLER: At least one of the did.

VICE CHAIRMAN WALLIS: Did they have to face this thin bed business in their analysis?

MR. BUTLER: Yes, they all --

VICE CHAIRMAN WALLIS: They all have thin bed problems?

MR. BUTLER: -- they all calculate thin bed and --

VICE CHAIRMAN WALLIS: But -- and then -- so they know how to do that?

MR. BUTLER: Certainly, yes. I mean --

VICE CHAIRMAN WALLIS: You do?

MEMBER KRESS: Again, you have a thousand, several thousand square feet did not talk about thin bed, I'll bet.

MEMBER SIEBER: Yes.

MR. CULLISON: It solves, again, the industry's perception of thin bed is not a multi-layered thick. It's just the thin bed. And when they go to large areas, they solve not only the thick bed but they also solve -- I mean the thin bed exists but with a larger area, with their calculations --

MEMBER SIEBER: So the drop is less?

MR. CULLISON: -- they drop -- the pressure drop --

VICE CHAIRMAN WALLIS: So they get less than a millimeter of cal-sil or something? Or what do they get?

MR. BUTLER: I'm not an expert so I would preface my remarks with saying that. But one of the consequences of significantly increasing the area is you number one decrease the approach velocity which directly impacts the head loss. And obviously with increasing the area, you're minimizing the impact of the large debris loads because you're spreading it out over a larger area.

But the approach velocity is the dominant effect on the thin bed effect, head loss --

VICE CHAIRMAN WALLIS: Well, we don't know yet. But if you look at the Los Alamos database, if you're talking about the same thing I think you're talking about, is some sort of anomalous increase in pressure drop. And it looks as if the particles are somehow getting closer together.

This increases as you increase the

velocity in all those tests. So if you get down to a velocity of less than .1 feet a second, based on that database, there might be some hope that you wouldn't have this effect at all. I just don't know.

MEMBER SIEBER: Well, even if you don't, you know the lower the velocity, the lower the head loss. And so the larger the screen you make, whether you have a thin bed or not, the lower the pressure drop and the higher the NPSH will be.

And so even if you get a thin bed that's uniformly deposited that does exhibit a pressure drop at those very low flows, those large screens, the NPSH loss is de minimus.

And so that's really what the advantage is. It's not trying to avoid making the thin bed because of the low velocities. It's the low velocities that cause the pressure drop to be very low. And so there's, to me, that's where the advantage of a large screen is.

MR. BUTLER: And I don't want to minimize the engineering aspect of this problem. I mean there are actual losses that are introduced by having an extremely large screen that wraps around your containment. And they have to be taken into account.

VICE CHAIRMAN WALLIS: If you really

understood the debris transport, you might be able to show that with these very low velocities, everything falls out before it gets to the screen.

MEMBER SIEBER: Right.

VICE CHAIRMAN WALLIS: Or it falls out enough so that it only covers the bottom of the screen and you don't get a uniform layer which --

MEMBER SIEBER: That's right.

VICE CHAIRMAN WALLIS: -- is extraordinarily conservative to assume a uniform layer. It's probably going to fall to the bottom of the screen. The top of the screen may be clear.

There are all kinds of ways in which things might be good.

MEMBER SIEBER: Well --

VICE CHAIRMAN WALLIS: But the thing I'm concerned about is how do you prove it?

MEMBER SIEBER: Well, the key parameter is the velocity. And that's most impacted by the screen size.

VICE CHAIRMAN WALLIS: Yes. It's the most obvious simple thing that you can do is reduce the velocity.

MEMBER SIEBER: Yes, well -- that's right. It's a continuity question.

MEMBER APOSTOLAKIS: I would suggest we let John complete his presentation. And we're just --

VICE CHAIRMAN WALLIS: Yes, well I think George --

MEMBER APOSTOLAKIS: -- taking away his time.

VICE CHAIRMAN WALLIS: -- had a good point.

MEMBER APOSTOLAKIS: I'd like to have him the chance to present what he wants to present.

MR. BUTLER: All right. I will continue. First off, we have not had a lot of time to look at the draft safety evaluation.

Unfortunately, the staff's review schedule did not offer them or us the luxury of having a lot of interaction during the review process kind of counter to the normal review process where you meet, have RAIs, and discuss things. So we are surprised by some of the actions taken in the safety evaluation.

VICE CHAIRMAN WALLIS: That's another -- I think a very important input for the committee. You haven't had this interaction and yet we're asked to sort of approve something when it appears that you may have some significant questions about it.

You are the guys who have -- or you at

industry --

MR. BUTLER: We just sent in a -- you know, we did take a few days and placed down some of our major comments. And we did provide those to the staff.

We are going to continue to review the SER and one of our major focuses of that review will be to make sure that we have a way to explain to the industry how to apply the combination of the industry guidance document and the staff's SER on how that modifies the evaluation guidance because plants -- the clock starts ticking as soon as the SER is issued. And plants will need to start using this guidance.

So we're hoping between now and the workshop that we have planned in December that we can have a good enough understanding of the SER that we can provide that guidance to utilities on how to apply it.

VICE CHAIRMAN WALLIS: So this factor of 1,000, if the SER goes through on the schedule, and you come up with some very good arguments that it should be a factor of two, is the staff going to change its position after the SER has been issued?

Are you really going to really listen to the industry? And if there is a really good argument that you've made a mistake in assuming a factor of 1,000, you'll go back and change it?

MR. LATELLIER: If I can speak for the staff, I think the flexibility is offered in the SE to review any information that is beneficial to the defensible reduction of conservatism. And, yes, the staff will accept that information whether it's formally implemented as a change to the document remains to be seen.

MR. JOHNSON: Michael Johnson speaking. That's true, of course. I was actually responding to talking about an earlier point that John made with my staff so I didn't really hear the question. But we, as Bruce indicates, we will -- we always would consider additional information submitted by licensees.

VICE CHAIRMAN WALLIS: But it wouldn't be good to have too many of these things that you have to adjust.

MR. JOHNSON: Well, I mean I guess, I think is the answer to your question -- but remember, keep in mind, we deal with, as John has made a great point, each of these plants is unique. We expect that. And we routinely deal with, even where we have generic guidances used, we routinely deal with a large number of unique differences where licensees have applied, to some extent, or not applied, to some extent, the guidance.

And so we deal with that as a routine.

VICE CHAIRMAN WALLIS: Yes, John.

MR. BUTLER: All right. Well, I'm trying to speed my way up through this. I made the point about simplifications in the evaluation guidance earlier. The staff's safety evaluation also has a tendency to remove some of those simplifications by requiring plants to provide plant-specific information.

The example I provide here is in recognition that for unqualified coatings, which we conservatively assume all fail and all fail in a highly transportable particle size and something that biases it toward aggravating the thin bed effect, we, for simplification's sake, assume a three mil thickness for those coatings, recognizing that there are hundreds of items inside containment that have unqualified coatings, motor, motor centers, junction boxes, all these surfaces have to be accounted for. And a simplification that is assuming a three mil thickness, we felt was an appropriate simplification.

VICE CHAIRMAN WALLIS: All these coatings come off?

MR. BUTLER: Yes.

VICE CHAIRMAN WALLIS: Some of those coatings contain materials that you probably wouldn't want in a chemical soup. They're not all the same coatings. Some of the electrical coatings contain materials like chloride -- chlorine or lead or something or other, whatever it is, which, I understand, you don't particularly don't want to see in the chemical soup that get in the sump, you're going to put all those coatings in the sump?

MR. BUTLER: Yes. Unqualified coatings, they are assumed to fail.

MEMBER SIEBER: I think if you are using the sump, that you need not worry about the chemical effects of chlorides on stainless steel because you aren't going to use the plant after that I don't think.

MEMBER FORD: I think Graham is talking about the formation of gels.

MEMBER SIEBER: Well, that's a different matter.

MEMBER FORD: Sure.

MR. BUTLER: Continuing, the --

VICE CHAIRMAN WALLIS: I guess I just want to be sure that when we do these chemical tests, we evaluate chemistry, we put in if it's relevant, make it compatible with this model for the coatings.

I didn't know they were going to consider electrical coatings and all kinds of other coatings. I think that --

MEMBER SIEBER: Yes, insulation.

VICE CHAIRMAN WALLIS: -- complicates the chemical problem.

MEMBER SIEBER: Insulation is a factor.

VICE CHAIRMAN WALLIS: Okay.

MR. BUTLER: Okay.

MR. MURPHY: Mark Murphy from Material and Chemical Engineering Branch.

In the chemical effects test, there is a generic addition of hydrochloric acid to account for some of the electrical coatings. And then the epoxies have been shown to not degrade. They are tested and they don't break down, you know, in solution.

VICE CHAIRMAN WALLIS: Thank you.

MR. BUTLER: The last point on this slide I would like to make is section 6, which we titled the Alternate Evaluation in recognition that it's not a risk-informed evaluation. So we're very cognizant of that. And we just call it an alternate evaluation.

It's still within the design basis realm and it just provides a more relaxed but still conservative treatment of a less likely spectrum of breaks within the design basis.

One aspects that the section 6 allowed would be a more realistic treatment of NPSH, a more realistic calculation using nominal input parameters.

The SER kind of restricts that use in that you'd still need to go through a 9118 evaluation any time you exceed a nominal parameter, which will tend to make plants go with their bounding tech spec values to avoid having to constantly go into an operability evaluation. So it really reduced the usability of that section 6 analysis.

I've made the point that we're still reviewing the SER and that, you know, we're going to start focusing on the application of the guidance so that we can continue on.

The combined impact of the changes on the result, it really isn't known. That's an uncertainty we're just going to have to deal with at this point if it is finalized in its current form.

The calculations that have been performed to date, I imagine as we continue on, some assessment can be performed using those calculations to get an idea of how significant these changes are. But that hasn't been performed to date.

And lastly I'd like to point out the uncertainties that have to be somehow accounted for with the test programs that are ongoing. The chemical effect testing that is -- well, should get underway very shortly. The initial results should be available before the end of the year. And the final results are going to be available sometime, hopefully the first quarter of 2005.

And the second item is the downstream effect testing. I'm uncertain about the schedule there.

Both of these test programs have the impact of effecting the overall resolution process. The issuance of the SER for the guidance will start a clock. Plants will be required to respond within 90 days of that issuance. And basically start their evaluation.

They have until September of next year to complete that evaluation. So anything, any uncertainties they have to deal with during that process complicates the final evaluation of the resolution options. So we're concerned about that.

And then the schedule for implementing any modifications as necessary is also shown in this timeline. But the main point is the short time period between the issuance of the SER between now and September 1st of next year, there are a lot of uncertainties that need to be addressed.

MEMBER FORD: John, could I just repeat what I said at the beginning? That if one of the modifications is to remove the cal-sil, if that is one of the options being taken, you are aware that by removing silica, you will increase the possibility of chloride stress corrosion cracking of the stainless steel? That might be an unexpected consequence of doing this that should be evaluated either in terms of a test program or within the Reg Guide 1.36 guidelines.

MR. BUTLER: I've made note of your comment. I admit I don't appreciate it. I will take it back to those who can appreciate it.

VICE CHAIRMAN WALLIS: Okay, John, does that conclude your presentation?

MR. BUTLER: That's is.

VICE CHAIRMAN WALLIS: We're almost approaching the time when the Chairman said we had to stop. So --

CHAIRMAN BONACA: Well, the staff maybe

has a closing statement?

VICE CHAIRMAN WALLIS: Well, yes -- well, that's it, I was just hoping we could end up with the staff. If you can do it before twelve, Michael?

(Laughter.)

MR. JOHNSON: That's okay. Yes, actually --

CHAIRMAN BONACA: We come back at one but if you guys want to go further now, that's fine.

VICE CHAIRMAN WALLIS: For those who are impatient to learn more, we can stay here.

MR. JOHNSON: My comments are simply conclusionary actually.

And has been said a number of times today, and we would stipulate to the fact that there is always more that can be learned, and we are going to learn as we go forward, and we'll deal with what we know.

And, for example, we're not opposed to -- in fact, we'll consider issuing even a supplement to the SE if that becomes appropriate based on something that we learn. That's certainly within the realm of possibility.

We recognize that there are areas where there is not a lot of data. And that's, again, something that we're going to continue to learn as we go forward.

But having said, and we've said a number of times today, we believe that we know enough about this issue such that the staff's conclusion is that based on the GR and the SE that there is reasonable assurance of adequate protection for someone exercising the methodology and then making fixes.

The plants that do that will be in a safer place. The plants will have an understanding of whether they have a problem. That was one of John's points. That was really the industry's thrust in terms of developing the methodology.

We agree that with the fixes pointed out in the SE, that the staff -- the plants will have an understanding of whether they have a problem. And will certainly have a sense of comfort that fixes that are made as a result of this SE, again, will result in plants that are safer.

We've had lots of interaction. I want to go back -- I don't want you to leave with the impression that, again, staff has not had a lot of interaction.

We've had from the first draft report that was submitted on this, we've had a full round of REIs.

We've gotten written response on those REIs. We've gone back and had additional discussion.

There are many, many areas of this evaluation where we've had extended dialogue with the industry on the evaluation. We're going to continue to dialogue.

One of the points that was indicated in a letter from Tony Petrangelo to us last week was, for example, that we have dialogue with vendors to understand what vendors are proposing in terms of the fixes. We think that is a good thing.

We're going to work -- we're going to set up that dialogue. We've talked to Tony and they're going to orchestrate that dialogue with the staff so we understand what folks who are going to be fixing these problems are coming up with and the challenges and so on and so forth. That's a good thing.

But I guess my bottom line is we've had lot of interaction.

I do want to make the point that -- again, I tried to make this point in my opening in terms of what we see as our regulatory responsibility. You know, we -- again, we're faced with resolving problems and, you know, sort of looking at justifications is always the responsibility of the licensee to provide an adequate justification.

It's the responsibility of the NRC to decide whether that justification is adequate. And whatever fixes they put in place that would correct that problem are adequate. And so that's the approach that we will have.

I'm very encouraged by the industry talking about additional tests. And we want the industry to do additional testing if they feel it's appropriate. That would benefit the process. We would certainly look at whatever comes from that.

It's not the responsibility -- we don't feel at this stage that it is the responsibility of the NRC to develop some new unthought of test program to address these issues.

We very much want the industry to continue to do what is necessary and particularly could be beneficial to address some of these refinements in some of these areas where there is policy of data.

And so again, I just wanted to say we believe it's time to go forward. We are going to learn a lot going forward. But we believe it's time to go forward with respect to the evaluation.

VICE CHAIRMAN WALLIS: I want to raise two points.

You make statements about plant safety. You are sure the plants are safe. We haven't discussed any of that. We have no evidence. I don't quite know how you make a statement until you see the consequences.

You can't make a statement on plant safety based on these documents because they haven't been implemented yet. So I don't -- that's just a comment. I think that's irrelevant to what we're here for today. The ACRS isn't going to take any position on plant safety because we haven't seen any evidence.

But we have taken an issue with some technical issues. And it seems to me that you say you are comfortable. Are you comfortable with proceeding without resolving what seem to be quite a few technical issues that we have raised? Are you really comfortable proceeding without resolving technical issues that we have raised?

MR. JOHNSON: Well, of course a few of the technical issues you've raised are issues where we have ongoing work. For example, chemicals. And that's built into the resolution process.

We talked about downstream effects and John -- and we have also indicated that there is some ongoing work on downstream effects. And that's actually a part of the evaluation going forward.

In general, we have made changes in response to the comments we've gotten. And yes, I think the answer is that the staff is comfortable going forward. Not to say that this document is so perfect. We're still incorporating minor changes to the document.

VICE CHAIRMAN WALLIS: I'm so surprised, Michael, because I come from a different environment maybe where if I review of a technical paper for a journal or if I review a student thesis, and if has these sort of fundamental technical questions about it, it doesn't get accepted.

Maybe this is a different environment? Or maybe you know something more?

I don't want to continue the conversation. Just personally I'm a little puzzled by your comfort. But it maybe because of the background I come from.

CHAIRMAN BONACA: Yes, I guess my discomfort a little bit is due to the fact that I really was left with the impression that we do not have a full appreciation of the dimension of the problem.

I mean we came up with dimensions of debris of different quantities, et cetera. So -- and then that leaves me with uncertainty about the adequacy of proposed fixes.

So the sense I'm getting is that you do have an appreciation of the problem from what you're saying even if it is the burden of the licensees to address the problem. If you do not have a full appreciation of what the problem is, I mean how can you make a judgment on the adequacy of the fixes, you know?

MR. JOHNSON: Well -- and I appreciate Tim's comments sort of explaining the differences. You know we were all struck by the numbers that Tim used at the end of the subcommittee meeting. And so we wanted to go back and look at where we thought a fiber plant, for example, would come out using the same evaluation.

And it is plant specific. And I think that helps. I don't think you heard from anyone that we need to add additional conservatism on the various aspects of this evaluation. What you heard, in fact, from the industry is that in some cases, they believe that we're overly conservative.

And, of course, the staff's response to that is we may be overly conservative. But if that is the case, it's because there are these unknowns that somehow have to be accounted for.

CHAIRMAN BONACA: I don't worry about the baseline calculation. At some point, everybody will have to come in to the refinements. And that's really what I'm wondering how they're going to apply them based on what I've heard today. I realize I'm not a member of the subcommittee but it was left to some puzzlement in my mind.

MEMBER KRESS: Well, let me ask a question about the head loss correlation. This is to you, Mike.

You've treated one of the parameters, the specific surface area, to beta points on various debris mixtures. And you come up with the different values for that depending on which test it was and the mixture.

Will you require the use of the value for that that gives the most conservative result? The biggest head loss?

MR. LU: Could you repeat your question again? We are trying to discuss what exactly you mean in terms of the specific area there.

MEMBER KRESS: The head loss correlation has parameters in it that were adjusted to fit the data. And depending on which test you adjust it to, you had different values. For example, for the specific surface area.

Now my question is there is a range of these or a choice to be made about which specific surface area for which debris type you will use. And my question was will you require that they use the value that gives the most conservative result, that is the biggest head loss?

MR. HARRISON: Looking at the calcium silicate test in particular, the specific surface areas we came up with were identified with the worst conditions that we found. And the recommendations had an addition ten percent factored in.

Ten percent in the specific surface area could be as much as 21 percent in the head loss because it's the number squared in the correlation.

MEMBER KRESS: So actually you're making them use the most conservative guidance for that.

MR. HARRISON: And I believe we're also recommending some enhancement on the actual number determined from the tests, add a safety factor to that.

MEMBER KRESS: Okay, thank you.

VICE CHAIRMAN WALLIS: Mr. Chairman, it's yours. It's up to you to decide what to do next.

CHAIRMAN BONACA: Okay. Any other questions or points the members want to make? Please.

MR. CULLISON: I would like to make a point and that is that I think that there is a safety problem here today. And I think that the Advisory Committee has to be careful that we not allow the progress to move forward rapidly.

It's taking too long. I think we recognize there is a real safety problem today that effects us within the design basis envelope.

On the other side, I think it's also clear that there are various aspects of this where the staff believes there is conservatism with very little justification for that belief that there is conservatism.

And there is a need for clearly more work beyond what exists as the basis that the staff would use today for its evaluation.

So, again, let me point out that there are two sides to this. But I think we have to be very careful that we allow the industry to move forward or we force the industry to move forward aggressively to solve a problem that does exist today.

We often deal with hypothetical problems that -- this is a real problem.

VICE CHAIRMAN WALLIS: Yes.

CHAIRMAN BONACA: That's why I was asking those questions regarding is there going to be additional work to be done to close some of these issues.

MEMBER KRESS: Like confirmatory research.

CHAIRMAN BONACA: Yes. Because if that is the case, the downstream risk is the one that the licensees may have to do additional modifications to their sumps. But still the trend is going to be in the positive direction.

MEMBER APOSTOLAKIS: But again I'm not an expert in this area but I'm puzzled by a couple of issues. First of all, we've heard the time pressures. And I agree with Rich. After 25 years, all of a sudden there is time pressure?

Second, some of the proposed actions to take care of the problem, which is something that Graham keeps coming back to, I mean what are you going to do at the end? Not just analyze the thing. Are they very expensive to do? Are they -- all of them are?

MEMBER SIEBER: Yes.

MEMBER APOSTOLAKIS: All of them are? I understand the issue of increasing the surface area but all of them are expensive? I mean the barriers that Mario mentioned to limit the --

MEMBER KRESS: Well, I mean that's --

MEMBER SIEBER: It's all relative.

MEMBER APOSTOLAKIS: What?

MEMBER SIEBER: It's all relative.

MEMBER APOSTOLAKIS: Relative to how much pain you're going to get by not --

MEMBER SIEBER: You run the plant and make the mods or don't run --

MEMBER APOSTOLAKIS: Wait, wait, wait, there are various kinds of pain.

MEMBER SIEBER: Money is --

MEMBER APOSTOLAKIS: One is getting a negative ACRS letter on this safety evaluation and --

VICE CHAIRMAN WALLIS: George, you are very right. I think we need an -- if this were a student design project, I'd say you need an economic analysis. I want to know what is the risk. I want to know if we make a bad decision based on this SER, the industry may have to spend 200 million dollars. How much is it worth getting better information --

MEMBER APOSTOLAKIS: That's exactly my point.

VICE CHAIRMAN WALLIS: -- and working on

your research, which may coat me ten million dollars --

MEMBER APOSTOLAKIS: Yes, exactly.

VICE CHAIRMAN WALLIS: -- in order to save the risk of making a 200 million dollar mistake. That's the kind of thing I'd like --

MEMBER APOSTOLAKIS: And that's the way --

VICE CHAIRMAN WALLIS: -- because that's the way I'd think if I were a business man.

MEMBER APOSTOLAKIS: -- and that's exactly where I was going to. I mean --

MR. HAFERA: Excuse me. I think on my third slide, my last line, I projected some practical solutions and some of those are fairly -- are not necessarily -- it doesn't take a lot of engineering to go get a bunch of insulators and double jacket your insulation.

MEMBER APOSTOLAKIS: That's exactly --

MR. HAFERA: And remove all your fiber from the source term. There's a nice, inexpensive fix that every plant could do.

MEMBER APOSTOLAKIS: So why aren't they doing it then?

MR. HAFERA: Well, it's up to them to do it.

MEMBER APOSTOLAKIS: Well, I understand that.

MR. HAFERA: We can suggest it.

MEMBER APOSTOLAKIS: But that's my problem that --

MR. HAFERA: And, again --

MEMBER APOSTOLAKIS: -- I see here again a question that is open. There are strong disagreements. Do more tests. Do more research. And I'm wondering, you know, are there any solutions that, you know, coming back to the internal combustion engine. They didn't quite understand what was going on but they built it. Maybe there are some solutions here --

CHAIRMAN BONACA: The only problem in that example is that we will never know if the sump works until you have a LOCA and hopefully we'll never have it.

MEMBER SHACK: You know you've made it better. Have you made it good enough?

MEMBER SIEBER: Yes because you have to have the analytical methods and the data to know that.

CHAIRMAN BONACA: That's right. I mean we are never going to take --

MEMBER APOSTOLAKIS: So there aren't any

actual things they can do that would be convincing that result in adequate protection?

CHAIRMAN BONACA: Adequate protection?

MEMBER SIEBER: Until --

MEMBER APOSTOLAKIS: Without studying and expanding the methodology.

MEMBER SIEBER: -- until we have the database, which does not require extrapolation, and the analytical methods that make physical sense, you can't show whether you are good enough or not. Even though you can physically make improvements to the plant.

And so I think that you need to work on both ends of it. I think there are more pieces of data that need to be developed. I think there are improvements to the models that need to occur.

On the other hand, I think that licensees could be thinking in terms of not running tests to avoid the requirement to extrapolate but to come up with designs that will pull the operating parameters into the realm of test data they already have, reducing flow velocities, increasing screen area, eliminating debris to the extent that you can.

VICE CHAIRMAN WALLIS: Absolutely.

MEMBER SIEBER: And so those are the kinds of approaches that I expect. But moving forward the way the SER now says and the guidance now says I think will lead to a quagmire.

CHAIRMAN BONACA: Well, I think we need to take a break now.

MEMBER APOSTOLAKIS: For what? Ten minutes or lunch? Lunch?

CHAIRMAN BONACA: Yes.

MEMBER APOSTOLAKIS: What's going to happen to the schedule now? Can we --

CHAIRMAN BONACA: Like I said before, we're going to take a recess now until one.

At one sharp, we're going to get together and review ACR-700. And hopefully we can do it in an hour. You know, that's the time we're allotted now.

And then we'll just resume the schedule as we had it.

But I will start the meeting at 1:00 p.m. sharp. So with that we can recess.

(Whereupon, the foregoing matter went off the record at 12:16 p.m. to be reconvened in the afternoon.)

CHAIRMAN BONACA: Okay. We are continuing the meeting and we have a quarrel. So we will start the meeting with the next meeting on the

Agenda, that's Pre-Application assessment report for the advanced of 100 design. Dr. Kress?

MEMBER KRESS: Thank you. We, the staff, since about 2002 or so has been working on the pre-application review for ACR-700. And they've been looking at what's been called focused issues.

And they have written -- this is a severe -- I mean an SAR instead of -- it's a safety assessment report. They've issued this. And you've gotten the copy of it.

And hopefully most of you have read it. And that's what we're going to hear about today, the results that is. And I guess -- are you going to lead off Laura?

MS. DUO: Yes, I'm just going to take a minute. Good afternoon, I'm Laura Duo. I'm the section chief for the new reactors group. Before we start, quickly, I just wanted to introduce Bill Beckner is the new program Director for our program.

Many of you remember Jim Lions going through this. This is Bill's first opportunity to come before you.

MR. BECKNER: I think you probably remember me from other jobs.

MS. DUO: Okay, I know that we are

compressed on time. So, I'm going to go just through this quickly and then turn this over to Belkys again.

Pre-application is in accordance with the Commission's policy statement on advanced reactors. It encourages the Staff to engage early on complex technical issues and start a good dialogue with applicants well before a design certification application comes in.

The goals of the activity we consider that we're presenting today is sort of our completion of phase two. Again, completion in the concept of pre-application is the identification of a path forward in design certification.

I don't think you're going to be hearing any firm regulatory conclusions today, nor does the report have any firm regulatory conclusions. So, with that, I'm going to turn that back over the Belkys.

MEMBER KRESS: But, before you do, I would like to -- in the spirit of identifying the way to move forward, I would like the committee to look upon this meeting as a way to identify the things we need to review and the issues we might be most interested in when we get to our part of the review, the certification of ACR Weather 700.

Thank you, with that now you can turn it over.

MS. SOSA: Thank you, good afternoon. I am Belkys Sosa. I'm the Department Manager for the ACR-700 pre-application review. And, as Laura said, the purpose of today's meeting is to brief the Committee on the status of the pre-application review, to provide information to the Committee on the major issues identified in the pre-application safety assessment report, the PASAR, as we called it for the ACR-700 design, and to also request that the ACRS provide a letter on the Staff's assessment on the design and the feasibility of completing the design certification review.

Our Agenda is being modified somewhat due to the time limits. I'm going to try to go very quickly. What I have prepared today is an overview, very general type of presentation on the different focus topics.

We are planning a presentation by Don Carlson. And we also would like you to hear from the Applicant at AECL on the same topic. They have prepared a letter of presentation on what they intend to engage us on in the transition phase.

The approach for the pre-application review, again, was to identify some terms. We are not trying to resolve the issues, we were trying to facilitate the design certification review.

Phase one was the familiarization phase. That lasted approximately a year. We also tried to develop an understanding of the differences between the ACR-700 and other CANDU plants, to identify existing regulations that may not be met by this design and to identify new regulations that will be required in order to provide and ensure adequate --

VICE-CHAIRMAN WALLIS: This bullet of differences, it seems to me you need to be clear about what appear to be differences, but may be superficial because it looks differences, and what are real differences about approaches to safety or defense in depth, or the principles.

And so, somehow separate those out for us so we don't get lost in the details and we can see these are the main key differences that affect something at a higher level. Maybe that would help us too.

MS. SOSA: That's a good point. The ACR-700 is light water cooled. It's not heavy water or -- so there are some differences that we need to point out.

And we also have been engaged with the

Canadian Nuclear Safety Commission as another resource in the pre-application review, including several technical interactions with them.

They are -- designing -- now, the pre-application review scope was selected by the Applicant. And there's 13 different areas. What you see underlined are, for instance, the boundary design, the computer codes and validation adequacy, the power fueling confirmation of negative void reactivity and the fuel design, are issues that AECL determined to be key focuses.

Again, that was done in response to some concerns from NRC resource limitations. They said, please focus on this first. The item that you see in red is design basis access and acceptance criteria.

It was determined by the Staff to be the NRC priority. And the items that you see in blue, focus topics five, ten, and twelve, essentially do not have distinct sections in the report.

What we did is we wrote that information with the other focus topics. So, you won't find a separate chapter on that. The report for every focus topic contains a review scope section where discussion on what was reviewed and the guidance that it was reviewed again to the extent that it exits.

There's a section on regulatory issues that were identified for each focus topic. Again, rules, rulemaking are exemptions that will need to be resolved, are listed on there.

Potential policy issues, again, there's a section that discusses items that could potentially require upper management or Commission dinosaur resolution.

At this point we feel it is pre-mature to call any kind of policy issue because we haven't seen the application yet. Technical issues, again, it discusses significant technical items identified that will require additional data tests or analysis in order for a resolution to be issued.

And the conclusions section is nothing more than identifying what the feasibility of successfully completing the design. The Staff feels at this point that nothing that we've reviewed would preclude certification of -- not certification -- but moving forward with design certification.

Here are the major milestones in the pre-application. Phase one complete in July of 2003. Phase two is currently ongoing and scheduled to complete at the end of this month with issuance of the report.

The draft report was provided to the Committee for review September 16th. Essentially, the PASAR will be issued at the end of this month. In November we will start with what we call the transition phase.

And that will go through until we actually receive the application.

MEMBER KRESS: Do we have a target date for completing the whole certification process yet? Or is that too soon?

MS. SOSA: I think that is a little soon. Once we receive the application we will develop our estimate on the schedule. Now, again, this is a very general overview for each of the focus topics.

For class-one pressure boundary design we have a couple of regulatory issues involving 50-55A, the use of ASME. Essentially, for areas where ASME code requirements are not applicable or need to be supplemented, the Staff will evaluate the acceptability of Canadian codes and standards.

Again, for the ACR-700 they don't have a reactor vessel, they use pressure tubes. So, there's a regulatory issue there. But, the Staff feels that, in accordance with 52-40A, the technical requirements specified in 50-61, the pressurized thermal shock, the fracture toughness and the materials surveillance requirements are not technically relevant.

MEMBER APOSTOLAKIS: Excuse me, come back to the first bullet. Does Canada follow the ASME standards?

MS. SOSA: In some areas of the design because of the unique aspects, for instance, the material, the use in the pressure tubes, that's not in -- by ASME. So, a lot of it is --

MEMBER APOSTOLAKIS: But let's say that the issue is -- is it possible that Canada will apply its own standard? Or is that covered by what you say there?

You say for those areas where the ASME code requirements are not applicable.

MS. SOSA: Correct.

MEMBER APOSTOLAKIS: You look at the Canadian standards. What about the areas where the ASME code applies but they have their own standard?

MS. SOSA: For those areas we will use our standards. So, only for areas where we don't -- where it's not covered in ASME, then we use --

MEMBER APOSTOLAKIS: I'm sure you'll teach them to have their own standards. And they have agreed to this --

MEMBER KRESS: Evaluate the acceptability of the Canadian standards. When you do this evaluation of the Canadian standards, normally what you do is compare those to ASME standards.

Are you going to basically be developing what you think our standard ought to be and see if this meets it? How are you -- what is your acceptance criteria.

MEMBER APOSTOLAKIS: Yes.

MEMBER KRESS: That's yet to be determined, I guess.

MS. SOSA: I'd like to refer to Ted Sullivan to give you an explanation on that. Actually, why don't I have Victor?

MR. SNELL: Victor Snell with AECL. Just to answer the question briefly, and sorry for -- Belkys time -- for areas in Canada where ASME would apply, we use it. So, it's just as simple as that.

MEMBER APOSTOLAKIS: Okay.

MEMBER KRESS: But it doesn't help the issue of how do you go about evaluating the acceptability of a standard.

MS. SOSA: Of the Canadian Standard?

MEMBER POWERS: Yes. For instance, if you're working with the zirconium alloy, so you look at a Canadian standard for that alloy. Have you given thought at all to what it requires to review and assess that standard?

MS. SOSA: I think the approach is going to be to try to evaluate the standards to an applicable requirement, to the same level of requirements that we have.

But we will be using Canadian standards for that.

MEMBER POWERS: Do you have a standard -- is there a requirement in particular on deuterium take-up by the alloy?

MR. FAIR: Yes, this is John Fair. I'm not going to answer your specific one on the materials. But, those design aspects that are not covered by the code, specifically we tried to review and see that they meet the intent of the code, which is the margins of safety, etcetera.

For the materials aspect, they're going to have to look at details of materials, testing and stuff like that, and the type of detailed review you would do when accepting the materials that are accepting the ASME code.

But we do not have specific criteria for doing this evaluation.

MEMBER POWERS: I guess this sounds like it can be either an easy job or an impossible job. I mean, if I go in and look and say, okay, here's the standard, and here's a bunch of data, and sure enough they bound it up with the standard, it's not too difficult to do.

If, on the other hand, they go in and say, well here's the data to use, but here's the database that really exists and here's all the testing methods that hey did, and how good and reliable those testing methods are, and the bias that's inherent in the various testing methods, and the bias that was applied because the samples were not really pressure tubes but little plantchets that people tested and things like that. This could well be a lifetime occupation.

MR. FAIR: Well, we think that some of the areas are going to be difficult, but not impossible. I think there's a lot of test data out there on some of the areas that we don't have covered by the ASME code that ADCL has referenced.

And I believe that we're also going to be looking at doing some confirmatory stuff with our research. So, it's not an easy job and we agree with you, it's going to take a lot of effort to review some of these areas.

MEMBER POWERS: The problem I see inherently in it is there are data that are not cited either in the standards or by AECL that also exist on these materials. And finding them is a chore.

MR. FAIR: Well, you may be correct on that. I mean, I can't speak to things I don't know exist. Other than the fact that, when we get into the review, we'll probably do document searches and try to get as much of the information as we could find out there.

MEMBER KRESS: Is the process that Canadians went through to develop their standards similar to the process we go through to develop ASME standards?

MR. FAIR: I'll leave that to AECL to answer, but I believe so.

MR. SNELL: Victor Snell again. I can try and give a general answer, because I'm not a standards expert. But I think the general answer is yes.

By in large, where ASME applies, we use it. So, the Canadian standards have been developed over a large number of years with operating on research experience, and basically come from initially steps at the labs, and confirmed by operating experience, and get formalized into standards by a group consisting of the Canadian ministry -- playing a sort of puddles over a park controlling role.

So, by the time the Standard comes out, what it represents is an industry consensus that has the input, if not the formal agreement of the regulator, incorporates operating experience and research experience, and stands as subject to revision as things change.

That's basically the process that's been followed.

MEMBER KRESS: Sounds very similar to the process we did.

MEMBER POWERS: A skeptical person might, not that I am one, might say the old boys club gets together and sets the standard in Canada, just like the old boys club sets the standards in the United States.

They cannot be considered consensus of the entire --

MEMBER ROSEN: In the United States there are safeguards that are implied by ANSE to attempt to keep the old boy network under control.

MEMBER POWERS: Another old boy network oversees the first old boy network.

MEMBER ROSEN: Well, there are certain

criteria for who can be on the standards committee and the representation and that sort of thing.

MEMBER APOSTOLAKIS: I thought Professor Wallis this morning raised concerns about one of the ANSE standards.

MEMBER ROSEN: Well, that's always possible.

MEMBER APOSTOLAKIS: Didn't you say that you looked at the models and --

MEMBER KRESS: I think we're going to need to move on on this issue. We've discussed the standards enough.

MS. SOSA: Thank you. The PASAR also discusses various issues on degradation mechanisms that will require additional information and further review for resolution.

Design basis access and acceptance criteria, focus topic number two -- again, this was the NRC priority during the pre-application period. AECL proposed -- risk informed reactor accident and clarification scheme, essentially introducing the limit the core accidents as a new category.

The Staff recommends to adapt a probabilistic event selection for ACR-700, this is a line within the new risk inform initiatives. Severe channel flow blockage and the stagnation --

MEMBER APOSTOLAKIS: Wait, the second bullet there says the Staff recommends a probabilistic -- you're going to select design basis accidents using --

MS. SOSA: No, we are going to look at the limited core accidents in between category that AECL is proposing, and make a determination based on the probability and frequency, whether they belong in DBA or severe --

MEMBER APOSTOLAKIS: Yes, so you're using probabilities to define the DBAs aren't you?

MEMBER POWERS: No, categorizing the hypothesized accident into one of two categories, DBA or severe accident. The accidents already exist.

MEMBER APOSTOLAKIS: The analysis, you mean.

MEMBER POWERS: The scenario already exists.

MEMBER APOSTOLAKIS: Yes.

MEMBER POWERS: The question is, is the design basis accident that's subject to conservative deterministic evaluation.

MEMBER APOSTOLAKIS: But it's an interesting thing, though. I mean, you're saying that as if it's the easiest thing in the world. I mean, tomorrow we have a whole presentation on licensing future reactors that will be risk --

MEMBER POWERS: We know that the academic community can complicate any subject.

MEMBER APOSTOLAKIS: I think there are some skeptical members of this committee that do that very well. There seems to be a disconnect. On the one hand we have a major research project trying to do that for future reactors.

And here we're saying, no, we're going to adopt a probabilistic approach and do it. I'd like to see that. I think we were supposed to have seen it already.

MEMBER KRESS: They will also have a PRA.

MEMBER APOSTOLAKIS: Of course.

MEMBER KRESS: The PRA will look at the whole range of accidents, like PRAs do.

MEMBER APOSTOLAKIS: I understand that. But, I thought that's an issue that our staff is facing is that DBA isn't PRA. What do we do about it?

MEMBER KRESS: What DBAs are supposed to do is render the design into an acceptable safety. What the PRA does is validate that, tell you whether or not you have a risk.

MEMBER APOSTOLAKIS: I know.

MEMBER KRESS: So, I think the process they're talking about may be workable. They may have to -- they have to decide on what probabilistic value they'll use for the break.

MEMBER APOSTOLAKIS: Yes.

MEMBER KRESS: And then that may be an issue, I don't know. They may choose one of them, the PRA and tell them, maybe we should have used a different one.

They may have to adjust that. I don't know what they plan on doing. I'm just throwing out words.

MEMBER APOSTOLAKIS: Well, as I say, tomorrow we will cover a whole presentation on the issue.

MEMBER KRESS: Yes.

MEMBER APOSTOLAKIS: Maybe we can tell them it's trivial, go, find out from these guys and do it.

MEMBER KRESS: Say again?

MR. BECKNER: I don't know that Belkys said it was going to be easy. I think she said that was we intend to try. But I think we would concur that it's not an easy task.

MEMBER DENNING: Could you give us some idea as to what the threshold might be between what's a design basis accident and what's a non-design basis accident?

MS. SOSA: I'd like to defer to Jerry Wilson. He was the chair of a working group that we established specifically to look at this.

MR. WILSON: Jerry Wilson, first of all, I'd like to remind the Committee that the Staff has been before the Committee several times on these non-NRWR policy issues, one of the issues of which was selection of accidents for finite reactors.

And this Committee approved that proposal. And the Commission approved that proposal. And so, the Staff is proposing to do is adopt that approach for this particular design.

And the specific answer to your question, I think the range that we're looking at for design basis accidents would take us down into a frequency of ten to the minus five.

MEMBER KRESS: I recall that this was a process that Exxon proposed.

MR. WILSON: Something like that, similar. But, we haven't worked out the details, but this a proposal for going forward at this point.

MEMBER KRESS: And the selection of ten to the minus five is based on what?

MR. WILSON: Well, a range that was discussed in those policy papers that have been sent to the Committee on the frequency it feels appropriate for design basis accident.

MS. SOSA: Okay. As an alternative to meeting the requirements of 50-34, the Staff may propose a mechanistic fission product source term for commission consideration.

Computer codes and validation adequacy were focused up in number three. This involved the neutronics tools, as well as the thermal hydraulics codes.

The current physics codes that AECL brought in, the WIMS codes, DRAGON, RFSP, staff determined will meet modifications and revalidation for ACR-700 conditions.

Experimental database on header and fitter inventory on fuel distribution, horizontal fuel bundle thermal hydraulics and RD-14M integral test is required for a successful completion of design certification.

Now, modifications to test facilities, such as the RD-14M and CWIT, and the LASH facility, may be required to correctly scale the ACR-700 design.

MEMBER KRESS: Now, I'm sitting here with perhaps a mis-apprehension about these facts. Do these requirements, the current physics codes, and these modifications, for example, are those things that you expect the Applicant to do?

MS. SOSA: Yes, they are currently working on that.

MEMBER KRESS: Now, on the scaling question, are you going to require that the AECL do a scaling analysis?

MS. SOSA: The staff is currently doing a scaling analysis.

VICE-CHAIRMAN WALLIS: Did you say something about thermal hydraulic codes that I missed, or are you just talking about physics codes?

MS. SOSA: The thermal hydraulic codes were also reviewed, the ATHENA code was. Several runs were performed. And, the outcome is what you see here. Essentially it was determined that the database would still need to be worked on to make sure that it represents ACR-700 conditions, and that the test facilities will have to be verified to make sure that they are scaled correctly.

VICE-CHAIRMAN WALLIS: You have the ATHENA code?

MS. SOSA: Yes.

VICE-CHAIRMAN WALLIS: We can use your --

MS. SOSA: Yes, we have. And the Staff is working on their own independent tool.

VICE-CHAIRMAN WALLIS: How do you verify or decide to accept a code?

MS. SOSA: How do we verify?

VICE-CHAIRMAN WALLIS: How do you decide that a code is acceptable?

MS. SOSA: Well, I'd like to defer to the lead on the thermal hydraulics review, Walt Johnson.

MR. JOHNSON: Yes, we're going to apply the -- Walt Johnson, reactor assistance branch. We're going to apply the draft reg guide, 1120, which --

VICE-CHAIRMAN WALLIS: Is this the one that has never come out yet?

MR. JOHNSON: The reactor --

VICE-CHAIRMAN WALLIS: We have been working to get it out for eight years or something, is that the one?

MR. JOHNSON: This is the one.

VICE-CHAIRMAN WALLIS: Maybe if you used it, then that would be sort of -- day factor whatever they say issuance.

MR. JOHNSON: I suppose it would. It seems like a good way to go as -- be done and requires that the code be validated against the important areas in the PIRT.

And we're going to follow the approach because it seems like the appropriate way to go.

VICE-CHAIRMAN WALLIS: That would be very good. I think we'd be happy to see this document used.

MS. SOSA: Thank you. Severe accidents definition, adequacy of supporting research and developing, focus topic number four. The NRC PIRT process identified a number of key technical issues that must be addressed for successful completion of design certification.

The PIRT process also identified potential deficiencies in the experimental database used to validate the analysis codes. And the Staff will use MELCOR, will model on MELCOR to model the unique characteristics of the ACR-700 configuration for independent validation.

And, the Staff is not planning to conduct additional experimental work. We anticipate that the AECL experiments are going to be sufficient to validate the analysis.

MEMBER KRESS: Now, the Canadians use a version of the map code for this?

MS. SOSA: Yes.

MEMBER KRESS: Have you reviewed that?

MS. SOSA: Yes. I'd like to defer that question to Sid Basu. And he can elaborate a little bit on what the plan is.

MR. BASU: Okay. This is Sid Basu from research. I guess I missed Tom's question.

MEMBER KRESS: I wondered to what extent you plan on reviewing the map code that the Canadians use for their severe accidents?

MR. BASU: We are going to be looking the mapped ACR version that they are either developing currently or probably just about completed the development.

And we're going to look into the code to see whether all the phenomena are adequately modeled there. That's currently the extent of our review process.

MEMBER KRESS: With respect to no experiments needing, are there any experiments being done to look at LCI steam explosions in heavy water?

MR. BASU: Yes, they have planned -- which is mostly -- interaction experiments. They have about half a dozen experiments planned. They were going to run the commissioning test with a smaller amount of melt mass just to see, you know, how the system facility works.

And I believe the test was planned some time in August. I don't believe it has been run yet.

MEMBER KRESS: Do these tests include substantial amounts of the caladium two and pressure two metal components?

MR. BASU: Yes.

MEMBER KRESS: I think --

MS. SOSA: Canadian design codes and standards, focus topic six. The Staff believes that SECY-47 has direct applicability to the use of Canadian codes and standards for the ACR-700.

In response to that, the Commission directed the Staff to review the international codes and standards only as part of applications or pre-application reviews.

So we believe that the ACR is covered by that. Now, we expect, as you mentioned earlier, that the review of Canadian codes and standards will have a significant impact on the time and technical resources of the Staff -- certification review.

So we are preparing for that. The next

focus topic is distributed control systems and safety critical software. The Staff raise a question in their review on how the design complies with NRC's position on defense in depth.

Since it appears at the trips head points for both the shut-down systems are the same, the Staff question whether shut-down system one and two are developed to meet the same systems functional and software requirements.

AECL's presentation the last time we came to the ACRS in January of 2004, indicated that reliability of safety critical software is demonstrated through particular quantitative reliability goals.

This may raise an issue, since current NRC position does not provide the use of digital reliability goals.

MEMBER KRESS: But, is it precluding them? Is the NRC position precluding the use of goals?

MS. SOSA: I'd like to defer that question to Mike Chramel, he can elaborate.

MR. CHRAMEL: I'm not sure. We say that we don't allow quantitative reliability to be the only means of verifying the quality of the reliability of the system.

It could be used as an added incentive. But it should be both qualitative and quantitative.

MEMBER APOSTOLAKIS: Well, in order to do a quantitative analysis, you have to do the qualitative first. So, it shouldn't be that hard to satisfy that requirement.

I remember there were some funny words in the regulations about the reliability goals related to software. It didn't quite come to the point where they said don't use them.

But, it clearly sent the message that you guys were very cool toward the idea. Well, that was a long time ago.

MR. ARNDT: That was seven years ago.

MEMBER APOSTOLAKIS: Seven years ago. Steve, do you want to say something?

MR. ARNDT: Steve Arndt. The other issue, of course, was the particular methodology they use is not something we've specifically looked at, although we're in the process of looking at similar things.

MEMBER SHACK: But, do the two systems meet the diversity goal? Are they using the digital reliability to substitute for diversity? Is that --

MR. CHRAMEL: Yes, they are using two different codes and different mechanical systems.

But, the thing we are looking for is the requirements are the same or not.

MS. SOSA: On power fueling is focus topic number eight. The Staff's approach was to compare the design of the ACR-700 on power fueling systems to the design related regulations in part 50 and part 52.

The Staff determined that existing regulations are adequate to support design certification on power fueling for the ACR-700. Now, the on power fueling process could be a relatively high probability initiator for limited core damage accidents. That's something that's --

MEMBER ROSEN: Now, in reading the PSAR, what I learned was that, for on power fueling, the components that 10CFR would require in terms of isolation were the -- not be available in the current design of the ACR-700.

Am I correct in that some additional design work may be necessary to bring it into full compliance, mainly in the area of double isolation and those kinds of --

MS. SOSA: I'd like to refer that question to Steve Jones or John Fair as well.

MR. FAIR: Hi, John Fair. We've reviewed it, the pressure boundary in accordance with 50-55A designation in the regulations. And some of the lines that were coming off of the refueling machine didn't have double isolation valves.

And I believe AECL was considering whether they were going to change some of those designations to conform with U.S. regulations are not.

MEMBER ROSEN: Well, isn't it time to stop considering and kind of fix on a design weakened review?

MR. FAIR: I'll leave that to AECL.

MS. DUO: This is Laura Duo again. Again, pre-application was looking at some of the larger issues in having -- forward. Once we had the design certification application, that's where we start to get into those issues more deeply.

But, until we have that application submitted, we have to review what we have before us.

MEMBER ROSEN: Well, I think you can say that isolation isn't important. But, if it's clearly not in conformance with some of the requirements of part 50, I mean, that's a show stopper, isn't it?

MR. ARCHINOFF: Can I just interject for second. It's Glen Archinoff, AECL. As far as I know, that one has been taken care. That change has been made in the design, that particular one.

MEMBER ROSEN: Well, there are few others. In reading -- I don't have a mental picture of it right now. But, I'm reading -- when I read that I was concerned there were a number of things.

None of them looked like terrifically big hitters. But, if they weren't fixed, they simply wouldn't comply. So, I think we'll have to focus on that in the future.

MEMBER KRESS: Well, they'll either have to comply or get an exemption.

MEMBER ROSEN: Right. They can always get an exemption.

MEMBER KRESS: I know it's unheard of.

MEMBER ROSEN: Well, I mean, exemptions have to have due cause and all that shown.

MEMBER SIEBER: On the last bullet you have on this slide, what's the scenario of the core damage actions to --

MS. SOSA: Okay, I'd like to defer to Steve Jones for that one.

MR. JONES: Steve Jones, NRR. The -- both operational experience and AECL's preliminary probabilistic safety analysis indicated a couple type of events may result in failure of the end fitting, either due to failure of the refueling machine to properly re-seal the fuel channel, or due to impact of the fueling machine with the end fitting.

In that case, events such as fuel ejection from the fuel channel are possible.

MEMBER SIEBER: Okay.

MS. SOSA: Okay. Thank you. Confirmation of negative void reactivity, focus topic number nine. And, again, Don is here to provide you more detail.

We heard you and got some feedback last time we were here in January, where you referred to this issue as probably the number one issue to look at during the pre-application review.

So, based on that feedback, we prepared a more detailed presentation for you. Now, the Staff feels that, again, the design that they reviewed during pre-application is a preliminary design.

So that's important to recognize. If the AECL comes in with a design that's still -- has not eliminated the potential for substantially positive reactivity during the initial checkable reading, they feel that they would raise a similar issue as that in SECY-92.

MEMBER KRESS: Could you elucidate us on what checkable --

MS. SOSA: Yes, I think that Don has

prepared a detailed presentation on that. So, I will defer him. So, again, the challenge here will be what level of confidence are needed for establishing compliance with GCD11.

Here is focus topic number 11. The issues, again, after review is the treatment of limited core damage accidents. And risk objectives should be expanded to address both the limited core damage accidents and the severe core damage accidents.

And the definition is there. Limited core damage accidents are accidents that involve just a single channel, by design, do not propagate to the entire core.

And, severe core damage involve the entire core.

MEMBER SIEBER: What are the consequences, however, of limited core damage accident? Is it limited to inside containment and contamination? Or is there a potential for external consequence?

MS. SOSA: That's a good question. I think I'm going to defer to Marty Stusky. Is he in the room.

MR. STUSKY: This is Marty Stusky from NRR. The Applicant stated that the consequences of limited core damage accidents are confined inside the containment building itself, which would be small because it's only one 296th of the core inventory or so, single channel. It's something we'll look at.

MEMBER SIEBER: Okay. Thank you.

MEMBER POWERS: What are consequences outside the containment then are all relative or dependent on what the leak rate from the containment would be.

MR. STUSKY: That is correct.

MS. SOSA: Okay. The last focus topic is the fuel design. The design certification process for the ACR-700 fuel will deviate from past practices.

The reason is that AECL does not have a referenced CNSC approved ACR-700 fuel design or fuel performance methodology. The fuel design criteria deviates from SRP 4.2.

And the ACR-700 design and operating conditions deviate from operational -- as well.

MEMBER POWERS: That's the whole set of things, right?

MS. SOSA: Yes, it's very different. Their fuel design is very different.

MEMBER KRESS: I understand the CANFLEX shown has a much thinner clad around it.

MS. SOSA: I'd like to defer the question to Paul Clifford.

MEMBER KRESS: The question is what are the implications of that with respect to, say, appendix K type acceptance criteria.

MR. CLIFFORD: Yes, Paul Clifford, NRR. Yes, the cladding for the CANFLEX is about 30 percent thinner than typical LWR cladding. The cladding is thinner, it is designed to collapse instantly during initially due to system pressure right onto the fuel channel.

MEMBER KRESS: Minus the heat transfer?

MR. CLIFFORD: Correct. They have a very high heat rate. And that's required to transfer the heat.

MEMBER KRESS: What are the implications of that with respect to the 17 percent clad oxidation criteria?

MR. CLIFFORD: The clad is our force, so we're familiar with the behavior. As far as clad rupture or burst during a LOCA, we don't expect it to do any worse than what we've seen in a current white water reactors.

We expect the 2,200 and the 17 percent to be applicable.

MEMBER KRESS: Oh, that's my question.

I'll have to think about that one.

MS. SOSA: Now, AECL's limiting reactor experience database for higher burnout slightly enriched uranium fuel bundle designs may -- a reliance of ongoing irradiation programs, which are not going to be completed until 2009 timeframe.

MEMBER KRESS: You talked about higher burn-up SEU fuel there. My impression was that the burnouts were on the order of 25 megawatt days per ton. Now, I wouldn't call that high burnout.

MR. CLIFFORD: Well, I think it referred to higher -- the current --

MEMBER KRESS: Oh, higher than the current.

MR. CLIFFORD: Right, the current is about 2,000. And the AECL would be looking somewhere between 25 and 30,000.

MEMBER KRESS: I see, much higher than the current -- database on that fuel.

MR. CLIFFORD: Right, well within our experience database for the reactors.

MEMBER KRESS: Okay, I understand that.

MEMBER SIEBER: And that's due to the slight enrichment?

MS. SOSA: Yes.

MR. CLIFFORD: Right.

MS. SOSA: So, in conclusion, the Staff has prepared carefully for reviewing the ACR-700 design certification application. Based on the information provided by AECL during the pre-application review, the Staff identified a number of issues that will require more detail for resolution.

But, we did not identify any issues that would preclude certification of the ACR-700 design.

MEMBER APOSTOLAKIS: What are the top are the top two issues, the most important ones? You have identified a number of issues.

MS. SOSA: I think what the presentation has kind of touched on today is probably gives you a good idea of where we are.

MEMBER APOSTOLAKIS: But there are several.

MS. SOSA: Is one issue.

MEMBER APOSTOLAKIS: The what?

MS. SOSA: The coolant reactivity is one issue that will have to receive a lot of attention during the certification. Everything else we have discussed today.

The fuel design is another significant area.

MEMBER POWERS: And she failed to mention the most important one, which is the aqueous chemistry of iodine in the containment building.

MS. SOSA: I'm saving that one.

MEMBER POWERS: She has an entire presentation on that one.

MEMBER KRESS: If there's one thing the Canadians know about it's that.

MEMBER POWERS: They probably got it wrong, so we need to review it carefully.

MS. SOSA: The Staff is currently preparing a SECY paper to inform the Commission on the issues identified during the pre-application review in preparation for design certification.

MEMBER KRESS: Thank you very much.

MEMBER POWERS: If I could, I'd like to ask a question.

MEMBER KRESS: Yes, sir.

MEMBER POWERS: It's a question of you.

MEMBER KRESS: Oh, well in that case, no.

MEMBER POWERS: As you are acutely aware, I am aging, and so my memory suffers.

MEMBER KRESS: I hadn't noticed.

MEMBER POWERS: Do we have within the regulations for advanced reactors considerations of issues of non-proliferation and other national policies regarding nuclear materials?

MEMBER KRESS: I don't think so. I don't think those are in the regulation. Now, somebody may correct me.

MEMBER APOSTOLAKIS: They are not.

MEMBER KRESS: I don't recall ever seeing any questions about proliferation in the regulations.

MEMBER POWERS: Does the Committee have obligations in regard to the issues of nuclear materials for proliferation?

MEMBER KRESS: I would think our Committee ought to think about everything having to do with issues of public health and safety.

MEMBER POWERS: That's a safeguards issue.

MEMBER APOSTOLAKIS: I don't think so. I think that's an issue of national policy. .

MEMBER KRESS: That's a policy issue.

MEMBER APOSTOLAKIS: It's not us.

MEMBER KRESS: I don't think it's something we have to -- if I'm going to review something like the CANDU, I'd normally ask that question in terms of my certification review.

We might ask why not put them underground, because they are less susceptible to terrorist attacks.

MEMBER APOSTOLAKIS: Is that stuff part of 10CFR?

MEMBER KRESS: No.

MEMBER APOSTOLAKIS: No, it's not. So it's none of our business.

MEMBER KRESS: I think we stick to 10CFR.

MEMBER APOSTOLAKIS: And it's not the Agency's business either.

MEMBER KRESS: That's probably right.

MEMBER SIEBER: No, I think proliferation is explicitly part of the Atomic Energy Act, George. So, it very much is part of our business.

MEMBER APOSTOLAKIS: It's Commission, but it's not --

MEMBER SIEBER: If you look legislation, you are definitely covered by the Atomic Energy Act.

MEMBER POWERS: That's where the limits on fuel enrichment come from.

MEMBER APOSTOLAKIS: I didn't hear that.

MEMBER POWERS: That's where the limits on fuel enrichment come from.

CHAIRPERSON GEOFFREY: Just a second. I hate to break in, but we were supposed to gain back half an hour. And, it took 45 minutes to deliver a 20 minute presentation.

Now, there is going to be how many other presentations.

MEMBER KRESS: We're going to hear from the Canadians.

CHAIRPERSON GEOFFREY: We have 30 minutes left. I'm sorry. Somebody has to manage the time. And we'll certainly go over the hour at this point. But, I need to watch the time.

MEMBER KRESS: I think this is certainly legitimate questions.

CHAIRPERSON GEOFFREY: I understand. I'm not arguing. I'm only saying that --

MEMBER KRESS: I think it might be better to ask the Staff if they're going to consider those things in their certification review. And I think the answers going to be, leave those to the safeguards people.

CHAIRPERSON GEOFFREY: Yes.

MEMBER KRESS: But, anyway, we'll give you the floor now.

MR. CARLSON: I'm Don Carlson. I'm in the Office of Research. And I'm going to be talking about pre-application focus topic nine, confirmation of negative void reactivity.

That's chapter eight in the PASAR. Okay, let's jump right into the highlights from my pre-application review. As you've heard before, we've conducted PIRT processes for ACR-700.

There's actually three coordinated PIRT sub-panels, one on nuclear analysis, which is what I'll be talking about. And we mentioned already thermal hydraulics in severe accidents.

A major insight that emerged from the nuclear analysis PIRT was the importance of checkerboard voiding of alternate channels in ACR-700 large LOCAS.

And so, there was already a question asked about that. As you recall, the CANDU reactors and ACR-700 in particular are horizontal pressure tube reactors.

ACR has one inlet header at one end, and another inlet header at the other end, and, likewise, outlet headers. And so, the flow of coolant and the flow of fuel during on-line fueling is in opposite directions in alternating channels.

When you have a large break LOCA, let's say it's an inlet header, then the channels that are connected to that inlet header, void very quickly, say in a about a second, more or less, depending on the size of the break.

And, that's every other channel in the core. And the other channels remain cooled for several seconds. The insight from the PIRT panel was you go from half voiding to full voiding.

But, what really counts in LOCA analysis is the half voiding, because, by the time you get to full core voiding, hopefully you will have -- the reactor, after which time of course inherent reactivity effects like void reactivity are of no consequence whatsoever.

So, as soon as we identified this, we actually had a PIRT meeting right after the last time we briefed the Committee on ACR-700 in January, in which this came up.

And, out of that meeting, we did a number of calculations of checkerboard void reactivity. Now, the AECL design analysis that was presented to us for the pre-application review reported a full core void reactivity, that is all the coolant in all of the channel is voided, not checkerboard, of minus seven milli-K.

And this is based, it seems, on a tradition in Canada of analyzing traditional CANDUs that way. And it's probably appropriate to do that.

But, as I'll explain in a moment, the physics of checkerboard reactivity substantially different in this design.

And, it turns out the checkerboard void reactivity gives you positive effects. So, the results of our calculations were reasonably consistent with AECL's.

Analyzing similar cases, similar models of full core reactivity, roughly in agreement with them on the slightly negative full core void reactivity, in our calculations discovered that the checkerboard reactivity was positive.

And we did these calculations doing different models, different methods, different analysts, and got consistent results. So we're confident that we're correct in this assessment that -- positive that there is a positive checkerboard void reactivity.

Now, I should interject too that there is no such thing as pure checkerboard voiding. You get void fractions of maybe 90, 95-99 percent in the voided channels.

And the cool channels will have void fractions of a few percent. But, again, the insight from the PIRT was, rather than focus on full core void reactivity, let's find -- let's focus on another figure of merit that's relatively simple to define in calculating. That's the checkerboard void reactivity.

VICE-CHAIRMAN WALLIS: What's the pattern of this checkerboard? Is it just a like a checkerboard?

MR. CARLSON: It's exactly like a checkerboard.

VICE-CHAIRMAN WALLIS: Alternate channels run across the whole matrix?

MR. CARLSON: The whole face of the reactor core you have alternate channels with coolant coming at you and going back in the opposite direction.

VICE-CHAIRMAN WALLIS: It seems to me that can happen, there must be a lot of other modes besides perfect checkerboard.

MR. CARLSON: Just about all -- it happens over a large range of large break sizes and locations. The term checkerboard voiding is a reasonably good description of those patterns.

CHAIRPERSON GEOFFREY: The patterns come about because of the way the headers are set up?

MR. CARLSON: Yes.

MEMBER KRESS: It's a little bit of

surprise to me that that gives you positive, whereas the whole core gives -- voiding gives negative.

MR. CARLSON: Yes, it came as a discovery. Nobody really foresaw this.

MEMBER KRESS: Do you want to explain that to us?

MR. CARLSON: Yes.

MEMBER SHACK: Why wouldn't you be concerned about that in a conventional CANDU? It's got the same thing, right?

MR. CARLSON: Well, I could let AECL explain it. But I think it's fairly simple. In a conventional CANDU void reactivity effects are more linear.

So, if it is say 20 milli-K or 18 milli-K positive in a conventional for full core voiding than half core voiding, regardless of whether it is checkerboard or other pattern it's roughly half.

MEMBER KRESS: That's what I was going to guess. But there are reasons why it's not in this one.

MR. CARLSON: In this case it's not linear.

MEMBER KRESS: Obviously.

MR. CARLSON: That was the major insight.

Before I try to explain a little bit about the physics and other technical insights, I want to make a few key points.

First of all, as Belkys mentioned, this is a preliminary design. It evolved somewhat during the pre-application review and may evolve further. We'll see what AECL submits for design certification.

Another point worth mentioning -- and this is one area that distinguishes ACR from conventional CANDUs -- conventional CANDUs have a fuel temperature coefficient that is very small, essentially zero.

This design has a more negative Doppler fuel temperature coefficient. It's maybe a half to two thirds as strong as what we're used to in PWRs and BWRs.

But it's clearly negative. And so, the effects of fuel temperature, fuel heat-up, may tend to limit the power surge just by positive checkerboard voiding.

MEMBER KRESS: And the material design criteria, this is 11, is it?

MR. CARLSON: Yes.

MEMBER KRESS: It doesn't necessarily preclude a positive void coefficient?

MR. CARLSON: No, it does not. I mean,

coolant density effects figure into the assessment in relation to GDC11. But, one could approve a design with positive power coefficients based on GDC11 -- not positive power coefficient, power void reactivity.

GDC implies some power coefficient, and is considered to be met in light water reactors with the existence of negative Doppler and negative power coefficient.

And you might have that in a CANDU even though there is positive void reactivity.

MEMBER SIEBER: On the other hand, the Doppler is weaker than a standard light water reactor.

MR. CARLSON: Somewhat weaker.

MEMBER SIEBER: And so, an accident limited by Doppler in standard light water reactor Doppler may go a few milliseconds to a pulse power of 1,000 percent.

Maybe you would get more than that in this case.

MR. CARLSON: We are evolving a capability to do the transient analysis. Everything that I'm going to be presenting now, and everything we've done to date really is static calculations of K effective voided versus K effective cooled.

PARTICIPANT: That's what I was going to

ask you. This checkerboard -- you impose a checkerboard, and then you see what happens? Or do you let it evolve in a transient?

MR. CARLSON: So far the question confirm negative void reactivity. That was the pre-application --

PARTICIPANT: The next thing is, how does this checkerboard evolve, what does it do? The question is, how does it evolve, and what does it do?

MR. CARLSON: The question is, how does it evolve, and how does the overall transient play out when you consider the effects of void reactivity and Doppler reactivity.

And so we're evolving the capability to do that, so is AECL. I wouldn't describe ours or theirs for what I've sent to date as yet to the level of best estimate.

PARTICIPANT: Do you let the checkerboard evolve naturally as a sort of instability from a steadier situation, a more uniform situation? Or do you impose a checkerboard on someone?

MR. CARLSON: Well, it's -- the thermal hydraulicists hypothesize a break in size and location, and calculate the break flows. And it's a thermal hydraulic calculation.

PARTICIPANT: How does it checkerboard?

MR. CARLSON: The header breaks.

PARTICIPANT: And then it must be a checkerboard, no matter what, because the way the flow has to go.

MR. CARLSON: Or something above the header.

MEMBER RANSON: Is the coolant borated in this reactor?

MR. CARLSON: No, it is light water coolant. If you like, I have some back-up slides if you want to spend a minute reviewing what this design is in relation to conventional CANDUS.

MEMBER KRESS: I think we're okay.

MEMBER RANSON: Is there boric acid in it?

MR. CARLSON: Not in the coolant under some operating conditions they have very small amounts of boron or gallium in the moderator, not the coolant.

MEMBER KRESS: Okay, moving on.

MR. CARLSON: And that does have a positive effect on void reactivity. So, some of the technical insights. First of all, I think I mentioned this when we were talking to you in January, the void reactivity is a combination of large positive and large negative contributors.

MEMBER KRESS: Yes, when I see that it always scares me. Do you plan on doing an appropriate uncertainty analysis when you --

MR. CARLSON: Exactly.

MEMBER KRESS: Okay.

MR. CARLSON: I can talk about that as we go on. It is, because of that actually, non-linear with partial voiding. It can be positive during checkerboard voiding, even though it is negative for full voiding.

And it is sensitive to void distribution not only between channels, like checkerboard void reactivity, but within channels. You get different void reactivity, substantially different between stratified versus uniform density reduction within a channel.

And, again, it is sensitive to core design, operating parameters. For example, whether there is boron in the moderator. Burn-up effects, it is sensitive to some uncertainties, perhaps, in the fuel burn-up isotopics.

So, another important point to make is the confirmatory measurements of coolant void reactivity have never been done in operating CANDUs because it's inherently difficult and may not be done for ACR-700, although we are considering novel ways of doing it, say in an initial core.

But, AECL has not identified any plans to measure it in an ACR-700 operating core.

MEMBER KRESS: But you will do some critical experiments?

MR. CARLSON: Well, hence the importance of code validation based on benchmarks against critical experiments in zero power critical facilities.

MEMBER KRESS: And you can rely on your calculation tools.

MR. CARLSON: But those experiments have to be representative. And there -- AECL has identified some existing data from Italy, from Japan, from the UK.

And they are in the middle, or early stages of a rather extensive program using their ZED2 critical experiment facility at Chalk River, specifically aimed at validating void reactivity and other effects for ACR-700.

MEMBER POWERS: When you say that the measurement is inherently difficult, are you implying that, if I did the test, I would get data that were sufficiently scattered that I might not be able to use it for confirmation?

Or are you saying it's just extraordinarily difficult to get --

MR. CARLSON: It's a feasibility issue. Here we're talking about voiding of an entire channel. Actually, half the channel is in the court.

And there's a small number to measure there, right? The small void reactivity. Well, imagine voiding a channel -- voiding channels in existing power reactors.

It has never been done. Now, we have thoughts about how it could be done. But, you know, it's not cheap, it's not easy, and it's not --

MEMBER SIEBER: It would be easier in a CANDU than it would be in any other.

MEMBER POWERS: I guess what I'm asking is, suppose that I found a way to do it, would the data be sufficiently precise that I could arrive at a confirmation of my model?

Or would they be sufficiently scattered or replica tests that I might come up with, well, maybe it's okay?

MR. CARLSON: I think you're saying it's hard to get a clean experiment. And I think that's a valid observation. So, not have a clean experiment, you know, not having a clean measurement, because not an experimental facility, you could get scatter.

So, the bottom line is, we're looking at relying on critical experiments in CIE to validate this.

MEMBER POWERS: If it's just difficult, then that's one thing. But, if it's difficult and I'm not guaranteed to get my answer, then it's not worth pursuing.

MR. CARLSON: We could discuss this at length.

MEMBER KRESS: I'm certainly leaning towards the end of that spectrum that says you're not going to get good data.

MR. CARLSON: Yes, that's why -- a huge heroic effort. You come back a little bit like you kissed your sister, you know, it didn't leave you with a great deal of thrill.

MEMBER KRESS: I wouldn't know, I've never tried that.

MEMBER POWERS: I wouldn't know, I don't have a sister.

MR. CARLSON: It's an important observation. It's a significant observation. It's never been done for any operating CANDU to date. And they have reactivity issues in operating CANDUs.

It's positive -- strongly positive in operating CANDUs. So, the checkerboard void analysis requires, in our case, some changes to our methods and models.

And we're starting to implement those. And specific testing is part of the ZED2 test program. The specific experiments have to be done to address checkerboard void reactivity.

And the way they do validation traditionally needs to be modified because of the checkerboard void reactivity issues.

MEMBER POWERS: Have you looked at the consequences yet of the reactivity excursion during your checkerboard?

MR. CARLSON: I mentioned earlier that we're evolving a capability to do that based on parts coupled with trace. We're getting there. And we should be -- have some good progress on that in the next year or so.

MEMBER POWERS: I mean, can one do just like a back on the envelope -- give me some feel for -- like the amount of energy I put in?

MR. CARLSON: We've seen preliminary calculations from AECL on that. But I wouldn't regard them at a level that I would draw really good insight from that.

So, we're evolving so we have models that are of adequate quality that we can develop real insight.

MEMBER POWERS: I mean, if I put in two calories per gram, I'm not going to get too excited. If I put in 200 calories per gram then maybe my pulse rate -- it's a little better than kissing your sister in that case. That's a hot time in the old town tonight.

MR. CARLSON: Well, the problem is it is very sensitive to the magnitude of the coolant void reactivity.

MEMBER KRESS: But you could use that as a parameter. And I think you could handle the Doppler coefficient at a relatively simple way. You could probably d your calculation.

MR. CARLSON: It's very hard to a priori develop a point kinetics model that mean anything. You have to do a spatial kinetics.

MEMBER POWERS: I'm afraid it's like all these things. I can get you a number, but it's the tails of the distribution that count here. And they go up to the point that something unkind happens.

MEMBER KRESS: Right.

MR. CARLSON: They're very slim reads. Okay, let's talk a little bit more about physical impact --

MEMBER KRESS: This is why you get the checkerboard positive void coefficients?

MR. CARLSON: Not exactly. These are calculations done for full core void reactivity, very simple ones. But we're trying to understand where AECL, our CANDU are going, from where they've been.

And where they've been in conventional natural uranium, NU, natural uranium CANDUs to ACR-700. And so we did some simple calculations of the neutron spectral shift that happens upon voiding the coolant, 100 percent, not checkerboard.

And, in a conventional CANDU, this spectral shift fairly subtle. And I won't discuss it at length, although it does make for a interesting discussion.

The main point here is, for ACR-700, that coolant is very much a moderator also. So voiding it increases, really changes the spectrum very dramatically, and you get a great increase in the fast end epithermal region and a decrease in the thermal -- it's a slight softening.

And that's hard to talk about. It's easier to talk about if you do your calculations and then edit out what the four factor formula spectral contributors are to all this.

And we did it again for conventional CANDU and for the reference pre-application design, actually for a very simple case of a lattice of fresh fuel.

So this doesn't correspond exactly to irradiated fuel. But the trends, overall observations are valid here. In a conventional CANDU -- you see in the first two columns, we got something from the 1995 paper presented by Whitlock & Company from the AECL showing that -- what the spectrum components were of void reactivity in a conventional CANDU.

We did calculations with HELIOS 1.8 at Purdue University and got very similar results. The observation is that the positive void reactivity in a conventional CANDU is the summation of moderate positive contributors, the largest one being increase in residence escape probability with voiding.

Now, with ACR-700, for full voiding we see in the third column there that -- of large positives and large negatives. And, interestingly, what was formerly the strongest positive contributor in conventional CANDUs is now the strongest negative contributor, 72.4 milli-K negative.

Fast fission factors, high thermal utilization factors, high reproduction factors, small. And it all adds up to give you a few milli-K negative.

We then analyzed the case of 50 percent uniform voiding. We just did uniform density reduction in the coolant channels. And you see that the contributors are somewhat linear, but not perfectly.

And so, they all go down by half or a little more. And it sums up to actually a void reactivity that is more negative than full core voiding.

And then, for checkerboard voiding, the minor -- again, it deviates from linearity, but each of the contributors, but in a different way. And now it is positive, 3.5 plus.

And the biggest change has been in the residence escape probability. There are six point one milli-K right there, which counts for the difference by itself.

But there are other factors that balance it out. This has, of course, uncertainty implications. Uncertainty and contributors add up to big uncertainties in the small sum.

MEMBER KRESS: That's what worries you about that sort thing.

MR. CARLSON: And it gives you sensitivity -- just now that it's very sensitive -- to operating conditions to voiding patterns, etcetera.

MEMBER KRESS: But this, even if you do an uncertainty analysis to ensure that the range of the coefficient -- reactivity coefficient -- is not too big, for example, but still submitted a positive on one end, that doesn't preclude the acceptance of that, does it?

MR. CARLSON: Yes, I mean, we will analyze what it all means.

MEMBER KRESS: I guess the question is, how positive does it have to be before you really -- I guess it depends on the other power coefficients.

MR. CARLSON: Yes. I mean, to put whatever the source of the reactivity is in perspective, whether it is voiding or Doppler, one dollar, you know, the effective delayed neutron fraction, is about five milli-K in an equilibrium core.

So, these numbers are large in relation to a dollar. The reactor period is a strong function of how far over you are -- key effective -- in the prompt neutron lifetime.

So, in this reactor, the prompt neutron lifetime is ten times longer than what we're used to in conventional -- in light water reactors. But it is three times shorter than what you have in conventional CANDUs.

Again, you'd be able to balance it out when you have the parts coupled with trace and do some parametrics. So, that table was just for the simplified case of a lattice of fresh fuel bundles in both cases.

Then we proceeded to do some calculations taking into account burn-up for both uniform and mixed burn-up lattices. Because you have refueling from both ends, in the middle of the core you will have roughly similar burn-ups in neighboring channels.

At either end you will have very different burn-ups in neighboring channels. So we did cases with kind of a mid-core burn-up of 12.3 gigawatts per ton and then a checkerboard or a mixed burn-up of 1.6 and 24.4, which would be very much near the ends of the reactor.

These are simple two dimensional infinite array cases, but they provide good physical insight. It carries over quite nicely into three dimensions in some cases.

So, for the uniform burn-up, we get -- it doesn't -- we did the two voiding patterns, checkerboard one voiding pattern is we voided the lighter shaded channels.

And the checkerboard two voiding pattern in the second column there is where we voided the darker shades ones. So, of course, it doesn't matter in the case of uniform burn-up.

It's plus 4.7 milli-K for uniform burn-up. And full voiding is minus 3.4. For mixed burn-up it makes a great deal of difference whether you are at one end of the reactor versus the other.

Where you're voiding the higher burned fuel, then it is plus 65. milli-K. What this means then is, if you have positive void reactivity and a LOCA, the power-surge that happens will also have a significant axial tilt.

This 6.5 end of the reactor will be more reactive than the other. But the whole reactor probably will go up and might tend to be turned abound by Doppler.

So, the main conclusions on this focus topic was that the reviewed preliminary nuclear design of ACR-700 does not have negative void reactivity in large LOCAS.

As we mentioned, the design changes could be made to reduce LOCA void reactivity. Those design changes would involve increasing dysprosium and enrichment in the fuel design.

MEMBER KRESS: Some things that probably don't want to do?

MR. CARLSON: Well, Alaska AECL. And, again, very important, CDR bias and uncertainties are potentially large in relation to nominal values. And, AECL's ongoing experimental work, particularly at ZED-2, but also their fuel irradiations, and isotopic assays that will come out that will be important benchmarks for quantifying -- and uncertainty.

MEMBER ROSEN: What can you say about the effect the large LOCAS and negative void reactivity as a function of power? In other words, compare two cases, a full power case and a zero power case.

MR. CARLSON: Well, are we talking strictly about void reactivity? Void reactivity seems to be a fairly weak function of fuel temperature.

And, low power to a neutronics person means lower fuel temperature.

MEMBER DENNING: But, in -- the tangent makes a lot of difference where the power levy --

MR. CARLSON: Oh, I see what you're saying, yes. And, yes, that's one of those things we'll have to analyze with a transient analysis capability, like we're developing with Park's.

You can't really do it with static calculations and draw meaningful conclusions.

MEMBER ROSEN: Well, my experience with positive coefficients, in the case I know of, moderate temperature coefficients, reactors -- PWRs these days are often designed with positive moderator temperature coefficients.

But they only are so for part of the cycle, usually up to mid-cycle, and usually only at very low power. So, I was wondering if there's any sensitivity like that here, certainly not -- there's no boron in these reactors, so it's not the same.

MR. CARLSON: Well, only the moderator under some conditions.

MEMBER ROSEN: Yes, under some conditions. But there's no sensitivity in power --

MR. CARLSON: Well, it's not no sensitivity, but it's not a strong sensitivity.

MEMBER ROSEN: Okay.

MEMBER DENNING: A comment on the uncertainties, and that is that, you know, I think even today you could have done a fairly simplistic uncertainty analysis.

Obviously it depends upon state of knowledge to what you do. And I think it would be interesting to see that. And, obviously, as time goes on people would be able to do that better.

But I think, as good practice, we ought to really try to look at the uncertainties on these numbers, because we could be -- I mean, all of those cases might be positive.

Or all of those cases may be negative, as my guess based upon the realistic assessment of uncertainties.

MR. CARLSON: I think you're leading into my next slide. We actually do have -- the path forward is we're going to continue trying to develop our analysis capability and, of course, in parallel review analyses of these transients by AECL.

But, our capability involves modifying the Park's code and coupling it with a suitable trace model of ACR-700 and MELCOR where needed for simulating operations and accidents, including the combined effects of void and Doppler reactivity, and including parametric sensitivities on uncertainties or biases in void reactivity and other effects.

But the second bullet here is interactions with AECL to assess the applicability and adequacy of the existing and planned sets of experiments for validating code predictions of void reactivity and other effects, and to provide timely identification in gaps in what they're trying to do with those two experiments in their fuel irradiations.

And we'll be doing that using state-of-the-art methods that the research has developed over the past eight years or so in the code modules called Scale Tsunami.

They are sensitivity uncertainty analysis methods based on generalized perturbation theory to join solutions to the transport equation. And that type of approach, I think, is a sophisticated and very useful way of doing what you're talking about.

MEMBER KRESS: I think we need to move on. Thank you very much. Now I think we're going to hear from the AECL.

MR. ARCHINOFF: Good afternoon. My name is Glenn Archinoff. I'm the ACR Licensing Manager with AECL Technologies. I'd like to thank the Committee for giving us the opportunity to say a few words here today.

Before I begin, just let me introduce the other folks that are here as well. John Paulson is the President of AECL Technologies right there. Victor Snell is the Director of Safety and Licensing.

Peter Boczar is the Director of Reactor Core Technology. And Ben Rouben is the ACR Physics Manager. Robert Yan, ACR Licensing, and Kyle Reed from Bechtel is here with us as well.

I'm going to start with a very brief presentation discussing the pre-application phase, just a very brief overview. Belkys has pretty much covered what I was going to say.

So I'm going to be very brief. And then we'll get to Peter, who's going to talk about the work that we're doing to improve our reactor physics methods.

And then we'll continue the discussion on coolant void reactivity. The objective that AECL Technologies had for the pre-application phase was essentially to determine if the design of the ACR-700 could be certified within the U.S. Regulatory framework in a reasonably timely manner.

There were two particular areas of emphasis. We know that some parts of the regulatory framework aren't really a good fit with the underlying CANDU design.

So we need to see how that was going to work out. Another aspect was that NRC Staff -- or not all NRC Staff are familiar with the underlying technology.

And we knew it was going to take time for Staff to come up to speed. And Belkys covered the activities in the two phases of the pre-application phase.

She mentioned the focus topics. So I won't go over those. But, just to get to where we are now, as we come to the end of phase two of the pre-application.

We believe that the main objective of pre-application, in fact, has been met. Our view is that the certification of the ACR-700 design within the regulatory framework is feasible.

Belkys talked already about CANDU specific aspects, where the regulations just don't fit or don't exist. And we would apply Canadian requirements.

And we believe we will be able to show that they meet the intent of U.S. regulations. There was a tremendous amount of interaction with NRC Staff during pre-application phase.

Something like 34 formal deliverables over 300 additional documents were submitted. 23 in-depth technical meetings were held, a lot of interaction.

And we believe Staff are now quite familiar with the technology. And so, that would facilitate a timely design certification process. Now, of course, there are still issues to address.

And Belkys has discussed some, and Don discussed some as well. And so, that will be the focus of our next phase, which we call the transition phase, which will be from now until the time we actually submit the application.

Our objective for that phase is to make sure that we have high confidence that the certification application we submit will be acceptable to NRC.

And, for this phase, we've identified a smaller set of focus topics. Right at the top of the list there, reactor physics codes and coolant void reactivity, but a number of other ones as well.

And, once we have received the pre-application safety assessment report, there may be other focus topics, depending on what's in it, and depending on our further discussions with NRC Staff.

So, that's a really quick summary of what we feel was achieved in the pre-application phase to date, and where we intend to go from here. If there are any questions on that, I'd be happy to take them.

But, otherwise, we could move on to the other presentations.

MEMBER RANSOM: What is the status of the certification in Canada?

MR. ARCHINOFF: There isn't an analogous formal certification process in Canada. What's happening in Canada is what we call a license ability review, where the CNSC is reviewing pretty much the same material that we've given to NRC for the purpose of making the determination of whether they think the design will be licensable.

That will culminate essentially in a letter, identifying if there are any major concerns or impediments to licensing. So, it's analogous to pre-application, but it's not as formal.

MEMBER RANSOM: What do you mean by licensable? Licensable in Canada?

MR. ARCHINOFF: Canada, yes.

MEMBER KRESS: This question may be out of line, but do you intend to build one of these in the U.S. at a U.S. site? Or are there other reasons for -- there are other reasons for certification I know.

MR. ARCHINOFF: Yes, our hope is that one of these will be built -- maybe more than one, maybe a whole bunch will be built in the U.S.

MEMBER POWERS: I'm pretty sure they're counting on 25 in Tennessee.

MEMBER KRESS: UVA will buy anything.

MR. ARCHINOFF: I'm going to turn it over to Peter Boczar now.

MR. BOCZAR: Thank you. Good afternoon ladies and gentlemen. It's a pleasure to be here. I have responsibility for physics and fuel in AECL. Given that these are two of the focus topics, I have an interesting life.

I'm going to talk about physics in this one. Just a very, very short overview presentation to give you an idea of where we are and where we are going with respect to the physics tools that we're using.

After me, Ben Rouben will describe some details of the actual LOCA analysis in response to some of the earlier questions that you had. In terms of our current tool set, it's based on three stages to the calculation, a lattice calculation using WIMS 2D transfer code, a multi-group transport calculation, condense the two energy groups averaged over the cell.

There are some devices in the core that are vertical in the reactor, they are perpendicular to the fuel channel, so they're not normally represented in the lattice calculation, in the 2D calculation.

So we use a 3D transport calculation to represent those effects. And then those shell average cross sections are used in the reactor calculations RFSP two group diffusion theory.

This code does a number of different kinds of calculations, time average calculation, refueling simulations, the day-to-day fuel management calculations, xenon transients, kinetics calculations.

And our kinetics calculations include thermal hydraulic feedback in accident analysis such as LOCA. An important part of the tool set is MCNP. There are obvious limitations to the reference tool set.

We will use MCNP to benchmark the reference calculations, determine the uncertainties, the applicability of the analysis approach.

MEMBER KRESS: Would that be equivalent to the park's code?

MR. BOCZAR: No, MCNP is a -- sorry, MCNP is a fundamental, theoretically rigorous code. There are no approximations in MCNP. It's a Monte Carlo simulation.

So it's only limited by the detail in your modeling and the nuclear data. So it's used as a numeric benchmark.

VICE-CHAIRMAN WALLIS: Well, there's an approximation in treating nuclearized -- of their spheres or something. I mean, at some level there's approximation.

It's not exact model of anything. The level of approximation is normally acceptable.

MR. BOCZAR: Yes. It is as accurate a calculation as one can achieve.

MEMBER DENNING: But, of course, there's this statistical uncertainty associated with the Monte Carlo element of it.

MR. BOCZAR: Yes, of course, there's a statistical uncertainty which one can address by --

MEMBER DENNING: If you wanted to know.

MR. BOCZAR: It's used by Los Alamos for the things they do there.

MEMBER RANSOM: Has there been any comparison between like RFSP and the Park's code that you've heard about?

MR. BOCZAR: TO date we haven't undertaken comparisons of our toolset with Park's. We've done comparisons with -- namely with MCNP, because any other codes that has approximations compared to MCNP.

Now, as we go forward, as you'll see, we will be engaging independent assessments of the adequacy of the analysis. So, of course, we're keenly interested in the accuracy of the code, the suitability of our modeling.

And assessment of the tools has been an important part right from the onset. The key ACR physics phenomena that we have here compared to the current CANDU, there's a tighter neutronic coupling between adjacent lattice cells, because the lattice pitch or the separation between adjacent channels has been reduced from 28 centimeters to 21 centimeters.

There can be greater heterogeneity between adjacent cells. And that's not necessarily the case for normal operating conditions. But there are scenarios such as checkerboard voiding where there is greater heterogeneity between adjacent channels.

And that has to be accounted for. Leakage tends to be greater as well. Our assessment to date is that the toolset is adequate for most applications.

So, for normal refueling, for the normal design calculations, the toolset is adequate. There are enhancements that are desired for certain heterogeneous configurations.

And this is alluded to in Don's

presentation as well. So, speaking to those enhancements then, I mentioned the three parts of the calculation.

The first part, the fundamental part, is the lattice calculation. Normally we model a single lattice cell in isolation. So, this might be what one normally models in isolation of -- the assumption is an infinite lattice of that cell and the effects of the adjacent cells is normally accounted for by some sort of leakage correction.

MEMBER POWERS: Just use a mere boundary condition or something like that?

MR. BOCZAR: Something like that, yes. So we've just released a beta version of WIMS, which has considerable enhancements, considerable theoretical improvements over the version of the code that we've been using till now.

It has an improved residence treatment, a more detailed geometrical representation. So, for example, we can represent explicitly a bundle that, for some obscure reason, sits at the bottom of the fuel channel, rather than concentrically suspended in the middle of the fuel channel.

We are putting in place what we call this multi-cell capability where, instead of just modeling one cell in isolation, we can model -- in this case I have shown a two-by-two checkerboard where the properties of one cell might be different from the others.

So, this cell is cooled, for example, and the adjacent neighboring cells, if we reflect this, are voided. So, in doing this, one can explicitly model the effect of the environment on the properties of the cell of interest.

VICE-CHAIRMAN WALLIS: That's easier than the problem where it's partially voided and you don't know where the water is.

MR. BOCZAR: We can also model -- the assumption here is that we do know where the water is. So, when we do a couple RFSP ATHENA transient calculation, we get feedback from ATHENA as to the voiding.

VICE-CHAIRMAN WALLIS: And mostly a sort of annular flow where liquid films on the walls. Is that what you have most of the time?

MR. BOCZAR: It's -- the blow down happens very quickly, within about a second for the voided channel. And the void distribution, we believe, is fairly uniform because of the high turbulence.

Okay, so we believe this capability by

itself will be sufficient to address most of the issues that we have encountered. In terms of the whole reactor code, the RFSP calculation, we've developed an improved treatment of burn-up, which we call micro-depletion.

So this modeling takes into account the local history of the fuel at that point in time. And, the local conditions on the history, so the coolant density, the fuel temperature.

And we're also adding specific enhancements to address heterogeneity between adjacent cells, so, to be able to use this information from the last calculation and the full core calculation.

And it's this enhanced toolset that will be used for the DCD, for the analysis that supports the DCD. And we'll be validating this toolset, of course.

MEMBER ROSEN: Hold on for a minute. It occurs to me that, if you're thinking about fuel depletion and using the exact state of a CANFLEX module in a calculation, this is different than a light water reactor in this country because these CANFLEX modules move along the channel during the course of their --

MR. BOCZAR: Yes.

MEMBER ROSEN: -- in reactor times. So, it's not like a fuel assembly in a PWR, for example, where you put it and it stays put.

MR. BOCZAR: Yes.

MEMBER ROSEN: This one changes not only because of the burn-up and flow changes, perhaps --

MR. BOCZAR: Yes.

MEMBER ROSEN: -- but because it moves.

MR. BOCZAR: Right.

MEMBER ROSEN: And so, you have to keep track of all of that.

MR. BOCZAR: Exactly. So that's what RFSP does.

MEMBER ROSEN: I see.

MR. BOCZAR: It simulates the actual movement of fuel in the channel as a result of -- the main thing is refueling. And, of course, it models the effect of depletion and isotopic changes.

And it reflects the actual local environment and the history.

MEMBER ROSEN: So, when you start a transient in a given instant, where all these channels -- what was it, a dozen assemblies per channel?

MR. BOCZAR: Yes. There are twelve bundles per channel.

MEMBER ROSEN: Which all have moved and, you know, so you've got this huge array of 156 channels, or whatever it is, with 12 assemblies per channel, all of which have moved and have a history.

MR. BOCZAR: Yes. And, typically, if you look at eight channels, suppose this is a channel and you start refueling at this end, and the ACR-700 is a two bundle shift.

So you add two bundles at one of the channel. So, at one end of the channel the fuel I relatively fresh, it's relatively new. And, as that fuel gets moved down the channel with a result of subsequent refueling, you know, it burns up.

So, the fuel at the other end of the channel is depleted. So, the fuel management simulation, RFSP, accounts for that. So, our analysis approach, we use WIMS 3.0.

We'll be incorporating enhancements to RFSP to reflect the environment. We'll supplementing that specific analysis with MCNP analysis to get a better handle of the calculation uncertainties.

And, of course, I mean, you can look at the calculational uncertainties. But there's only one way to find out what reality is. And that's to measure it.

So, the foundation of our qualification is based on measurements, experiments, cold, clean, critical experiments as Don mentioned in his ZED-2 facility at Chalk River.

And this is a very, very flexible facility. We'll be measuring everything that moves. So we'll be measuring the effects of checkerboard voiding.

We'll be measuring the effects of partial voiding. We'll be measuring the effects of different burn-up distributions, using fuel and using simulated burned-up fuel.

We'll be measuring temperature coefficients, all the reactivity coefficients. And with that, the whole intent there is that, for each of the -- parameters, we'll establish a bias and an uncertainty.

Then that bias and uncertainty will be reflected in the safety an licensing analysis. Don mentioned other critical facilities. We'll be getting some information from NRU irradiations.

So, for example, information on depletion of the fuel. We have dysprosium as a neutron absorber, which is unique, in our reactor. We'll be getting validation data for that depletion from NRU irradiations.

MCNP for filling in the gaps, for scaling, for extrapolation, from ZED-2 conditions to reactor power conditions. And I mentioned previously that we will be engaging independent assessments to confirm the adequacy of both the modeling and the adequacy of our qualification.

We believe that the series of experiments we have planned at ZED-2 are fully adequate and sufficient to validate the toolset. But we'll get independent confirmation of that. And these are the conclusions.

VICE-CHAIRMAN WALLIS: When you say it's not adequate, do you have some criteria about how accurate it needs to be?

MEMBER APOSTOLAKIS: Come on.

MR. BOCZAR: That's really, in my view an iteration --

MEMBER APOSTOLAKIS: This is the physics. It's not thermal hydraulics. This is science.

VICE-CHAIRMAN WALLIS: I don't care if it's the size of fork for lifting manure, it's still got to be adequate on some basis.

MR. BOCZAR: The final basis is the safety analysis. We have to show that, with the uncertainties and the biases that we have the margins that we believe we have.

So, it's hard to establish what the acceptance criteria is a priori and in isolation of the subsequent use of that information in a safety analysis.

MEMBER DENNING: Isn't reactivity coefficients your ability to predict reactivity coefficients that's critical to us from a safety viewpoint, as opposed to fuel depletion or things like that, which we don't care about?

MEMBER APOSTOLAKIS: Then we'll use multi-group theory.

MR. BOCZAR: We use --

MEMBER DENNING: No, but isn't that the criteria? Your ability to accurately give us credibility in the reactivity coefficients that you calculate theoretically.

MR. BOCZAR: The reactivity coefficients are certainly important. But, the process we follow I think is very similar to the U.S., where, for each of the important accidents, we define the phenomena.

And, for each of those phenomena, the important contributors to those phenomena from each of the disciplines. So, in physics, the reactivity coefficients are obviously a very important parameter.

But, the ability to measure power is another important parameter, so that you control the compliance with bundle power and channel power limits.

So, reactivity coefficient are important, but there are other things too. Your ability to calculate the depletion of dysprosium will impact on the accuracy of your void reactivity calculations.

MEMBER APOSTOLAKIS: No, but I think the point of the comment was that we are reviewing safety here. So, the purpose of your presentation, as far as we are concerned, is the reactivity coefficients.

MR. BOCZAR: Well, those parameters that impact safety --

MEMBER APOSTOLAKIS: I mean other things are for different things.

VICE-CHAIRMAN WALLIS: And the question that would be -- what's the risk of being wrong in those coefficients? I mean, what's the uncertainty? Is it a very low probability that you'll exceed some criteria and, whatever?

MEMBER APOSTOLAKIS: I think so. That's when the parameters come into the picture, so much more uncertain.

MR. BOCZAR: We establish the --

MEMBER APOSTOLAKIS: The least of your worries should be the calculation of the reactivity coefficient.

VICE-CHAIRMAN WALLIS: Well, probably this is a much more certain area than many other areas we get into.

MEMBER KRESS: When it comes to reactivity insertion accidents, can they revert back to the old criteria or acceptability, because this is almost fresh fuel and it has 25K burn-up, mostly.

MEMBER POWERS: In the end, the old criteria really is a pellet clad interaction criterion. And their clad collapses down --

MEMBER KRESS: It's already collapsed on to the --

MEMBER POWERS: -- onto the fuel. So I can't imagine the mechanics are anywhere near alike.

MEMBER KRESS: Do we need to do reactivity insertion tests for this kind of fuel?

MEMBER POWERS: Well that's -- I mean, the issue is one of magnitude here. And, before I started asserting a need to look at pellet clad interactions in this configuration -- and it is a little softer fuel on top of that.

You need to get this magnitude issue down.

It's less bothersome here because you're talking about a voided channel. And so, what are you going to disperse your fuel onto, a cooled zirconium clad? I mean, it's not quite the same issue.

MEMBER KRESS: It's not the same issue.

MEMBER POWERS: It's a different issue here. So, I think you really -- that is why I was anxious to know whether we are working with a two calorie problem or a 200 calorie problem, because my reaction to them are completely different.

We did look at source term consequences of having a reactivity insertion felt like you're just not in the same league with a little diffusion release.

MR. BOCZAR: I think that's a perfect segway into the next presentation. Ben Rouben is the manager of the -- one of the two physics branches at AECL.

Is the manager of the Physics branch at Sheridan Park, and he's also the ACR Physics Manager.

MR. ROUBEN: Good afternoon. I have a short presentation to pursue the question of void reactivity. Now, for the ACR-700, the choice of the void reactivity was made to provide a good balance of nuclear safety or nuclear protection between one kind of accident, the LOCAS, and another category of accidents, the fast cool down accidents.

And so, one requirement which stands from that objective of balance was to ensure that the power transient in any design basis accident would be mild before the tripping of the reactor, before all accidents.

Just to repeat what was said before, in the ACR-700 the design of the coolant system is two passes in a figure of eight so that, in adjacent channels, the coolant is flowing in opposite directions.

And, if we have large loss of coolant, which would void a lot of channels, nonetheless, one pass will generally void faster than the other. And that is what is called checkerboard void reactivity, because the density in all the channels going in one Director is different from the density of the coolant in the other channels.

This checkerboard void reactivity gives rise to non-linear effects, as Don Carlson mentioned. And so, the reactivity that you would get from 50 percent voiding by voiding one pass is certainly not the same as you would get by voiding 50 percent of all channels.

And Don demonstrated that. The point that I would like to make, though, is the extreme case when one talks about checkerboard voiding, one often thinks of complete voiding in one pass and complete full coolant density in the other.

That's not really a physical occurrence, because you cannot lose all the coolant in one pass instantaneously. Now, our LOCA analysis is done by calculating coolant densities with a thermal hydraulics code.

And we do that with ATHENA. And then we input those coolant densities into a kinetics code. The kinetics code is RFSB ISD, as Peter mentioned. It has a kinetics capability.

And, generally speaking, the coolant densities -- the coolant density transients are a function of the pass, of the channel, and even actually within the channel.

All that information is passed on from ATHENA to RFSB. I'm showing in the next two slides a particular case. I'm showing the system reactivity and the resulting core power transient from a large break, a reactor outlet header break, a 100 percent break, which should give a large value of void reactivity because the coolant is lost very quickly in the 100 percent break.

So, in the next slides, -- what happened? There should be -- there they are. This slide shows the reactivity as a function of time for the first three seconds for this 100 percent outlet header break.

The reactivities here were not calculated with RFSB. But they were calculated with MCNP using a full court model of the reactor and using the densities as provided by ATHENA to this full core model.

Okay, so this is the best calculation of the reactivity versus time using the actual densities from ATHENA.

MEMBER KRESS: Those look like the height that I used.

MR. ROUBEN: Oh yes.

MEMBER KRESS: Are they calculated here?

MR. ROUBEN: Well, the difference is --

MEMBER KRESS: Oh, this is the whole reactivity. I see.

MR. ROUBEN: This is the entire area.

MEMBER KRESS: I'm sorry. It's not just the void, it's the whole reactivity.

MR. ROUBEN: No. And it starts out

negative because the first phenomenon is voiding and also leakage of neutrons. And that starts out with a negative reactivity for a few tenths of a second.

Then the checkerboard voiding phenomenon comes in as the difference in density between the passes takes over. And so, the reactivity does go positive around one second and reaches about 1.4 milli-K.

Now, this whole calculation was done without the shut-down system action. So, there was an assumption in the calculation that the shut-down system didn't act.

In actuality, the shut-down system would be tripped around .7 seconds. The trip time would be about .4 seconds or so. And so, just the delays in the circuits, in the electronics, would actuate the shut-down system at .7 seconds, and the shut-off rods would enter the core around one and a half seconds.

But, again, this whole calculation is just for the assumption of the voiding without shut-down systems.

MEMBER POWERS: Just to make sure I know what I'm looking at here, the blue squares represent some average over time of the voiding that's predicted by ATHENA?

MR. ROUBEN: No, it's not average, it's instantaneous values.

MEMBER POWERS: Okay. So, but I guess what bothers me is, if they are instantaneous values, I would have assumed that I would have seen large fluctuations in the curve, rather than a very smooth curve. Am I just looking at connected points?

MR. ROUBEN: These are just connected points, yes.

MEMBER POWERS: So, in between the dots there were no calculations done. That's just a curve for the eye there. Is that correct?.

MR. ROUBEN: Yes. ATHENA does the calculation for the entire time. But we picked certain --

MEMBER POWERS: Points and then you did your MCNP calculations for --

MR. ROUBEN: That is correct.

MEMBER POWERS: If we -- if you had done things more densely, would we have seen a lot of variation between the points, or is it relatively smooth in there?

MR. ROUBEN: I would think it's relatively smooth.

MEMBER DENNING: Is the height of the box one sigma? It looks like the height of the boxes are different. Is that one sigma in the MCNP?

MR. ROUBEN: No, this is just the --

MEMBER DENNING: That's just the box. You don't have the -- how big is the MCNP.

MR. ROUBEN: It would be about .2, .3 milli-K or so. So, not far from the height of the box, but I would say it's about .2 or .3. You can reduce that, of course, by increasing the number of histories.

These histories were done with about 30 million histories in these calculations. These results are preliminary in the sense that the ATHENA transient here was calculated assuming constant power.

Now, we took this reactivity curve and we put it into a point kinetics calculation. So, the power decreases for the better part of a second. And then it does go above one as the checkerboard voiding reactivity becomes positive.

But, the transient is self-limiting. And, after a few seconds, will come down.

VICE-CHAIRMAN WALLIS: One would be very careful about this plot because a novice examiner might get the impression that the shut-down system caused the transient.

MR. ROUBEN: The calculation was done without shut-down system. So, the power transient was self-limiting. And, again, if the shut-off valves were actuated at .7, they would come in around here at 1.5 seconds.

And they would cut off this peak even more.

VICE-CHAIRMAN WALLIS: Well, I think it is probably true. But it would be nice if you could see it going on for a bit longer so we know it doesn't come up again.

MR. ROUBEN: Definitely -- I don't have these numbers here, but when we do the full analysis, we go beyond three seconds.

VICE-CHAIRMAN WALLIS: You'll go beyond three seconds, a bit more beyond the peak to make sure it's not coming up again.

MR. ROUBEN: The thermal hydraulics calculation goes a long way. The physics LOCA calculation goes to a few seconds.

VICE-CHAIRMAN WALLIS: And then you're happy?

MR. ROUBEN: Yes.

MEMBER DENNING: But there are a couple of full power seconds potentially in there. What's the enthalpy of the fuel? Did you look and see what the enthalpy is in the fuel at this point?

MR. ROUBEN: I don't have the numbers with me. This would, of course, be reduced a lot with the shut-down system. So, it would be even less than a couple of full power --

MEMBER KRESS: The core is still voiding there? I mean, there is significant flow in the core to cool -- to take heat out of the bins in that period?

What is the thermal power? Is it about three times the 700?

MR ROUBEN: Of the ACR?

MEMBER KRESS: Yes.

MR. ROUBEN: It's around 1,950 or something.

MEMBER KRESS: So multiply that by two seconds and -- I don't know what the MCNP is, but you can get some idea.

VICE-CHAIRMAN WALLIS: But we don't know when it comes down. It' still up at 1.2. At the end of the graph it may go on for ten seconds. We don't know the integral on that.

MEMBER KRESS: Right, I think if you continued that it would repeat itself, if you continue, wouldn't it?

MR. ROUBEN: Well, it would certainly be arrested very quickly --

MEMBER KRESS: Yes, if you put the rod. That's what I was counting on, the rods going in.

MEMBER FORD: If I could interject, for a licensing calculation, we would certainly credit the shut-down system action.

MEMBER KRESS: Certainly.

MEMBER FORD: This is to help understand what's going on. This is to help understand the phenomena.

MEMBER KRESS: Right.

MR. ROUBEN: The safety analysis would credit the shut-down system.

MEMBER KRESS: Sure.

MR. ROUBEN: In terms of conclusions, I just wanted to say that MCNP, being the best calculation we can find, has given us a good handle on the physics of checkerboard voiding.

And, as far as our other tools, as Peter mentioned, we are working to further develop the capabilities, especially for checkerboard voiding, generally for heterogeneous, but the most important one being checkerboard voiding.

So, we are developing methods to cater to the heterogeneous in RFSP, for instance. And, the effect of the checkerboard voiding, as we saw here, is a mild power transient, which is self limiting and turns over, even without a shut-down system.

MEMBER KRESS: Thank you. Well, the Staff, I think, is expecting a letter from us. And I think the nature of the letter will be some sort of comment on your -- the job you did with the SAR.

Perhaps I would like to, in the letter, identify what I would at this time call focus topics for ACRS review. Maybe that would help. I guess we can turn it back to you, Chairman.

CHAIRPERSON GEOFFREY: Okay. Thank you very much for the presentation. The next presentation we have is on the GSI-185. Before we get to that, we have clearly schedule problem.

We are running over two hours late. And we need to get to the letter before close of day, because, otherwise it will not have information on what to put in the letter.

And it he only has tomorrow available with us. He's not going to be here on Saturday. So, the problem we are having is that I need to stop the presentations at six p.m.

We need at least one hour to work on, which means the next two presentations have to be within time. I have to depend on you to control time within one and a half hour.

You have it on the agenda, but please make an effort. We need to really be able to get to the letter by six p.m. That also means that that puts into question a break.

Do you want a break? But then you'll have to eat some other break for your presentation. You have to be tough. All right. So let's take a break until ten after three, and then start with the next item on the agenda. So, please be here at ten after three.

(Whereupon, the above-entitled matter went off the record at 2:56 p.m. and went back on the record at 3:10 p.m.)

CHAIRMAN BONACA: Back in session. The next item on the agenda that we're going to cover is GSI-185, and Vic is going to lead us through that.

MEMBER RANSOM: The concern about the issue of warm dilution dates back quite a ways to like 1995, and --

MEMBER SHACK: Strictly newcomer.

MEMBER RANSOM: Right. The current

general safety issue 185 was established in roughly 1999 as a request from NRR, and then RAS performed a prioritization study, support of establishing general safety issue 185, which is titled "Control of Recriticality Following Small Break LOCAS in PWRs", and both the prioritization study and everything that had been done before assumed no mixing between deborated water and the steam generator, and the borated water in the reactor vessel. And this led to some concern about the power that might be deposited in the fuel and possibility of fuel damage.

Subsequent to that, RAS has conducted research to improve the mixing ability, and also the neutronic capability calculating the core power. These were the two elements that were key to potentially resolving this issue. And the staff and our contractors met with our Thermohydraulic Subcommittee in 2002 twice, and also twice this year to review the details of the research and the results of system simulation, mixing core neutronics and the probability considerations for the occurrence of these events, and as a result of these meetings and the documented research contained in a draft NUREG report, it was the consensus of the committee that this should be brought to full ACRS, and that's where we are today. So with that, I'll turn it over to Jack.

MR. ROSENTHAL: I'm Jack Rosenthal. I'm the Branch Chief of the Safety Margins and Systems Analysis Branch in the Office of Research. So we're talking about GSI-185, which is a boron dilution scenario, and it's a pressurized water reactor issue in which one postulates that one's had a small break LOCA. There has been some time when you're in a reflux cooling mode. You're essentially discerning boron, boron in the primary system and it was in this case of loop seal.

You have to form a diluted slug. Mr. di Marzo will be talking more about slug formation in a few minutes. You have to somehow transport that diluted slug into the primary system, and you can do that either by the start of natural recirculation, or by the operator's turning on reactor cooling pump. And then we asked ourselves the question, if you form a slug and you transport the slug, will there be a recriticality, and will that recriticality form teal damage.

I want to draw the distinction; there's a fair amount of work going on in Europe on the issue. And there's a fair amount of work that we did on the issue. It was focused on the thermohydraulics of the issue, and only recently -- actually, I think I have to give credit maybe most to Professor di Marzo for saying wait a minute, it's not simply a thermohydraulic issue. The real issue is I'm going to bust up fuel, if you have the event. So we really do five components; the probabalistic risk assessment, there's systems analysis - things like just by looking at the size of piping, size of the loop seals, mixing transport analysis, a really very simplistic RELAP model - just enough to drive the PARCS code.

Research made an investment in building a 3D space time kinetics capability, and this is an application where the ability to do that sort of analysis is paying off. It's more realistic than point kinetics. And last is a fair amount of fuel work that we've also done, so we see for this somewhat simplistic problem, it really is a very multi-disciplinary problem, where we're taking advantage of work that was done in prior years in Maryland, and in Germany, PKL, the development of PARCS at Perdue as 3D kinetics model, in this case coupled to relap, but we also couple it to TRACE, the same code. Some code work that we did at Kurchatov that gives us confidence that we know how to do stuff. And then all the work that we did on reactivity insertion events gives us a contemporaneous idea of what the fuel failures might be, so it's quite an integral program.

With the Subcommittee, I did the introduction, then David did a lot of the -- Dave Besette, the Systems work, and we decided just in the interest of time that I would speak quickly, and then we'd go on. So what's the probability?

If you have a large break LOCA, you depressurize, the event is over, so you need a small break LOCA. And, in fact, you need a small LOCA, which you can get by a pipe break or opening a valve and leaving the valve open. And the small break LOCA alone isn't going to cause this event. You have to fail ECCS; either you have a hardware failure or the operators turn it off, in order to get in a condition in which you're distilling water. And so what I want to leave you with the idea is that this is a subset of all small LOCAs, and not equal to LOCA for --

MEMBER POWERS: Well, it's a trivial subset because everything you set up there is exactly TMI.

MR. ROSENTHAL: It is TMI.

CHAIRMAN BONACA: But the PORV, that includes the assumption that the operator doesn't realize that he has a stuck-open PORV.

MR. ROSENTHAL: Later on I'm going to show you a slide with 10 to the minus 7 on it, and --

CHAIRMAN BONACA: I'm trying to understand this 2 to the minus 3.

MR. ROSENTHAL: And Dana is right, that I'm describing Three Mile Island. And, in fact, we discussed that at the subcommittee with Dr. Wallis, that in fact this sounds like TMI, so it's hard to deny that it could never happen.

CHAIRMAN BONACA: Well, TMI the number wasn't 10 to the minus 3, it was 1 in 50, I mean.

MR. ROSENTHAL: Just the probability of a stuck-open valve.

CHAIRMAN BONACA: Yes. All right.

MR. ROSENTHAL: Okay. Now I'm starting to repeat myself. In order to get into this scenario, and I'm describing TMI, you'd have to have a condition with a small break LOCA. You interrupt high pressure injection. You then terminate the small break LOCA somehow, and HPSI is off for a period of time.

We know from the difficulty of conducting the experiments at PKL and at Maryland that, in fact, it's somewhat difficult to form a slug, and it would take at least an hour to form a slug, which is time for action to take place. And, in fact, the experimenters have difficulty running an experiments that formed this nice slug and held it where they wanted it until they wanted to move it. Well, when you look at this, you say the best prospect of this happening is a stuck-open PORV with a terminated and restarted HPSI.

MEMBER POWERS: It seems to me that this is not very difficult to do at all, in the simple sense that that's exactly what happened.

MR. ROSENTHAL: They went into reflux cooling for some period of time.

MEMBER POWERS: A long period of time.

MR. ROSENTHAL: Well, this is what drives it. You're going to hear a deterministic argument in a couple of minutes, but I'm just setting the stage for where we perceive it in terms of probability.

MEMBER POWERS: Yes, but I'm having troubles with the probabalistic statement. I'm looking at it this way - if it's happened once, then surely the probability must be extraordinarily high that it will happen relative to things like 10 to the minus 4, and 10 to the minus 5. It's relatively -- since it has happened once in 2000 reactor years of operation, I mean being a Baysian here.

MR. ROSENTHAL: No, you're being a

Classicist, and it would be 1 in 4,000 or so at this point, which gives you like 2 times 10 to the minus 4 or something. I mean a Classicist would argue --

MEMBER POWERS: Classicist would, but I would simply use the event as a Baysian update, in which case my probability is a lot higher than 2 times 10 to the minus 4.

CHAIRMAN BONACA: Well, they did make significant changes in that type of plant to the change the very number, because they had no trips or secondary size parameters, and that's why they were opening the PORV and sticking it open once every 50 times. That was the history before TMI for the B&W plants. Now what they did, they implemented feedwater trips, so if you loose feedwater you will have a scram before you have a transient at the primary site, so therefore, they stayed away from the PORV. Now that's why I was asking the question before. I mean, they made changes that resulted in that number you're showing us --

MR. ROSENTHAL: The minus 3 number is a hardware valve number. That's of an initiating event. TMI is a full sequence. I just want to set the stage here, so you're concerned about the event.

For Westinghouse and combustion plants,

the loop seals are just plain smaller. The volume of the piping involved is smaller, so that if you postulate the maximum slug size and you inject that into the core, you don't go recritical. So it's just plain not a Westinghouse or a combustion issue.

Now I will give the subcommittee credit because we were so focused on B&W that we hadn't looked at Westinghouse, and CE, and under some prodding from them we went back and did look, and did some analysis. And then finally at the end, looked up the size of the piping, which is probably the most persuasive thing, that the volume is just not there, so it's a B&W problem, B&W lower loop problem.

VICE-CHAIRMAN WALLIS: Is it also a problem with the lower loop --

MR. ROSENTHAL: Lower loop, because we said the raised loop will have a smaller volume again. But I do want to leave the very strong -- it's a B&W issue, not a CE and Westinghouse. Not to pick on them, it's just that's how the piping looks.

MEMBER ROSEN: Even a subset of B&W.

MR. ROSENTHAL: Yes, sir. Okay. So now let's look at B&W for just a minute.

MEMBER ROSEN: How many of the B&W plants are lower loop, of the six?

MR. ROSENTHAL: Five out of the six.

MEMBER ROSEN: Five out of the six?

MR. ROSENTHAL: I think Davis-Besse is the only raised loop.

MEMBER ROSEN: Oh, good. Okay.

MR. ROSENTHAL: Okay. So now you have to transport the slug and there's two ways; one is natural circulation, which is a slower event, and one is by the operators turning the pumps on. So for the case that we're most concerned with, there's explicit procedures in their EOPs not to turn on those pumps until they have acceptable conditions.

Okay. Having said all that, that it's a B&W lower loop problem, where we think we're robust that it is not a combustion or Westinghouse problem - let me just go on. And one can argue that this is argumentative.

You take a small break LOCA as about 2 times 10 to the minus 3, if it's the valve. It's got to be early in the fuel cycle, about the first 20 percent of the fuel cycle, which also was TMI, in all fairness. It was early in their fuel cycle, because that's when the boron is holding down more reactivity. For slug formation, in order to get in this condition, you need equipment failure - one or more pieces of hardware fail, typical train is 10 to the minus 2, so it's some number of that order of magnitude.

VICE-CHAIRMAN WALLIS: It's either equipment or it's inappropriate operator action.

MR. ROSENTHAL: Well, my P4 is the restart of the reactor coolant pump --

VICE-CHAIRMAN WALLIS: By shutting off the HPI or whatever it is that you need to do.

MR. ROSENTHAL: Yes. Yes, sir.

VICE-CHAIRMAN WALLIS: That's also in there.

MR. ROSENTHAL: Yes. Okay. And then you have to restart the pump, and for that we looked at the human -- we got the human factor experts.

MEMBER POWERS: You're going to create these things as independent, and it's just no way that they're independent.

MR. ROSENTHAL: Go on.

MEMBER POWERS: Well, I mean, that's what you do. Right? And why do you think that P3, P4 are independent?

VICE-CHAIRMAN WALLIS: The guy who's going to shut off the HPI is probably under some misapprehension about what's happening. He might equally well start the pump under the same misapprehension. That's what happened at TMI; because they misunderstood what was going on, they did things that had a common cause.

MEMBER RANSOM: I think the procedures right now are for the operator to call for not to restart the pump and kill natural circulation that's existed for a given length of time, and so it would have to be in violation of that procedure.

MR. ROSENTHAL: So this would be an estimate that we would use to get a sense of the likelihood of this boron dilution event in which the operators turn back on the pumps. One can argue what is the magnitude on the number, and I just wanted to give you a feel for this, because in a little while we're going to talk mechanistically about what would happen. And I think that what we're saying is that we believe that mechanistically, the consequences of such an event would be low in terms of the extent of fuel damage, and as a basis for dismissing the issue. And that if you combine that with our perception of the probability of the event, it further supports dismissing the issue.

MEMBER ROSEN: Do you think if one were to say that it's a common mode failure of operators' cognitive processes, so that P3-P4 is not 10 to the minus 4, it's 10 to the minus 3. Would that change your answer?

MR. ROSENTHAL: No.

MR. BESETTE: The human factors people actually gave two numbers for P4. One is like a standard error rate, the other one is what you might call a highly stressed error rate. And the one that is shown is the highly stressed error rate. The standard error rate is lower. I guess another factor to consider is that by this time, the emergency response center at the plant would have been actuated and there would be a lot of people --

MR. ROSENTHAL: Let me make another point, and that is that you're used to seeing core damage frequencies of 10 to the minus 4, 10 to the minus 5. Sometimes people will get up here and argue seriously about 10 to the minus 6 for core damage frequency. This number here is an estimate of an event in which you put an unborated water slug back into the core, and that's not core damage. And, in fact, it's a scenario in which to get in this scenario I've interrupted HPSI, and then I've recovered high pressure injection. So if I cause fuel damage, which I will show mechanistically we think is of low likelihood, it's into a situation in which I have operable ECCS, so we're far from a core damaging scenario.

MEMBER POWERS: It seems to me that if I were going to try to redo this calculation, I would take P4 as one.

MEMBER ROSEN: One.

MEMBER POWERS: If I've interrupted high pressure injection, at some point, for whatever reason I did that, at some point I'm going to turn on the reactor coolant pump. Guaranteed, just flat guaranteed that I'm going to do it.

VICE-CHAIRMAN WALLIS: The only evidence we have for how operators behave under really high stress would seem to be TMI. That would be another incidence --

MR. ROSENTHAL: We also have the Crystal River event, which was a very telling -- I'm sorry, Dr. Rosen.

MEMBER ROSEN: No, no. We have TMI for sure, but we don't have this circumstance anymore without having had TMI, and having had the corrective actions, and having had the training and the procedural changes, so we're in a different world. You can't use a pre-TMI number any more.

MR. BESETTE: At the time of TMI, there

were no procedures one way or the other in terms of tripping reactor coolant pumps, or stopping reactor coolant pumps. If you had a LOCA, you didn't have to trip reactor coolant pumps. Now you're directed to trip them, and so there were no procedures one way or the other at the time of TMI.

CHAIRMAN BONACA: But in order to have a slug formation, you've got to have the operator terminating HPSI. Right?

MR. di MARZO: You have to have several concurrent things. You have to have the primary higher than the secondary in terms of energy. IN other words, secondary has to be a sink. You have to have HPSI interrupted, you have to have break isolated, and you've got to maintain this kind of situation for a relatively long time with an eventary range which is very tight.

MEMBER ROSEN: And then you have to restart --

MR. di MARZO: And then you have to restart HPI, so the inventory in which you've got to be has to be such that you don't cool the core, and you do not go into resumption of natural circulation.

CHAIRMAN BONACA: I was just dealing with the probability issue. What I'm trying is that right now with the formation of the two margin and adequate core cooling, et cetera, the probability that he will cut off HPSI is extremely low, I think. But what is that small number there? I don't see that. I see a small break LOCA.

MEMBER ROSEN: Slug formation.

MR. ROSENTHAL: WE've lumped all the hardware and human into some estimate of slug formation. As I say, this is to give you a perception that we're working on a infrequent event.

CHAIRMAN BONACA: I understand, but the point is that yes, I have more credit than that to the slug formation. I would go to 1 in 10 to the minus 3 almost, because you would have to have this intent and no recognition of circ cool margin, et cetera. These guys are trained so heavily on this issue, I mean it's just not going to happen. But the other points, however, that I think about is that RCP. Yes, I mean there are steps and procedures to do that. That's going to be closer to one, I think.

MR. ROSENTHAL: To one?

CHAIRMAN BONACA: Well --

MR. ROSENTHAL: If failure to follow the procedures over an hour into an event?

CHAIRMAN BONACA: No. No.

MR. ROSENTHAL: It's at least 10 to the minus 2. This was the standard methodology that -- human factor methodology.

CHAIRMAN BONACA: Okay.

MEMBER ROSEN: We're assuming here that this is not a cognitive failure of the whole crew. If you have cognitive failure of the whole crew, as you had at TMI, then you're going to get higher numbers, but if you -- it's very much harder to do that in post event environment than in a pre-event environment.

CHAIRMAN BONACA: Yes, also a very different situation in the control room. You have three people there with the --

MEMBER ROSEN: Four o'clock in the morning.

MEMBER RANSOM: Correct me if I'm wrong, but I didn't think this improbability argument was really key to resolving the safety issue.

MR. ROSENTHAL: Correct.

MEMBER RANSOM: It's only frosting on the cake.

MEMBER ROSEN: Frosting on the cake tells us it's about 10 to the minus 6. You can argue it could be as low as 10 to the minus 7, might be 10 to the minus 5.

CHAIRMAN BONACA: All right. I see somewhere --

MR. ROSENTHAL: Okay. So now from --

MEMBER ROSEN: We don't care whether it's any of the numbers.

VICE-CHAIRMAN WALLIS: You're going to tell us it can't happen, not the consequences of not happening.

MR. ROSENTHAL: Okay. Now let's talk about the consequences. Now we've said that for CE and Westinghouse, just based on the slug size that you can form, you're not going to recritical. B&W lower loop you could have 40 cubic meters of unborated water that you could put into the core. And if you do that, there's two cases two consider; one is natural circulation, and the other is the restart of the reactor coolant pump.

So now we use the PARCS code, and we can calculate the reactor kinetics, and we can calculate the enthalpy deposition in the fuel. And what you find for the natural circ case, things happen slow enough, the normal feedback mechanisms in the core are fast enough that, in fact, we don't think that you'll fail fuel.

For the restart case, which is faster,

where you've got a pump that's stuffing unborated water into the priming system, there is a potential for fuel failure. And that would be limited to some peak region of the core, and that would be in a scenario in which you have high pressure injection available by definition of the scenario.

So we think of the consequences of the event are modest, and one can argue over the frequency of the event, but we also believe that that is modest, and that with the explicit procedures already in the B&W EOPs, enough has been done that we do not have to require more be done.

Okay. So I'm now repeating myself. No problems with CE and Westinghouse. B&W is a plant that's vulnerable. B&W is the one that's addressed the issue already with explicit procedures which suppresses the probability of the event. And based on that, we concluded that no further regulatory action was necessary.

MEMBER DENNING: I have a couple of questions. One of them is where is the Boron that got left behind when the water evaporated and then recondensed? Is it supposedly stuck up in the -- where is it?

MR. ROSENTHAL: It's in the reactor vessel in the core.

MEMBER DENNING: It's in the core, so you have an unusually high amount of Boron in the core.

MR. ROSENTHAL: Yes, but we're not going to -- you have a LOCA. Dave Diamond is going to present the criticality in a few minutes.

MEMBER DENNING: Okay.

MR. ROSENTHAL: But you've had a loss of cooling event, and you've had an ECCS injection, so you're starting with like 2,000 ppm that you've been putting into the core from the injection of the ECCS. The little bit from the distilling, the little bit extra Boron --

MEMBER DENNING: But that's the difference between -- that little bit of difference is the difference between why you've got a problem. I mean, that's why you have dilution, is because you left some Boron behind someplace.

MR. ROSENTHAL: And you're postulating that you're putting an unborated water slug, not 2,000 ppm but close to zero ppm.

MEMBER DENNING: I know, but you increase the concentration some place in the system of Boron to come up with that slug of water that's unborated. Am I wrong? So it's a matter of the distribution of where it's in the system.

MR. ROSENTHAL: Oh, yes. Yes.

CHAIRMAN BONACA: Yes, because you make the assumption that when it comes in, it doesn't mix.

MEMBER DENNING: Well, then that's not --

CHAIRMAN BONACA: It comes to the core.

MEMBER DENNING: Does it not mix in the downcomer or what are your --

MR. ROSENTHAL: Okay. Now Professor di Marzo is going to talk. This is an introduction.

MEMBER DENNING: Okay.

MR. ROSENTHAL: In due course, we'll talk about where you would form a slug, how big the slug could be, how you could transport the slug from the pump, through the pipe, downcomer, lower plenum, and back up --

MEMBER ROSEN: And what happens to the Boron that got -- came out of the slug, and whether that matters; where it went, and whether that matters.

MEMBER DENNING: Jack, one other question that's important; and that is, reactor coolant pump - this current requirement that they not restart the reactor coolant pump until some particular time, is that implemented specifically to avoid this problem, or is there for another reason?

MR. ROSENTHAL: It's in the B&W -- I'm sorry, it's the bases, it's the EOP bases document that told that this is the reason that they shouldn't do it.

MEMBER DENNING: And this is the reason they shouldn't do it.

MR. ROSENTHAL: I don't want to use the word --

MEMBER DENNING: The thing that I'm worried about is, are there situations where we wish they really had started that reactor coolant pump, that they did not have a prohibition against it? If this is an unreal problem, if mixing and stuff like that really mean this isn't the real problem, and we've imposed a requirement that they not start the pump because of a non-real problem, then I want to know, you know -- you're telling me that from your analyses, it's not too bad. I want to find out is it really important, and if this is a fake problem that we've just set up by the boundary conditions, I'd like to know have we really done the wrong thing from a safety viewpoint by prohibiting the restart of that coolant.

MR. ROSENTHAL: I guess you could postulate. You have those 40 cubic meters of water in your seal, and if you did have a core recovery, it would be really useful to get that over to the core, if you had no other way of getting water there. It would just buy you some time. Ultimately, you need to get some ECC injection back.

MR. BESETTE: It's interesting these restrictions have been in place since 1996, though. Framatome put them in place at that time, and I believe based on possibility of Boron dilution --

MEMBER RANSOM: The only reason the procedure is there is because of Boron dilution.

VICE-CHAIRMAN WALLIS: The Chairman wants to finish by 4:30.

MR. ROSENTHAL: No problem.

MR. DIAMOND: I'm David Diamond from Brookhaven National Lab, and I will be very brief. I'd like to give you an idea of the analysis that we did at Brookhaven that Jack alluded to.

We wanted to understand the consequences of the event given a particular slug, and what we mean by the consequences are calculations of the fuel enthalpy throughout the core as a function of time. The fuel enthalpy that we're talking about is averaged over a pellet. That's how we define fuel enthalpy, but we look at it as a function of position within the reactor. And as I say, it's a function of time during the boron dilution event. And, of course, we look at fuel enthalpy because that is generally used as a failure criterion for reactivity initiated accidents.

We, of course, did best estimate studies and, of course, parametric studies to determine the effect of different assumptions, such as flow rate, Boron concentration and reactor types. And I'll, of course, only touch on one or two calculations here today.

As Jack mentioned, we use a methodology developed by RES, and it couples in this particular case Relap 5 with PARCS. PARCS, of course, providing the neutron kinetics, and I have some attributes of the PARCS code listed here, which I won't go into. I have more on these slides than I will touch on, but the information is there for your perusal at a later time.

This slide shows something that is important in developing a PARCS model, and that is the fact that the assemblies are represented as homogenized regions, so that a true assembly which is heterogenous, one does a calculation over the full spectrum of neutron energy, and over this assembly, and then averages the cross-section information, averages it spatially in order to get a uniform representation of the assembly, and averages it in terms of energy in order to reduce things down to two neutron energy groups. And that is the way in which the core calculations are done. There is a way of backing out information on the pin-by-pin power, but I'm not going to get into that in this.

MEMBER RANSOM: It might be important to touch on the validation for this model, how much faith can you have in this.

MR. DIAMOND: All right. The PARCS code has been validated by comparisons with many different benchmarks, both experimental and numerical. For this particular calculation, of course, one doesn't have direct validation. However, we did do some code-to-code comparisons against a Russian code using a totally different methodology, just to give us a certain level of confidence in the ability of the methodology used in PARCS to be able to calculate the core under these conditions. And these conditions are extreme relative to --

MEMBER RANSOM: It's my understanding that those were reactivity transients that you compared it against. Is that right?

MR. DIAMOND: They were specifically for

Boron dilution.

MEMBER RANSOM: Okay.

MR. DIAMOND: The calculations that we did modeled a B&W design. It's 177 assemblies in the core. It happened to be TMI-1, and it's modeled at the beginning of cycle because as mentioned, that's when a Boron dilution has consequence. Indeed, we did some analysis to show that the consequences are of concern only in the first maybe 10 or 20 percent of the cycle. It depends on the type of fuel cycle one has.

MEMBER POWERS: If I have a core that's 60 percent fresh fuel, 40 percent old fuel, I don't need to worry, uniformly distributed.

MR. DIAMOND: No. This has nothing to do with the fuel in the core. It has to do with the cycle which starts out with a high concentration of Boron, and then eventually goes down to Boron concentrations that are so low that a dilution doesn't really add much. And it turns out that that point is reached fairly early in the cycle.

MEMBER ROSEN: Do you know how high the Boron concentration is at the beginning of life? Is it --

MR. DIAMOND: Typically, Boron

concentration is about 1,500 ppm, and that's generally true --

MEMBER POWERS: That's hot full power?

MR. DIAMOND: Hot full power, yes. And that's generally true even as one goes to longer fuel cycles.

MEMBER POWERS: The previous speaker put up things that said gee, all I did was pop this fuel with perhaps as much as 185 calories per gram, full of numbers for different assumptions, and I was supposed to walk away with a lot of comfort; that 25 calories per gram, I'll walk away with a little bit of comfort. When you cross on how many calories per gram I'm starting to get real nervous.

MR. DIAMOND: Okay. Let me go through the calculations and qualify those numbers a little bit, put them a little bit in context, and then we can get back to your question, perhaps.

Anyway, the starting point for the calculations that we did is late in the scenario; that is, it's after the dilution has taken place in the cold leg. At this point, all the control banks are inserted, the control and shutdown banks. The fuel has cooled a little bit and is down to 500 -- excuse me, the moderator has also cooled by virtue of the injection of the ECCS, and it's at 500k for this calculation. And at a Boron concentration of 2,500 ppm, which corresponds to the ECCS concentration. So at this point in time, the reactor is about 15 dollars shut down. And then the transient boundary conditions that we imposed in order to do the calculation is a Boron concentration as a function of time at the lower plenum. And how we get that, Marino di Marzo will explain after I'm finished here. And then we looked at flow rates based either on assuming natural circulation or the restart of a pump in the diluted loop.

This is the layout, and I just want to show you that the numbers represent control banks, and so we have a checkerboard pattern of assemblies with control rods, and checkerboard with those without control rods.

The reason that one-eighth of the core is highlighted here is that we did have one-eighth symmetry, and although we calculated in PARCS, we calculate the result for every assembly in the thermohydraulic calculation that this is coupled to; namely, RELAP5.

We only considered thermohydraulic channels representing each of the assemblies in a one-eighth core. And they're listed here for two purposes; one -- well, first of all, note the yellow assemblies, those in which one has control rods initially. And the number at the bottom of the assembly is the burn-up in this particular core.

The assemblies here, these two assemblies that are shaded have low burn-up. They're fresh fuel in this particular core. This is at beginning of cycle, and it's in these two assemblies where the peak fuel enthalpy occurs. And also I might say at this point, it also occurs at the bottom of the core. And I think that's what Jack was referring to by saying that this is not a core-wide -- that one doesn't get to high enthalpy throughout the core. One gets it in these two assemblies, and at the bottom of the core.

MEMBER ROSEN: And those are megawatt days per ton numbers?

MR. DIAMOND: Gigawatt days per ton.

MEMBER ROSEN: I mean, gigawatt days per ton.

MR. DIAMOND: Yes, that's correct.

MEMBER SIEBER: So that's 16 assemblies for the whole core, two per one-eighth segment.

MR. DIAMOND: That's correct.

MEMBER SIEBER: Okay.

MR. DIAMOND: Okay. So here is your lower loop plant, and what we do is in the RELAP5 calculation to model each of the fuel assemblies in that one-eighth core as a thermohydraulic channel. These are, of course, one dimensional models, and they are coupled at the top and bottom. And we have an explicit representation of the inlet plenum and the outlet plenum.

MEMBER RANSOM: As I understand it, there are 29 channels. Is that right?

MR. DIAMOND: Well, actually a 30th for bypass flow. Yes. And this shows you a result when the flow goes to 25 percent of nominal value. And the blue curve here is Boron concentration, the ordinate is on the right side here. That's ppm, and you can see that it starts off at 2,500 and goes down in about just a few seconds to about 450 roughly ppm, and then comes back up to 2,500. And the resulting reactivity versus time is shown here in red. And that starts off, as I said, at 15 dollars subcritical --

MEMBER ROSEN: I'm sorry, Dave, to be so stupid, but I don't know what this 10 second or 20 second transient is. What happens during that 20 seconds?

CHAIRMAN BONACA: The Boron goes --

MEMBER ROSEN: I know, but what in the plant --

MR. DIAMOND: Yes. This is starting from one pump starting at time zero.

MEMBER ROSEN: Okay. And this is in the core, which starts at 2,500, and it's being flushed, basically.

MR. DIAMOND: That's correct.

MEMBER ROSEN: Okay.

MR. DIAMOND: We impose this Boron concentration versus time at the inlet plenum, the lower plenum.

MEMBER ROSEN: Okay.

MR. DIAMOND: And then calculate the consequences in the core in terms of power.

MEMBER ROSEN: This is essentially the startup of 1 RCP. Is that what this --

MR. DIAMOND: That's correct.

CHAIRMAN BONACA: That's an average core, the whole core?

MR. DIAMOND: I'm sorry?

CHAIRMAN BONACA: I mean, you have a finite amount of water coming in from the slug.

MR. DIAMOND: Yes, that's correct.

CHAIRMAN BONACA: Okay. And where is it

placed?

MR. DIAMOND: And it's placed in the lower plenum, and then flows up through the core. This is a B&W case which is 40 cubic meters.

CHAIRMAN BONACA: Assuming the whole core to be affected by this.

MR. DIAMOND: Yes.

MEMBER ROSEN: Now these are huge pumps, great big motors. From the time you actually press the button until the time it gets to full speed, is that taken into account?

MR. DIAMOND: In this particular calculation, yes. This takes about 10 seconds.

MR. di MARZO: Yes, but the problem is there is water before the deborate, so the pump gets to full speed before the deborate arrives. In other words, you have to start flushing the downcomer and whatever you had in the cold leg downstream the pump first, and then you get that. So essentially, it's full speed almost.

MEMBER ROSEN: The pump starts up and it pushes a lot of borated water in first, and then incomes the non-borate.

MR. di MARZO: Right.

MEMBER ROSEN: And that whole -- the nonborated water gets to the core is time zero here.

MEMBER POWERS: That's zero.

MR. DIAMOND: That's correct.

MEMBER ROSEN: Not the pump start time.

MR. DIAMOND: That's correct.

MEMBER ROSEN: Okay.

MR. DIAMOND: That's correct. And the result on power is shown in the red curve here, and the scale here is, 100 percent, of course, is nominal power. And one gets to a prompt critical situation, and that's the reason that the power rises so rapidly. You have a very sharp burst. And, of course, that burst is turned over rapidly, as well, because this is a characteristic of light water reactors, the doppler feedback is extremely powerful and very fast.

Having said that though, you could also notice that it did get up to 2,700 percent before being turned off. Now it then goes through a series of, like you could almost call them oscillations, as a result of the conflict between the dilution that's taking place and all of the negative feedback that takes place as a result of the increase in fuel temperature, and then the decrease in density as you get voiding sporadically in the core.

And what I said earlier, what we're most interested in, though, is to take a look -- this power here is a global power, and we're really interested in something that's happening locally; namely, how is the individual fuel rod behaving. And we judge that according to what the fuel enthalpy is, and in the blue curve here we're looking at the fuel enthalpy in the rod that has the maximum value. And what we see initially is a rise in fuel enthalpy from about 17 calories per gram to an increase of about 30 calories per gram, to about 47 calories per gram. And that's this initial jump here. It's almost hard to see because we're talking about a jump in less than one second. This initial pulse here is a very narrow pulse relative to this time scale here. So that initial fuel enthalpy increase by which a lot of people judge fuel behavior is only on the order of 30 calories per gram.

However, in this particular case, because there is so much diluted water that's coming into the core, we see that - and it's coming in so fast, at 12 seconds we're up to about a maximum fuel pellet enthalpy of about 190 calories per gram, or an increase of about 170 calories per gram.

MEMBER POWERS: And what turns it over there is the re-boration.

MR. DIAMOND: Yes.

MEMBER POWERS: As the pump continues to run, it starts putting back in borated water.

MR. DIAMOND: That's correct. The slug is a finite volume, and --

VICE-CHAIRMAN WALLIS: In other words, the pump is now turned off.

MEMBER POWERS: Oh, my gosh, I made a mistake on that one. I turned it on for 12 seconds and trip it.

CHAIRMAN BONACA: How do you get the pin value?

MR. DIAMOND: I'm sorry?

CHAIRMAN BONACA: How do you get the pin value? I mean, you do have a calculation here and a cross-match.

MR. DIAMOND: Yes. Right.

CHAIRMAN BONACA: And then you go to fine-mesh. How do you -- I mean, you superimpose --

MR. DIAMOND: You can impose a peaking factor on the assembly calculation.

CHAIRMAN BONACA: That's what you did.

MR. DIAMOND: In this particular case, no. This is not --

CHAIRMAN BONACA: Is this an average?

MR. DIAMOND: This is averaged over assembly, and so it would be in the neighborhood of maybe an additional 20 percent peaking factor to account for what it might be at a pin.

VICE-CHAIRMAN WALLIS: Let's look at that. If they did bump the pump as they did at TMI, I guess, 5 seconds, and they turned it off and left the diluted borated water in the core, you wouldn't get Boron in the core now. It would take its course, presumably, in some way.

MR. DIAMOND: It wouldn't be going through as rapidly, that's true.

VICE-CHAIRMAN WALLIS: Now circulate as natural circulation or something?

MR. DIAMOND: Yes. Well, I mean, there was some momentum built into the flow, so --

MR. ROSENTHAL: Let's be very careful in describing the scenario. We're an hour into the event, and we've distilled enough water that we formed this maximum 40 cubic meter slug of water in the loop seals.

MEMBER POWERS: Deborated.

MR. ROSENTHAL: Deborated water, and now you've turned it on, and Dave is trying to show what might happen. And now your -- and there's only 40 cubic meters max to play with, so now you're postulating exactly what? You trip the pump --

VICE-CHAIRMAN WALLIS: You squirt it in and then you stop.

MR. ROSENTHAL: And you stop --

MEMBER POWERS: No, he starts it at the wrong moment, and then it trips 10 seconds later, which is possible, because he doesn't have all the auxiliaries set up. He's made a mistake.

MEMBER SIEBER: But that's not what was analyzed.

MR. ROSENTHAL: That's good. That's good, because Dave is showing you the pump case, the natural circ case is a more benign case.

VICE-CHAIRMAN WALLIS: No, I'm saying - he said it turned around because you started to bring in borated water. I'm saying will that happen if you turn off the pump, or if the pump trips? Does it turn around if the pump trips?

MEMBER SIEBER: Well, the peak won't be as hot in the natural circulation case.

MR. BESETTE: Once a pump is going, there's a coast down that lasts for about another 30 seconds or so. The flywheel will keep --

VICE-CHAIRMAN WALLIS: And that's enough

to keep the fluid out.

MR. BESETTE: And plus, you've got a pretty strong natural circulation when you have 100 percent power, too.

CHAIRMAN BONACA: I'm sorry, just to understand. Those 40 cubic meters, what is it, a volume of the vessel?

MR. BESETTE: The 40 cubic meters is about the volume of -- the core region has about 36 cubic meters or 40 cubic meters.

CHAIRMAN BONACA: You're talking about the whole amount of the core region. Okay. That's fine.

MEMBER ROSEN: What saves you is the flywheel.

MR. ROSENTHAL: Yes. The volume that we're talking about is below the inlet of the cold leg - I'm sorry --

MEMBER POWERS: Right there, that one.

MR. ROSENTHAL: So it's this volume in the steam generator and in the cold leg below this level.

VICE-CHAIRMAN WALLIS: I don't know why the flywheel saves you, because you could turn the pump on for two seconds, and then the flywheel will put the rest of the slug in.

MR. di MARZO: Then you don't get the max speed.

VICE-CHAIRMAN WALLIS: If you wanted to be extraordinarily pessimistic you could say absolutely the worst possible thing happens.

MEMBER SIEBER: And I don't think that is, because a transient is much slower. When the transient is slower, you don't get to the peak power. And it's self-limiting.

VICE-CHAIRMAN WALLIS: We don't have the spectrum of transients.

CHAIRMAN BONACA: I want to think about the slug going through from the narrow pipe down the downcomer. We're assuming that it fills the downcomer, and then it comes up. I can imagine, for example, a slug going in locally in the region of the core, so have a more drastic effect, because it could last a longer time.

MR. DIAMOND: Well, I think Professor di Marzo discusses the slugs.

MEMBER RANSOM: Well, isn't there an issue, too, that if you have like 40 cubic meters of the deborated water, and you turn on the pump, the pump is going to cavitate at some point because there isn't any fluid behind that slug. It's going to pump down until it starts to cavitate, and I would think operational procedures would call for shutting it down.

MR. di MARZO: That would depend when you do it, because you would do it at some level of -- you may have some level of refueling. You do it or you may have just the slug itself.

MEMBER RANSOM: You mean you're assuming that you would have the slug sitting there, but then refilled with borated water above that?

MR. di MARZO: The slug can be at any position up and down the steam generator if you're starting refueling, for example, and then at that point start the pump. Or you can postulate that you start the pump exactly at the final time when the slug has just finished forming. That introduces another probability there. You have to factor that in, I suppose.

MR. DIAMOND: All right. And perhaps the consequences will become also a little bit clearer if I show one case where the flow rate is only at 3 percent, representing natural circulation. And in this case, again the Boron concentration starts at 2,500, and it takes much longer for the slug to go through the core. And this acts in your favor in terms of making the event more benign.

Again, the red is the reactivity versus time, which is just the conflict between the Boron dilution and the feedback. And the result, though, is different for the pump-on case. The red is now the power. Again, in terms of having a prompt critical pulse initially, that's the same except that this one only goes up to about four or five hundred percent, and then it goes through a series of oscillations over a longer period of time because this is a slower event. But also, if you look at the peak fuel enthalpy as a function of time --

VICE-CHAIRMAN WALLIS: At the end of that process when the slug is now in the core. The core's coolant is unborated water. Is it the voiding which is filling the reactivity rather than the Boron?

MR. DIAMOND: It's a combination - yes - the voiding and fuel temperature. And also the fact that yes, we think in terms of a monolithic slug going through, but it's actually a very spatially dependent process. So this initial rise of the fuel enthalpy is only about 25 calories per gram here, and then the peak value of the fuel enthalpy, which again is in those fresh fuel assemblies at the bottom of the core, it's only about 90 calories per gram in this particular case.

One other case, this is the no-never-mind case. This is the simulation for a Westinghouse cold leg design, where the Boron concentration versus time is a much shorter dilution. This comes right back up in about 2 seconds, because we're talking about an order of magnitude difference in the volume, going from about 40 cubic feet to about less than 4 cubic feet.

MEMBER POWERS: Could we go back to your previous slide?

MR. DIAMOND: Sure.

MEMBER POWERS: You show a very sharp initial transient, some minor oscillations, and then a period of very short passes in the power. Are those oscillations such that, and the time is wrong here. It's 10 seconds for those hash marks there, such that your fuel is successfully disposing all of its enthalpy into the coolant, and not getting any --

MR. DIAMOND: Yes. Well, this is the enthalpy here, and so the enthalpy levels are relatively low. Don't forget, full power enthalpy is about 45 calories per gram, so okay. You have a situation here where you're hotter than normal.

VICE-CHAIRMAN WALLIS: Well, it's 10 percent power, but you've got something like a BWR.

You're boiling off the voids in there. You're cooling the --

MEMBER POWERS: Really, I'm just asking if heat transfer was operational here.

MR. DIAMOND: Yes. I mean, as calculated by RELAP5. So you have two-phase cooling, sure.

Okay. As I mentioned, this is really the no-never-mind, because the volume of the borated water is so small.

VICE-CHAIRMAN WALLIS: If you have a very, very big slug and you put it in there slowly, you just boil, and boil, and boil and fuel will be cool, and there will be no greater power.

MR. DIAMOND: You would reach an equilibrium power.

VICE-CHAIRMAN WALLIS: No, but you've got it there. You got in the last few seconds of the previous slide, essentially cooling it.

MR. DIAMOND: Yes.

VICE-CHAIRMAN WALLIS: But the boiling -- you don't care if there's any Boron in there or not.

CHAIRMAN BONACA: If you have the slow transient, you --

MR. DIAMOND: Yes. I mean, the -- that's correct.

MR. ROSENTHAL: My memory serves me that in a pressurized water reactor, you hold out about half the reactivity with soluble Boron, and about half with rods. You shutdown to about 350F, 400F on rods alone, so I think that if you have the rods in there and totally deborated forever, you're going to end up with some temperature about 400F system pressure, and some power, and you'll sit there.

VICE-CHAIRMAN WALLIS: It doesn't matter, you don't need any Boron.

MR. DIAMOND: To go to the cold shutdown you need the Boron.

VICE-CHAIRMAN WALLIS: You can't get the cold shutdown, but at least it's -- it doesn't get overheated or anything.

MEMBER POWERS: It's called N-O-P-N-O-T almost, Normal Operating Pressure and Normal Operating Temperature; 450 degrees Fahrenheit, and you go up to 2,000 psi, and sit there. You lift the release.

MR. DIAMOND: All right. This slide repeats what I've already said, and what Jack presented earlier, so I just want to have three bullets here. One to remark that RELAP5/PARCS is a viable method for this analysis. As Jack pointed out, it's important to recognize that RES does have methodologies now that can analyze very complex transients in which both the neutronics and the thermohydraulics interplay.

From the point of view of fuel enthalpy, the increase is only significant if the volume of the diluted water is large enough, namely one has the B&W lower loop scenario, and the rate of injection is large enough; namely, one has the RCP restart. And as I already mentioned, the effect is only possible on the first 20 percent of the cycle, which also comes out of consideration of panasonics.

MEMBER ROSEN: Very good, Dave, nice stuff.

MR. DIAMOND: Thank you.

MEMBER POWERS: Well, I'm still sitting here saying they sure are happy with 173, 180 calorie per gram percs on the fuel. And I keep wondering why are they so happy? I mean, what is it that makes you say gee, I've got no -- life is good, got no trouble. I just rattled the fuel - I'm just not real happy about taking.

MR. ROSENTHAL: I think my argument was that I think I have a reasonably low likelihood event. And for that reasonably low likelihood event, I think of the extent of heating damage would be limited to some region of the core. And I have a scenario in which by definition I have ECCS available.

Now let's talk more specifically about the enthalpy deposition. For 30 years, we've had on 1177 on the books, which says 280 calories per gram is an acceptable enthalpy deposition. It was associated with a reactivity insertion event of an ejected rod, which is a very fast event, the order of milliseconds. And we've recently done work at Cabris, we're co-sponsors of Cabris, which says that the high burn-up fuel, that number might more likely be 80 or 100 calories per gram as a value at which you might damage clad. That's the high burn-up fuel, although I cannot guarantee the fuel loading pattern in some future reactor. I think that the one that David used is a typical reloading pattern, and so that the peak is more likely to occur in the fresh fuel for which there's more likely some margin than the older fuel.

In the Cabris test, we argued over is it 10 milliseconds or is it 30 milliseconds is the right pulse - time frame to run these tests at, because if you run the test fast enough, there's time for the pellet to heat up before the clad has time to heat up and start to grow, and become more ductile. If you can heat it, you can transfer the energy to the clad, so that the clad warms up. It's more ductile, you can put more energy into the pellet.

The kinds of scenarios that we're running here are slow compared to the Cabris test, and that's why Dave focused on pointing out that first bullet, the less than a second, the so many milliseconds blip, because that's the enthalpy deposition that you should think of in terms of when you're comparing it to the Cabris, in which case the experimental evidence - it looks like it's okay. So it's the sum of those considerations. And then the last thing is that we put in place explicit operator procedures to tell them don't do it.

MEMBER POWERS: Let me follow-up on my question, please.

MR. ROSENTHAL: I apologize.

MEMBER POWERS: I think you persuaded me that you have a 5 times 10 to the minus 6 event. Okay. I took half of my 1 times 10 to the minus 5^{th}. You aren't going to get P4 out of me. For 30 years, you've had 280 calories per gram on the books. You've known it's wrong. It has always been wrong. It's a flat wrong number. You've worked at Cabris. You understand that you have to be very careful about the power inputs, because if you leave power-off into the clad, then it just doesn't count too much on the fuel, so you worry about short transients, in which all the energy goes into the fuel. But here in these analyses, you're telling me I'm getting reasonable hits on my fresh fuel, which can be adjacent to fuel that's not so fresh, but you haven't told me anything about that not so fresh fuel. Okay. Is it doing nothing? Are you getting no energy whatsoever into that?

MR. ROSENTHAL: I think there's one item value in the core from what I learned in school. David, can you address that?

MR. DIAMOND: It turns out in this particular core, all of the burn fuel has a control rod in place in there, so there's going to be quite a large difference in terms of the fuel enthalpy rise in the spent fuel versus what's going on in the fresh fuel.

MEMBER POWERS: Now have you imposed a requirement that all burn fuel have a rod in it?

MR. DIAMOND: No.

MEMBER POWERS: You left something out of your analysis.

MR. DIAMOND: Right. And in a different fuel management scheme, you would certainly have rods with higher burn-ups suffering not as high as an enthalpy as a fuel assembly with zero burn-up or low burn-up, but it would be --

MEMBER POWERS: But see, even if I go out and find new experiment, let's say if I put 50 calorie per gram into this fuel, I broke it apart. Okay. Now you can wave your hands and say okay, there are reasons for that, maybe the water was cool, things like that. I mean, my point is there's something missing from your analysis here. You haven't given me enough information to make your case. That's the point I'm making here.

MR. ROSENTHAL: Even if you said well -- if you applied those enthalpy increases that we have for fresh fuel there to high burn-up fuel, you're still less than the enthalpy step increases which led to cladding cracks --

MEMBER POWERS: Oh, no, I'm not. If I've got 173 calories per gram in the 50 gigawatt day fuel, it's going to be pulling apart.

MR. ROSENTHAL: No. You're talking about --

MEMBER POWERS: What do you mean no? It's not no, it's yes. It's guaranteed.

MR. ROSENTHAL: I mean those experiments

are single pulse experiments. You're talking about from an experiment at a single pulse which put in 170 calories per gram which cracked the cladding. Here we're getting multiple pulses. Each one is contributing maybe 25 calories per gram.

MEMBER POWERS: Okay. Now show me all your experiments which say that that will not crack the clad.

MR. ROSENTHAL: We've had the -- you can look at the wide pulse data where you don't get cracking.

MEMBER POWERS: It has nothing to do with multiple pulses. You're making a case that says multiple pulses won't crack the cladding. You've got no data to support that argument.

MR. ROSENTHAL: No, but there's no data to contradict it either.

MEMBER RANSOM: I didn't think that's what they were trying to make. I thought they said there would be fuel damage, just not loss of coolable geometry. And that satisfies the G-68.

MR. SCOTT: David, this is Harold Scott. The Japanese did do one test in NSRR, where they did have multiple pulses. I think it was called I-11 or something, so there's at least one thing like that.

MEMBER POWERS: I think they'd be hard pressed to use it to make their case here though.

MEMBER ROSEN: And what was the result of that Japanese test?

MR. ROSENTHAL: Did not fail.

MEMBER ROSEN: Okay.

MEMBER SIEBER: But that's not the criterion that you're using here. You're allowing failure. You just want to make sure it cools.

MR. BESETTE: The main objective is coolable geometry, that's the governing objective.

MEMBER RANSOM: Is that right?

MEMBER SIEBER: Well, the clad may not be the only effect. For example, when --

MR. di MARZO: I am Marino di Marzo. This is a presentation, the objective of this presentation is essentially three objectives. The first objective is to give you an idea of the mathematical models which are very simple, and provide some interpretation of the physical reality, and at the same time give you a tool to essentially end of mixing in a way that you can scale it from the U-Scale experiments that are available to the typical scale without too much of a controversy.

The other objective is to then assess the model that was presented in RELAP5/PARCS as reasonable for the vessel. And then the third part is to show how we're going to determine the boundary condition to the vessel depending on each of the scenarios, as far as the deborate movement. So as far as the model goes, this is very old material.

MEMBER POWERS: I said it once, I'll say it again - anybody that cites Levenspiel is okay in my book.

MR. di MARZO: All right. I'm blessed. It's extremely old material in the sense that what we want to do is to look at it in a very simplistic fashion, and look at true limiting condition. On the one end we want to look at the situation where we have plug flow. That basically means that an input signal enters the volume and exits exactly the same without any alteration, just the time delay. On the other end of the spectrum, that would be a totally unmixed-type process.

On the other end of the spectrum, we have something that we call backmix flow. You can call it a mixing cup. You can call it a completely mixing reactor, or in any other way. But basically, you have a totally steered volume in which you put new and then you get whatever comes out of the other side.

The formulation for that is the listing here, where you have a current here that is multiplied by the input function that you have. Now it is convenient for what we are going to do to define a time, an undimensional time which is the ratio, the volume of the slug divided by the volumetric flow rate. That we call a transit time. That will be the time it takes the slug unborated to go through a cross-section. So that way we can eliminate essentially time from your equation, and just get a generic type profile of what the concentration look like during the transient.

VICE-CHAIRMAN WALLIS: You're scaling.

MR. di MARZO: Right. So now the nice thing about the equation that is up there is that the only thing that matters are volumes. We are not making any statement in this approach as to the amount of mixing.

VICE-CHAIRMAN WALLIS: Ratios of volumes.

MR. di MARZO: Ratios of volumes, is either totally mixed or totally unmixed. And that's very important because it enhances the portability of what we do at one scale to another scale, provided that we retain the same volume.

So now to the left here is what has been done in PARCS lot as shown before. You have a time-dependent volume where you put your first function in terms of dilution that feeds one node which represents the lower head. And then there is a junction which has no volume, it's just a junction that feeds all this part of the channels which are your core. So that's basically what's in there. That's the RELAP modeling of the vessel.

Now what we tried to do here is to look at the vessel in the following simplified way, to look at plug flow in the core that is in these channels, look at the backmix flow in the lower head that is in the portion at the bottom, and then plug flow in the downcomer. This is by no means an attempt to actually model what it is, but it's just simply a concoction, if you wish, of mathematical tools that give an answer and a series of assumptions that we then have to test against, some data and some experimental areas.

Now as far as the lower head goes, the geometry is quite important. What you have is if you wish a spherical angle here, or the region between two hemisphere, which is reasonably free from impediments for the flow, and then you have a highly constricted region going from this inner structure here through a number of sets all the way to the vessel. You can count one, two, three, four, and five screens essentially where the flow has to go through.

VICE-CHAIRMAN WALLIS: Marino, that lower colander has a lot of holes.

MR. di MARZO: A lot of holes.

VICE-CHAIRMAN WALLIS: Using jets which are likely to produce --

MR. di MARZO: That's right. So there are jets through all this --

VICE-CHAIRMAN WALLIS: Particularly through the lower colander.

MR. di MARZO: The lower colander is the first, and then --

VICE-CHAIRMAN WALLIS: You're not mixing in that lower volume.

MR. di MARZO: Absolutely. In this volume here there will be a lot of mixing, and there will be also mixing in the region in-between, this region here and this region here. So an analogy of what you're looking at is a distributed head, if you wish, with extremely strong resistance on the distribution, so that is a typical reasonably well distributed head in our way of putting it. So that's the configuration of the lower head, and that's why the idea is to use it as a backmix flow there.

Now let's move forward and concentrate on this picture here, which is again from Levenspiel. What we did is this; we took an F function. An F function is essentially a step going from zero to one at time zero, and we fed that into RELAP, into the model of RELAP as presented. So as this step function goes through that one volume that represents the lower head, we measured the output out of the RELAP calculation. That output is this thick line over here. Okay. If you put that in the context of this picture, you can see that this line here is very close to the backmix flow line, which has a dispersion of infinity. In other words, it's a completely mixed volume. In any case, it's in a region where you will say there is a large amount of dispersion, or a large amount of mixes.

Now as far as the reactor vessel goes, where you basically have the stack of nodes, we did the same problem. We essentially sent a step function through, and we look at how this is mixed as it moves through. We compared that solution with a solution given out by G.I. Taylor of a flow of a certain concentration following a flow of a different concentration, and we compare the result of RELAP with the results of the theoretical case. And again we find low levels of dispersion. In other words, we are in a situation close to this line over here, between this line over here, but less.

VICE-CHAIRMAN WALLIS: Since RELAP models complete mixing - doesn't it - why doesn't it lie on the line?

MR. di MARZO: Because when you have a stack of nodes, basically it's like having a series of --

VICE-CHAIRMAN WALLIS: A stack of nodes.

MR. di MARZO: It's a stack of nodes, so in that sense you get something -- your arithmetical diffusion but --

VICE-CHAIRMAN WALLIS: This the lower plenum plus the downcomer?

MR. di MARZO: No, this is just the vessel, inside the core. Inside the core there are only channels. Channels behave --

VICE-CHAIRMAN WALLIS: It says lower head, that's why I was asking.

MR. di MARZO: The lower head behaves like this. Okay. Which is a fully mixed volume. The channels in the core behave like this line here.

VICE-CHAIRMAN WALLIS: I just wondered why that doesn't follow the --

MR. di MARZO: Because it's a stack of nodes. It's not just one -- the lower head is only one node.

VICE-CHAIRMAN WALLIS: And it's mixed.

MEMBER ROSEN: It's very totally mixed.

VICE-CHAIRMAN WALLIS: I'm just puzzled by why RELAP doesn't run exactly along the theory, since it's modeling a mixed node.

MR. di MARZO: That I do not know, but the problem is this - I just took the answer that RELAP was giving, because there are options in RELAP, and I don't know -- it must have been exercised in that particular node, so I do not know. But what I know is what comes out of it. And looking at that response, essentially what it does is what's depicted here.

MEMBER ROSEN: Now what you're saying is that in the core now there's very little mixing. It's axial flow.

MR. di MARZO: Right.

MEMBER ROSEN: No cross-flow, very little cross-flow.

MR. di MARZO: Very little according to RELAP. Remember, this is only what RELAP does. Now in the lower head we have total mixing according to RELAP again. The downcomer is not present in the model formulated by RELAP5 in that supply to PARCS. So the only way you can represent it in that model is essentially a plug flow, because it's just missing. So that's basically the model that has been coupled with PARCS. That's what's there. I'm just simply using these simple mathematical tools to explain what RELAP is doing; no more, no less. No attempt to say it's right or wrong. It's just something like this.

Now on the other end, we have performed experiment at Maryland, actually it wasn't even me, it was another crew when I was not into the project any more, in the framework of a CS&I experiment, where essentially front was sent through the cold leg, went down the downcomer, and then was measured at that elevation. In research there was a CFD computation performed of the same geometry, exact same geometry of the experiment for all the downcomer, the lower head, up to the core entrance. Those two --

MEMBER RANSOM: This is a model of the Babcock & Wilcox system. Right?

MR. di MARZO: It is a model of the Maryland facility, which is a model of the Babcock & Wilcox.

MEMBER RANSOM: Okay. Right.

MR. di MARZO: And the results I have, but in the interest of time, I'm going to move on and not show. But basically, those two -- the CFD computation and the Maryland experiment are in extreme agreement. There's very good representation of that.

So here what you have is the simplified model where there is a totally unmixed downcomer, and then there is a fully mixed lower head going to the core. And superimposed on this is the CFD calculation.

Now what these bars represent is the distribution that you have about that difference, about that -- remember, this is just a location across the entrance of the core, so there's a distribution.

VICE-CHAIRMAN WALLIS: No experiment in this --

MEMBER RANSOM: That's data you're talking about.

MR. di MARZO: This is CFD calculation validated against data.

VICE-CHAIRMAN WALLIS: No, against the MM. It's the MM versus CFD. The experiment must be somewhere else.

MR. di MARZO: Yes, you want to see the experiment --

VICE-CHAIRMAN WALLIS: It's not shown in

that figure.

MR. di MARZO: It's not showing in that figure, but I can go --

VICE-CHAIRMAN WALLIS: No. I'm just pointing out that you haven't said over there, but it's not --

MEMBER SIEBER: We did it already.

MR. di MARZO: You did it already. Okay.

MEMBER RANSOM: For the error bars or just from the CFD calculation?

MR. di MARZO: The error bars to the CFD calculation. Okay. Now refer to the previous presentation. What it is that gets you into trouble here are two things; is the magnitude of the slug, and essentially how low does it go in terms of Boron concentration, one. But most important is the sharpness of the entering flow.

Now in the model that we have used to generate the input that generated the result that you've just seen, basically we used a black line and look how sharp the entering slug is compared to what it would be if you use a less conservative, if you wish, approach of using the CFD calculation. So that already there introduces a quite conservative element in the results that you're getting.

VICE-CHAIRMAN WALLIS: I suppose they could be sharp if you actually use those error bars. You could create a --

MR. di MARZO: You could go there, and then if you use the top --

VICE-CHAIRMAN WALLIS: No, use the top of one, and then you zip down to the bottom of the other.

MR. di MARZO: What does it -- what the error bar means is this; is essentially 10 percent of -- there are fingers of high concentration and low concentration. That's basically what that means. Now on the low end we bound the lower edges of those concentrations, and so essentially we are conservative again. So this representation is a very simplistic mathematical representation, has the feature of adding a sharper edge here, and has the feature of adding a low concentration over here. So in a sense, it's very simply. It enables us to port it from this use scale to the large scale because the only argument we have to make is volumes, and therefore, we use that as input to the RELAP/PARCS computation. So that is what we are doing for the vessel.

Now the first -- we've seen the conclusion, but what we have said is that the model that's present in PARCS/RELAP is reasonable, albeit conservative with respect to what reality could be, at least for the data and the computation that we're performing.

Now we have to solve the problem of what do we feed to the downcomer. And that depends on a variety of initial conditions. You start the pump, you start the - whatever the situation, where the slug is, and how do you form the slug and all these things. So in order to do that, we conducted a series of experiments again at Maryland, and they were based on a set of assumptions. And the assumptions were as follows; this is the steam generator, the lower portion of the steam generator. This is the steam generator outer plenum, and these are the two legs. So for the pump case, this pump will be activated and essentially will draw from the tubes and we also draw from the other side, typically. So what we're trying to establish here is can we use simple models like before in order to represent this situation. And the idea is to use plug flow in the PARCS, because PARCS do not mix much. And to use instead completely mixed volumes in the steam generator outer plenum, because there are two effects here that comes into play. First, the flow comes out of all the tubes, and those are again jets coming into the plenum; and therefore, enhanced mixing. And second, there is flow from the adjacent leg coming in and mixing, stirring up the volume as well. So this will be a fully mixed volume.

The other fully mixed volume is the volume of the pump. Now there is nothing magic about the volume of the pump. It's just an assumption; the idea being that the pumping pellet being moving or addressed will generate some volticity and therefore some mixing in the flow downstream. The equivalents of all this mixing is a fully mixed volume of the pump. That's just the assumption that we're making. So we made these two basic assumptions, and then we ran a test. And I have the results of the test if you want, but basically we activated the pump and measured what was going through. And then we calculated with this simple model that they explained to you what happens, and the front of the slug, which is here at this point as you activate the pump, we go only through the pump, so the mixing that the front experienced is only one mixing volume, the volume of the pump. Therefore, it maintains its sharpness. Depending on the slug, which is back into the steam generator on the other end experience mixing because it goes through the steam generator outer plenum first, and then through the pump second, so it's a much more slanted-type process. What we get is this line.

VICE-CHAIRMAN WALLIS: That's your measurement at the outlet site.

MR. di MARZO: Yes, the dots are the measurement at the outlet pipe. And the line is what you get from -- these are close form solution of the same equation that I showed you on the very first slide. And again, I've got the case if you want.

So this gives me a tool to predict the input to the calculation, so that's basically the methodology of the tools that were used to generate the results that David Diamond just showed you for a variety of conditions.

VICE-CHAIRMAN WALLIS: This is also a peer review document, and --

MR. di MARZO: This is -- right.

MEMBER RANSOM: This has got the equivalent of an ACRS standing ovation silence.

MEMBER POWERS: That's the best thing that could happen to you.

MR. di MARZO: These are my conclusions. So the RELAP/PARCS model for the in-vessel mixing is reasonable, albeit conservative. These mixing models are used to generate the boundary condition to it, and basically what we do with that, we fill them in the time dependent volume, that's at the bottom of -- the input of the RELAP code, as shown.

MEMBER RANSOM: Thank you. I think this was really the key behind resolution of this issue, because all of the work that had been done in the past that brought this issue to the front assumed plug flow throughout, so the step change, instantaneous entrance to the core, and that did create reactivity transients that would bring about core damage, and so this was a very important contribution I think, and it adds realism, as well as still some conservatism to the analysis. So thank you.

VICE-CHAIRMAN WALLIS: If he had also done the other extreme, like putting in a slug with no mixing, it's showing that that gave a bigger reactivity transient. So you didn't do that actually, you just put in a more realistic one. It's perfectly okay, just careful about the word "conservative" if you didn't do the alternative thing -- it's been done before. You did it, that's right.

MR. ROSENTHAL: Right. And they use a point kinetics, we used a 3D kinetics model. In my branch, of course we do reactor physics and we do thermohydraulics code development. I also have a substantial fuel program underway. And Ralph Meyer sits on the other side of a partition from me.

MEMBER ROSEN: Never heard of him.

MR. ROSENTHAL: It would have been I think improper to use the regulatory limits on acceptable fuel enthalpy deposition when research itself had issued a letter to NRR, and we're working with NRR advising them for high burn-up fuel, the permissible enthalpy deposition in a reactivity insertion event, you have an injected rod specifically might well be lower. Dana, of course, is familiar with that work, and I asked Ralph to come down, Dana, because I thought that he might be able to better answer questions on relative fuel time constants, et cetera, than I am.

MEMBER POWERS: Ralph, the question that I have posed is that in the course of discussing this resolution some power inputs to the fuel over relatively long time schedules compared to what we're used to for reactivity transients are predicted, but -- and there was a confidence that this was okay. And the articulated basis of that confidence was the 280 calorie per gram geriatric criterion. And that's only one issue.

The other issue is that it seems to me the analyses have been done not looking at the most pathological configuration of fuel; that is, the configuration that was examined, clearly the most energetic events occur in the fresh fuel, but the question is what about adjacent assemblies that have some burn-up, the adjacent assemblies that were examined had rods in them. So one obviously asks the question what happens if the adjacent burned-up assemblies don't have rods in them. And so I guess the question being put to you is, is it, in fact, okay to have a fairly potent energy inputs to fuel rods that over some protracted period of time - well, protracted, of course, is measured in seconds, but not measured in milliseconds - and how do you know? And not necessarily single impulses, but multiple impulses.

MR. MEYER: Okay. I'm Ralph Meyer from NRC's Research Office. Harold showed me out in the hall before I came in, showed me slide 11 in David Diamond's presentation, and pointed out the two pulses that you were thinking about. The first one was the initial pulse, which is very sharp, but had an energy content of something on the order of 40 calories per gram.

Now at 40 calories per gram, we know from test pulse experiments that 40 calories per gram is not sufficient to cause cladding failure or crack in the cladding, or perforation of the cladding in any way. So I would say that you can rule out any concern over that initial spike, because the energy content is too low.

The next one that Harold points out is very broad, if I'm looking at the right figure, and has a peak fuel enthalpy of 180 calories per gram. Okay. So you can see that on the scale here on the right-hand side. And as you mentioned, Dana, it's the half-width of this pulse, the full width at half-maximum is several seconds.

MEMBER POWERS: I'm not sure that's a good measure for this particular scenario. I don't think you want the half-width - I mean, pulse width at half-height. I think you want the ramp time here.

MR. MEYER: Well, you want the which?

MEMBER POWERS: The ramp, how fast you get up to the peak. And it's over 2 seconds. It's slow.

MR. MEYER: Actually, what matters is how much time elapses until you cause a failure of the cladding, and now it depends on several variables, and we could talk about whether this is high burn-up fuel or low burn-up fuel, whether it is heavily corroded or lightly corroded. And all of those would make a difference --

CHAIRMAN BONACA: This is fresh fuel. Right?

MEMBER POWERS: It's fresh fuel.

MR. MEYER: I can't guarantee the loading pattern, so for the purpose of this meeting you have to assume that the enthalpy deposition that we're showing in the fresh fuel, in fact, could conceivably occur in burned fuel where we know the limit is lower.

MEMBER ROSEN: Typically in accord with a thrice burning fuel, this would -- the beginning of the third cycle.

MEMBER SIEBER: But the key point is that the fuel after it goes through this transient has to only be in coolable geometry, and so that's a different criteria than the burn-up one, and enthalpy limits that we're talking about here.

MR. BESETTE: Actually, there's only really one pulse in this event. And basically, you're sitting around 100 percent power, and you heat up over the course of about 5 seconds. And, in fact, these other things you start to think about, and most importantly, some of these rods end up in DNB for a period of 10 seconds or more. So you're no longer dealing with a reactivity pulse after 8 seconds or so, you're dealing more with a --

MR. MEYER: The thing that we're really -- that's relevant. And the thing that we're really concerned with here is not the failure of the cladding, but whether you're going to eject fuel in a manner that would cause a fuel coolant interaction.

MEMBER SIEBER: That's right.

MR. MEYER: Because if you just lose a few fuel particles rolling out into the coolant, this is benign. And now that I see that picture clearly, we're not talking about 180 calories per gram, except in -- wait a minute.

CHAIRMAN BONACA: Some things that could be actually 20 percent above. That's an assembly-wise average enthalpy, as we heard before.

MR. MEYER: Okay. What I see in this figure is different from what I thought I heard from Harold, so maybe we're going to have to recalibrate here. Th is initial pulse reaches 180 calories per gram?

MEMBER ROSEN: No.

MR. MEYER: Is it the red line or the blue line that --

CHAIRMAN BONACA: Blue is --

MR. MEYER: Oh, the blue line. Okay. Yes, yes, yes, yes. I see it now. I see it now. Okay. Yes. Okay, so now I see where the 40 is, now I see where the 180 is. And it's starting at hot conditions around 16 or 18 calories per gram. Okay.

The overriding factor is the time here, and the dispersal of fuel is going to require one of two things. It's going to require either enough fuel enthalpy to cause significant melting, which we know from experimental work is about 230 calories per gram, so we're nowhere near that; or it's going to require a lot of fission gas on the grain boundaries, which can only come from high burn-up.

MEMBER SIEBER: Right.

MR. MEYER: And a narrow pulse with a fuel enthalpy of greater than about 80 calories per gram. Now what you have is a very broad pulse with a fuel enthalpy of 180 calories per gram. This pulse may result in cladding damage and cladding failure from a high temperature excursion, but based on the test results, would not be expected to drive hot fuel particles into the coolant, so it would be benign.

MR. ROSENTHAL: Maybe we should stop at this point and just summarize, because we're not doing frap tran analysis as we sit here, but we will be able to couple that in a year or two, and do an integrated total picture. Okay. So what we've argued is that for CNE and Westinghouse plants, just based on the size of the piping, you don't have a recriticality. You can dismiss the event. But B&W plants, it's an issue, not for raised loop, for lower loop plants, the majority of the plants.

We've argued that the likelihood of the event is reasonably low. Should we have that event, I can end up in natural circulation for which we've mechanistically shown that we've have low enthalpy deposition, and the fuel will survive.

I cannot be dispositive that for the perverse pump case that I won't damage some fuel. We've argued that the damage of that fuel would be limited in radial and axial extent, and coolable and with ECCS available by virtue of the scenario we're talking about here. So for the one case where I cannot be dispositive, the B&W pump case, we know that there are procedures that have been put into their EOPs, and the bases document explains why they're there, and that's the basis that we think that no further action is necessary.

MEMBER ROSEN: The only thing I can quarrel with with all of this is the use of the word "benign" when one talks about this event. It would be a very unbenign thing for the plant manager and his staff.

MEMBER SIEBER: Yes, but getting there is --

MEMBER ROSEN: I think I know what you mean, but it's not a benign thing.

MEMBER POWERS: Getting there is not benign either.

CHAIRMAN BONACA: Vic, are you going to wrap it up?

MEMBER RANSOM: I think we're through.

MEMBER POWERS: Very good. I guess I still have one question. I have a lot of questions, but I'll ask one question. The famous blue line here which isn't that some place in Baltimore - reflects an assembly average the worst broad looking line.

MEMBER DENNING: The black line, that's not assembly average.

MEMBER POWERS: Oh, that's not the assembly average.

CHAIRMAN BONACA: Well, we were told it was assembly average.

MR. MEYER: I said there was a difference of about 20 percent.

CHAIRMAN BONACA: Okay. So the assembly average would be lower than that, and the 20 percent is added on top. Okay.

MEMBER SHACK: But Jack's slide said that you will get some fuel melting, center line melting.

MEMBER SIEBER: That's right. That's where the hotter --

MR. MEYER: Let me comment on that because we have experimental data for fairly narrow pulses that address this. And I'll just repeat it again. You've got to get about 230 calories per gram in there, which would involve already some incipient melting which may start around 150 calories per gram; but we know experimentally that you need over 200 before you start really breaking up the fuel, and putting small pieces into the coolant.

MR. ROSENTHAL: The last thing I'm reminded that we've made a reasonable technical, multi-discipline case, and what we need from the ACRS is a letter.

MEMBER RANSOM: Thank you, Mr. Chairman.

CHAIRMAN BONACA: Okay. Thank you. If we have enough time at this meeting, we'll have a letter. We're struggling with that. You will have a letter from us. Okay. We still have one presentation on the agenda, and as I said, at 6:00 I'm going to head out, and we need to absolutely have a discussion tonight about some issue, so we'll try to do the best we can with the next presentation.

MEMBER SHACK: We have one hour. Is that what you're saying, Mario?

CHAIRMAN BONACA: Yes, we have just about one hour. And if we need two, then we'll have to postpone the rest of the presentation.

MEMBER SIEBER: I'll give the introduction, by the way, in an effort to cut off at the pass things we've already -- are we ready to begin?

CHAIRMAN BONACA: Yes.

MEMBER SIEBER: Thank you. Our last subject today is a review of a document that is provided to each of us at Tab 5 in our books, which is a draft NUREG entitled FX-XXX, that reports on the analysis of the results of the pilot program along with six recommendations that the staff believes should be incorporated into a final mitigating system performance indicator program.

I would point out that this project has been going on since September, 2002, and originally started in 1999 when Chairman Jackson gave the suggestions that the regulations be risk-informed. One of the early projects was the development of the ROP, which relies first on inspection findings which through the significance determination process are color-coded for risk-importance, and performance indicators which initially were not risk-informed and, therefore, did not meet the original guidance where risk information was to be used to the extent possible.

In the Mitigating System Performance Indicators area, there was a safety system unavailability which for PWRs tracked the unavailability of high head injection, low head injection, RHR, diesel generator, service water, and so forth in the equivalent pieces of equipment for BWRs, HPSI, RCSI and emergency power and so forth. So you ended up in the Mitigating or in the safety system unavailability indicator a number of indicators which now under the Mitigating System Performance Index will all be rolled into one.

The new proposed index, the development of that was started in September, 2002. We met twice on that as a subcommittee, and at one time had a full committee presentation to describe what those were. And briefly, the Mitigating System Performance

Indicators are risk-informed. They are based on SPAR models which have been indexed to the plants' PRAs, and therefore, are plant-specific. And they really are a summation of the Birnbaum Importance Factors, and take into account both unavailability and unreliability of each of the components selected to be a part of this.

During the development phase, which lasted about six months, the basic structure of the index was developed and a 20 plant pilot program was begun, which again lasted for six months, during which a lot of Lessons Learned occurred, and now we have this draft NUREG which I'm sure we've all read, and I have read it. And it provides a number of suggestions, many of which were already transmitted to us previously; the idea of front stops and back stops, and sensitive and unsensitive parameters were important factors that have been known for probably about a year now, and incorporated into the process. So what we'd like to do this afternoon is to review the draft NUREG report which is the analysis of the results of the pilot program, and see the extent to which the MSPI is now ready for integration into the ROP.

I would point out that the ROP does not

represent anything safety-related or safety-significant. It is an administrative tool that is an outcome of the ROP action matrix to guide the staff in allocation of resources toward licensees. So from that standpoint, the MSPI may not and need not be perfect in every respect, but suitable for the purpose for which it's intended, which is the operation of the inspection and enforcement part of the Commission's mission.

So with that, Pat, I think you can proceed. Anything I've covered, you may skip because we must end at 6 p.m. The microphones are shut off at 6 p.m.

MR. BARANOWSKY: Okay. I am Patrick Baranowsky, Chief of the Operating Experience Risk Analysis Branch, and I have Donald Dube, who is a Senior Risk Analyst in my branch here who will talk about the MSPI, and Stu Richards from NRR's Inspection Program Branch who will talk about implementation issues. And I'd like to thank you for the introduction because it's going to make my job a lot easier. I don't have to repeat things that you said, and we will move along accordingly.

We are going to give you the status of implementation. Stu will actually present that.

We're going to go over a few technical issues that were still open, I think, at the last meeting. You did have the report which provides a fairly significant discussion of those issues. And at the end, we want to talk about having a letter from the ACRS. So this is the content. We made a few changes in order here. I'm going to do the MSPI overview, Don will do the technical discussion, I'll get back to the summary, and at the very end we'll follow-up with the implementation issues.

We think that the work that we've done indicates that the MSPI is a robust performance indicator that can differentiate risk-significant changes in system performance, and is reasonable for the intended application. It's been tested, evaluated through the pilot program, as you mentioned. We have a good understanding of its characteristics, its strengths, its limitations, and we have pretty significant documentation on all the issues that are associated with MSPI that we did quite a bit of study on during and after the pilot, and that's in the report that we sent.

We think it's pretty clear that the indicator is a better measure of system performance for many reasons than the safety system unavailability indicator, and that it addresses the known problems of the safety system unavailability indicator.

I'm not going to go all the history that John gave a great discussion on, but I do want to mention that the MSPI was formulated to address known issues with the safety system performance unavailability performance indicator, specifically the way fault exposure time was used, the fact that unreliability elements were not in the indicator. There were some definition differences and unavailability in that indicator, and some other indications, such as what's used in the Maintenance Rule and INPO WANO indicators. There was a cascading of failures using the SSU from support system to front line systems, which gave multiple hits for a single issue and was problematic in terms of dealing with the action matrix. And the thresholds were minimally risk-informed, and certainly not plant-specific, so we went through the history of developing the indicator, as discussed.

Our conclusions are that we've tested, evaluated this through a pilot program. I'm at the wrong thing. And now I go to Don. I' almost skipped the whole hour.

MR. DUBE: Thank you. I'll go through

this quickly. The MSPI accounts for unavailability and unreliability which occur indicated as an I, and it uses the plant-specific PRA model to derive risk-important measures, so it really captures the plant-specific configuration and performance which the current indicator does not.

The data will be consistent with current PRA methods which is not necessarily the case of the current SSU, and will be consistent with the maintenance rule. The data will be integrated with the consolidated data entry program under INPO's jurisdiction, so it's going to be kind of a one-stop shopping for data. Licensees will send their data to INPO and it will be used for a number of things looking at equipment performance, system performance, but also part of it will be used for the MSPI.

MEMBER APOSTOLAKIS: I am a little bit surprised that you guys don't put as part of your advantages for going with MSPI the fact that, I think it addresses the - what flaw was that, fundamental flaw of the ROP. What do we call it, it was another adjective.

MEMBER SIEBER: Which one?

MEMBER APOSTOLAKIS: The ACRS identified a fundamental flaw, which was changing each indicator, and then seeing what happens to CDF and then based on that, setting the thresholds. And we argued that you can't do it one at a time. You shouldn't be doing it one - because the core damage will not occur because one indicator or one unavailability went too high. It will be the combination of things. And I think by putting this Birnbaum measure there, you're actually addressing this issue.

MEMBER SIEBER: That's right.

MEMBER APOSTOLAKIS: And I would make a big deal out of it.

MR. DUBE: I would say an ACRS letter could make a big deal out of it.

MEMBER APOSTOLAKIS: No, but you go back to that letter. I mean, our major complaint in all the letters we've written on ROP has been that. You understand the issue?

MR. DUBE: Yes, I understand --

MEMBER SHACK: If you set the threshold based on this, you still have that problem, if you're looking at the -- if you use the Birnbaum, it integrates it, but you're still looking at the change due to this specific set of --

MR. BARANOWSKY: We are holding other factors constant.

MEMBER SHACK: Other factors constant.

MR. BARANOWSKY: We are holding them constant, but we're adjusting them to whatever they are at that time. They're not being held constant forever. They get updated --

MEMBER SIEBER: It's the combination of these factors that go in there.

MEMBER APOSTOLAKIS: But it's a step toward resolution of that.

MEMBER SIEBER: And rather than look at peer comparisons --

MEMBER APOSTOLAKIS: The other guys are on the PRA, so you better not refer to it.

MEMBER SIEBER: Rather than look at peer comparisons for green and white threshold, you're looking basically at risk information, which I think is an improvement. And that's certainly in there, and it's one of the features.

MEMBER APOSTOLAKIS: Do you have to tie this to train unavailability? Is it to be able to, as you say, be consistent with what other people are doing?

MEMBER SIEBER: It's train unavailability.

MR. DUBE: It's train unavailability --

MEMBER APOSTOLAKIS: I know what it is,

but does it have to be? It doesn't look like it has to be. I mean, it can be a component, internal component, couldn't it?

MEMBER SIEBER: There was arguments in the paper why it was better off being train rather than --

MEMBER APOSTOLAKIS: Well, one of the reasons is the Maintenance Rule, I think.

MEMBER SIEBER: Yes.

MR. DUBE: Won't have to collect extra data.

MR. BARANOWSKY: The way the formulation is, we could actually take any set of items in the plant. It doesn't make any difference whether it's trains, or components --

MEMBER SIEBER: And apply those.

MR. BARANOWSKY: So that's a kind of unique thing about it.

MEMBER APOSTOLAKIS: No, because you have some limitation that you don't include common cause failures. But if you went to a component level, then you could include it.

MR. BARANOWSKY: Well, we include common cause failure as a factor to recognize the importance of failures, but what we have trouble doing is taking a common cause failure event and as a result of it, making a change to the common cause failure parameters, because the time frame for updating information is too short to get a good estimate of the common cause parameter.

MEMBER SIEBER: One of the other factors is the back-stop provision in a way provides a mechanism so the common cause factors aren't overlooked all together for insensitive parameters. So even though it's sort of in the abstract there, there is a consideration, a process that must be gone through when people analyze what the MSPI really means as it's applied to the matrix for a given plant, as I see it.

MR. BARANOWSKY: Yes. The methodology presumes that common cause failures can be treated through correlations of single failures.

MEMBER SIEBER: Right.

MR. BARANOWSKY: But that the occurrence of a common cause failure where multiple components fail is so significant that we want to look at that separately, so we put that off to the significance determination process. It's a blend of things.

MEMBER SIEBER: And that's because the common cause failure of that nature is probably a cross-cutting event.

MR. BARANOWSKY: Yes, it has big implications.

MEMBER SIEBER: That's right.

MEMBER SHACK: Well, the back-stop is also purely a performance measure.

MEMBER SIEBER: That's right.

MEMBER SHACK: So it does solve some of the problems that we originally had with the ROP.

MEMBER SIEBER: Yes.

MR. BARANOWSKY: Well, we were listening to you guys, and we --

MEMBER SIEBER: Well, the way I addressed all that in my draft letter was to say you have listened to and incorporated our comments in the past, which include all of these things.

MR. DUBE: I decided to use a layman's definition, so there are no equations here. But a good way to relate what the MSPI is, it's a measure of the deviation of plant system unavailability and component unreliabilities from historical baseline values, so you have HPSI pump unreliability at a plant, minus a historical value. If it's positive, that's bad because unreliability of that pump at the plant is worse than the industry norm. But we can relate unavailability and unreliability by their impoortant, their risk-importance, so that factor, if you will, that coefficient is what relates unavailability and unreliability, and makes them an apple-to-apple comparison, which I think is somewhat unique. And then we can also compare the importance of a pump in a system, or the valve in a system again by the importance, weighting by the importance measure. So it's an interesting way to combine unavailability and unreliability into a single system measure.

MEMBER SIEBER: The valves have been excluded from the analysis.

MR. DUBE: Well, low risk important valves can be excluded, because --

MEMBER SIEBER: Even though they're active.

MR. DUBE: Yes, because we determined that if we excluded low risk important valves, it would not change the index by any measurable amount. It would be insignificant.

MEMBER SIEBER: Just so that's clear.

MR. DUBE: And so in that way, if a valve is important to the PRA results it will be included. If it's below some truncation level, some threshold, we decided that the cost of collecting the data did not outweigh whatever impact it had on the MSPI.

MEMBER SIEBER: Right.

MR. DUBE: It would leave out.

MEMBER ROSEN: The low risk importance of a valve is known in every plant? I mean, the risk importance of each valve?

MR. DUBE: There's a threshold. It would be a Birnbaum of 10 to the minus 6, so licensees will calculate this, and if they're below -- if a valve is below it, they can leave it out of the system.

MEMBER ROSEN: I'm trying to get to the question of is there a plant out there still who is so non-PRA informed that they can't tell you the risk importance of their valves?

MR. DUBE: No, they should all have it.

MEMBER ROSEN: They all have them.

MR. DUBE: Yes.

MEMBER ROSEN: Maintenance Rule forced that.

MR. DUBE: Oh, yes, definitely.

MR. BARANOWSKY: Where or not their PRA is complete --

MR. DUBE: It can be easily calculated.

MR. BARANOWSKY: Yes, whether the PRA is adequate or not, we have an issue on that. But they have something.

MEMBER SIEBER: That's for another day.

MR. DUBE: So I'm on the technical approach, I'll go quickly. But basically, I mentioned it before - it's an approximate change in CDF, and it's not an exact because it's tail expansion, if you will, and we're only using the first term, and there are other terms. But for what we're looking at, which is trying to look at deviation of system performance from the norm, we feel that it does a good job. It includes unavailability and unreliability, and as I said before, it accounts for plant-specific features, and plant-specific core damage frequency.

MEMBER RANSOM: Is the baseline that it's compared to plant-specific, or is that an industry baseline?

MR. DUBE: Industry baseline, generic industry baseline on unreliability.

MEMBER SIEBER: The system and component level depends on whether you're talking unavailability of unreliability.

MR. DUBE: Yes, there are some differences, but basically it's generic industry data.

MEMBER APOSTOLAKIS: Let me understand that a little better. Aren't you updating as you go?

MEMBER SIEBER: Yes.

MR. DUBE: No, we're using data that's roughly representative of 1995 to 1997 industry performance which has been deemed by policy to be acceptable.

MR. BARANOWSKY: Plus the standard that was set during the ROP development, the Commission actually bought into that. And even though we're using data that's more current, what we've done is benchmarked it to see whether it's -- it's a little bit conservative, so we got somewhat conservative improvement over that `95 to `97.

MEMBER APOSTOLAKIS: So the SPAR model is plant-specific only in the sense of the full event is being plant-specific.

MR. DUBE: SPAR models currently don't have plant-specific failure rates. It could. That's the next step.

MR. BARANOWSKY: And when we put the MPSI data in, that is plant-specific failure rates, and then we compare that to the baseline, which is a generic number of `95 to `97 time frame.

MEMBER SIEBER: But the SPAR models have been benchmarked and are within a factor of 2 to 4 of the plant's PRAs as I understand it.

MR. DUBE: WE've had a major effort on that.

MR. BARANOWSKY: Yes. We're actually able to get a lot closer but where we are factors of 2 to 4, we've identified the factors within the models that cause that difference, and that's part of our PRA adequacy resolution activity to get those things worked out.

MEMBER SIEBER: But this has been addressed by the staff as an issue.

MR. BARANOWSKY: Yes.

MEMBER SIEBER: An ongoing issue in the development of the MSPI.

MR. BARANOWSKY: Yes.

MEMBER SIEBER: And it's in-hand now.

MR. BARANOWSKY: Yes.

MR. DUBE: These are the systems, I won't spend any time, but it's basically high pressure systems, aux feed. Generally, the most risk-important systems. And what we have that's not in the current ROP are support system cooling water systems, service water, emergency service water, component cooling water.

Now I'm going to shift over to the resolution of the key technical issues. Some of them we've discussed before, but we've reached a decision on these, both the NRC as an agency, as well as the working group with the industry.

Velocity behind the front stop is that expected performance variation should not result in crossing a performance threshold. In other words, there is some distribution, a component, an automobile, a pump, there's some normal distribution to failure rates, and within some range, one would expect some variation. And just because it's slightly worse than average, or slightly better than average, that's a normal expected variation.

MEMBER SHACK: But why didn't you define the front stop as sort of the inverse of the back stop? I mean, you defined the back stop in exactly the way I thought you would. You would look at sort of the number of failures you would expect to get, and if you got more failures, you knew you had a problem. Here, why didn't you do it in the same way - define the sort of number of failures you expected to get, and accept it. And you somehow introduce this artificial capping or the risk cap, and I can't quite figure out --

MR. DUBE: Well, because the expected number of failures typically is like .1 or .2 on many components on many systems, so --

MEMBER SIEBER: And it can result in a risk number greater than what you would normally expect, and that's why the cap is there.

MEMBER SHACK: So you're really saying that one is the smallest integer that corresponds really to what you're expecting there is.

MEMBER SIEBER: Right.

MR. DUBE: That's the challenge.

MEMBER SHACK: That's the challenge. Okay. That's reasonable enough.

MR. DUBE: So the front stop is a mechanism and it is just that it minimizes the likelihood that one failure or one failure beyond baseline, which is generally about one or two, in a three-year period results in white. But we built into this the allowance that the index could still become white with one or even zero failures if there's significant system unavailability, so I mean it was -- there's so many degrees of freedom, but we built into it an allowance that even with the front stop, if the particular system had a large amount of unavailability, it would still become white. And that's why we thought it was a better mechanism than having a white failure, a hard and fast one failure not be white, and so we think it's kind of the best of all worlds.

So a decision has been made to move forward with the front stop. It's one of the recommendations in the NUREG report.

MEMBER SHACK: They're going to still do an SDP on that failure. Right?

MR. DUBE: Yes. That was the big difference between four months ago, six months ago when we met and now.

The back stop is a recognition that there are some lower risk significant components, but the algorithm would allow a large number of failures before it turned white, but we just didn't feel that that was appropriate, so the back stop is a mechanism that results in white if a component type exhibits a statistically significant departure from the expected number of failures in a three-year period, regardless of risk-significance.

And just quickly moving on - the decision has been made to move forward with the back stop as recommended. And actually, there wasn't any controversy on that.

MEMBER SIEBER: Yes. ON the other hand, that does take you out of the risk-informed area, except to the extent that it deals tangentially with a common cause failure, but it takes some management insight to get there in each case.

MR. BARANOWSKY: I think but everybody agreed that when you have performance that's degraded to that extent, it's hard to say it's just oh, one component. There may be a lot more to it, and so pretty much agreement, industry and everybody else that that's something that we want to correct.

MEMBER APOSTOLAKIS: Let's go back to what Don just said, that if there is a statistically significant deviation from what's expected, it moves on to white, so it's not tied to CDF then.

MEMBER SIEBER: No, it's not --

MR. DUBE: The back stop is performance-based.

MEMBER SIEBER: It's not risk-informed.

MEMBER APOSTOLAKIS: So it's really performance-based, which is good.

MR. DUBE: And it's an or situation. You could turn white --

MEMBER APOSTOLAKIS: Which is what -- we also argued that.

MEMBER SIEBER: That's right.

MR. DUBE: It could turn white because you exceed the CDF threshold, or it could turn white if you exceed the performance-based back stop.

MEMBER APOSTOLAKIS: Now from the pilots, which one did you see dominating?

MR. DUBE: Well, we designed the back stop so that it would be invoked infrequently, and we didn't see it - we came very close. San Onofri had a back stop limit on the salt water pumps of seven, I believe, and they had six failures in a three-year period. They could still get that seventh one sometime in the future.

MEMBER APOSTOLAKIS: So the delta CDF.

MR. DUBE: Was low.

MEMBER APOSTOLAKIS: No, my question is there are two ways of getting into white, as I understand.

MR. DUBE: Yes. Delta CDF.

MEMBER APOSTOLAKIS: Delta CDF, and the other is the deviation.

MR. DUBE: Or the deviation.

MEMBER SIEBER: Or the back stop. Yes.

MEMBER APOSTOLAKIS: And you say that the Delta CDF was the one that put it to white --

MR. DUBE: Most of the time in the pilot, yes. And the back stops invoked a fraction of --

MEMBER APOSTOLAKIS: But isn't that a little strange?

MEMBER SHACK: Well, they have a fairly high -- you know, that would be one thing I'd quibble over is you're asking for a lot of statistical confidence. I don't know how you came up with those numbers and whatever judgment, but you could have made those numbers a little lower, and then your back stop would have gotten you there faster.

MEMBER APOSTOLAKIS: We first -- let's say a plant starts deviating, wouldn't you first deviate from the industry average significantly before you hit a Delta CDF? I mean, that's what I would expect.

MR. DUBE: It's a function of the risk-importance of a particular component. It's a strong function of the risk-importance of the component too.

MEMBER APOSTOLAKIS: Well, intuitively I would expect it the other way.

MR. DUBE: But we specifically designed the back stop to be infrequently invoked as a last measure.

MR. BARANOWSKY: And you'll recall, we are tracking some component, specifically valves with very low risk-importance.

MEMBER APOSTOLAKIS: Essentially there you are saying --

MR. BARANOWSKY: Those are the ones where you could have a lot of failures before you ever get near risk.

MEMBER APOSTOLAKIS: So that's where you see the move to white before.

MR. BARANOWSKY: So at least these have some measure of risk-importance that's worth looking at, but it's not that high.

MEMBER SIEBER: Okay. Why don't we go to the short-term back stop.

MR. DUBE: Well, when we did a benchmark and we took all of the whites and near white from the pilot plant, and tried to understand them, tried to compare them to what SDP, Significance Determination Process, showed, what the SSU showed, there was one that we couldn't explain where the SDP gave it a very clear white. It was a high white, and the MSPI for a number of reasons showed it to be a high green. More unavailability or one more failure would have made it a white, but we tinkered around with the idea of a short-term back stop, but we reached the conclusion, which would have been expected number of failures over one or two quarters instead of three-years. And the long and short of it is we felt that it would complicate the index. It was not in keeping with the monitoring trend over a three-year period, which is what -- we kind of went into this pilot program with the understanding. And then the decision to keep the SDP somewhat obviated the need, because this event would have still been white. It would have been top white by SDP and not by MSPI, but it wouldn't have snuck through the cracks, if you will.

MEMBER SHACK: Suppose I settled for a one chance in 25 of a false positive for my back stop, would I have caught it then? I mean, you've got one chance in a hundred now.

MR. DUBE: No, I don't think so.

MEMBER SHACK: You still wouldn't have gotten it.

MR. DUBE: No.

MEMBER SIEBER: But the only reason you got a white out of the SDP is because an inspector had an inspection finding to which the SDP was applied, so now you're relying on the inspector and the inspection findings to determine the most significant weight that you would apply to the specific events.

MEMBER ROSEN: But isn't it true, Jack, that four EDG failures in the third-quarter would likely catch an inspector's attention?

MEMBER SIEBER: I would think so.

MEMBER ROSEN: So I don't think --

MEMBER SIEBER: Caught my inspector's attention, except it was only two that caught his attention.

MEMBER ROSEN: But the point is it wouldn't slip through.

MEMBER SIEBER: That's true. And I think that's justification for not further messing with the concept of a short-term back stop. I think it's okay as is, what you've done.

MR. DUBE: There was some staff concern on the use of a constrained non-informative prior. This is the prior distribution that's used, that we used plant-specific data, the Baysian update.

MEMBER ROSEN: By the way, I'm glad you didn't have that word in your definition of MSPI, "constrained non-informative prior".

MR. DUBE: We had looked at the CNIP along with others. It had the best false positive/false negative characteristics in our earlier report. With no prior, NUREG 17.53 found the index would have been much too volatile leading to very high false positive probability, so we decided it's good enough to proceed.

Now there are other promising possibilities, one of the authors, Dr. Atwood is here, but it would require much more data. We'd have to basically -- we are now with that where common cause parametric models were with calculating the parameters 25 years ago perhaps, so it has promise, but it would require much more data analysis and more development. So we feel that the CNIP is adequate to move on, and so the decisions have been made to move forward the CNIP, knowing that it's not perfect, but it seems to be the best of what we can do.

The final open issue had to do with PRA quality,and so as not to hang up the implementation of the MSPI, a separate working group has been formed that consists of three members from the NRC staff and two from industry. Basically, it's to determine the PRA quality needs for the MSPI application, the appropriateness of the ASME standard, what kind of documentation is needed, what are the main modeling issues. And they are building upon some of our insights from some of the SPAR and plant PRA modeling benchmarks that we did. And so that's moving forward, Garreth Perry is the chairman of that committee.

Finally, a couple of slides. We received comments from six persons or organizations. They were supportive of the MSPI technical concepts, the nuclear industry reps endorsed all six recommendations in the draft NUREG report. We did get some comments from Dr. Vesley who has his name on the importance measures so he knows something about it. The cohort effect, which has to do with the fact that it's only a linear approximation to change a core damage frequency, and there perhaps synergistic effects that could result. And we spent a lot of time and effort, did a lot of analysis, and ended up putting a whole appendix in the report, Appendix M, that we feel addresses those concerns.

We recognize that the MSPI is a linearized approximation to the change in CDF for given change in system unavailability/unreliability, but as I said, with the basic definition of the MSPI, is that we use the plant-specific importance measures as weights to look at the -- weighting the difference between actual plant performance and generic baseline. And that's their primary purpose, so they're derived once when a PRA model is updated, the values will be derived once and can be input into the consolidated data entry program at INPO.

CHAIRMAN BONACA: Is the cohort effect a result of your use of mains rather than components?

MR. DUBE: No. It's more a function of the fact that when you do a tailor expansion, we're only looking at Delta -- we're literally adding Deltas from Pump A, Pump B, Pump C, Valve A, Valve B - but if you look at cut sets, there are changes in Pump A, and changes in Pump B in certain cut sets.

MEMBER APOSTOLAKIS: So all you need is one extra term.

MR. DUBE: We could go to second order --

MEMBER APOSTOLAKIS: Second order are three terms. Two of them drop out, don't they, because they require a second derivative.

MR. DUBE: No, we don't have second derivative.

MEMBER APOSTOLAKIS: So it would be only one term, the cross-term, so it's not a big deal.

MR. DUBE: Implementation-wise it would be a big deal. And Dr. Atwood wrote a nice treatise in Appendix M on how one might do it in theory, but it does add a significant complication because you need to do -- get that second derivative, and for 50 components getting that second derivative of various combinations would be a PRA practice nightmare.

MEMBER APOSTOLAKIS: What do you mean 50 components?

MR. DUBE: Well, the MSPI has 50 components on it, typically.

MEMBER APOSTOLAKIS: System.

MR. DUBE: Total.

MEMBER APOSTOLAKIS: For one system.

MR. DUBE: For all six systems. You get cross-terms of Diesel A with Aux B, Pump B and so on and so forth, so it could get very complicated. It could be done, in theory --

MEMBER APOSTOLAKIS: Because the diesels are --

MR. DUBE: Right.

MEMBER ROSEN: This MSPI - excuse me, George.

MEMBER APOSTOLAKIS: Go ahead.

MEMBER ROSEN: This MSPI-PRA quality task group, is that going to hold up the train leaving the station? Is it something that needs to get done before we go ahead with MSPI?

MR. BARANOWSKY: It's being done. Are you going to address that or do you want me to say anything about that?

MR. RICHARDS: Well, in short the answer is yes, it has to be done before MSPI can move along its timeline.

MEMBER ROSEN: And how long is that going to take?

MR. RICHARDS: We scheduled it, I believe, to roughly go until the end of this month, so it's in the near term.

MEMBER ROSEN: It's a couple of weeks then.

MR. RICHARDS: Mike, do you have better information?

MR. CHEOK: This is Mike Cheok. We are supposed to come up with a -- we're scheduled to come up with a draft recommendation in December to be discussed with, I guess, the agency and industry reps.

MR. DUBE: Bottom line is we feel that the formulation as is is good enough for its intended use. And if this were a, let's say an online risk monitor, clearly just using the first term would be inadequate, because here when you remove a component from service, we're not talking about Delta CDFs of 10 to the minus 6. We want to be talking about risk achievement factors of two and ten, meaning doubling, or even ten-fold increase in core damage frequency in that time frame when that equipment is removed from service. So obviously, this formulation wouldn't be adequate to that, but for the range of changes in CDF that we expect and that we've seen from the pilot plant, we feel that it's adequate. And that's all I have.

MR. BARANOWSKY: Okay. So now we get to --

MEMBER APOSTOLAKIS: Your assignment on the frequency of initiators, it appears to me you can handle them the way you're handling the unavailability, because all you're doing is you're finding the --

MR. DUBE: Right.

MEMBER APOSTOLAKIS: If at all that has a problem. You can't find it all for the frequency of initiating events, but you could include them in this.

MR. DUBE: You mean a change in initiating event frequency?

MEMBER APOSTOLAKIS: Yes. Why not?

MR. DUBE: Well, the next generation --

MEMBER APOSTOLAKIS: It's already an indicator.

MR. DUBE: The next generation might do that to combine an MSPI-type formulation with a --

MEMBER APOSTOLAKIS: No, I'm not saying combined. Have an MSPI for initiators.

MR. DUBE: We could do that.

MEMBER APOSTOLAKIS: Nothing would change.

MR. BARANOWSKY: That would be an initiator indicator.

MEMBER SIEBER: I'm not sure that that adds much to the ROP. Now you can make the ROP so complicated that it doesn't --

MEMBER APOSTOLAKIS: Well, the ROP already has an indicator, doesn't it?

MR. BARANOWSKY: Well, I think the better -- I like the way we did this one, because there were specific problems that were identified, and we tried to design something that addressed the problems, and met the objectives of being risk-informed. And I think there are, as I identified, some other problems with other indicators. We would work with them to try to come up with some improvements.

MEMBER SIEBER: Good luck.

MEMBER APOSTOLAKIS: So the major improvement here is that the thresholds are not generic any more?

MR. DUBE: I think the major improvement is that we now account for unreliability.

MEMBER APOSTOLAKIS: Yes, that too, absolutely. Absolutely.

MR. DUBE: We now take into account the fact that every plant is different, and they have different plant-specific configurations and that is reflected so they have threshold -- the number of failures that they need to reach the threshold will be different from plant to plant, depending - and system to system depending on the --

MEMBER APOSTOLAKIS: That's what I'm saying, that the thresholds are not generic any more.

MEMBER SIEBER: Right.

MEMBER APOSTOLAKIS: Is that correct?

MR. DUBE: In terms of the number of failures they're not generic. But in terms of 10 to minus 6, 10 to minus 5, 10 to minus 4 they're --

MEMBER APOSTOLAKIS: Yes.

MR. DUBE: But the number of component failures and the percent increase in unavailability will vary from plant to plant, depending on how important it is.

MEMBER APOSTOLAKIS: Only to the extent that one plant has two diesels and the other has three diesels. But not including the data action, because you are using the data from `95 to `97 as a reference.

MR. DUBE: Right. Data will have an impact in the deviation of their performance --

MEMBER APOSTOLAKIS: From that point of reference.

MR. DUBE: From the baseline.

MEMBER APOSTOLAKIS: Which is a point of reference for everybody.

MR. DUBE: Right.

MEMBER APOSTOLAKIS: Not plant-specific.

MR. DUBE: Correct. So it accounts for unavailability, it accounts for unreliability, plant-specific configuration, and plant performance deviation from the norm. Those are the strengths.

MR. BARANOWSKY: I would also add that we're using plant-specific PRAs, including looking at PRA adequacy issues in a way that could be done consistently across all plants here. We're learning a lot about that.

MEMBER SIEBER: That's a secondary effect.

MR. BARANOWSKY: Yes, it is, but it's --

MEMBER SIEBER: It's important to the ultimate outcome, that failure to do that in a timely fashion would not prevent initiating the MSPI. I mean, it's not a precursor step.

MR. BARANOWSKY: I think a decision has been made that we need to have adequate PRA quality for the application of MSPI. So it was a fallout thing that we didn't expect when we first started this --

MEMBER SIEBER: That can add to the timeline.

MR. BARANOWSKY: It's adding to the timeline, but we've learned a lot about what causes folks to have differences of opinion on the risk associated with plant operating issues, that might have taken years to discover without a systematic way that we've looked at it.

MEMBER SIEBER: So when do you think the MSPI will become a fact of life as far as the matrix that is on the NRC website?

MR. BARANOWSKY: Well that's why we're going to listen to Stu Richards as soon as I do the conclusions.

MEMBER SIEBER: Okay. Do the conclusions, and let's listen.

MEMBER ROSEN: I think you left an important thing out of that page, which is the support system. It includes cooling water support system. That's another big event.

MR. BARANOWSKY: Okay. So to conclude, as you've heard, we've tested and evaluated in a pilot program the MSPI, and discussed it at numerous public meetings. There were many issues that were raised, and we looked at them fairly thoroughly and documented that in the report. The problems associated with the current PIs are clearly addressed, and we know a lot about the capabilities, strengths, and limitations of the MSPI, which is why I think I'm safe in saying it's a fairly robust performance indicator.

We looked at the sensitivity of how the MSPI performs when you vary certain issues about common cause failure, and putting valves in and leaving them out, and whether or not you get the same outcomes. That makes it robust, if you get the same results by making a few changes, and it's not really twitching, it's a robust indicator.

As we mentioned, it has desirable qualities with respect to plant-specific risk implications, reliability and availability treatment, captures system performance degradation. The computation has some complexities, but it's structured and programmable so you can easily implement it with a computer.

MEMBER APOSTOLAKIS: You mean individual licensees will not have to worry about cross-train, non-informative --

MR. BARANOWSKY: No, it's algebra. I presume that we can do algebra.

MEMBER ROSEN: I suppose you're going to

issue a template some place --

MR. BARANOWSKY: There will be a template --

MEMBER ROSEN: Plug your failures in and it will do the calculation.

MR. BARANOWSKY: I think INPO is making the template.

MEMBER SIEBER: Yes, I want --

MR. DUBE: And what will have the official calculation, I believe, the licensee will have their own mini programs for what-ifs, but the official will be with INPO.

MEMBER SIEBER: Before we conclude this session, I'd like to review some of these details as to what has to be in place, what steps you will take, so we can decide if there's anything else we need to look at, or if we just give a global blessing or criticism in the letter that you're requesting.

MR. BARANOWSKY: Okay. We think based on discussions that we've had internally and with the industry, MSPI is consistent with the Maintenance Rule implementation, technical specifications, and SECY 99-007. The PRA adequacy issue is being addressed. It's not completely addressed yet, but it will be. And so we get to the last thing, which is we'd like to get --

MEMBER SIEBER: Yes, you did show that.

MR. BARANOWSKY: This thing here. We'd like to request an ACRS letter on this, which you knew even in our prior meeting. You might recognize the MSPI as a significant development in the application of PRA methodology in the regulatory program, and endorse it for the intended use in the reactor oversight process, or something like that.

MEMBER APOSTOLAKIS: Now when you say -- let me understand something.

CHAIRMAN BONACA: Non-constrained --

MR. BARANOWSKY: Just came off the top of my head.

MEMBER APOSTOLAKIS: You say it's consistent with the Maintenance Rule.

MR. BARANOWSKY: Yes.

MEMBER APOSTOLAKIS: In what way?

MEMBER SIEBER: Same data.

MR. BARANOWSKY: The definitions of unavailability and you don't get --

MEMBER APOSTOLAKIS: But the Maintenance Rule uses different thresholds, doesn't it?

MEMBER SIEBER: Yes.

MEMBER APOSTOLAKIS: Based on raw.

MEMBER SIEBER: Yes.

MR. BARANOWSKY: But you don't get going off in two different directions.

MEMBER APOSTOLAKIS: The data collection is the same on unavailability, and so on.

MR. BARANOWSKY: Yes.

MEMBER APOSTOLAKIS: Does the Maintenance Rule include unreliability? I don't remember. I think it's only unavailability.

MR. BARANOWSKY: Yes, it includes unreliability.

MEMBER APOSTOLAKIS: Includes unreliability.

MR. BARANOWSKY: Yes.

MEMBER SIEBER: The concept of it.

MR. BARANOWSKY: The concept of it.

MEMBER APOSTOLAKIS: What does that mean?

MR. DUBE: It means you have so many failures, you elevate your action.

MR. BARANOWSKY: And in particular one of the things we talked about was unavailability during our operations versus shutdown, for instance, and why those should be separated when you're trying to look at thresholds, because the risk is different, and the drivers are different. Okay.

MEMBER SIEBER: Okay.

MR. BARANOWSKY: So now Stu will tell you about what's happening implementation-wise.

MR. RICHARDS: All right. I'm Stu Richards. I'm the Chief of the Inspection Program Branch in NRR, and we're along with industry the end-user for MSPI, so when Research goes home, we're going to still be using it. And we've had a lot to do with it.

Slide 19, I'll go through this pretty quick. We have three slides. It was already mentioned, we piloted this at nine sites and 20 units. We've touched on it briefly at two commission meetings. The commission gave us some guidance in two SRMs and they have encouraged us to go forward and work with industry to make this happen.

MEMBER SIEBER: That last one was a good one.

MR. RICHARDS: It was already mentioned, we have monthly meetings with industry on MSPI. I think we've had over 35 meetings over the last couple of years. Some of these meetings take all day. There's been a tremendous amount of hard work that's gone into this, and I'd like to compliment Research on their work. They've done a real good job.

For us it's cumulated in NRR sending a

letter to NEI just this past month, September, agreeing to go forward with MSPI implementation. And they said they needed that letter in order for the industry to start making some investment in the process it's going to take to set this up.

MEMBER SIEBER: Let me ask a question about NEI. They have a document 99-03 which is part and parcel to this. It's mentioned in your analysis report, and it says that revisions will be needed to 99-03. Is that really true? Does NEI have to do something?

MR. DUBE: It's been significantly revised.

MEMBER SIEBER: Okay. So the revision is done. It would meet the recommendations that's in your report.

MR. DUBE: Definitely.

MR. RICHARDS: Is that different than 99-02?

MR. DUBE: No. In 99-02, Appendix F is the NEI guidance.

MEMBER SIEBER: 99-03 is the number I have. Is that the right --

MR. DUBE: It's 99-02.

MR. RICHARDS: We'll touch on that briefly on the third slide. Next slide, please. We already touched on agreeing with industry for creating the front stop, and we already touched on the concept of this task group working on what constitutes the minimum PRA requirements for MSPI.

On the implementation side, we see that as important because we're counting on that task group to provide us some insights on what we need to inspect as far as implementation of MSPI, and what we should be looking at long-term current feeding of it.

MEMBER SIEBER: What will you send the licensees to inform them that the MSPI is now in effect, and that the data will come through the INPO process? Is that going to be a generic letter, or something like that, or what will it be?

MR. RICHARDS: It will probably be a Reg INPO summary, and we'll touch on that a little bit further down the line here. Well, it really touches on the last bullet we have here.

MEMBER SIEBER: And along with that, how will you inform the public that you're switching over and when they look at the action matrix results on the website, how will they interpret this new indicator, and how will they know what it means?

MR. RICHARDS: Well, we plan to have a

communication plan. The indication has said they will have a communication plan also. One of the challenges of MSPI is to explain it to the public in a way somebody can understand.

MEMBER SIEBER: Yes, that will be a challenge.

MR. RICHARDS: That will be a challenge, so we are going to put together a communication plan. We intend to put information out to the public and make it available through our ROP website. We're in the formulation stages of that.

MEMBER APOSTOLAKIS: Don's interpretation is a first good step.

MR. RICHARDS: I'm sorry.

MEMBER APOSTOLAKIS: Don showed a definition without any equations. That was a first good step on the way of informing the public. I mean, what else can you do? It's a measure of this and that, and this and that.

MR. RICHARDS: Part of the ROP is the idea that somebody, an interested stakeholder can take the inputs and understand how you came out with green, white, yellow, or red. Of course, in this case it's not going to be so simple to do, and now because of the security restrictions, we're no longer allowing public access to a lot of PRA information, so that pretty much precludes anybody from going through that exercise. So it will be a challenge.

MEMBER SIEBER: Well, there's the public, and then there's the public. There's the public that, for example, when I was in industry, the financial people looked at all the SALP reports and performance reports to make their own judgment as to how well the company is running the plant. There are other public that want all the details in preparation for allegations and so forth, and then there's a third class of public that takes general views. So I think somehow or other, you've got to recognize all three are out there and tailor communications to reach all three.

MR. RICHARDS: We agree.

MEMBER SIEBER: Okay.

MR. RICHARDS: All right. The last bullet on this slide, I'd like to touch on very quickly, but it is important for us. We agree with the industry that the implementation of MSPI has to occur at all sites at the same time. We're not going to end up with two different Pis, one for plants who can't there, and one for plants who can.

Because the PI program is a voluntary

program, the burden to get all the plants lined up and ready to implement basically falls on the industry, and the industry understands that.

MEMBER SIEBER: Right.

MR. RICHARDS: Next slide. Some of the challenges that we're working on right now in concert with NEI, we need to get the interpretation issues as much as we can straightened out. We found out from other Pis that once a PI is in place and you start arguing about what the details mean, and it makes a difference about a plant going green or white, sometimes that can be tough, so we want to iron that out on the front end, hopefully, and minimize the amount of resources it's going to take to answer those kind of questions down the road.

I mentioned already we're working with NEI on their implementation guidance, which is contained in their 99-02 document. I mentioned already the communication plan and the reg INPO summary to tell the industry what we're doing in this area. I think there is a minor detail as far as aligning the data entry for MSPI and the Maintenance Rule that needs to be worked out.

The industry plans to have three public workshops primarily to inform the industry on how to implement MSPI. We'll probably participate or at least attend those, and when we do have some internal training that we're going to have to do to get the inspection staff up to speed in implementing the MSPI.

MEMBER SIEBER: You have to modify the inspection manual too, do you not?

MR. RICHARDS: Yes, we'll have to change our -- we have a procedure to go out and verify PI entry data. So, of course, we'll change that for MSPI.

MEMBER SIEBER: You've got to change that.

MR. RICHARDS: The one question that will answer my last bullet, when are we going to implement this. The industry proposes that we implement this in the first quarter of calendar year 2006, so that data would be received by us after that quarter is over in April of 2006, and that's when we would post it.

MEMBER SIEBER: I'll be an old man by that time.

MEMBER ROSEN: Did you agree to that time? Have you agreed to that time frame?

MR. RICHARDS: We have agreed to that schedule, as long as all the things that have to occur in-between now and then occur. We're not locked into that.

MEMBER ROSEN: It sounds like a pretty leisurely schedule to me.

MEMBER SIEBER: It certainly does. I'll be an old man before you're done.

MR. RICHARDS: Well, part of the schedule is actually driven by outages at plants. When you look at them having their three workshops and when they have to schedule that, the work that has to be done by industry to go and make sure peoples' PRAs are ready to use MSPI, and the fact that everybody has to be there, I think you could probably argue that maybe most of the plants right now are in good shape. But there's going to be some population that's going to have to do some work.

MEMBER ROSEN: Did you say the first quarter of 2006?

MR. RICHARDS: First calendar quarter.

MEMBER ROSEN: I would think that people would -- that most of the industry is already there participating in pilots and whatever, and the ones that aren't there need to get hot, I'd say.

MEMBER APOSTOLAKIS: It's only a year. I mean what's the big deal. It's only a year, right?

MR. RICHARDS: We had 20 units out of 103 units.

MEMBER SIEBER: There were some adjustments even with those 20 units, because things weren't working out properly initially, and so there had to be some interaction. I can understand some time, but in a way I'm a little frustrated, as probably Steve is also, that that seems to be a long time. Okay. Anything else that you want to add?

MR. RICHARDS: No. Thank you for the opportunity.

MEMBER SIEBER: If we write a letter, it will -- I don't know whether you would issue that NUREG with or without our concurrence, but that would probably be one factor that would be in any letter we might write, provided my colleagues would agree with it.

MEMBER APOSTOLAKIS: What is the condition?

MEMBER SIEBER: The concurrence with the NUREG that's Tab 5 in our manuals, and some kind of concurrence that the staff should proceed with the implementation of the MSPI. I think we would be interested in the future in knowing progress, but I don't think in the future we need to have meetings to deal with technical issues upon which we would write you additional letters. I think we're now far enough along that those issues are behind us now, and satisfactorily concluded. Steve.

MEMBER ROSEN: Jack, I only want to say one more thing. I think the staff and the industry both need to be congratulated on bringing this issue to this kind of resolution. I think the ROP will be quite a bit stronger with the new MSPI, and that's in part why I wanted to get on with it.

MEMBER SIEBER: Okay. Well, that will be in the record, and maybe in our letter, too. So if no one has any additional questions or the staff has no additional comments, Mr. Chairman, I turn it back to you, and I've gained 35 minutes.

CHAIRMAN BONACA: Good for you. Appreciate the presentation.

MEMBER SIEBER: Six o'clock is not until five more minutes. I request a break.

CHAIRMAN BONACA: Yes. A short break, because we need to get to this, but let's get a break until five after, 10 minutes.

MEMBER SIEBER: That's good.

CHAIRMAN BONACA: And thank you very much for the presentation again.

MEMBER SIEBER: Thank you.

(Whereupon, the proceedings in the above-entitled matter went off the record at 5:55 p.m.)