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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON THERMAL HYDRAULICS

MEETING

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WEDNESDAY,

September 22, 2004

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The meeting was convened in Room T-283 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Graham B. Wallis, Chairman, presiding.

MEMBERS PRESENT:

GRAHAM B. WALLIS   Chairman
F. PETER FORD   ACRS Member
THOMAS S. KRESS   ACRS Member
GRAHAM M. LEICH   ACRS Member
VICTOR H. RANSOM   ACRS Member
JOHN D. SIEBER   ACRS Member

ACRS STAFF PRESENT:

RALPH CARUSO   ACRS Staff
SPYROS TRAIFOROS   ACRS Consultant

C-O-N-T-E-N-T-S

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

8:33 a.m.

CHAIRMAN WALLIS: On the record. Could we please have quiet? This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Thermal Hydraulic Phenomena. I am Graham Wallace, Chairman of the Subcommittee. The Subcommittee Members in attendance are Tom Kress, Victor Ransom, Jack Sieber, and Peter Ford. Also attending is our consultant Spyros Traiforos.

The purpose of this meeting is to discuss the staff's approach to resolution of several generic safety issues related to loss of coolant accidents. During the first part of this meeting, the Subcommittee will consider the staff's safety evaluation report related to Generic Safety Issue 191, Pressurized Water Reactor Sump Performance During A Loss Of Coolant Accident, and the Nuclear Energy Institute Guidance Report titled "Pressurized Water Reactor Sump Performance Evaluation Methodology."

During the second part of this meeting, the Subcommittee will consider the proposed final report related to the resolution of Generic Safety Issue 185, Control Of Recriticality Following Small Break LOCAs in PWRs. The Subcommittee will hear presentations by and hold discussions with representatives of the NRC staff, the Nuclear Energy Institute, and other interested persons regarding these matters.

The Subcommittee will gather information, analyze relevant issues and facts, ask many questions, and formulate proposed positions and actions as appropriate for deliberation by the full committee. Ralph Caruso is the designated federal official for this meeting. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on August 20, 2004.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. We have not received any requests from members of the public to make oral statements or written comments.

Now, I believe that Michael Johnson is going to start off for us today. Michael, it's always a pleasure to hear from you. We heard from you last time on the same issue when you were issuing a generic letter. That was a somewhat interesting meeting because you assured us that you had a nice generic letter and the next time we saw it, it was utterly different.

I think we have a lot of time for questions on this matter we're going to discuss today. So it's quite likely you might want to change the SER as you changed the generic letter. So perhaps this is a work in progress as well as being your best job up to today.

MR. JOHNSON: Well, it certainly is our best job up to today, I'll say that. My name is Michael Johnson. I'm here to, as indicated, introduce the GSI-191, work that the staff has done on the SE. I'm joined by Mark Giles to my right who will state some words in terms of overview. I'm also joined by the team of folks who have worked in terms of preparing what the staff has put together and what has been provided to you in terms of the SE.

You are right. We did speak last on June 22. At that time, we talked about the issue and the urgency of the issue and in fact the Commission's desire that we address the issue quickly. We talked a little bit about the bulletin and the work that had been done by the staff in the bulletin and the real purpose of the bulletin which was to have licensees confirm compliance on a mechanistic basis with the regulatory requirements for their ECCS and CSS systems and recirculation and compensatory measures that they should consider to reduce the risk.

We focused on the main objective of that meeting which was to review the generic letter. You are right. That generic letter changed a little bit. I'll say "a little bit" from June. We think we got an improved product based on the interface that we had with you and with stakeholders. In fact, that generic letter was issued on September 13, 2004, with the blessing of the ACRS.

CHAIRMAN WALLIS: Actually what we blessed was any generic letter that you had finally come up with as I remember because they seemed to be varying.

MR. JOHNSON: Right. You were sold on the concept of it.

CHAIRMAN WALLIS: On the concept. We liked the concept, yes.

MR. JOHNSON: Members of the GSI-191 Industry Task Force talked about the Generic Evaluation Guide. We said some stuff also about the Generic Evaluation Guide, although that clearly wasn't the purpose of our meeting in June. We're here today to talk in detail about the results of our review of the draft SE. One of the first points I wanted to make is - and Mark, would you skip ahead to the very last slide --

CHAIRMAN WALLIS: Could we look at this slide? Are you going to talk about this slide?

MR. JOHNSON: I'm going to come back.

CHAIRMAN WALLIS: Oh, you are going to come back to it, okay.

MR. JOHNSON: The first point I wanted to make is that the work that was done to develop the SE was done with the involvement of a large number of folks, some of which are present today but many of which are not present today including people, representatives from the Office of Nuclear Reactor Regulation, of course. We also got outstanding support from the Office of Research in supporting this activity. Of course, LANL did a lot of the work in support of the SE.

In addition to that, we've had frequent and close communication with the industry and other external stakeholders and getting the generic guidelines that were prepared by them and in fact having discussions in terms of various aspects of the evaluation and the work that went into preparing our SE. In fact, we made a draft of the SE public on

September 20 to make sure that external stakeholders still are aware of how that SE is unfolding.

Let's go back, Mark, to the first slide, if you would. We reviewed the Generic Industry Guidance very carefully as you asked us to do and was our intent. In general, we think that the overall approach that was used by the industry is a good one. We did find areas, in fact, we expected to find areas where additional guidance would be necessary and is necessary to make that guideline be acceptable and provide an acceptable approach for the staff. We'll focus on those areas as we go throughout the presentation.

Also there continue to be, as you are well aware, areas where our knowledge is limited. As a result, there are uncertainties in some parts of the analysis. That challenged us. In those areas, we used our judgment to reach a regulatory decision that will support resolution of this generic issue in a way that I believe is appropriate.

CHAIRMAN WALLIS: This conclusion that you have up here is your conclusion.

MR. JOHNSON: Yes.

CHAIRMAN WALLIS: The staff's conclusion. Now, there are some important words in there. It says, "Technically sound and accept all methodology." I think you'll find ACRS has quite a few questions about the technical soundness.

It may be that the methodology is acceptable despite numerous shortcuts in the technical analysis. Or maybe it's not acceptable because of those shortcuts. But I think you may find that we have some debates about what you mean by "technically sound."

MR. JOHNSON: Absolutely.

CHAIRMAN WALLIS: I think there's also a question about "realistic" because at certain points I think in the analysis it's pointed out that we're not being realistic. We're looking for a bounding estimate. That's quite different from a realistic estimate. So if you are going to say it's realistic evaluation, is that what you mean? Or do you mean that it's okay because it's conservative?

MR. JOHNSON: By that we mean that we tried for an approach in areas where we didn't try for an absolute conservative approach. We tried to make where we needed to be conservative to make that conservatism as realistic as possible.

CHAIRMAN WALLIS: Well, I think that there is something here because I think in parts of the SER you asked them to assume that Cal-Sil has the worst possible specific surface that's ever been measured rather than the average or most realistic one. So it appears as if the SER is being conservative. In which case I think you ought to say so.

MR. JOHNSON: We're going to talk about this as we get into the various sections. I ought to point out, in fact, my very next point was going to be that we're well aware that there are areas of the SE and the Industry Guideline perhaps even that the ACRS has particular interest in. We're going to focus on those as we go throughout the presentation and try to touch on those.

As you indicate, we did look at various areas in terms of how we wanted both the baseline and any refinements to the baseline to come out so that at the end we could be comfortable that a plant exercising the baseline or taking refinements could resolve this issue in a way that could provide assurance to the staff that the issue at hand could be resolved. That was the goal for us in terms of the way we approached the issue.

In the end, the staff has to issue the SE and get into the hands of licensees, put the onus on licensees, to go out and do the evaluation --

CHAIRMAN WALLIS: Can I ask you about this "realistic" word? Maybe you want to change it because it seems to me in numerous places when you are looking at, let's say, the debris transport, in order to avoid - I forget just what the words are - but essentially it says in order to be conservative enough, you have to assume a certain thing. That's not a realistic analysis as I understand it.

A realistic analysis is based on what you think really happens not on limiting it with some bounding assumption. And that occurs several times in the SER. I'm trying to get at the philosophy behind the SER because I think we need to establish that at the beginning. Is it realistic or is it conservative or don't you know?

MR. JOHNSON: I think it's realistic and conservative.

CHAIRMAN WALLIS: It can't be both.

MR. JOHNSON: It's conservative but we tried to move in the direction of being realistic. That should indicate that we weren't trying to go with an approach that was overly conservative.

CHAIRMAN WALLIS: So it's not unnecessarily conservative.

MR. JOHNSON: That's right.

CHAIRMAN WALLIS: Okay.

MR. JOHNSON: Again, in the end, the staff does need to issue an SE. We're driving towards that. We have a slide where we can talk about the milestones going forward. But in fact, that takes me to the last point that I wanted to make. The issuance of the SE is not the end of the effort.

In fact, I would argue it just marks the end of a phase and the beginning of probably a more challenging phase which is to then have licensees do the evaluation, to conduct our review of that evaluation, what licensees are in fact implementing in the field, and ultimately leading up to our close out of the issue in 2007. There's a lot of work and a lot of planning that needs to go into those aspects. There will be a lot of continued dialogue with licensees and certainly with the ACRS as we go forward.

CHAIRMAN WALLIS: That was a concern of the Committee was that you are going to get 69 different submittals all based on different analyses and it's going to be a nightmare to sort them out. Can I ask about the words "technically sound?"

MR. JOHNSON: Yes.

CHAIRMAN WALLIS: There has to be some

criteria for soundness. Maybe we'll touch on this throughout the day, I think, because if I see, say, ten experiments and two of them show something or other that I'm interested in and the other eight do not and I make a conclusion based on two of them, is that technically sound or not?

What are these ideas of what is technically sound? Is it taking the biggest thing I have ever measured although it may be an outlier of everything? Is that a technically sound decision or not? There has to be some sort of mutual understanding which is justifiable in the public domain of what is technically sound and what is making some regulatory-type decision because you have to because it's the best you can do now and it's conservative and therefore it's okay?

That's quite different from what maybe the engineering community might regard as technically sound. So I think we're going to touch on that. I'm warning you. But you are going to try to disappear and leave it to somebody else.

MR. JOHNSON: No, I'll be here.

CHAIRMAN WALLIS: But since you put the words up.

MR. JOHNSON: But there will be someone

more directly in your line of fire as I sit on the side. We do ask that the main objective - and this goes back to the first point on my slide - is that we get from the ACRS your endorsement of the staff's SE. That's the objective for this meeting and the meeting before the full Committee.

CHAIRMAN WALLIS: Yes.

MR. JOHNSON: We think we're ready with an approach that is sufficient for our regulatory purposes to go forward with implementation that licensees use in their evaluation for implementation of fixes that will resolve this issue at their plant should the vulnerability exist. So that's really the objective of the meeting today and tomorrow.

CHAIRMAN WALLIS: And I hope that when these presenters present they won't just present a lot of words. I hope they will present some evidence which goes to this technically sound issue.

MR. JOHNSON: Absolutely. Having said that, I'm going to turn it over to Mark.

MEMBER RANSOM: May I ask you a question about your previous slide? What was the role of RES in this work? I have seen things from LANL and from NRR and from NEI but I haven't seen anything from RES. Is there anything written up?

MR. JOHNSON: I look and I see Tony and maybe even Mike Mayfield will be in the room at some point. You will see throughout the presentation a Research presence. Research was particularly helpful I think in helping us deal with the issues of head loss and helping us in fact deal with the issue of destruction pressure in two phase flow.

As you are aware, they have taken really the leadership role in terms of chemical precipitation effects and the concerned raised by the ACRS in terms of that research. In fact, Tony can talk to that research. We have a point in the presentation where we talk to that.

So Research has been particularly helpful throughout and in many other aspects of the review of the SE. In fact, one of the things that we did in preparing for this meeting was to send out the SE to Research as well as the other divisions within NRR to get their comment and input.

MR. HSIA: This is Tony Hsia from Research. Dr. Ransom, Mike said correctly our staff is here. We will be supportive of NRR today to discuss in particular the head loss that's in the agenda and also in the downstream effects and chemical precipitation effects.

The chemical effects, we expect that test to begin either next week or the week after next. At this moment, we have no data to present on that aspect but we have done a lot of work. Today, later on, you will see how we were involved in coming up with the head loss and the correlation of the 6224 versus some other data.

MR. JOHNSON: Thanks, Tony.

MR. GILES: Good morning. My name is Mark Giles. I'm the lead project manager for GSI-191. I'd like to provide you a brief overview of the safety evaluation report. The purpose of the safety evaluation report is to provide an NRC approved methodology to allow PWR licensees to perform the plant-specific evaluations regarding sump screen debris blockage for the emergency core cooling systems and containment spray system operation while on sump recirculation.

This is following loss of coolant accident or high energy line breaks. The SE is designed to take into account the most limiting events. As far as the plant-specific evaluations, these evaluations are required per the generic letter. The generic letter, as you probably know, was issued earlier this month in 2004 Tag 02, issued on September 13.

The generic letter requires licensees to perform actually within 90 days of issuance of the SER to provide a description of the methodology that's going to be used to perform these site-specific evaluations. It also requires the licensees using this evaluation approach to be able to confirm their compliance with regulatory requirements for ECCS and SCC functions by September 1, 2005.

The evaluation methodology that is illustrated throughout the SER is a combination approach using the NEI submittal, the guidance report, and the SER. This is a little bit untypical. Normally the NRC issues an SER to determine the acceptability of submittals from either a licensee, a vendor, or a nuclear organization. We are using a combination approach in the SER. This is going to allow for a more proactive and timely resolution of GSI-191.

A little on the SER development. There's been several public meetings that staff has engaged in for GSI-191 that start back in 1997. These interface meetings have discussed resolution strategies with regards to the issue and also some issues of concern.

Some of the involvements include the GSI-191, the parametric evaluation which was later issued as NUREG CR-6762, also the previously issued guidance for sump screen issues REG GUIDE 182 Revision 3, NEI's draft evaluation methodology ground rules, and also issues that we have already mentioned.

Tony Hsia mentioned some of the more complex issues for the head loss, correlation equations, the chemical testing, precipitation effects, data collection, and evaluation guidance. The last part is NEI submittal, the guidance report, NEI 04 TAC 07, PWR Containment Sump Evaluation Methodology, and that's really the subject and core element of the SER.

The staff reviewed NEI submittal and concluded that portions of the guidance report, the baseline guidance were acceptable as written based on their technical justification. However, the staff determined there were certain portions of the document that needed additional supplementation because the methods did not contain sufficient guidance, data, or analyses to justify the technical bases. As you will notice in the SER for these areas, the staff has provided additional comments, assessments, evaulations and refinements in order to provide an acceptable methodology for those areas.

A little bit about the integration of the

SE. As Mike said, this is just one part in the resolution process. The NEI submittal was submitted May 2004 except for the Chapter 6, the alternate evaluation which actually came in July 2004. The NRC has issued the final generic letter. That was September 13.

The review for the industry guidelines has also been completed. Moving ahead after issuance of the SER which is proposed for October 29, we'll look for the licensees to start analyzing sumps with the approved guidance. That should probably happen sometime in the first quarter of 2005.

They have the 90 days to give us the description of the methodology and how they intend to make the evaluation. Then we expect licensees to start making the modifications, if needed, using the approved guidance. This should begin in 2006. The generic letter states that the latest these corrective actions can start would be the first refueling outage after April 1, 2006.

Sometime in 2005, the NRC plans to review the responses and start inspecting on an auditing-type basis. That would allow, facilitate for the final closure of GSI-191 by December 31, 2007.

This is a list of the topic areas and the lead presenter. There will be several presenters that come up at the time that these topic areas come up for discussion. I can just briefly go down these. For pipe break characterization, the lead would be Mark Kowal.

For zone of influence, the lead would be Ralph Architzel. For debris characterization, the lead would be Angie Lauretta. For latent debris accumulation, the lead would be Tom Hafera. For debris transport, the lead would be Hanry Wagage, along with the head loss.

For physical refinements and alternative evaluation methodology, the lead is Mark Kowal. For sump structural analyses, the lead is Tom Hafera. For upstream and downstream effects, the lead is Joe Golla. For chemical precipitation effects, the lead is Ralph Architzel. At this time, I would like to go ahead and introduce Mark Kowal and the group supporting staff.

MR. KOWAL: Good morning. My name is Mark Kowal. I am a reactor systems engineer in the plant systems section of NRR. I'm going to be speaking this morning to Section 33 and Section 421 of the guidance report and safety evaluation report.

Basically these sections get into break

selection and identifying limiting break locations to be analyzed. Joining me at the table here is Dr. Bruce Latellier from Los Alamos who also participated in the review of these sections.

Section 33 of the guidance report provides guidance and considerations regarding the overall process for selecting and identifying the limiting break location. In summary, the staff finds that the guidance provided in this section of the guidance report is acceptable and notes two exceptions. First, the guidance report does not provide guidance for plants that can substantiate no-thin bed effect.

CHAIRMAN WALLIS: Do you understand what that means?

MR. KOWAL: Well, yes, I do. This is actually something that is going to be discussed into the next presentation.

CHAIRMAN WALLIS: Well, I'm not at all clear on what are the criteria for knowing when you do or do not have this thin bed effect and what it is. The first thing you have to do is to say, do we or do we not have a thin bed effect? Apparently if they can establish no thin bed effect, then they don't have any guidance. So what good does that do them? If they can establish that they don't have a thin bed effect, then there's no guidance.

MR. KOWAL: Right.

CHAIRMAN WALLIS: And if they do have a thin bed effect, then presumably they are in trouble because that gives them a high head loss. So I'm not quite sure what this does to the plants. I'm not even sure that they know how to determine whether or not they have a thin bed effect.

MR. LATELLIER: If I may interject, at this point, we're simply speaking about whether or not the plants have sufficient fiber that arrives on the screen to support the accumulation of particulate matter.

CHAIRMAN WALLIS: Is there criterion for that of some sort?

MR. LATELLIER: There are criteria based on one-eighth of an inch dry fiber.

CHAIRMAN WALLIS: Now, one-eighth of an inch is enough to support particulates. And there's another part of the SER that used to say there was overwhelming evidence that Cal-Sil alone can produce a bed. Presumably Cal-Sil alone is a thin bed because that's the stuff that makes the thin bed effect, isn't it? The Compressed Cal-Sil alone is what makes the thin bed.

MR. LATELLIER: Cal-Sil has both a fiber and a particulate constituent so it is capable of forming that effect by itself depending on the screen opening size.

CHAIRMAN WALLIS: So it's not a question of having enough fibers. If they have Cal-Sil alone, they could still have a thin bed effect.

MR. LATELLIER: The guidance could be more clear on the treatment of Calcium-Silicone.

CHAIRMAN WALLIS: I think it needs to be more clear. So it means if they have any Cal-Sil in the plant at all and if it's enough to produce a certain thickness on the screen, they have a potential thin bed effect, is that it?

MR. LATELLIER: I believe there is a potential for that to occur, but generically speaking, they are assessing their vulnerability to various sub-blockage phenomenon. Some plants also have the opportunity to substantiate no appreciable fiber accumulation at all because of their particular insulation type.

MR. JOHNSON: Can I suggest something? What we really wanted to do with Mark's presentation was to provide an overview.

CHAIRMAN WALLIS: Well, I'm sorry but this is a technical matter. We said we were going to look at the technical validity of these decisions.

MR. JOHNSON: Absolutely and we want you to. We actually have a presentation that is going to enable you to get into a lot of detail, as much detail as you want.

CHAIRMAN WALLIS: Well, I think that we need to do this. To start with, this thin bed effect appears throughout the SER. We need to be pretty darn clear what it is. And we need to have clear criteria for what it is so everyone understands it so it can be used. Then apparently if this doesn't happen, which maybe if there's a plant with no Cal-Sil, if there's no Cal-Sil, there's no thin bed effect. Then there's no guidance according to this statement. That's not very good guidance. What do they do if they don't have any Cal-Sil? They have no guidance.

MR. LATELLIER: What that bullet suggests is that the industry guidance report did not provide guidance if the plants could substantiate no appreciable accumulation of fiber. There is a criteria stated in the SE. I think we can get into the acceptability of that criteria.

CHAIRMAN WALLIS: Yes, I do think as well we need to get into that. Well, we'll get into that later.

MR. LATELLIER: Yes.

MR. JOHNSON: Dr. Wallis, if I can just ask your forbearance, we are going to get into all of these issues.

CHAIRMAN WALLIS: Yes, but again "this GR does not provide guidance for those plants that can establish no thin bed effect" is an overview. We're not going to get into that again, are we?

MR. KOWAL: The next section and recharacterization and also in the head loss section later this morning --

CHAIRMAN WALLIS: I would expect it to read the other way around that unless you gather thin bed effect, you are okay. If you do have the thin bed effect, then you better do something more substantial.

MR. CARUSO: What's the staff position regarding the section in the guidance report that's that silent regarding plants that can substantiate no thin bed effect? What does the staff think about it? It says that it's acceptable with that exception. Well, so what's the staff position then?

MR. JOHNSON: Angie, do you want to address that?

MR. WAGAGE: Hi. This is Hanry Wagage.

I'm from NRR. I reviewed debris transport and head loss sections. This thin bed effect, what the guidance is is that if there is fiber to form a thin bed, if there is sufficient fiber for one thin bed, then licensees have to consider effect of thin bed.

If there is more fiber, then licensees consider the head loss across the debris bed except in these Cal-Sils as Dr. Wallis mentioned. We recognize that in the SE there is some experimental emittence (PH) that Cal-Sil can form a thin bed even without fiber. Then we do have some conditions where Cal-Sil cannot form a thin bed. Those are when the velocities are low.

When the Cal-Sil fraction containment is low, the thin bed cannot be formed. That's an exception. Otherwise licensees have assumed that Cal-Sil can form thin beds. The question is when it comes to head loss. If there is no thin bed, the licensees have to calculate the head loss --

MR. CARUSO: I think the question is, this section here deals with the break location, right?

MR. KOWAL: Yes.

MR. CARUSO: That's what you were talking about.

MR. JOHNSON: Right.

MR. KOWAL: I think the question is, you say right there that there is no guidance about break locations for plants that don't have a thin bed effect issue. So what's the staff position about that? Is that acceptable? They can do whatever they want. Or did the staff provide additional guidance about break location?

MR. KOWAL: Not really with respect to break location, Ralph. This section documents the overall process of how you identify the limiting break. For example, in doing so, you consider each of the phases of the act: the transport, the regeneration, the accumulation at the sump screen.

Some of the assumptions that are made in these later sections of the GR. For example, codings is one of the areas where particulate sizes are assumed. When you have a thin bed, that tends to increase the head loss. That's a conservative assumption. For a plant that can't substantiate a thin bed, if they do not get a thin bed, then what I'm saying is those particles could pass right through the sump. Maybe those aren't the conservative particle sizes --

CHAIRMAN WALLIS: But then another part of the guidance says that Cal-Sil can block the screen without the fibers there. So you can't just assume it all passes through.

MR. CARUSO: How does this affect limiting --

CHAIRMAN WALLIS: Again, they are supposed to consider the worst combination of debris mixes that are transported to the sump. You look at all the break locations and say what's the worst thing that can happen. I don't see why you need this exception at all. It just confuses everything.

MR. KOWAL: Perhaps we don't need it here then. The limiting break location is going to be identified through surveys, through as I mentioned walk downs, considering worst locations, those types of factors.

CHAIRMAN WALLIS: But really they have to consider a lot of locations to find out what's the worst.

MR. KOWAL: Right. They will be doing that.

CHAIRMAN WALLIS: So what you are really saying is make a comprehensive analysis who considers lots of break locations bearing in mind those which are next to places where there's a lot of insulation, see what happens, and find out the worst one.

MR. KOWAL: Right.

CHAIRMAN WALLIS: I don't know why you need any of these peculiar exceptions like this one which seem to be addressing something else.

MR. KOWAL: Okay.

MR. SOLORIO: Dr. Wallace, this is Dave Solorio. We hear your comment. We will go back and look at our SE and see how we can improve the clarity.

CHAIRMAN WALLIS: Well, what you will do is simply leave it out because then we won't have to discuss it anymore. I don't even understand why you put it in in the first place.

MS. LAURETTA: This is Angie Lauretta with the Plant Systems Branch. I'll be going into the details of the effects of the thin bed on the next presentation.

CHAIRMAN WALLIS: Okay. Will you explain to us what a thin bed is?

MS. LAURETTA: Well, we'll be talking about it and the different aspects.

CHAIRMAN WALLIS: You'll explain what it is.

MS. LAURETTA: Yes, I think we will.

CHAIRMAN WALLIS: Okay. Thank you.

MS. LAURETTA: This consideration was

included in the Reg Guide 1.82 as a criteria for break selection which is why --

CHAIRMAN WALLIS: Thin bed appears in 1.82 as well.

MS. LAURETTA: Right, and that's why it was included in this presentation and in the SER under this section.

MR. CARUSO: And what does 1.82 say about break location with respect to no thin bed? What does it say you are supposed to do? How does break location compare with no thin bed? I think that's the question. It's not clear to us how the fact that you can't form a thin bed. How does that effect --

MR. JOHNSON: I very much welcome the recommendation from ACRS to take out this. I'm sorry that this bullet is on this slide. Dr. Wallace, the way you described it is the way we intended.

MR. CARUSO: Maybe we just misunderstand it. That's why we're asking.

MR. JOHNSON: I don't think so. We'll get more into thin bed later on.

CHAIRMAN WALLIS: I think your answer to most of our criticisms is going to be to simply leave them out which is a little peculiar because presumably they were in for a technical reason. Let's proceed.

MR. JOHNSON: Before we do, I do want to make sure you know we are going to talk about thin bed in a couple of presentations. Angie will certainly do it in hers. We were actually going to talk most about thin bed in the head loss presentation. So I don't want you to be disappointed when we get to the next topic and we say wait until a later topic on head loss to talk more about thin bed.

MR. KOWAL: Okay. The second exception I had listed is, for plants needing to evaluate secondary size piping breaks such as main steam and feedwater pipe breaks, the location should be evaluated consistent with the guidance for LOCA pipe breaks.

CHAIRMAN WALLIS: So the overview really is that they have to consider a lot of breaks in a lot of places. They have to consider proximity to insulation. They have to do an intelligent analysis in order to try to find out the worst that could happen. That's really the substance of your SER.

MR. KOWAL: That's correct. As Dr. Wallis said, this section provides guidance and considerations on identifying limiting break size and location. What we're trying to find is the break conditions that present the greatest challenge to the sump screen and to sump performance.

The criterion for identifying limiting break location is the head loss across the sump screen and finding the break location. What we're trying to do is find the break location that results in the maximum amount of debris transported to the screen and the worst combinations of debris transported to the screen.

So we're really looking for what arrives at the screen itself. In doing this, all phases of the accident scenario have to be considered: the debris generation, the debris transport, and the accumulation.

CHAIRMAN WALLIS: So it seems to me in reading this I concluded this was not sequential. You have to propose a lot of breaks. You have to go through all the rest of the analysis with debris generation, transport and calculation. Then you have to go back again to see whether you have picked enough good breaks.

MR. KOWAL: That's right.

CHAIRMAN WALLIS: You can't just sequentially do it and say we'll pick all these break sizes and go down and calculate everything because which ones you pick depend on the subsequent calculations of other phenomena. So it's all tied together.

MR. KOWAL: Yes.

CHAIRMAN WALLIS: So really you are saying consider all break sizes. I don't see that there's much else to it.

MR. KOWAL: Right.

CHAIRMAN WALLIS: All the reasonable break sizes and locations and see what happens.

MR. KOWAL: On the next slide, as far as the break size considerations, for RCS, main loop piping and attached auxiliary piping, double-ended guillotine breaks with full separation and off-set are assumed. For secondary system breaks, for those plants that need to evaluate those scenarios, the guidance report suggests that either double-ended breaks in those systems or conditions consistent with the licensing basis be used for break size.

Staff agrees with this and notes that the licensing basis analyses for these secondary side breaks do typically evaluate the full spectrum of break sizes up through the double-ended ruptures of those lines. Basically the staff concludes then as far as break size that this is acceptable because it should provide for large quantities of debris and worst combinations.

CHAIRMAN WALLIS: It seems to me that if I were a plant I could work backwards from my screen. I could say here is my screen. If I understand the worst conditions for blockage in terms of getting all the Cal-Sil there with a little bit of fiber or whatever it is, I can work back to where in my plant could this happen. Then I could pick the break sizes. So it's almost as if the break sizes comes later in your decision rather than in the beginning.

MR. KOWAL: I guess that's possible.

CHAIRMAN WALLIS: I guess the bigger it is the worse it is so it's location that we're picking. But if it's next to a steam generator covered with Cal-Sil then maybe that's a good location to study.

MR. KOWAL: Right. That may be a good starting point for doing this type of systematic approach actually. Break location considerations. The staff position is that any break which satisfies the following three criteria must be considered: basically a break that's incorporated into the plant's licensing basis, both LOCA and non-LOCA, if they rely on sump recirculation, is capable of generating debris, and leads to a recirculation demand on the sump.

The piping systems that should be considered include all RCS piping and attached piping and the secondary side non-LOCA pipe ruptures that's part of the licensing basis. The guidance report also offers numerous other considerations for licensees. Pipe breaks must be postulated in pre-existing pipe break exclusion zones.

This would include locations that are typically subject to more rigorous inspection and normally aren't considered in break analysis, for example, piping that runs between isolation valves. Staff finds this acceptable. This implies that all locations would be considered.

Additionally, application of NRC branch technical position MEB 3-1 shall not be used for determining break locations in the baseline analysis. This MEB 3-1 basically identifies locations of high stress or high fatigue. The staff agrees with this consideration also as it leads to all locations being considered.

As I mentioned before for plants needing to evaluate secondary side piping such as main steam or feedwater lines, break locations should be postulated in a manner consistent with LOCA piping. The guidance report had suggested that plant licensing basis locations could be used. This was the exception that I noted on the first slide.

The reason for this is that these plants would rely on sump recirculation to mitigate these events. Basically these break locations assumed in the analysis probably were not performed for evaluations of the sump. They could not have foreseen all the issues that we're talking about now for GSI-191.

The GR states that pipe breaks shall be postulated at locations such that each location results in a unique debris source term. In general the staff agrees with this consideration, however, notes that the debris transport is a consideration performed in this. There certainly can be elimination of some efforts through doing comparisons of the different phases of the event.

Pipe breaks shall be postulated in locations containing high concentrations of problematic insulation. Staff certainly agrees with this and notes that both larger and smaller piping in the vicinity of the zone of problematic insulation should be considered because the debris compositions might not be identical.

Pipe breaks shall be postulated with the goal of creating the largest quantity of debris and the worst case combination of debris arriving at the sump screen. These are the two attributes mentioned earlier. The staff certainly agrees and notes that that quantity of debris may not be --

CHAIRMAN WALLIS: Can I ask about that? The pressure drop on a screen depends upon how the debris is layered. If you have fibers on the screen and then the Cal-Sil comes later, you get a different answer than if the Cal-Sil comes first and the fibers come later, I believe, right? Do you have anything about timing in any of these considerations? We just have to consider the largest quantity, but it makes a difference how the sandwich is made up, doesn't it?

MR. WAGAGE: This is Hanry Wagage. It comes in the head loss section. What this different section does is to transport a lot of debris onto the sump screen. During the head loss evaluation, licensees have to evaluate when the debris is a mixture of fiber and Cal-Sil. After that, they have to consider the thin bed effect. That means that is the limiting one. They have to assume that first there is a layer of fiber and then the --

CHAIRMAN WALLIS: So this worst case combination is not just a matter of quantity. It's a matter of timing. Why don't you put it in here?

MR. WAGAGE: Dr. Wallis, the timing is not taken into consideration in the baseline evaluation.

CHAIRMAN WALLIS: Well, the sequence of making the sandwich. If you put the bread on first before the salami, it makes a difference to the head loss.

MR. WAGAGE: Yes, I agree with that. But the licensees have to assume that it is a limiting condition.

CHAIRMAN WALLIS: So I'm trying to gather this. It's the largest quantity in the worst sequence of something that they have to consider. It's not a homogeneous sandwich. It's layered maybe. That makes a difference. Are they supposed to consider this layering or not? It's not just a matter of quantity as stated on the screen. Is it or is it not?

MR. ARCHITZEL: Dr. Wallis, this is Ralph Architzel. I think you have raised the point. It's accurate. I'm pretty sure the SE does not address debris coming preferentially at different times, for example, insulation first and then particulate later. It's perhaps a realistic but not necessarily always going to happen-type assumption that it comes in a homogeneous form distributed evenly over time sort of like it was done --

CHAIRMAN WALLIS: Now, when we get to this thin layer discussion, that's going to be a homogeneous layer. Or is it going to be a sandwich?

MR. ARCHITZEL: Well, the actual physics --

CHAIRMAN WALLIS: Is it going to be a sandwich in the thin bed or not? I still don't know where the thin bed is. Is it a sandwich or is it homogeneous?

MR. ARCHITZEL: In reality and when it really happens --

CHAIRMAN WALLIS: Well, what are you asking them to do?

MR. ARCHITZEL: Homogeneously arrive and not --

CHAIRMAN WALLIS: But the thin bed itself, does that depend upon how the sandwich is made?

MR. WAGAGE: Dr. Wallis, this is Hanry Wagage. It depends on how the sandwich is made. But during the calculation if there is a one-eighth inch fiber, even if it's mixed, what is going to control is the debris which is that particulate which has --

CHAIRMAN WALLIS: Okay. So I do this. I calculate the largest quantity of debris and I get a cubic yard of fiberglass and a cubic yard of Cal-Sil. Now I have to calculate my head loss because that's the worst thing or something.

MR. WAGAGE: That's not the worst thing. The worst thing is when there is a one-eighth inch thick fiber.

CHAIRMAN WALLIS: Well, okay. So do I take some of this fiber and put it on the screen first and then put the Cal-Sil on? Do you see what I'm getting at? Maybe we'll get into this later. Will we get to this later?

MR. WAGAGE: Yes, we can get to it later during the head loss evaluation.

CHAIRMAN WALLIS: You see, when you say "worst case combination" here, it seems to me you cannot avoid getting into the question of how it's sandwiched. It's not just quantity that matters.

MR. JOHNSON: If I can interject --

MEMBER SIEBER: Well, if you go through the SER, one of the statements that's in there is that the thin layer effect initially comes from latent debris which when you pass that through the screen, to my way of thinking, automatically separates the particulate from the fiber. Early arriving particulate will go through the screen whereas the fiber will stay on the screen. Then you build up the layer in that process. I would suggest that when we get to the latent debris that that would be an opportunity to discuss how this material is formed.

CHAIRMAN WALLIS: Bring this up again.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: That's fine.

MR. JOHNSON: And we have noted that question also. We have some other folks who can bring to bear some input to the conversation.

MEMBER FORD: Could I ask an overriding question? I'm hearing these arguments about the timing component of how the debris is made up and the different types of debris. I keep hearing the word "calculations." Are there any experiments to back up the calculations?

MR. WAGAGE: Yes.

MEMBER FORD: Are there a lot of data, not just one set of data, to back up these statements I'm hearing about calculating this and calculating that?

MR. WAGAGE: That's in different sections. For example, in the head loss evaluation, there are experiments to calculate the head loss.

MEMBER FORD: Sure. But in relation to how the sandwich is made up, are there data?

MR. HAFERA: There's real world data.

MEMBER FORD: There's real world data.

MR. HAFERA: Limerick (PH) had a thin bed effect.

MR. HAFERA: Right.

MEMBER FORD: Okay. That's one set of data.

MR. HAFERA: Larsabeck (PH) had a thin bed effect. So it's an honest to God phenomenon.

CHAIRMAN WALLIS: So only data from reactors not from experiments in a lab where you made up different sandwiches?

MR. JOHNSON: Bruce, can you talk to that?

MR. LATELLIER: Yes, let me interject. This is Bruce Latellier from Los Alamos National Lab. A great deal of our experimental database is founded on the testing that was done for the resolution of the BWR strainer blockage issue. At that time, various combinations of debris were introduced to a suppression pool environment.

It was found in general that homogeneous combinations of fiber and particulate induce less head loss than a thin layer of fiber that's supporting a thicker layer of particulate, up to some limit. Of course, you can always dominate the head loss by a very large amount of fibrous debris.

The transport scenarios, we are not asking the industry to assess the time dependents in an explicit manner. We believe that those cases where large amounts of fiberglass insulation debris arrives on the screen that it will be more or less homogeneously mixed with the particulate. So we're asking them to assess their bed head loss on a homogeneous manner.

MEMBER FORD: Okay. That seems a reasonable engineering approach. But when you are doing these calculations and backed up by the limited data that you have, have you done a sensitivity analysis to show that it does not matter as to how the debris is made up? Or you can realistically say that it's just a mixture.

MR. LATELLIER: I think it's more accurate to say that the sensitivity of studies have been done to show that yes it does matter. In fact, in one early recommendation for the BWR closure, it was suggested that the head loss of various debris types be added in linear combinations to maximize their separate effects.

At that time, it was judged to be unrealistically conservative. The intent of the guidance was to ask the industry to assess homogenized beds. The one important exception to that is the formation of a thin layer of fiber which we know from some test experience can happen in almost unintended fashion from the suspension of individual fibers either from latent debris or from residual LOCA-generated debris.

MEMBER FORD: Right.

MR. LATELLIER: Now, there are scenarios where if large amounts of fiber are present on the screen then they will certainly continue to filter particulates. It's our belief, it's our understanding at this time that thick beds of fiber will accommodate particulates within the body of the media and they will not collect on the surface in a manner that induces the so-called thin bed behavior which we'll describe later.

CHAIRMAN WALLIS: Now, what's the evidence for that?

MR. LATELLIER: There's always a limiting particulate loading for any porous media. If that limit is reached, then of course it will filter on the surface.

CHAIRMAN WALLIS: So if you had a thick bed that had enough particulates in it, it would behave the same way.

MR. LATELLIER: That's true.

CHAIRMAN WALLIS: So there's nothing magical about this being thin. It could be an inch thick, not an eighth of an inch.

MR. LATELLIER: That is true.

CHAIRMAN WALLIS: So the eighth is the minimum possible layer.

MR. LATELLIER: There is always a limiting particulate level for any medium.

CHAIRMAN WALLIS: So it would be better to call this the clog bed effect rather than a thin bed effect. The thinness is a misleading term.

MEMBER SIEBER: Well, it seems to me that when you describe the thin bed effect you are describing the fact that the head loss curves are non-linear. They are isotropic. They have a dip in them. The very front piece of those head loss curves is the thin layer effect whereas gross accumulations occur further out in the flow regime. And there is a difference. You can get more of a head loss out of the thin bed effect under certain circumstances than you can with heavier loadings.

MR. LATELLIER: That is a fact. And Dr. Wallis makes the point as well that particulates can form on the face of a thicker bed of fiber and induce the same behavior.

MEMBER SIEBER: That's right.

MR. LATELLIER: We need to explain at this point that use of the term "thin bed" is somewhat of a misnomer. It's historical in nature. It's semantics that were chosen to emphasize the industry's potential vulnerability to small amounts of debris. Where previously we had defined our worst break locations based on maximum debris volumes, this now emphasizes that there are alternatives that can give you equivalent effects.

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: And that is when you happen to have a clogged bed which has the maximum amount of Cal-Sil you can stuff into the fibers and clog them up, isn't it, which could occur at any layer in the sandwich.

MR. LATELLIER: I'm trying to think of transportability scenarios that would lead to a late introduction of particulate.

CHAIRMAN WALLIS: But it could happen. You could in the lab make a bed of fibers and then put Cal-Sil on top of it in which case you would get a big head loss.

MR. LATELLIER: You certainly can create those effects artificially in the lab. In those cases where transportability is sufficient to establish a thick mat of fiber on the screen, we also believe that the particulate will arrive at the same time during the same transport phase.

CHAIRMAN WALLIS: You believe or you have analyzed it.

MR. LATELLIER: It has not been specifically analyzed for the resolution --

CHAIRMAN WALLIS: It seems to me it has to be analyzed not just believed. Belief is not part of the lexicon here.

MEMBER SIEBER: Faith-based.

CHAIRMAN WALLIS: I know I don't believe anything. I don't think you should until you have tested and analyzed it.

MR. LATELLIER: In our testing experience which included integrated tank testing, while we have observed the accumulation of a thin mat of fiber supporting particulate collection, we have never observed the reverse at least not over the time scales over which we have tested. We are continually thinking about the sequencing of debris generation and debris introduction to the suppression pool. The primary mechanism of transport which we may talk about is spray actuation which washes this material into the pool.

CHAIRMAN WALLIS: You see, in your reports, I have seen stuff introduced and you get a pressure drop and it has various characteristics. But I haven't seen a report where you say we put in the fibers first and then we put in the Cal-Sil or we put in some fibers and then some Cal-Sil and then more fibers or we put in the Cal-Sil and gee whiz it made a bed and then we put fibers on top of it. You have had Cal-Sil make a bed without fibers. You've had it put in together. But you haven't had these different sequencing of things which would seem to me fairly important.

MR. LATELLIER: Well, as I said, the separate effects of each debris type have been tested and their limiting conditions have been established to some level of understanding. It is true that the maximum head losses induced can be approximated by the linear combination of worst case effects.

CHAIRMAN WALLIS: I think you would get the worst case if you actually put the Cal-Sil on top and let it be compressed to its max. Well, it doesn't compress. It already is at it's max because it doesn't compress, right?

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: So you put a blanket of fibers. If you could then make a blanket of Cal-Sil on top of everything, that would be the worst thing you could do.

MR. LATELLIER: What you are describing is a mechanism for providing the maximum compression of the fiber which would be assumed under the --

CHAIRMAN WALLIS: Right. It also makes the maximum pressure drop, I think, because putting the Cal-Sil all together makes the maximum pressure drop. So one could require that they do that.

MR. LATELLIER: I'm sorry. Could you repeat that?

CHAIRMAN WALLIS: One could require that they calculate it that way if that produces a maximum pressure drop.

MR. LATELLIER: Please repeat the last scenario.

CHAIRMAN WALLIS: I thought you would already know it. You put the fibers on. You put the Cal-Sil anywhere really. It's a sandwich, only Cal-Sil. I think that's when you get the maximum pressure drop if it's all together.

MR. LATELLIER: That is true.

CHAIRMAN WALLIS: So what you want to avoid is having it all together anywhere.

MR. LATELLIER: Yes, but what you describe is a physical means for inducing the maximum compression of the fiber. And that supports my suggestion that you can approximate worst case effects by a linear combination of the worst case.

CHAIRMAN WALLIS: If you put the Cal-Sil on top.

MR. LATELLIER: Certainly.

CHAIRMAN WALLIS: But that's not what you are requesting that they do. That's the worst possible combination, but you are not requesting they calculate it that way.

MR. LATELLIER: That is true because under the scenarios of transportability for large amounts of fiber, we believe that they will arrive together --

CHAIRMAN WALLIS: Don't say "believe."

MR. LATELLIER: We assume --

CHAIRMAN WALLIS: Don't use the words "we assume." What's the basis of your statement?

MR. LATELLIER: The basis of our statement is the testing that was done for the BWR suppression pools. The transport conditions in that condition, we acknowledge, are much more turbulent than the PWR pools which leads to a separation of the debris.

MEMBER KRESS: More turbulence leads to separation or less turbulence?

MR. LATELLIER: Less turbulence can leave settling in the PWR sump pools.

MEMBER KRESS: That's what I thought you meant.

CHAIRMAN WALLIS: Everything is very well mixed and everything stays very well mixed if it's all stirred up.

MR. LATELLIER: Yes.

CHAIRMAN WALLIS: But PWRs, you have bigger places where things can settle out.

MR. LATELLIER: That's true. And in those circumstances, the large amounts of fiber are less likely to accumulate thick mats.

CHAIRMAN WALLIS: The problem with settling out is that you can make dams of stuff and then the dam breaks and you get a big rush of stuff all in one surge as you can see if you look at the way that storms wash things down roads. They get dams of stuff and then they get a surge of stuff and so on. So again, I'm not always convinced of having it one way is always better than another because it's a very complicated phenomenon.

MR. LATELLIER: Indeed it is. That effect that you describe would perhaps be relevant during the containment spray wash-down phase from upper containment levels.

CHAIRMAN WALLIS: Right.

MR. LATELLIER: That is a limited duration phase of the accident scenario by which time the sump pool may be substantially full to depths of four to six feet by the time that this large charge or the amount of debris that you describe might reach the pool. At that point, the transport velocities would not be sufficient for it to reach the screen depending on the location of its introduction and depending on the geometry of the sump screen.

There are some very unfavorable geometries. It must be considered. The combination of transport during spray wash-down and its location of introduction must be considered in combination with the geometry of the screen. For example, there are plants that have well-defined return water pathways in close proximity to the sump screen. That would be considered an unfavorable circumstance.

MEMBER KRESS: Are those details spelled out in the guidance report?

MR. LATELLIER: These interactions are emphasized. The staff encouraged the industry to provide examples of the interactions between the steps. We have made an attempt to supplement that where we thought it appropriate. The issues have not been ignored or forgotten. We can argue about whether the information is sufficient to ensure attention to the matter.

MR. KOWAL: Okay. I'll move on. Piping smaller than two inches in diameter does not need to be considered for identifying limiting break location. The staff agrees with this guidance.

CHAIRMAN WALLIS: Why is that? Because you don't have to recirculate, isn't it?

MR. KOWAL: Well, that is true. There are some PWRs that may not even need to go into recirculation --

CHAIRMAN WALLIS: Because you could certainly transport debris. But if you didn't have to recirculate, then you wouldn't have a problem, is that what you are trying to say?

MR. KOWAL: That is true. That is part of the reason. Also, some of the large dry PWRs may not need to use containment sprays in that situation. If there are fan coolers or safety grade, you would have less transport --

CHAIRMAN WALLIS: If this were a risk-informed submittal, then you would probably have to do this because these are more likely things. You really couldn't exclude small breaks if this were a risk-informed submittal.

MR. KOWAL: Well, we also feel that the large breaks with bound conditions --

CHAIRMAN WALLIS: But that's not the way you do risk-informed analysis to look at bounding large breaks. You look at probability of all breaks and consider the risk.

MR. ARCHITZEL: I'd just like to make a comment on the risk-informed comment. I don't know that we really know that risk-informed would give you a different answer. When we did a study on the risk associated with this issue, it was with the existing screens that the PWRs have.

So this assumption is you have analyzed and you have addressed the problem. So those vulnerable plants may not be anywhere near as vulnerable anymore to those small breaks and you might get a different answer. I don't know. We haven't done it, but it's not necessarily risk-informed to ignore the smaller breaks is the only point I was making.

CHAIRMAN WALLIS: But if you are not using the risk-informed approach, this is fine because the large breaks are going to be limiting anyway. But if you are going to start whittling away the large break, then I think you might have to revisit this business about what you need to consider.

MR. ARCHITZEL: You are correct in that in the study the risk was dominated by small breaks. That's correct.

CHAIRMAN WALLIS: Right. Thank you.

MEMBER RANSOM: Another problem I see - and it extends throughout this discussion - is the source-term (PH), basically the modeling of the jet and the damaged mechanisms that take place. From everything I have read, they seem so simplistic and possibly even wrong that it would be hard to base a break based on what happens in that scenario.

This may not be the place to discuss it, but you can see that what goes on in terms of debris generation affects all the rest of the analysis downstream in terms of selecting whether or not you have a thin bed behavior or not. Even in terms of the two inch diameter, you never see time come into play into this because on a two inch break you will have a much more energetic break for a longer period of time.

That never comes into the analysis. You don't know what effect that has. A large break is going to be over much more quickly but to much larger an extent. So I question I guess how you can actually make decisions based on such a cavalier model. I think that needs to be discussed.

CHAIRMAN WALLIS: We're going to get to the ZOI, aren't we?

MR. JOHNSON: Yes, we are.

CHAIRMAN WALLIS: You are going to have quite a few questions about that too. We'll revisit some of these questions later in the day.

MR. KOWAL: Other considerations provided include a consideration of debris and material locations with respect to the break. NEI-02-01 walkdowns have probably already been performed to identify these types of locations. The next consideration is the thin bed effect that we have already discussed to some degree and will discuss further later on.

There's a recognition that latent debris inventory may be a limiting source for plants that have little or no fibrous insulation. Attached piping beyond isolation points does not need to be considered. The staff agrees with this. Breaks in these locations should not require sump recirculation assuming the isolation valves --

MR. CARUSO: Could you give an example of what that might be?

MR. KOWAL: In an attached safety injection line or HR line or something.

CHAIRMAN WALLIS: While we're on this slide -- Well, answer his question.

MR. KOWAL: I'm thinking of a safety injection line that has contained isolation valves that --

MR. CARUSO: You don't have to consider a break upstream of the isolation valve.

MR. KOWAL: Right.

MR. CARUSO: But downstream of the isolation valve to the loop, that all has to be considered.

MR. KOWAL: Yes.

MR. CARUSO: Okay.

CHAIRMAN WALLIS: On this second bullet, what you mean is generate enough fibrous debris to filter particulates. "Thin" has no place in that sentence, does it? It's simply enough fibers.

MR. KOWAL: Right.

CHAIRMAN WALLIS: And how do we know that a sixteenth of an inch layer won't filter particulates?

MR. LATELLIER: This is an engineering judgment based on --

CHAIRMAN WALLIS: Have you tested anything thinner than an eighth of an inch?

MR. LATELLIER: The eighth of an inch, first of all, it's important to understand that that is based on the dry fiber packing density, a theoretical density, if you will.

CHAIRMAN WALLIS: Well, we know that no fibers will filter Cal-Sil because you have Cal-Sil deposit with nothing. And then is there a vacuum between no fibers and an eighth of an inch of fibers where the fibers can't filter the stuff. It seems to me there's always a thin bed effect potentially.

MR. LATELLIER: The one-eighth of an inch was chosen as a practical point of evaluation, a rule of thumb judgment. It had been our earlier experience that thinner beds of fiber could not sustain higher pressure drops approaching 20 feet of water.

CHAIRMAN WALLIS: In spite of the overwhelming evidence cited in an SER that Cal-Sil alone can form on a screen. It doesn't make sense.

Obviously these statements are incompatible. Cal-Sil alone can form on a screen. Then you need an eighth of an inch of fibers to make Cal-Sil form on a screen. Those are not compatible statements.

MR. LATELLIER: I don't disagree. The treatment of Calcium-Silicate has been and should be an exception to our previous understanding of combinations of fiber and particulate of the types of iron oxide and silica-based dust and dirt that are present in latent debris.

CHAIRMAN WALLIS: So it seems to me you are retracting the statement about an eighth of an inch being necessary. I'm sorry I'm behaving like a lawyer, but that's what I have to do.

MR. LATELLIER: I'm suggesting that we should clarify our treatment of Calcium-Silicate.

CHAIRMAN WALLIS: I agree. Thank you.

MR. KOWAL: As far as break intervals to be used in the evaluation, the guidance report suggested three. The staff feels that five foot intervals would be acceptable. It still provides for a systematic approach.

CHAIRMAN WALLIS: Why hasn't staff asked the kind of questions that I'm asking when they review these guidances? I don't expect to get an answer.

MR. JOHNSON: I'm sorry. What was the question?

CHAIRMAN WALLIS: It seems to be obvious to me. They are saying compatibility between a clear statement that Cal-Sil can form by itself and another statement that you need an eighth of an inch of fibers to make it form. There's a clear incompatibility. I just wonder why the staff doesn't recognize this and why it has to come to us to ask that sort of a question, unless I'm being naive in some way. I don't expect an answer but I'm just puzzled.

MR. JOHNSON: No, I don't want to answer that. We have asked a bunch of questions. You won't get the benefit of those necessarily today. But we certainly come to you because we expect that you will ask questions that we haven't thought of. That's part of why we do this.

MEMBER RANSOM: A legitimate question here too is, to what degree has the chemical industry filtration technology been brought into play in terms of what it would say about some of these effects? It seems like the industry has tunnel vision. It stays within the nuclear industry. You can say the same thing about the jet behavior.

There's no evidence that you ever looked at the aerospace field to see what really happens in a supersonic jet. There has to be some crossover I would think and some valuable insight that could be gained by this sort of thing. The chemical industry, historically, has dealt with filtration which is exactly the kinds of things we're dealing with; how to separate fibers from particulate material, et cetera.

MR. LATELLIER: Indeed we do take advantage of information from the chemical filtration industry. But in those circumstances, they have the benefit of engineering and optimizing a porous media filtration bed. From that, we have learned a great deal about the limiting circumstances for head loss. However, we don't have the advantage of predictability of debris transport and what the morphology of the beds will be.

So we're at the point of compromising between our lack of certainty about what the realistic beds will look like and what the maximum filtration efficiencies might be if you design them to perform in that manner. Those are the compromises that we're facing.

CHAIRMAN WALLIS: Well, I think you didn't really answer his question. The references in your report are the two really that are in my book which is

45 years old or something. There must have been a lot more work in the chemical industry on filtration than just those two pieces of work which were cited then.

So it's just surprising that there's no broad literature review. And there have actually been books written on filtration where there are standard methods and so on. There's no reference to any of those in any of your work. It seems rather surprising.

MR. LATELLIER: We'll take the comment under advisement. It's always worthwhile to look for crossover advantages. But I would ask if you would have us postulate the optimum filtration efficiency that we can find in the chemical filtration literature.

MEMBER RANSOM: Well, it would be helpful if you simply had a consultant from that industry who could back up what you are saying whereas you are just out in the open the way it is, going on your judgment basically. You must consult the literature and the wealth of knowledge that's out there even if it says we can't do it. Then you have something to stand on.

MR. WAGAGE: Dr. Wallis, I would like to address your question on one-eighth thickness and not recognizing that the Cal-Sil effect in the regulation.

When you put out Reg Guide 1.82 revision 3 at that time there was not sufficient information on Calcium. Information came with the Cal-Sil report LANL put out with experiments.

Now we have both information coming from Reg Guide 1.82 revision 3 which says there has to be a one-eighth inch thickness fiber to form a thin bed. Then the new information is that Cal-Sil can form a thin bed without fiber because itself has fiber.

CHAIRMAN WALLIS: That's right.

MR. WAGAGE: We recognize the need to change that. But we didn't have that information at the time --

CHAIRMAN WALLIS: That's right. So all this stuff on thin bed simply should be if you have Cal-Sil in your plant, you have to calculate the pressure drop assuming that it's in the worst possible place, isn't that really what you are saying? The thin bed effect disappears once you realize that Cal-Sil alone can clog a filter. I was just puzzled by why this thin bed effect is invoked all throughout the guidance and the SER when really it's a misnomer and there's new experimental data which says that it's not quite the same as just a thin bed effect. You can always get Cal-Sil giving you trouble.

MR. JOHNSON: We'll look to clarify that if it's not clear.

CHAIRMAN WALLIS: So you are going to rewrite your SER.

MR. JOHNSON: We'll clarify the treatment of Cal-Sil.

CHAIRMAN WALLIS: Thank you.

MEMBER FORD: Could you go back to the previous slide please? The final bullet about the five foot intervals, it's my understanding that the industry wants the three foot intervals and you have relaxed that based on an earlier evaluation showing that Mariska (PH) perspective doesn't really matter, is that correct? If I read the SER, that's essentially what it's saying that your reasoning for allowing them to relax it to five feet is based on an earlier risk assessment that doesn't really matter, is my reading correct?

MR. KOWAL: There was some work done by LANL where they did evaluate some smaller intervals, I guess one to two foot intervals. That was part of the basis for this.

MR. LATELLIER: However, Dr. Ford, it was not based on a final risk-based estimate. It was based on the practicality and the variety of break types and debris compositions that you would achieve. We simply felt that the same objectives could be achieved with a less refined resolution. Now, if you are performing a risk assessment, as Dr. Wallis indicated, you would be interested in the proportion of linear feet of piping of different sizes and their break potentials.

MEMBER FORD: But then the sub-bullet says "the key factor may be containment materials," i.e. there's a certain uncertainty in that statement. My question really is, how much are you compromising the safety issue by allowing this five foot interval from three foot?

MR. LATELLIER: Although we have not quantified it, it should not have an important effect on the safety outcome as long as the variety of breaks has been adequately examined. By "variety," I mean both the quantities of debris and the composition of debris and their locations. If you think about containment piping, three feet versus five feet, there are not substantial changes in the composition of insulation application over that interval. It's a practical judgment.

MEMBER FORD: So why did the industry elect to go to three foot or were willing to do three foot?

MR. KOWAL: There really wasn't a good strong technical reason in the guidance report for the three foot interval.

MR. TRAIFOROS: I guess there might be another way of defining the break location that is knowing where the material is and how the piping is running at the plant. One might consider the concept of the destruction pressure and the related issue of zone of influence to the break diameter.

This way, one might be able to eliminate possibly looking at too many locations and at least start with the ones that are the most important. Do you think that that might be a feasible way to start looking at the important break location, that is, looking at the material that is being affected, the zone of influence, and then draw a line where you can intersect the pipe that runs around?

MR. KOWAL: I'm not certain what industry will do, but I think that would be a reasonable way to do it. I would expect that licensees would probably proceed in that fashion.

MR. LATELLIER: I would like to add that as we get into our discussions of zone of influence I think you will begin to understand that our uncertainties in that potential volume are much greater than this spatial resolution.

MR. TRAIFOROS: Absolutely, yes.

CHAIRMAN WALLIS: I like my consultant's suggestion. Take the zone of influence for a certain pipe size and roll it around the containment to find out where it has the worst effect and see if there are any pipes there rather than looking at every pipe everywhere.

MR. LATELLIER: There are a number of ways to improve the efficiency of this systematic investigation. I have also proposed the inverse vulnerability approach where you ask yourself what can you accommodate on the existing screen and go look for it.

CHAIRMAN WALLIS: Yes, work backwards from the answer.

MR. LATELLIER: Work backwards. I think that could be a very effective way. And we're not precluding that approach.

MR. JOHNSON: This is Mike Johnson. I don't think there's anything in the industry guides or the SE that would preclude them from taking a course like that.

CHAIRMAN WALLIS: Okay. Should we move on then? I think we're going to have a long day but that's all right. This problem is important enough that I think it deserves it.

MR. KOWAL: Okay. As I mentioned before, in identifying the limiting break location, we're actually looking at all the phases of the event which is the generation, transport, head loss. In reviewing this section of the guidance report, the staff also did consider the Regulatory Guide 1.82 and those locations recommended in that document. Based on the criteria and considerations that we discussed this morning, the staff finds that the guidance report guidance reasonably addresses that spectrum of break locations.

CHAIRMAN WALLIS: Here we get thin bed effect again twice.

MR. KOWAL: Yes, it's in the reg guide.

CHAIRMAN WALLIS: It's everywhere.

MR. KOWAL: So in summary, I will just repeat the staff finds that the guidance is acceptable with the one exception now of the secondary side break location should be performed consistent with the recommended guidance in this section for LOCA pipe breaks also.

MEMBER SIEBER: Do you think there are

plants that can substantiate no thin bed effect?

MR. LATELLIER: Those plants that have an opportunity to do so. And here, by "thin bed" we mean they can rationalize that there will be less than an eighth of an inch of fiber from any source. The only plants that can do that are primarily reflective metallic insulated plants that have good plant cleanliness programs so that they don't have an issue from their latent fiber loadings.

CHAIRMAN WALLIS: But Bruce, I thought that we found that was no longer important because you believe you can get a Cal-Sil build up with no fibers at all. So there is no justification for this one-eighth of an inch.

MR. LATELLIER: We've acknowledged that we need to refine our treatment of Calcium-Silicate and treat it as an exception.

CHAIRMAN WALLIS: Yes, but this is an important thing. This thin bed effect appears on almost every page and yet we have discovered that it's really not properly defined.

MEMBER SIEBER: Well, that was the starting point as I understand it. Cal-Sil was another thought and is not necessarily related to whether you can form a thin bed or not. There are some plants that don't have Cal-Sil. So from that standpoint, you can ignore that.

On the other hand, since the thin bed effect comes from latent fibers - and I don't know of any plant that runs the vacuum cleaner around their containment after each refueling - I'm curious as to whether anybody would claim that they can substantiate no thin bed. I guess I have to read all their responses to see who has the nerve to make that claim.

MR. KOWAL: Okay. We'll proceed now to Section 4.2.1.

CHAIRMAN WALLIS: Well, are we going to get to this point of latent debris?

MR. KOWAL: That's a separate discussion.

CHAIRMAN WALLIS: Okay. So maybe we can come back to this question about what is it a plant would have to do in order to substantiate no thin bed effect.

MR. KOWAL: Okay.

MEMBER SIEBER: Well, in order to calculate how much latent debris you have, you do have to sample surfaces, primarily horizontal surfaces, in containment with either wiping it up or a little vacuum cleaner or something like that. On the other hand, I can't imagine people crawling up on top of steam generators to try to get all the dust off of them.

MR. KOWAL: We'll get into that when we talk about latent debris.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: Presumably they have never been cleaned. That's where most of the dust is.

MEMBER SIEBER: That's a pretty good assumption.

MR. KOWAL: Section 4.2.1 of the guidance report proposes a refinement to the break selection --

CHAIRMAN WALLIS: Are you going to do the --

MR. KOWAL: Yes, there's a separate handout for this section. Basically the refinement proposes to allow the use of branch technical position MEB 3-1 for the break locations to be considered in the sump performance evaluations. In summary, the staff does not accept this refinement. It is not acceptable to the staff. The staff concludes that the guidance of section 3-3 should be followed as is for break selection purposes.

Really the application of SRP 3.6.2 and MEB 3-1 would focus attention on break locations, high stress, and high fatigue, for example, such as the terminal ends of piping, intermediate pipe ruptures, locations at high stress. Staff finds this unacceptable for a number of reasons.

First of all, the PWR sump performance evaluations are performed to insure adequate long-term cooling and compliance with 10 CFR 50.46(b)(5) which requires that a number of locations and size of breaks be considered. The appropriate SRP sections staff would follow to review those basically suggest that reviewers evaluate whether the entire spectrum of sizes and locations was considered. Considering only those locations with MEB 3-1 would not meet or satisfy the requirements of 50.46.

The second reason, the staff also previously rejected a similar proposal for the BWR resolution of this issue. In doing so, we cited two reasons: first of all that the SRPs don't provide guidance or acceptance criteria for how to meet the guidance of 50.46.

Actually compliance with GDC-4 is the only acceptance criteria discussed in those sections. Also, the BWR Owners Group had not demonstrated that these break locations would produce the bounding or most limiting locations. The same would apply for the PWRs.

As I mentioned before, Reg Guide 182 provides what the staff considers to be the complete spectrum of breaks to be considered. Considering only those locations of MEB 3-1 does not necessarily capture this complete spectrum.

The final reason is, the ongoing 50.46 rulemaking efforts to risk-inform 50.46 and the break size is not proposing to change this current regulation regarding the break locations. What we're trying to do with GSI-191 should be consistent with that. So in summary, the staff does not find this proposed refinement to be acceptable. The break selection process should proceed in accordance with section 3.3.

CHAIRMAN WALLIS: Thank you very much. Anymore questions or comments from the Committee or the consultants or staff members? Can we move to the next presenter? Thank you very much.

MR. KOWAL: The next presenter is Angie Lavretta.

CHAIRMAN WALLIS: Thank you for your patience with us and our questions.

MR. KOWAL: You're welcome.

MEMBER FORD: It's time for a break, isn't it?

CHAIRMAN WALLIS: I don't think so. I think we better move on.

MEMBER FORD: What's next? Is it zone of influence?

MEMBER SIEBER: Debris characteristics.

MEMBER FORD: Debris characteristics.

CHAIRMAN WALLIS: Well, this is the time we were scheduling a break. Is it sensible to have a break now?

MEMBER SIEBER: It might be necessary.

CHAIRMAN WALLIS: Before we get into something significant, okay. I'm sorry. We're going to have a break. We're going to take it until 10:20 a.m. So it's going to be something less than 15 but over 10 minutes. We'll start right on time at 10:20 a.m. Off the record.

(Whereupon, the foregoing matter went off the record at 10:05 a.m. and went back on the record at 10:21 a.m.)

CHAIRMAN WALLIS: Back on the record. We're looking forward to hearing about the zone of influence. I think that's what we're going to do. Are we going to hear about zone of influence now or is it debris characteristics? So we've dumped out of zone of influence. Are we passing over zone of influence of influence or are we coming back to it? We seem to have a presenter on debris characteristics so let's hear that.

MS. LAURETTA: Good morning. My name is Angie Lauretta with Plant Systems Branch. I'll be presenting the debris characteristics. This is Section 3.4 of the baseline in both the SER and the NEI guidance document and includes 4.2.2.2 in the Refinement section. Supporting this review with me are Martin Murphy of the Materials and Chemical Engineering Branch who is joining me at the table as well as Clint Shaffer of the Eris (PH) Corporation. Bruce Latellier is also available.

Three major topics are covered in Section 3.4. Debris characteristics is one of them, coatings which I also will be addressing and debris destruction which includes the zone of influence discussion that will be presented after this presentation by Mr. Ralph Architzel. Also as you noted earlier, latent debris is not included as part of this debris characteristics discussion.

CHAIRMAN WALLIS: Latent debris, however, is a very important, could be a very important actor in all of this.

MS. LAURETTA: It is. The three

presentations together I think are very interrelated. Slide two. As an overview, debris input parameters needed for transport and head loss calculations include destruction pressure, density, size and debris fractions or size distribution.

MEMBER RANSOM: Could I ask you for a definition before we get started? What do you mean by "destruction pressure"?

MS. LAURETTA: This is the damage pressure defined by the zone of influence which will be discussed later on.

MEMBER RANSOM: What is it though? Define it.

MS. LAURETTA: The pressure at which debris type --

MEMBER RANSOM: Pressure itself does not destroy anything. Pressure gradients, pressure differences, those are the things that are important or forces that act on the material and this is a problem that somebody has to define because throughout the discussion they use things like pressure, jet pressure, destruction pressure, stagnation pressure, all somewhat interchangeably. These all are quite different things and somebody has to define those and use them consistently.

MS. LAURETTA: As far as the discussion, perhaps Bruce can rely how it's used for.

MEMBER RANSOM: Who is? Somebody is going to define these terms, I guess.

MR. ARCHITZEL: This is Ralph Architzel from the Staff. When I get into zone of influence, really destruction pressure, this is a hard place to talk about it. So it's not necessarily in my discussion, but you're right. I mean we use impingement pressure as well. So we use a variety of terms and in the end, it's a surrogate for what really destroys the material.

It's not necessarily what really happens and I agree with you. It's not necessarily a pressure, but it has been empirically measured in testing at the face of different distances from discharges, air jets and things like that. We're using that surrogate.

Now we can maybe clean it up and say in different places, "Perhaps impingement pressure is the best thing to use because that's what's been measured in the test programs that have been done." But that is then empirically determined on the test procedures and that's where a major portion of the targets are destroyed and that's the pressure of interest. It's not necessarily a pressure. It's a characteristic we can measure. Some tests that's been done back calculating distances and using the ANS Standard. It's not actually been measured, but a lot of things are going to measure pressure where you actually take a pressure at a distance from a test setup.

MEMBER RANSOM: Well, for example, the ANS standard seems to actually imply these are static pressures throughout the jet.

MR. ARCHITZEL: Yes. Actually, throughout the jet, it's a brought to rest type of stagnation pressure is what's being used.

MEMBER RANSOM: Well, even in a supersonic jet, you never the stagnation pressure. You only see the pressure downstream of a normal shock that proceeds that.

MR. ARCHITZEL: I guess I can get into that a little bit, but I guess the point is here at this point what we used is a not only a surrogate. It's basically a metric that's been used that can be consistently applied in the analysis of this whole problem. I'll grant you. It's not necessarily a destruction pressure that destroys the targets.

MEMBER RANSOM: There are two pressures that quite honestly if you look in the literature are important. One is a blast-away pressure which is across a normal shock basically or a spherical shock that goes out ahead of a blast-away. That creates crushing pressure of course.

The other one is dynamic pressure which is what is used to correlate all aerodynamic forces that exists on destruction. That's --

MR. ARCHITZEL: Maybe it's preferred to hold this for 15 minutes until I'm up there with Bruce and to have this part of the discussion later on.

CHAIRMAN WALLIS: Would you agree, Ralph, though that if you had a coating on a wall and all you did was apply uniform pressure to it, nothing would happen.

MR. ARCHITZEL: I agree.

CHAIRMAN WALLIS: So there's something a bit weird about using pressure, but you're going to allude to that when you get up there.

MR. ARCHITZEL: I don't know if I'll do any better, but Bruce will help me out a little bit better on trying to.

CHAIRMAN WALLIS: Okay.

MS. LAURETTA: All right. The approach used in the guidance document for debris destruction and characterization varies between two debris types and they are coatings and all other debris types. That is the approach used by the NEI for coating is different than that used for the other debris types.

CHAIRMAN WALLIS: So a coated, something like Cal-Sil, isn't that coated too in some way?

MS. LAURETTA: I don't believe.

CHAIRMAN WALLIS: That's not a coating. That's part of the Cal-Sil. Coating to you is a paint or something thin stuck on a hard surface. It's not a coating on a insulation or something like that.

MS. LAURETTA: Exactly.

MEMBER KRESS: And is it true that you exclude qualified coating as a resource?

MS. LAURETTA: No, we're considering it, but I'll be getting into in the next couple slides our determination, our findings. Our overall finding for coatings is that lack of data leads to staff positions for either the need for plant-specific justification for a value used or use of previously accepted values.

CHAIRMAN WALLIS: Which is what? What are the previously accepted?

MR. ARCHITZEL: Is that the 10D?

MS. LAURETTA: Yeah, that's the specific case.

CHAIRMAN WALLIS: So you're basis for the

10D inference for coatings is based on a previously accepted approach. It's not something that came out of the air.

MS. LAURETTA: Right.

CHAIRMAN WALLIS: I didn't see that in this. It does actually refer to that.

MS. LAURETTA: We specifically made that statement in the SEA and also in the upcoming slides.

CHAIRMAN WALLIS: Okay. Thank you.

MS. LAURETTA: For all other debris types, the debris specific data and the default values, we find acceptable.

MR. TRAIFOROS: I would like to go back to your first bullet very quickly. You do list destruction pressure, but it seems to me that the important parameters are the result of this destruction pressure which you are describing as density, size, size distribution possibly. Because again, you are talking about the brief characteristics provided for transport and you list destruction pressure.

It's difficult to relate these two in terms of the transport events or the transport of the material and the position. So I understood listing destruction pressure as what causes basically the size, the density of the material and stuff like that. Is this the way? Is this why you listed that?

MS. LAURETTA: Yes. Also as I get into the presentation, you'll see that destruction pressure is a basis we use for conservatism of insulation type. It's used as a standard much like what Ralph described.

CHAIRMAN WALLIS: Okay. Well, what does it destroy? If I have a pipe that's wrapped in Cal-Sil, and you've seen Cal-Sil like this stuff here. It's that the pipe is wrapped in this stuff. He has it all around the pipe. Now pressure presumably is on one place. Does that blow off everything that is on the pipe or just some of it?

MR. ARCHITZEL: Dr. Wallis, I have some pictures in my presentation.

CHAIRMAN WALLIS: When you say, well, okay. So you're going to explain what you mean by the effect of destruction pressure. It blows off everything on the pipe if you have a certain pressure.

MR. ARCHITZEL: The major portion is considered the destruction pressure. There is some discussion like, for example, in the Nukon. There is a controversy between the ten pounds and the six pounds destruction pressure in the URG and the difference there was --

CHAIRMAN WALLIS: But it's all or nothing. It's all or nothing --

MR. ARCHITZEL: No, it's a lot or not much, but it's all something. The point is the six was not all. The ten was quite a bit. So when it's quite a bit that's when you're saying that's the destruction pressure.

CHAIRMAN WALLIS: So destruction pressure means that it's enough whatever the potency of the jet is measured by pressure in some way to remove all the insulation from it.

MR. ARCHITZEL: The major portion of it.

CHAIRMAN WALLIS: Well, it must be all. A major portion doesn't mean anything.

MR. ARCHITZEL: No, because I'll show you some pictures.

CHAIRMAN WALLIS: But for calculation purposes, you say it all comes off.

MR. ARCHITZEL: Yes, for calculation purposes.

CHAIRMAN WALLIS: Okay. Thank you.

MEMBER RANSOM: Well, there's another point along that line that you read in the testing that was done with air jet testing, the major destruction occurred in the blast wave that proceeds the actual jet impinging on it. It's out in front. It's basically a normal shock, but yet in the ANS standard and throughout the rest of the analysis, blast wave effects are completely ignored. So you wonder what is the damage mechanism that you're looking at.

CHAIRMAN WALLIS: Does the Staff have any answer to that or are you going to come back to that? We'll come back to that later. Okay.

MR. ARCHITZEL: We really would prefer to hold it when we're up there because Bruce will have some answers and I have some discussion.

MR. LATELLIER: Maybe I could add just a brief clarification. The damage pressure as Dr. Traiforos mentions is more a characteristic of the installation targets that we're interested in, not a characteristic of the debris. And also it's important as Ralph mentioned to understand that our understanding of damage mechanisms is based on empirical evidence which are correlated to properties of the expanding jet field and we have chosen pressure which we will define and discuss in greater detail in just a moment.

MS. LAURETTA: Slide three. This has to

do with debris characteristics and does not include coatings. The NEI document recommended that specific values for debris types be used, but for those debris types that were not readily available bounding debris types would be used for conservative application. For example, for missing damage data would use damage pressure of 4 psi which corresponds to the most limiting insulation type.

CHAIRMAN WALLIS: I couldn't quite figure this out. If you have a mixture of coatings in your zone of influence, some is metallic insulation. Some is Cal-Sil. Some is Nukon. Some is other stuff. You seem to saying that you calculate the pressure which will remove the stuff which is easiest to remove and then you apply to everything else?

MS. LAURETTA: No, it's the opposite.

MR. ARCHITZEL: Excuse me. Dr. Wallis, that's also my section.

CHAIRMAN WALLIS: Oh, you're going to do that too. Well, that was just here.

MR. ARCHITZEL: That's an accurate statement. You had an accurate statement.

CHAIRMAN WALLIS: So my statement is right. It seems very, very conservative.

MR. ARCHITZEL: But there's a refinement

that takes care of that, but that's in my presentation.

CHAIRMAN WALLIS: Okay, but it's on this one too.

MS. LAURETTA: Well, this is --

MR. ARCHITZEL: You can ignore it here and I'll talk about it.

MS. LAURETTA: This is what was proposed in the guidance in this section. We're going in parallel with the way it was proposed in the guidance report.

CHAIRMAN WALLIS: And Ralph's going to explain why.

MS. LAURETTA: Right. They touch on some areas in several places in the guidance report.

CHAIRMAN WALLIS: And Ralph is going to explain the two size groups as well as here.

MS. LAURETTA: No, that will be me.

CHAIRMAN WALLIS: Okay. Could you tell us what the two size groups' size is.

MS. LAURETTA: Yes, sir. Two group size classification and size distributions are assumed, the small and large. Small is considered to be that which could be transported through grading, trash racks and radiological protection fences that are less than 20 square inches in opening size with a nominal four inch by four inch square opening. The GR also omits consideration of two phase damage mechanisms which as we said will be discussed more in the next presentation.

CHAIRMAN WALLIS: So you have the debris in two classifications. One is really fine stuff which flows through everything until it gets to the sump or something.

MS. LAURETTA: Right.

CHAIRMAN WALLIS: And that other is wads of it that can get stuck on the way and trash racks.

MS. LAURETTA: And wouldn't make it to the sump.

CHAIRMAN WALLIS: And so on. And presumably, the interaction of the two isn't considered because you're being conservative or something that if the large debris blocks up a trash rack presumably it will also catch some of the small debris. But you're being conservative.

MS. LAURETTA: And assuming that all the small debris gets through.

CHAIRMAN WALLIS: Okay. What's the basis for assuming how much of it is one kind or the other? How much of the debris is big and how much of it is small, how do you decide how to distribute the debris into two categories?

MS. LAURETTA: Well, this slide describes what was proposed by NEI.

CHAIRMAN WALLIS: But how do they do it then? How do they decide how much of the debris is big and how much is little?

MS. LAURETTA: Well, the 60/40 split is consistent with what was used.

CHAIRMAN WALLIS: Sixty percent small?

MS. LAURETTA: Well, we're talking about for Nukon 60 percent small/40 percent large was used in the BWR URG and also tests were done at the Ontario Power Generating Station that show the 52 percent.

CHAIRMAN WALLIS: There's a long discussion in the SER I found sort of rambling about the Ontario tests and how they showed this and on the other hand, they showed that. Maybe they showed something else.

MS. LAURETTA: Depending on what the mechanisms --

CHAIRMAN WALLIS: That's right. So I didn't feel very confident that they had showed me something I was sure about, but presumably the 60 percent fine is based on some sort of conservative interpretation of the tests or something.

MS. LAURETTA: Exactly. The 52 percent, that was characterized as 60 percent was considered to be conservative and consistent with what had been accepted before.

CHAIRMAN WALLIS: Sixty percent is quite big. So if we assumed 100 percent with all the other uncertainties we have, it wouldn't make all that much difference perhaps.

MS. LAURETTA: And the 100 percent is assumed for some of the insulation types. Going on to slide 4, staff evaluation of those recommendations considered acceptable. First, that the bounding debris type be applied to all debris for which data is not available.

CHAIRMAN WALLIS: It is conservative. I think we would agree that's true. If you break Nukon with a pressure which would break fiberglass, you're certainly being conservative. But I'm sure why number two is conservative. Maybe that's where the long discussion of the Ontario hydro.

MS. LAURETTA: That, and also with number two we're talking about the --

CHAIRMAN WALLIS: See. I would say it's conservation if you assume it's all fines. But you

have a better justification than that for 60 percent.
MS. LAURETTA: Well, what we did is we did confirmatory analyses that are included in Appendix 2 of the SER.
CHAIRMAN WALLIS: This is analysis of how the fibers break up.
MS. LAURETTA: Right. We took a representative sample of certain insulation types.
MR. SCHAFFER: Dr. Wallis, this is Clint Schaffer. I performed some confirmatory research where I looked at the debris size distribution from the available test, for instances, what we call low density fiberglass in this one case and plotted out the size groups as a function of the pressure and correlated that to the pressure within its own influence and did the integral and showed that their 60 percent appears to conservative. So we've added some realistic research to back that up.
CHAIRMAN WALLIS: Okay. Good.
MR. SCHAFFER: The two size group, you should wait until you see the transport. The size groups go to the transport analysis.
CHAIRMAN WALLIS: Okay.
MS. LAURETTA: Also the last bullet --
CHAIRMAN WALLIS: I don't like the word

"plausible."

MS. LAURETTA: Well, the two phases as has been raised before, the damage mechanisms may not be clearly defined but based on plausible two phase damage mechanisms, we believe that's compensated for by the conservative function.

CHAIRMAN WALLIS: What's your definition of "plausible"?

MS. LAURETTA: Those that we've accounted for in testing which was supported by the --

CHAIRMAN WALLIS: So it's more positive than it sounds. Plausible usually has negative connotations. In other words, if my teenage daughter appears at 2:00 a.m. with all kind of excuses, I would say, "Your excuses sound plausible. Now tell me what really happened."

MS. LAURETTA: Well, perhaps I should have used a different word there.

MR. LATELLIER: Excuse me. This is Bruce Latellier. There's been a lot of discussion about the possible effects of two phase impingement that have not been tested thoroughly and various mechanism have been hypothesized from erosion due to droplet impaction, penetration in internal expansion because of the thermodynamic condition of the fluid.

Various mechanisms have been discussed. Although we do not have thorough data to assess them, that's the reason we're using them as plausible. We think that there perhaps are important effects we need to acknowledge.

CHAIRMAN WALLIS: I like that idea. I like the idea that the water is driven into the insulation at 1,000 psi. When the pressure drops to some lower value, it expands and blows it off and that's not represented by damage pressure at all. But it could happen.

MS. LAURETTA: Slide five. This begins the coatings discussion. The major recommendations offered in the baseline for coatings are a damage pressure of 1,000 psi with corresponding zone of influence of 1D. The failure assumptions are that inside the zone of influence all coatings fail both qualified and unqualified. Outside the zone of influence, the assumption is that qualified coatings remain intact and that the unqualified coatings fail.

Also default thickness is assumed for unqualified coatings outside the ZOI as an inorganic zinc equivalent of 3 mils. The guidance report also omits the consideration of no thin beds (PH) as has been discussed at some length and we'll continue on it. I will be discussing it or addressing it somewhat here but the main thrust of the discussion will be in the head loss presentation.

CHAIRMAN WALLIS: So this damage pressure, well, you're going to be very conservative about it. You're assuming that it's just pressure. It's not as if the jet picks up bits of Cal-Sil and throws them at the wall. That sort of thing is completely out. It's just it's a fluid pressure that washes off the coating.

MS. LAURETTA: Well, this is what has been proposed.

CHAIRMAN WALLIS: Yes.

MS. LAURETTA: Our evaluation.

CHAIRMAN WALLIS: So the assumption is, but your evaluation is much more conservative so perhaps I don't need to worry about it.

MS. LAURETTA: And that's on the next page.

MR. TRAIFOROS: I have one question on this 1,000 psi. This was the value listed. Is the value listed in the guidance document?

MS. LAURETTA: Right.

MR. TRAIFOROS: The NEI. However, in Table 3.2 of the Staff SER, there is no number there.

Instead it is to be determined both for protective coatings with epoxy and unprotected inorganic zinc. So there seems to be a difference between what you are discussing here as the damage pressure which is consistent with GR and the SER recommendation. I was wondering if you could maybe comment on that.

MS. LAURETTA: Yeah, I'll be touching on that in a couple of slides.

MR. TRAIFOROS: Beautiful. Thank you.

MS. LAURETTA: All right. Slide 6. As far as coating, the Staff evaluation of areas where we consider to be acceptable --

CHAIRMAN WALLIS: I think, are you mixing things up here? Coatings are the ones that you didn't accept. Don't you mean all whatever you call, what do you call collectively the Cal-Sil and the --

MS. LAURETTA: Debris characteristics.

CHAIRMAN WALLIS: But when you say coatings, I thought that was paints.

MS. LAURETTA: It is.

CHAIRMAN WALLIS: Because I think that's not acceptable what they submit for paint.

MS. LAURETTA: Well, I'm going to be presenting a list of what we find acceptable and what we find as needing alternative guidance. The first slide lists those aspects or those recommendations.

CHAIRMAN WALLIS: In the ZOI 1D?

MS. LAURETTA: That's not listed here as one of the acceptable.

CHAIRMAN WALLIS: Okay. So you are not going to redefine the ZOI later on.

MS. LAURETTA: Right. That's on the next page, on page seven. But on page six, I was just listing the recommendations that we found acceptable and those are the recommendations that the coatings fail within the zone of influence.

CHAIRMAN WALLIS: But it has to be redefined as you would redefine it.

MS. LAURETTA: Right.

CHAIRMAN WALLIS: Okay.

MS. LAURETTA: And that the qualified coatings outside do not fail. However --

MEMBER KRESS: Is there a technical basis for that? Do you have an experiment?

MR. MURPHY: Qualified coatings outside the zone of influence have been subjected to pressure and temperature testing, autoclave testing.

MEMBER KRESS: Yes, but that's different than intent. So you're basing it on the autoclave results.

MR. MURPHY: That's why we've chosen to separate it from outside the zone of influence and those qualified coatings inside the zone of influence.

CHAIRMAN WALLIS: No.

MEMBER KRESS: I think outside the zone of influence is more like the autoclave testing effects.

MR. MURPHY: Yes, that's right.

CHAIRMAN WALLIS: So they just fall off due to there's no flow effect.

MR. MURPHY: Outside the zone of influence, the qualified coatings do not fall off. That's the assumption.

CHAIRMAN WALLIS: Because the flow effects are small and it's just that they are heated up.

MR. MURPHY: That's correct.

CHAIRMAN WALLIS: That's the assumption.

MR. MURPHY: Well, they've been tested and shown that they will remain intact under the LOCA conditions of pressure and temperature.

CHAIRMAN WALLIS: On the static testing without any flow.

MR. MURPHY: That is correct.

MS. LAURETTA: The only stipulation here is that we ask that periodic condition assessment be done to ensure that they remain qualified.

CHAIRMAN WALLIS: And then this final statement means that all the paint falls off the entire containment if it's unqualified.

MS. LAURETTA: All unqualified coatings.

CHAIRMAN WALLIS: All falls off?

MS. LAURETTA: That's the assumption.

MR. MURPHY: Because they have not been tested and subjected to pressure and temperature.

CHAIRMAN WALLIS: So these guys even if they have a little pipe break, all the paint is going to fall off everywhere.

MR. MURPHY: No, all the unqualified coating.

CHAIRMAN WALLIS: Well, I know, but if they have unqualified. Do they ever have unqualified coating?

MR. MURPHY: Yes, they do.

CHAIRMAN WALLIS: They do.

MEMBER SIEBER: Some do.

CHAIRMAN WALLIS: That's a lot of material. It's a big place.

MR. MURPHY: That's correct.

MS. LAURETTA: Right.

MEMBER KRESS: Did you accept the default thickness for the unqualified coatings at 3 mil?

MS. LAURETTA: No, and again that's coming up in the next slide.

MEMBER KRESS: Okay. Sorry.

MR. TRAIFOROS: Going back to the outside the zone of influence, it appears that this particular coating is further away than the one diameter that you define here for the 1,000 psi. This is the definition. They are further away of the zone of influence and therefore they are not affected which is your definition of the distraction basically. Right? So they are further away.

MR. MURPHY: If I understand your question or your statement of that, you're correct. Because they are further away and they've been qualified, they will remain intact.

MR. TRAIFOROS: Yes. Correct.

MS. LAURETTA: Also for the unqualified coatings outside --

CHAIRMAN WALLIS: Okay. Let's go back to these coatings. They are qualified when they're new. Don't they age? Paints usually fall off of houses after a while and they fall off of nuclear plants after a while?

MR. MURPHY: There have been cases of that and we made a stipulation in the SER that if you have a degraded qualified coating you have to treat it as an unqualified coating and consider that it would then fall off.

CHAIRMAN WALLIS: How do they measure whether or not it's degraded?

MR. MURPHY: Currently, visible assessments.

CHAIRMAN WALLIS: Just look at it?

MR. MURPHY: They do plant walkdowns.

CHAIRMAN WALLIS: And that can tell them whether or not it's going to fall off when it's subjected to --

MEMBER SIEBER: Usually when they do that, you will find places in the plant where it has fallen off. Then you inspect that to see how well what remains adheres to the surface.

CHAIRMAN WALLIS: But this doesn't really tell them that it wouldn't fall off points subjected to pressures and temperatures on the LOCA.

MEMBER SIEBER: That's correct.

CHAIRMAN WALLIS: No. So it's a very crude way. Just look at it to see if it's still as good as it was before in an autoclave.

MS. LAURETTA: We had also --

MEMBER SIEBER: No, look at it to see if

it's still there.

CHAIRMAN WALLIS: But that's under normal containment conditions. That's not LOCA conditions.

MR. MURPHY: That's correct.

CHAIRMAN WALLIS: It still seems a little weak somehow.

MS. LAURETTA: We had also added the stipulation that a condition assessment be put in place to maintain.

CHAIRMAN WALLIS: Okay. Is this sort of an aging management program for coatings? Is that it?

MS. LAURETTA: Right. I don't think we've defined it.

CHAIRMAN WALLIS: Is there no aging management program for coatings?

MR. MURPHY: Not necessarily.

CHAIRMAN WALLIS: There is for almost everything else that exists in a plant.

MR. MURPHY: Correct.

MEMBER RANSOM: Is the zone of influence for coatings based on these water jet tests that you did on painted surfaces?

MR. MURPHY: The 10D zone of influence, is that what you're referring to?

MEMBER RANSOM: Yeah, or the 1,000 psi, I guess.

MR. MURPHY: Well, the 1,000 psi was the recommendation from industry which was based upon water jet testing. So it was based on some testing.

MS. LAURETTA: I'm going to move on to slide 7.

MEMBER RANSOM: Incidentally, in that case from the industry testing, I assume these were liquid jets and the 1,000 psi was really the stagnation pressure that was used they supplied.

MR. MURPHY: It was a liquid jet and it was at a higher pressure. I believe they used a pressure washer. It was around 3,500 pounds, I believe, at the discharge of the pump. I don't think they measured the actual pressure anywhere else.

MEMBER SIEBER: Right.

MEMBER RANSOM: But where did the 1,000 psi come from? You just backed down from 3,000 until the paint ceases to come off?

MR. MURPHY: Again, that was the supply industry suggestion. I'm not exactly sure how they got there. I think they reduced the pressure to provide some conservatives.

MS. LAURETTA: And we talk about that on slide 7. One of the areas where we propose alternative guidance to what was proposed by -

CHAIRMAN WALLIS: Now supposed I have a plan which has beautiful metallic insulation and it's all very rugged and none of it comes off and it has no latent debris. The only thing that comes off is a great pile of paint chips. Do I have head loss data for paint chips that I can use or does NUREG 6224 automatically take care of paint chips and flakes and all that stuff?

MR. SCHAFFER: My understanding is that there is a little bit of data out there for paint chips on the screens. It's older industry data, but that is one area, I believe, our head loss testing is lacking.

CHAIRMAN WALLIS: Is there any guidance about what you should assume for things like SV for paint chips?

MR. SCHAFFER: Not that I've seen.

CHAIRMAN WALLIS: So how is, The licensees then have to do their own tests of paint chips?

MR. SCHAFFER: That's the idea.

CHAIRMAN WALLIS: Okay.

MS. LAURETTA: And as we've discussed the destruction pressure of 1,000 pounds we don't believe is sufficiently justified. Testing was not performed at representative LOCA conditions that treated both temperature and pressure and no correlation was provided to extrapolate.

CHAIRMAN WALLIS: So let me go, I'm going to go back to head loss. I'm sorry. I'm just thinking. So there were experiments done with fibers and Cal-Sil and it was discovered that Cal-Sil could be bad. There was a bad effect or whatever you want to call it. That was not known until the tests were done.

Now you're going to say that we don't know what's going to happen with paint chips until some tests are done. Probably there will be some surprises there too and the Staff has to somehow deal with sort you have 69 plants and five of them have paint chips that don't affect the screen and two of them have unacceptably high, but they seem to be the same paint. You have anomalies appearing. I'm trying to think ahead that somehow is going to have to be sorted out by the Staff because there's no definitive work on filtration of paint chips through paint chips deposited on the screen.

MR. SCHAFFER: We obviously need to see some test data for paint chips in order to understand how this is going to shake out. My understanding is that the industry is building a test loop and they are going to conduct test data. Hopefully, they will cover paint chips.

CHAIRMAN WALLIS: And when will they have these results?

MR. SCHAFFER: I don't know.

CHAIRMAN WALLIS: So resolving the GSI is conditional upon the industry building successful test loops and getting acceptable data?

MS. LAURETTA: We have a default value that we're proposing that they can use.

CHAIRMAN WALLIS: You have a default value?

MS. LAURETTA: The 10D.

CHAIRMAN WALLIS: No, no, for the effect of the test of the paint chips on the head loss on the screen.

MS. LAURETTA: On size.

CHAIRMAN WALLIS: I don't know if you have a default value for that.

MR. LATELLIER: Let's keep in mind that the assumption of complete failure is artificial as you pointed out.

CHAIRMAN WALLIS: I know. I read that.

MR. LATELLIER: And that perhaps more

relevant to the issue than the head loss behavior is what the form of that debris will take. I think that needs to be determined first.

CHAIRMAN WALLIS: I believe that too. I think that if it's finally divided, it's very different flakes.

MR. LATELLIER: Exactly so. Under the guidance report, the industry position was to assume that degrades to the pigment basis, finest particulate available and that was done to emphasis the head loss effects in combination with fiber mats.

CHAIRMAN WALLIS: Which might then give you a lot of downstream effects in the reactor and all this swara of paint chips goes through the reactor.

MR. LATELLIER: Indeed, that is a result of that assumption, but again it's artificial. It's done to emphasize conservatism from one point of view. Now in the case that you describe of a plant that has no fiber and it has entirely reflective metallic insulation, the fine particulate may not be the most conservative form of the debris. It may be fine chips and platelets the tend to accumulate, but that hasn't been determined. It's not useful to discuss the head loss behavior until you know something about the debris.

CHAIRMAN WALLIS: Well, I'm just wondering if it's useful to resolve the GSI until we know something about the head loss behavior.

MEMBER RANSOM: Well, I bring up another point that if you read an I-6 appendix these are confirmatory appendices, the NRC has now discovered that you can higher than the stagnation pressure on a flat plate. I say this factiously because it's an error and the reason I bring it up is because this kind of error does not belong in anything with that the Nuclear Regulatory Division uses for regulation of nuclear power plants. Not only that when you see this kind of things in a report, it discredits everything. I couldn't get beyond that.

MR. LATELLIER: We will be discussing this in the next presentation for zone of influence, but I can say now at this moment that that assumption was made for consistency with the ANSI jet model and as we come to a common understanding of what that model does, I believe that you'll see that assumption is conservative from the point of view from our damage metric that we've chosen.

MEMBER RANSOM: I don't care. It's impossible.

CHAIRMAN WALLIS: Well, we can come to a

common understanding maybe.

MR. LATELLIER: I don't disagree.

CHAIRMAN WALLIS: We can buy in to the second law of thermodynamics. Then we build a heat engine and make free power.

MEMBER RANSOM: It's embarrassing.

MR. LATELLIER: The intent is to conserve the total thrust available from the orifice and that's exactly what's done in the jet model to emphasize for conservatism the thrust loading available on large structural objects.

MEMBER RANSOM: All it does is demonstrate there's a lack of understanding of how supersonic jets behave and the use of thrust coefficients and conservatism of thrust and trying to calculate what goes on in a jet is just not right. It's possibly conservative, but it's not realistic.

CHAIRMAN WALLIS: We're going to get into this discussion with Ralph later on.

MR. LATELLIER: I believe that's our next topic.

CHAIRMAN WALLIS: Okay.

MS. LAURETTA: As a finding for coating destruction pressure, we concluded that licensees may either use the 10D zone of influence for coatings or come in with plant specific justification for the value used based on experimental data. The next page, page eight, with regard to the default thickness for unqualified coatings outside the zone of influence, we consider that to be unsubstantiated.

CHAIRMAN WALLIS: What does "IOZ" mean?

MS. LAURETTA: Inorganic zinc.

CHAIRMAN WALLIS: Say that again.

MR. LATELLIER: Inorganic zinc.

CHAIRMAN WALLIS: Okay. It's interesting. It looks like ZOI backwards or inside out or in a mirror or whatever.

(Laughter.)

CHAIRMAN WALLIS: So it's inorganic zinc.

MS. LAURETTA: Yes.

CHAIRMAN WALLIS: That's what all the paintings are? They are all the same kind?

MR. MURPHY: No.

CHAIRMAN WALLIS: No.

MR. MURPHY: They use an equivalent for a default value of that.

CHAIRMAN WALLIS: Okay.

MEMBER: Why?

MR. MURPHY: The reasoning provided was because it has a higher density that it would provide an equivalent mass of roughly 13 to 15 mils of say epoxy or it's another type of coating that would be potentially thick that would be unqualified and therefore, it was potentially conservative. But there's enough instances where we don't think it's conservative that we chose not to accept it and requested the date coming with plant specific data to show what they actually had.

CHAIRMAN WALLIS: That would seem to me that what matters really is how the paint coming off. If it comes off as a powder, it's going to be very different than if it comes off in big flakes or sheets where some paints do. If it's a tough kind of paint, it feels differently than one that just sort of wears off and the rain washes off your house. Sometimes what comes off your house, certain kinds of paints, flake off in rather big pieces.

MR. MURPHY: That's correct.

CHAIRMAN WALLIS: That's quite different. If that gets on a screen, it goes cluck and covers up several bits of the screen right away and it's very effective as a screen clogger, flakes like that. Just like bits of plastic or something, they are very effective screen cloggers.

MS. LAURETTA: And that's --

CHAIRMAN WALLIS: So it's not really the mass of paint. It's the form it has that's most important.

MR. ARCHITZEL: Well, I think it's both because when you make the assumption that it's all found in this particulate, then it's a function of the mass and density that's failing and when you believe that there is a bed that forms on top of the sump, there the guidance report use of particulate for all paint was a conservative approach because --

MEMBER SIEBER: Hm-hm.

MR. ARCHITZEL: -- we raised the question that maybe you didn't have a bed, just what you were saying, where it could come off as chips or flakes. We asked the plants where they didn't have a thin bed that formed. They needed to look at chip or flake formation to see what kind of head loss that creates.

MR. ARCHITZEL: I think one thing the Committee has to consider is what we were presented with with the methodology that didn't do a very complex transport analysis. So some assumption is made up front to transport all this paint is fine, but are consistent with a simple transport analysis, we offer a more complex alternative in the chapter.

CHAIRMAN WALLIS: Okay.

MR. ARCHITZEL: So if you get into a debris size distribution like we could do at a volunteer plant that's brought in the back, then you could look at the transportability of these chips because it's not necessarily that the chips are there. They have to transport as well.

CHAIRMAN WALLIS: Well, transport. Yes, I know transport is an issue though, but if I have a drain in the street and there's a heavy rain and it washes a lot of sand along the street, it may wash right through the drain like a screen. But if it washes a few big leaves down, the leaves can cover between the gratings and it doesn't take many leaves to completely clog up the drain.

So if the flakes of paint come off as leaves instead of powder, it makes a big difference. I'm not talking about transport. I'm just saying that we don't really know how it comes off so how do we assess its effect on the screen.

MR. ARCHITZEL: But the point is with the simple models we had, this is what was done. So if we had more complex transport, we could address those questions. It's a triumph just to ask you to look at --

CHAIRMAN WALLIS: But you're making a

decision on what's acceptable without, it seems to me, knowing what it is you're dealing with physically.

MR. MURPHY: Well, the pressure wash data that industry did provide us showed that the coating failed as particulate.

CHAIRMAN WALLIS: So there is a good basis for it.

MR. MURPHY: There is some basis for it within the zone of influence that the coating will fail as particulate and one of the statements we make though is that it may be worthwhile to do additional testing at LOCA pressures and temperatures to see if it's going to fail truly as particulate or as chips are placed --

CHAIRMAN WALLIS: Can I ask my colleagues who've been into plants where the paint was peeling off what do they look like?

MEMBER SIEBER: Flakes.

CHAIRMAN WALLIS: They look like flakes.

MEMBER SIEBER: Yes, but those are during mild environment conditions. I think if you had a forceful jet --

CHAIRMAN WALLIS: But if they're lying if they are there?

MEMBER SIEBER: -- upon the wall you may

wash the paint off as opposed to have it chip and fall to the floor. So I think you're going to get a mixture. I really do.

MR. CARUSO: Do you have an idea of an acceptable method to this test? Is there an ANSI standard test method to perform these to make this determination?

MR. MURPHY: I'm not aware of one.

MR. CARUSO: So licensees have to develop a methodology to do the testing.

MS. LAURETTA: This is one of those areas identified up front by Mike Johnson that there is a real problem with the lack of data, lack of testing.

CHAIRMAN WALLIS: But now these flakes if there are paints which are flaking, they won't come off because of the zone of influence. They'll come off because of the sprays and the containment problem, won't they? I mean the sprays will be capable of washing them off if they are not very well attached.

MR. MURPHY: They could.

CHAIRMAN WALLIS: And that has nothing to do with the zone of influence.

MR. MURPHY: Well, if it's flaking and it's qualified than it's degraded and you have to treat it as unqualified and we've said you have to assume 100 percent of that comes off.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: And it might well come off as flakes rather than as powder.

MEMBER SIEBER: Once you get the first flake, then it's gone. Right?

MR. MURPHY: Yes.

CHAIRMAN WALLIS: They would peel off as flakes.

MEMBER SIEBER: I think one of the things that has an influence is the change in temperature. If you get a rapid change in temperature, it causes the paint to expand at a different rate than the underlying surface. Once you get a bubble, then off it comes.

CHAIRMAN WALLIS: It might come off as a sheet.

MEMBER SIEBER: It will come off as flakes. Generally, you can't support large newspaper sized sheets. I've never seen that.

CHAIRMAN WALLIS: Something like a leaf sized sheet.

MEMBER SIEBER: Yes, I think the size of a half of dollar.

CHAIRMAN WALLIS: Well, we may have said

enough about this, but I think that there might well be some tentacle uncertainties in this area perhaps.

MR. JOHNSON: Well, one thing that I think it goes without saying also, Michael Johnson speaking, is that you know even today if plants find this chipping, flaking paint, that it's remediated. There are plants today you are working on remediating that is visually degraded in their containment. So that's the other thing that we all also ought to bear in mind is that licensees shouldn't be watching the stuff chipping and falling without doing something about it.

CHAIRMAN WALLIS: That's right, but then there's the question of inspection intervals and how much is it degraded before you actually see it and all that. This is a somewhat nebulous area it seems to me.

MR. TRAIFOROS: I think also the point should be made that your choice of the inference of 10D is very conservative. It's two orders of magnitude in terms of destruction pressure because the way it was in the guidance report for coat use, you had 1,000 per psi at 1D. Now you are talking about 10 psi being the destruction pressure because that corresponds to 10D.

So it probably will be the licensees who will taking a great penalty in their considering that they can completely destroy paint at the 10 length of 10 diameter. Again as we all discussed, that some of these things hopefully will be ironed out during some experiments.

MR. CAVALLO: Dr. Wallis, excuse me. Could I offer something?

CHAIRMAN WALLIS: You have to identify yourself.

MEMBER SIEBER: Come to the mic.

MR. CAVALLO: My name is Jon Cavallo. I'm the Chairman of ASTM Committee D-33 and I would just like to offer some data concerning your questions and in response to your questions concerning the appropriateness of visual inspection of containment coatings. We've done a lot of work over the last 20 years in developing the family, if you will, of ASTM Standards which replaced the old ANSI Standards having to do with qualification of coatings and such.

There is a mother document called "ASTM D-51.44" which is a road map through this fairly complex issue. One thing that you had asked a question about the appropriateness of visual inspection as part of our condition assessment program, there's a lot of precedent for that. One of the things that's been done for many years is that ASME Section 11, Inspection of Containment Vessels, that inspection is primarily a visual inspection that looks at among other things the condition of coatings on the containment vessel.

We've used a lot of that data and our research has indicated, or our investigations I should say, has indicated that most coating failures have a visual precursor be it discoloration, cracking, checking, blistering that will indicate a degradation of the properties of the coating from the time that they were initially applied. That's been pretty well borne out in service.

So all the plants that I work with as a consultant and also other plants do a visual inspection in many cases every outage which is not a horribly time-consuming program, but we are able to very reliably determine if our qualified coatings have in fact degraded and take appropriate remediation action. It's simple as taking off the degraded coating or replacing it with properly applied coatings.

The other thing I did want to point is the terms "paint flakes" and "paint chips" has been used for years and years and really frankly we have been hard pressed to produce those paint flakes and paint chips or find them in fact in service. We have seen in service degradation of coatings that produce chips, but if you look at an accident scenario inside, for instance, the zone of influence, I participated in the water jet testing, and frankly, my colleague and I were shocked that we could not produce delaminated coating flakes or chips. We were unable to do it as hard as we tried. All the coating failures of the qualified coatings were, in fact, by erosion into very small sub-50 micron particles.

The delaminated coatings have been addressed for many years in licensing basis. If we go way back to Maine Yankee, for instance, Maine Yankee's FSAR notes that their coatings, although that's a decommissioned plant now, their structural scale was coated with an alkyd, an oil-based coating and they, in fact, said that any coating flakes that got into the post accident pool which was 200 degrees and acidic would dissolve and not be a flake with regard to transport to the sump. What we of industry have taken the position because of, as you point out, the lack of data on the failure morphology of unqualified coatings, that all coatings outside the zone of influence, unqualified coatings, will fail and be available for transport. Your point is well taken on the flake thing, but we've been able frankly to produce those flakes except theoretically.

CHAIRMAN WALLIS: But they could perhaps form. You said that coating failures have a visual precursor, but that was not under LOCA conditions in the entire containment.

MR. CAVALLO: No sir.

CHAIRMAN WALLIS: And then the flakes which are washed down by the sprays might be different from the ones that you looked at in the jet.

MR. CAVALLO: That would be outside the zone of influence, outside the destruction pressure.

CHAIRMAN WALLIS: I'm very interested in your assertion that at Maine Yankee, all the paint would dissolve because then it becomes available for chemical reactions in the pool.

MR. CAVALLO: Absolutely. That was in their licensing basis. That was how they justified not clogging their sump.

CHAIRMAN WALLIS: But it wouldn't clog with the paint, but it might clog with some product of chemical reaction.

MR. CAVALLO: This is prior to Barsevik.

CHAIRMAN WALLIS: Yes.

MR. CAVALLO: Yes.

CHAIRMAN WALLIS: Thank you. That's very helpful.

MR. CAVALLO: You're welcome.

MEMBER SIEBER: I think it makes a difference too as to what the original service is that's painted. For example, in a PWR, the crane wall is made of concrete which has a coating applied to it. If that coating comes off, so does the grains of sand or what have you in the concrete which adds to the particulate matter that's in the sump and available for transport.

MEMBER RANSOM: You know, if this discussion as well as the one about damage on insulation materials, there seems to be a lack of any mechanistic understanding of what goes on here. If you look in the aerodynamic literature, for example, you see parameters like flectural stiffness to dynamic pressure appear as governing whether or not you will get flutter or things that cause fatigue.

I don't see any of that here where there's been an attempt to utilize these mechanisms to correlate the data or put together models that would explain this kind of behavior. And even as paint business, I peeled paint off a house and you know how that happens. The jet penetrates behind it. You get a high pressure behind the layer and it pulls the layer off through creating again things like flutter in the paint. It rips it off.

But you see no mechanism in anything here, just simple things like this pressure which is used as a criterium which is not unsightful. It may be incorrectly used at times. It's not very useful.

CHAIRMAN WALLIS: Maybe the best that they have.

MEMBER SIEBER: I get the feeling that that was sort of a screening number anyway because main steam pressure is about 1,000 pounds. So anything that breaks in the RCS or the main steam system would create a jet that would qualify.

CHAIRMAN WALLIS: You want to move to the next. Are we finished with it?

MS. LAURETTA: Slide 9 we've already discussed, I think, as concern for sump blockage. For those plants that would be able to substantive no thin bed at the sump, it's recommended that the larger size is considered.

CHAIRMAN WALLIS: Does this mean that they have to consider big flakes?

MS. LAURETTA: Exactly.

CHAIRMAN WALLIS: That sounds pretty bad and big flakes really clog screens, don't they?

MR. LATELLIER: However, there is a transportability compensation.

CHAIRMAN WALLIS: So there's a quick passage to the screen through a stairwell or something. That's going to make a big difference to that licensee with flakes.

MR. LATELLIER: Depending on the geometry of the plant, that's true.

CHAIRMAN WALLIS: Yes. So when you say "realistically conservative coatings debris size assumptions" I don't know what that means. Does that mean that they can take flakes which are one millimeter across or one centimeter or meter or what? What's realistically conservative coatings debris size?

MR. LATELLIER: I don't know if this verbiage is presently in the SECY but I would propose that it's the minimum size that still is able to block the opening of the screen.

CHAIRMAN WALLIS: So it's not realistic. It's simply saying what's the worst that could happen.

MR. LATELLIER: That assumption would maximize transportability.

CHAIRMAN WALLIS: All right.

MR. LATELLIER: And also provide the opportunity for blockage.

CHAIRMAN WALLIS: So that might be more specific and would give some guidance as to what they should really do. That would be more useful perhaps to the licensee.

MS. LAURETTA: Next slide, Slide 10, as far as refinements, the only refinement operations are that debris specific values be used rather than bounding values which is acceptable and strongly recommended by the Staff. Slide 11 is where we summarize our conclusions where we find a need for alternative guidance. The Staff finds the approach acceptable for coatings and debris characteristics. Except that with regard to the zone of influence of 1D, we determined that we should either use plant specific values based on experimentation or use an equivalent 10D.

CHAIRMAN WALLIS: If we go back to what Bruce just said about this realistically, one sentence I pulled out of your section they're talking about here and I'm quoting now from the NES SER that I read, "Debris characterization should be realistically conservative based on the plant specific environment."

I felt that told me absolutely nothing. It's so vague that it doesn't really tell me anything.

MS. LAURETTA: That sentence actually go on to say "Based on the plant specific environment and susceptibilities identified by the licensee" and I guess the point there was susceptibility.

CHAIRMAN WALLIS: So you're putting it all on the licensee. There's no guidance. It says they have to start from square one and figure out what to do essentially.

MS. LAURETTA: So we hadn't come up with specific guidance at that point. The point that Bruce just made is an alternative that we're working with to try and --

CHAIRMAN WALLIS: So there is still the likelihood that different plants will consider different things to be realistic or conservative.

MS. LAURETTA: If they can justify based on testing something different than what we proposed, then that would have to be considered.

CHAIRMAN WALLIS: The Staff is going to have to exercise a lot of wisdom in evaluating these submittals. So how do we assure ourselves the Staff has that wisdom? How do you? How does the management assure itself that its people have the wisdom to assess all these extraordinary elaborate scenarios?

MR. JOHNSON: I'm sorry. Do you?

CHAIRMAN WALLIS: I would be bordered if I were a manager and I had people who had to assess all these extraordinary elaborate scenarios and figure out if they are believable or not.

MR. JOHNSON: Philosophically, going into this what we wanted was not 69 different evaluations that we had to do, but we wanted a limited number of specific evaluations that we had to do that could be used that used these guidelines that have been prepared. We will have to deal with what we get and the Staff will be ready based on the guidance that we will generate in here and the additional guidance that has gone into supporting this guidance to review it.

But you're right. We'll be challenged. We'll be challenged from a work load perspective alone even if we get a big population of different evaluations that are done using the evaluation methodologies.

MR. TRAIFOROS: I was wondering whether it would be feasible for the utilities to perform an analysis based on the guidance report and your additional guidance that you are offering through the SER and any other work that might have been done by the time that they get involved into that. And this report, this analysis then, might be a baseline if you will for something not all the utilities will be using.

CHAIRMAN WALLIS: Are you thinking of sort of pilot plan where you apply the methodology, you take a few different types and see what happens before you ask everybody to do it?

MR. TRAIFOROS: Yes.

MR. JOHNSON: Well, what we want to do is have again with the SECY the additional things that were provided by the Staff in the original guidance that is provided. We believe that is going to constitute an acceptable method. Now there are certain areas that we point to again where even the guidance here can be informed by additional things. Licensees can do additional testing. The results that come back from the things that are ongoing that can be and should be factored in as we go forward. So we expect that that's how this will unfold.

CHAIRMAN WALLIS: Let's follow this up a bit. I mean here we have an ANS ANSI Standard which appears to have some very strange features when looked at by us. Here there was presumably the product of wise people spending a lot of time. And we have some

LANAL (PH) reports where wise people spend a lot of time doing research trying to figure out what was going on and even after all that was done, there seemed to be still quite a few questions around.

Now we're going to have individual plants who are probably not as wise as the people I've just spoken of, each trying to do their own testing and evaluation of these phenomena and you're going to figure out if they're good enough. It seems to me you're putting an awful lot on the plants.

MR. JOHNSON: And in fact, we've had numerous conversations among the Staff. I mean our desire is that we limit areas where we ask the licensees to go off and do their own testing if you will. And in fact, in some cases where folks would look and say, "What's in the guideline or what's in the SECY is conservative." It's because we've chosen something to be conservative to provide an opportunity for licensees not to have to go do individual testing because we recognize the challenge that it places on our licensees and we recognize the challenge that it places on the Staff to review it.

That's been our philosophy now. Now again as you've pointed out throughout the conversation even thus this far, there are areas where we don't know where licensees, there is additional knowledge that can be had that would better inform us. We think that's okay if their knowledge comes. We think we know enough and again we'll talk about that in the next conversation that we have and throughout the rest of the day, but in the end what we want is a methodology that are in these areas that we don't know.

We want to either bound them or as we get information that shows, to point out just the vulnerabilities, we want to licensees to have considered the fact that the information could come and build that into the fix that they plan because we've also heard licensees say they only want to make this fix one time. I'm sure we'll have this conversation again as we get more into it.

CHAIRMAN WALLIS: Well, let's go back to that. That's sort of about unqualified coatings in the rest of the containment. You have to assume they all come off. Then Bruce was saying that the worst thing is that they come off as flakes which are just the right size to block the screen.

It seems to me that if you have flakes just the right size to block the screen, you probably have a layer which is a millimeter thick or less which is blocking the whole screen because the flakes just lie down like sheets of paper on the screen and cover it up. Then no one with unqualified coatings can ever pass if you have to make that kind of assumption unless they can show that they never get to the screen in the first place.

It's the transport which is going to pay. Transport itself is conservative assumptions of 15 percent and so on. So some of it is going to get there and it seems to me that those plants are never going to pass because of the way you've set it up just on the basis of unqualified coatings, could be flakes and some of them are going to get to the screen and so few of them it takes to cover the screen. Those plants don't have to do anything else. They just have to change those coatings.

MR. JOHNSON: One insight that we could offer is that basically the Staff has modified the existing proposal present in the guidance report. The industry proposed 100 percent failure of unqualified coatings. So in a sense, they've assumed the burden of the testing that's required. They've assumed that conservatism. If they would like to reduce it, that's on the table.

CHAIRMAN WALLIS: But you said that the

plants would not meet the criteria today and you're going to let them off by saying they're going to test. If they use this conservative business of the unqualified coatings, they wouldn't be able to show that they meet the 10 CFR 50.46 criteria. And you're going to say, "Okay, we're going to wait until you get results of tests before we ask you to do anything."

MR. JOHNSON: No, I think the determination of vulnerability and the need for testing are entirely separate issues.

CHAIRMAN WALLIS: That was also a puzzle I had with this whole issue. If it's the compliance issue, then how long can you wait for results of tests before you want to know are they not in compliance? That's maybe another question later on for the Staff, but we should perhaps put it off for the moment. It seems to me a fundamental question behind all of this. Okay.

MS. LAURETTA: One of the concerns we had in the treatment of unqualified coatings is some of the experience we've seen just recently where you have unqualified coatings without any damage mechanism winding up on the floor. I guess I'm talking about Okony (PH). With the other plants out there who could be approaching something of the same situation or condition with their coatings wanted to make sure that these plants would be bounded.

CHAIRMAN WALLIS: Well, you don't have any numbers. How much coating is there? No one has ever put this in perspective. When you have all these regulations about coatings, is it or is it not a potential problem?

MR. MURPHY: It depends on how much unqualified coatings the plant has and it encompasses a spectrum of values.

CHAIRMAN WALLIS: What do the customers think? Could you make yourself a calculation? Does it turn out that you have a hundred times as much coating as you need to clog the screen if it's flakes or you have a thousandth as much. What's the scale of things? If you have a thousand times as much coating in there which is unqualified then you need to clog a screen if it's flakes, then you're never going to analyze it away it seems to me. Just giving us some numbers to put it into perspective would help a great deal. I don't know whether we're asking questions about something that's relative or not.

MR. MURPHY: I don't have values to put out.

CHAIRMAN WALLIS: But it seems to me

that's the first thing you have to do is to make an order of magnitude. I used to say I liked putting all that effort into something that matters. It doesn't.

MS. LAURETTA: Transportability is such a big issue also.

CHAIRMAN WALLIS: But it isn't because you just assume 15 percent or 60 or something. It doesn't affect whether it's a thousand times as much as you need. That's tweaking it, but you can make some orders of magnitude.

PARTICIPANT: Does anyone in the industry have any idea what order of magnitude the coatings?

MR. MURPHY: I'm sure they do.

CHAIRMAN WALLIS: But do you?

MR. MURPHY: A couple people.

PARTICIPANT: On the order of 100,000 square feet.

CHAIRMAN WALLIS: Ten thousand square feet. How many square feet are on the screen?

PARTICIPANT: Total surface area multiplied by ten.

MR. MURPHY: Ten thousand square feet.

CHAIRMAN WALLIS: What is the screen area?

PARTICIPANT: Current screen areas vary from as little as about a dozen square feet up to several hundred square feet.

CHAIRMAN WALLIS: Say that again.

MR. MURPHY: It varies from plant to plant. The minimum may be as little as 12 square feet but sometimes it's several thousand.

CHAIRMAN WALLIS: Okay. That helps because 10,000 square feet of unclogged by coating. We only need to cover a 12 square foot screen.

MR. JOHNSON: You're probably speaking about 100,000 square feet.

CHAIRMAN WALLIS: Well, okay. So you have something between 100 and 1,000 times as much as we need just to lay it down optimally and effectively. So just on that basis, we would say, "Well, we can't make that kind of assumption."

MR. JOHNSON: Of we do, then you'd say you need to fix your coatings. You need to qualify your coatings.

CHAIRMAN WALLIS: Okay. That's right. So you can make that calculation right away.

MR. JOHNSON: Right.

MS. LAURETTA: Or modify your screen.

CHAIRMAN WALLIS: Right. But you can't analyze the problem away. You have to do something. And if you made it go from 12 to 100, that might not help you. Then you want to go to 1,000, but that might be over conservative. So you try to analyze these things, but at least you could start with some order of magnitude.

That's been helpful. Those numbers have been helpful. Maybe in all of these matters, it would help if you put up some numbers and say, "These are the kind of numbers that result from this kind of analysis." Therefore we have to worry about whether it's conservative or not and we have to worry about how accurate it is or not and so on. That would help us a great deal I think rather than just saying this is regulation.

MEMBER FORD: Probably what's going to happen is the uncertainties of the conservatism are we don't know how conservative it is. It's certainly not realistic and certainly it's --

CHAIRMAN WALLIS: And it's really helpful. We had a presentation once from Lona (PH). She told us that one cubic foot of material could clog a screen. That put things in perspective. I said, "Gee whiz. One cubic foot. It's just about one pipe one foot long with this stuff and there's a lot of more of that in that plant than that." So that help put it in perspective. Maybe when you get to the full committee you can put some of these subareas in perspective that way by giving us some orders of magnitude of the extremes or something.

MS. LAURETTA: We'll consider that.

CHAIRMAN WALLIS: Thank you.

MS. LAURETTA: Slide 12. Once again, the Staff findings were that the default coating thickness was no substantiated and that needed to be justified on a plant specific basis and also that licensees should periodically assess the condition of their qualified coatings inside containment.

The last slide, 13, also that if there is no thin bed formation, the licensees consider the larger size coating debris.

CHAIRMAN WALLIS: Are you going to give instructions to inspectors if they walk around the plant and they see, maybe they are already, signs of degraded coatings that they have do something. That must be already a part of their instructions.

MEMBER SIEBER: Yes, the inspections that licensees do are specific to coatings and the inspectors are trained to do that. They end up as nonconformances which there is a so-called qualified repair for a nonconformance. It's pretty systematized.

One of the things that I was thinking about when you talked about the 3 mil coating when you qualify a coating you qualify the materials and you also qualify the method of application. It's been a long time since I was involved in construction of the plant.

On the other hand, it seemed to me there were minimum coating thicknesses but no maximum. You could have a really thick coating there that would still be qualified. So when you assume a specific number, that means that would be the minimum number for a particular application of what's qualified coating from a pound standpoint.

MR. MURPHY: The data I've seen there's both. There's a maximum value on the coating thickness as well for qualification. You had to apply by the manufacturer's specifications which had a minimum and a maximum especially for things like that.

MEMBER SIEBER: I've seen them measure the minimum to make sure they made the minimum. I have not seen them measure for the maximum.

MR. MURPHY: At the plant that I was at, we had specifically had a maximum.

MEMBER SIEBER: A maximum. Okay.

MR. JOHNSON: I just wanted to make one

last point on coatings. You know I was walking around talking to the Staff who are familiar with what Davis Bessie did in looking at their sump with respect to coatings and Davis Bessie had a major activity to look and to fix their coatings in addition to the other things they did in addressing their issues that they had with their sump. We really do anticipate that there will be plants that need to do things. They need to fix their coatings. They need to have qualified coatings.

And that other point Louise London reminds me of is that it really is highly plant specific in terms of what qualified and unqualified coatings they have. So every plant is going to look at the coatings and their coatings maintenance programs to get after that issue because it can be an important part of the problem.

CHAIRMAN WALLIS: Are we ready to move onto the next topic? Thank you for all your efforts to give us good answers to our questions. Now Ralph, I don't know how long we'll take with you.

MR. ARCHITZEL: I'm going to have to invoke the ten minute rule.

CHAIRMAN WALLIS: I think, Ralph, we'll try to get out of here in a reasonable time for lunch and if you take too long or if we make you take too long, we'll just have to break during your presentation.

MR. ARCHITZEL: I don't know that I will take too long actually. It might be the questions sometime.

CHAIRMAN WALLIS: You might just resign.

MEMBER SIEBER: You'll be done by tomorrow.

MR. ARCHITZEL: My slides won't take too long. Let me put it that way.

CHAIRMAN WALLIS: Well, maybe we'll get through it in ten minutes.

MR. ARCHITZEL: My name is Ralph Architzel. I'm with the Plant Systems Branch. I'm going to be discussing the zone of influence portion of the guidance report in our Safety Evaluation.

MEMBER SIEBER: Maybe you could move those papers so that it's not in the way.

MR. ARCHITZEL: I would like to quickly go through a summary and I will ask if you could actually hold on the summary because I have that repeated at the end. So just to go over the summary first, so you're thinking about what the conclusions are and then hold those overall questions on this part until later. That's basically for the summary we consider generally any zone of influence approaches acceptable. We consider the refinements that are offered in the guidance report and the simplification steps that are offered there are also acceptable.

We've provided additional verification in the SER for how to use, and these are details for how to use the MALINDA (PH) ANSI Standard, but we do have that especially in Appendix I of that volume. And additionally we've determined that destruction pressure which are based on air jet testing alone should be reduced by 40 percent to account for two phase effects. That's my summary.

Now again it's the overview with the plant. Next slide. Now you can ask questions on the next slide. What I plan to do in the following slides is discuss and define the approach for estimating the zone of influence. The next step is to discuss the determination of volumes and conversion of these volumes to practical shapes. Well, actually it's not realistic, but what potentially might exist for shapes in a plant.

CHAIRMAN WALLIS: Are you going to show us some pictures, not just words, and some numbers?

MR. ARCHITZEL: I do have some on backups.

CHAIRMAN WALLIS: Okay.

MR. ARCHITZEL: And I have numbers as well.

CHAIRMAN WALLIS: Because when you're talking about volumes and shapes and so on, it would help to have pictures.

MR. ARCHITZEL: I have slides on back-up that have the ANSI pictures and graphs and I have pictures of destruction of the OPG test and things like that.

CHAIRMAN WALLIS: Thank you.

MR. ARCHITZEL: Let me get off the overview for a second. I'll be discussing the impingement pressures and the zone of influence. But the industries propose -- One thing to keep in mind. I do have a specific chart on here, a table, that when we talk about how complex this ANSI Standard, what all the licensees have to do, in the end with the approaches taken there is a simplification and it's provided for the materials that are well characterized and while we've adjusted it, it's not like every licensees has to go out there and calculate these. The idea behind that was that it would be available for analysis and wouldn't need to be redone. So we do have a chart that shows that.

CHAIRMAN WALLIS: Did they just use 12p or something? Whatever it is?

MR. ARCHITZEL: 12 Pressure. Destruction pressures or impingement pressures that are modeled off of what was --

PARTICIPANT: Are you going to define what these pressures are or try to clear up this issue?

MR. ARCHITZEL: I will in a little bit. I guess what I would want to say in there is that there's a chart. When we talk about complex, it's like Slide 10.

PARTICIPANT: But just a short time because I have to go back.

CHAIRMAN WALLIS: You might not get away from it.

MR. ARCHITZEL: Well, I'm not going to stay on it. I just wanted to say that, but most of the material is tabulated here and it does have diameters where there is destruction pressures.

PARTICIPANT: Can we get back to one?

(Laughter.)

CHAIRMAN WALLIS: It has diameters. So if it has diameters specified, you don't have to go then and calculate using the standard or anything else.

You've already just used the diameter.

MR. ARCHITZEL: Well, it was proposed by industry and we modified it and we have diameter there.

CHAIRMAN WALLIS: The diameter is based on the ratio -- Well, you're on this. When you say "10D," you mean the radius of ZOI. It's ten times the diameter of the pie.

MR. ARCHITZEL: The 10D is ten diameters of the pie.

CHAIRMAN WALLIS: The radius is ten times the diameter. Is that what you're saying?

MR. ARCHITZEL: Yes.

CHAIRMAN WALLIS: Yes, because it didn't seem to be defined anywhere as to what you meant by 10D or 12D. It's the radius --

MR. ARCHITZEL: Diameter.

CHAIRMAN WALLIS: Okay. Of the pipe.

MR. ARCHITZEL: Then I will discuss the refinements. I guess the first step, go to slide four please. Guidance report 342 recommends a spherical boundary for the zone of influence centered at the break. In addition to this recommendation, and I'm discussing the baseline, our presentations all follow the logic if we're discussing the topic.

CHAIRMAN WALLIS: Could I ask you about that?

MR. ARCHITZEL: Well, I'm just saying we're discussing the --

CHAIRMAN WALLIS: Could I ask you about spherical boundary?

MR. ARCHITZEL: Oh, can I just make one point first though? And the point is that just that we are discussing refinements together with --

CHAIRMAN WALLIS: Let me ask about a spherical boundary.

MR. ARCHITZEL: Yes sir.

CHAIRMAN WALLIS: Here I have a break and I have a jet coming out and a long, long way over there, I have some Cal-Sil. I don't have it anywhere else. This jet, we know that these jets can go a long way, but you're going to say, "Take all that and put it in a sphere." That sphere may luckily not contain something which happens to be somewhere where the jet could reach.

MR. ARCHITZEL: That's right.

CHAIRMAN WALLIS: So you're doing something that --

MR. ARCHITZEL: Well, the point is that we then translate that sphere through the plant to find

-- Now that particular break may not intersect at that point.

CHAIRMAN WALLIS: But to be sort of absurd here, if I were a fireman with a hose and there was a fire, it wouldn't make sense for me to assume that my jet is spherical because I can only put out the fire with a spherical volume.

MR. ARCHITZEL: These are not really going to be spherical jets.

CHAIRMAN WALLIS: No. That sort of assumes that the debris sources are kind of uniform. That's okay in that case. But if the debris sources are very localized --

MR. ARCHITZEL: Well, not only uniform, but that by moving it around to find the worst location, you will cover that situation with another break somewhere else. But there could be --

CHAIRMAN WALLIS: Maybe.

MR. ARCHITZEL: You likely will, but not 100 percent assurance.

CHAIRMAN WALLIS: You see my point is that the worst break may be here in terms of momentum and all that, but the Cal-Sil may be a long way away, but it could still be reached by that jet if you didn't make it into a sphere.

MR. ARCHITZEL: So that is a possible methodology and that came up, I think, on a AP-1000 and in that case, we decided you had to 30 away for any type of low destruction pressure type of insulation. I guess we don't have that caveat here.

CHAIRMAN WALLIS: But you've somehow rationalized that it doesn't matter.

MR. ARCHITZEL: Well, we didn't. We didn't address it in this SECY. If that situation is only long distance and you take the ZOI approach, I guess it's accurate that we didn't address that particular situation if it wasn't impacted by other --

CHAIRMAN WALLIS: Because what I read in the guidance document, the LANAL tentacle basis document says the jets were able to destroy some certain stuff 100 L/Ds away. It's possible, but none of your spherical boundaries ever get as big as that, do they?

MR. ARCHITZEL: Well, we allow as a alternative. We do allow and industries propose that this direct impingement model. I'm jumping ahead there.

CHAIRMAN WALLIS: They actually do that?

MR. ARCHITZEL: That's proposed, but that's not mandated. That's an allowable alternative.

CHAIRMAN WALLIS: But I just wonder what rationale you use for saying it's not acceptable to make it a sphere except that it's convenient. Is there some rationale?

MR. ARCHITZEL: Well -- Go ahead.

PARTICIPANT: Let me jump in. Let me just try for a second and Bruce, you can help me out. I guess the point is I'll go back to these damage, trying the ANSI model whether it's right or it's more like the photos and the shock waves that are in the papers you've presented and Dr. Ransom's presented. Basically, we're not dealing with a zone of influence.

We're dealing with a zone of no influence because if you have that shape you've had no damage. So it's a little bit conceptually out of line to talk about that type of a shape. There is no damage in that zone if you reach those boundaries. But then when you do reach a boundary.

So in practice when you reach a boundary and trying to maximize, you're going to have reflections and those reflections and those pipe widths take the angles at different locations and your zone is actually in the volumetric sense with the energy lost in the reflections, etc. are going to be much smaller than the equivalent volume zones. So we made this conservative assumption retaining that volume. You capture a lot of debris and a lot of targets within that zone and the other thing we have also is a really in area in fact.

CHAIRMAN WALLIS: No, no. Are you familiar with the Barsevik event?

PARTICIPANT: Yes.

CHAIRMAN WALLIS: Did the spherical zone of influence explain what happened there?

PARTICIPANT: I'm not familiar with the details of geometry. I could state and I guess -- Can you throw up the slide on the OPG test?

CHAIRMAN WALLIS: This would make it more convincing if you could say, "Here's the Barsevik event and if we use the spherical zone of influence, we can predicate what happened." But my impression is that the damage in Barsevik was a lot further away than was expected.

PARTICIPANT: Well, I guess I'm going to show you something that does --

CHAIRMAN WALLIS: Does the jet have the direction? Is that true, do you remember, Jack, about Barsevik?

MEMBER SIEBER: I thought it was further away and I thought there was more than they expected.

CHAIRMAN WALLIS: It was definitely further away than they expected.

PARTICIPANT: I don't know the details of Barsevik.

MR. TRAIFOROS: There is no doubt. I agree with the observations of Dr. Wallis.

MR. ELLIOTT: This is Rob Elliott on the subject of Barsevik.

MR. TRAIFOROS: Excuse me. I'm sorry. Okay. Go ahead.

CHAIRMAN WALLIS: Do you want to talk about Barsevik and then we'll go on?

MR. ELLIOTT: There's a lot of questions about Barsevik about what created the damage to the insulation and whether or not they had degraded insulation that was washed down by containment sprays or whether or not the insulation was actually damaged by the reflection of the jet from the safety relief valve. What they had was a stuck-open safety valve where they had a jet deflector plate on it. And clearly that damage to the insulation in the vicinity of the stuck-open valve, but I don't recall that the surprise was not how much was destroyed.

What was surprising was how much transported down to the screens and how little it took to clog the screens. They were surprised by the fact that their screens clogged inside of an hour and they were expecting them to last at least ten hours before they had to backflash. But I don't know that we can draw conclusions about the zone of influence from Barsevik because I don't think we have enough information about what created the damage.

MR. ARCHITZEL: One point on the spherical, next test down please. Can you make that big?

CHAIRMAN WALLIS: So these are directed jets. These are not spheres.

MR. ARCHITZEL: Well, okay. The point right now, this is Cal-Sil. The next one is going to -- test, but the point would be if you look at where that nozzle is, I think it's a three inch nozzle, and take any kind of concept about it, first off, notice that the damage is on the backside not the front side.

MEMBER SIEBER: Right.

MR. ARCHITZEL: So when we talk about them here, it's really "Did you get the insulation right? Did it peel off? What's really the damage mechanism?" Clearly, it's not a pressure. It's a little bit of tear and things like that, but there's a shock wave too, I'm sure. But the point is -

PARTICIPANT: Here the fuel goes to the side.

MR. ARCHITZEL: And to look how much broader the damage is out to the edges than what would be projected by the type of models that we have where it's a very enclosed type of phenomena, you're dealing with destruction in areas where the model would say there is no pressure. So that translation to a sphere is to try and take into account what really happens when you hit a target.

CHAIRMAN WALLIS: Well, I think what happens is that the jet penetrates the stuff and it makes a pressure inside it.

MR. ARCHITZEL: Right.

CHAIRMAN WALLIS: And then when it comes out on the backside where the pressure is low, it blows it off.

MR. ARCHITZEL: Exactly. But out beyond the range of the zone of influence that we're dealing with.

CHAIRMAN WALLIS: So it would help if there was some mechanistic understanding of what happens.

MR. ARCHITZEL: That will couple down too. That one test I had. This is first off --

CHAIRMAN WALLIS: No, not that one yet.

MR. ARCHITZEL: This is just to show you one of the key points we're raising as to why --

CHAIRMAN WALLIS: It's the backside that gets damaged.

MR. ARCHITZEL: The backside that gets damaged because of where the seam was. Like on a 45 degree angle, it could easily get inside there and then also clearly wider and more damage than you would expect. But the only problem with this test with fiberglass, it's close enough that if it was air it also would have been damaged.

CHAIRMAN WALLIS: Well, is it more damage than you would calculate? That's all we really care about.

MR. ARCHITZEL: I'm not sure about that in air. This is close enough where there would be damage in either case.

MR. CARUSO: Spherical -- I don't think is a problem but the problem is the range, how far away. How big is the sphere?

CHAIRMAN WALLIS: What?

MR. CARUSO: Well, presumably, you're setting the sphere radius based on how far it takes for the jet to dissipate to the point that it would not create this damage. Right?

MR. ARCHITZEL: Right, and that's why I'd like to show you another one. Just if we could go onto the test done in Europe. This is not science. It's really just observation. One of these tests, it's complex geometry to the two phase type of --

CHAIRMAN WALLIS: Excuse me. Observation can be very helpful to science and without observation, science is pretty helpless. We'd like to see more observation.

MR. ARCHITZEL: well there haven't been too many two phase tests.

CHAIRMAN WALLIS: That's right.

MR. ARCHITZEL: So this is one that was done over in Europe and I guess the point I'm trying to raise when you try and look at some of those targets, there's a mix of targets and there's some RMI and there's some fiberglass covered. You can see and there's like vessel sheeting on the bottom that's a little bit off. You can see how offset it is from the discharge pipe, how the right side is damaged and the left side is not.

I don't know if I'm making a point or not, but I'm trying to just illustrate that you get the seven -- here, the 12 -- here, the type of radii where you start get damage or you see the damage from these tests that have been precise, but you can see it, an area way or sphere way.

CHAIRMAN WALLIS: This is all very helpful though.

MEMBER RANSOM: L/D may be may be 14 or so.

MR. ARCHITZEL: This was the initial approach. If we go back to some of these approaches, you go to the approaches that were done historically where we've now gone away from these approaches if you up a slide or down a slide.

CHAIRMAN WALLIS: But the question is, Ralph, you're showing us good stuff because that data. I don't want to see that. I don't want to see that ever again, that part. The date you're showing us is very good because you're showing what really happens when you have steam impinging on the pipe with insulation on it. That's very good.

It should help us to resolve the question which we asked is "Is it okay to replace a directional jet with a sphere"? You compare that with the evidence. You compare your assumption that you can replace it with some evidence and if it works out, that's okay. The evidence is the key to the whole thing. All right. So the evidence that maybe this is being done.

MR. ARCHITZEL: But we can rule out the situation you're talking about which is a very long distance damage because we're allowing this other approach to be taken for practical reasons.

CHAIRMAN WALLIS: But the sphere makes it a shorter distance.

MR. ARCHITZEL: But the core volume and you would capture if you rotate it, I can't put your issue to rest because that situation could exist and then you would have to rotate that where that pipe --

CHAIRMAN WALLIS: But you can benchmark it. You can say we have Barsevik. We have the UNM/New Mexico test, all these things. Suppose we use jets. Suppose we use a sphere. What would we have predicted and what happened? And you can use a rational choice rather than all this judgment stuff where we believe something.

MR. ARCHITZEL: It's really a simplification for a convenience of calculations. I'll let Bruce talk about that.

CHAIRMAN WALLIS: That's not good enough.

DR. FOX: If you could put up the Battelle and talk about the slide.

MEMBER FORD: Would your spherical zone of influence explain why you get damage in the right-hand rather than the left? On the first question, would it have explained or predicted the damage on the right-hand side?

MR. ARCHITZEL: I can't answer that off the top of my head because I don't know the particular insulation of this product. I was just, maybe I should have thrown this one out with all the different insulations.

MEMBER FORD: As Graham said, it's fascinating because it's real. It's a real observation.

MR. ARCHITZEL: But it's one of the more complex geometries. They're normally not tested this way. They are normally tested dead on and things like that.

MEMBER RANSOM: Another problem with the damage modeling is in the test they believe or I heard the statement that it's the blast wave that actually caused most of the damage. You know it impinged on the structure which propagates out radially of course and is also a driven blast wave by the escaping gas which is coming out of the jet and the second mechanism of damage, of course, is the steady state jet which will cause drag on the structure and the dynamic pressure will create some damage.

The interesting thing in your morale is that this first mechanism is totally ignored. It was mentioned in the Los Alamos report, but then thrown out well at expense, weakens radially so that it was ignored. So I see a real contradiction between the two.

MR. ARCHITZEL: The other thing I have, the dilemma, I saw your paper and I don't claim that I can understand it real well, but I did also look at the work that was done on the BWR URG and they said for slow opening times, there isn't really going to be this shock wave and so that was one, I know, maybe perhaps you see the pictures that you had that you could clearly see those shock waves, but it's not a big volume with those shock waves. The type of zones we're talking about I think perhaps you are beyond the shock effect.

MEMBER RANSOM: Well, slow is relative you understand because even if it opens over a few milliseconds, still the pressure waves that are created they all travel faster and they coalesce into a shock. You do still get a spherical blast wave, let's say, out in front of that.

MR. ARCHITZEL: I don't think we're ruling out a blast wave.

MEMBER RANSOM: Pardon?

MR. ARCHITZEL: I think what we're saying is we can't quantify. What we can measure is the pressure on those tests we did.

MEMBER RANSOM: Right.

MR. ARCHITZEL: We got measurements of the static where the pressure right at the pipe and we moved it down. In the air jets that's how it was done.

MEMBER RANSOM: Is that with a stagnation probe or with a static probe?

MR. ARCHITZEL: My understanding, it was stagnation probe.

MEMBER RANSOM: Which would measure the pressure downstream of a normal shock. You do not measure the stagnation pressure in a case like that.

MR. LATELLIER: That's correct and the intent is that is the environment that the target would see at that location.

MEMBER RANSOM: Well, that's rather interesting too because even for a 22,150 psi jet, the stagnation pressure downstream of a normal shock is about 250 psi. And that's what's causing all of this destruction. It does not take all that much pressure.

MR. ARCHITZEL: Yes. Let's let Dr. Traiforos.

MR. TRAIFOROS: I would like to go back to the point that Dr. Wallis made regarding the validity of using the spherical zone of influence to calculate damage at material that based on experimental data there is a destruction pressure if you will. The bottomline, my understanding, is and you do have in your view graphs the figure that I will refer to. It is page 7 of your presentation.

MR. ARCHITZEL: Could you leave both open? I have it in front of me. We're not going to characterize this as being physically correct. We've actually made statements in our SE about this not being specifically correct.

MR. TRAIFOROS: Yes. Actually what I would just like to point out is the third line from the top is the isobar for 10 psi G. This extends to approximately 50 pipe diameters. That is L/D. At L/D equals 50, you can get a pressure of 10 psi.

MR. ARCHITZEL: If the jet has been allowed to expand freely and this is real.

MR. TRAIFOROS: Absolutely. Now --

MR. LATELLIER: And also as modeled by the

ANSI jet standard.

MR. TRAIFOROS: Absolutely.

MR. LATELLIER: And there are some discussions about that.

MR. TRAIFOROS: But then you use though the volume as calculated in order to calculate to equilibrium volume of your spherical model. What the equilibrium calculation for a sphere that takes this volume is equivalent to this volume over this strange figure there that we see, strange set, is approximately 10 diameters.

MR. LATELLIER: We have it on page 10 so we can see what it is.

MR. TRAIFOROS: Approximately. So there was an period between the 10 diameters of the sphere and 50 diameters of the direction that we are not considering this.

MR. ARCHITZEL: Well, if I could back to that first chart. Then the point I would make is if we go back to the plume. We'll call it the zone of influence and I'll call it the zone of no influence.

MR. TRAIFOROS: Okay, that's fine.

MR. ARCHITZEL: There is a region in space, you're right, between that diameter of 12.

MR. TRAIFOROS: 10D, I can see that.

MR. ARCHITZEL: Ten or twelve, yes.

MR. TRAIFOROS: To 50 D.

MR. ARCHITZEL: This region from here, this region, right.

CHAIRMAN WALLIS: Don't touch the screen.

MEMBER SIEBER: You need to talk to the microphone.

MR. ARCHITZEL: Okay. That would not be covered in that instance, in many instances, as you rotate that though the plant. The only comeback I'll have for the whole concept is that I look at it as a little bit more as instead of a volume, an area type of a situation. If you're going to hit a target, first off if you hit that target at that limit, there's very little material involved. So you have to hit targets early on and with the dissipation if it's not, how much really material can you get within that plume? Even if you distribute multiple times, how much area is available? Or if you want to take the volume, it's going to be less --

CHAIRMAN WALLIS: Well, this is steady jet. I mean everything is so rigid that the jet is always steady. Because if it has a 50 pipe diameter range and it's moving around because of the --

MR. ARCHITZEL: Well, the reason we do the standards is because it's moving around.

CHAIRMAN WALLIS: If it's hitting something or there's no pipe restraint, it sweeps out a sphere of radius 50.

MR. ARCHITZEL: Right.

CHAIRMAN WALLIS: Inside a sphere of radius 10.

MR. ARCHITZEL: But they only last for a very short time too and the initial shock is the one that really --

MEMBER RANSOM: Can I ask you a few questions about this? Is this for a 2250 psi system pressure?

MR. ARCHITZEL: Roughly.

MEMBER RANSOM: Okay. So it's the initial --

MR. ARCHITZEL: The parameters are liste in the GR. I think it was cold leg type temperatures and things like that.

MEMBER RANSOM: What are those isobars?

MR. ARCHITZEL: I'm sorry. The isobars?

MEMBER RANSOM: What are the definitions of the isobars?

MR. ARCHITZEL: I believe it's stagnation, but I can let Bruce talk on this.

MR. LATELLIER: These are the pressures computed by the ANSI jet model.

MEMBER RANSOM: And what is that?

MR. LATELLIER: After much deliberation and some confusion about how to implement the standard, I conclude that they are the impingement pressures that would be observed on a large structural object.

MEMBER RANSOM: Downstream of a normal shock you're saying that? This is a supersonic jet you're talking about.

MR. LATELLIER: These are --

MEMBER RANSOM: And I'll give you a choice of pressures. They could be static pressures. They could be isentropic stagnation pressures. They could be stagnation pressure downstream of a normal shock. They could be the static pressures downstream of a normal shock. They could the dynamic pressure.

MR. LATELLIER: These are not the isentropic stagnation pressures.

MEMBER RANSOM: Okay.

MR. LATELLIER: The assumptions that the ANSI jet model are built on are based on the conservation of momentum transfer from the orifice and so at some distance down range approximately at 7.5

L/D on this figure there is a so-called asentotic plain at which that thrust force is conserved. That's where we need to start when we assess the acceptability of this model. We need a common understanding of what the jet model can and cannot do.

MEMBER RANSOM: I don't agree with that because you know the thrust coefficient of a jet is defined immediately at the discharge from the jet for any supersonic flow and what happens beyond that depends on what the atmosphere and pressure is that it's expanding to. So I have some real grief with this model, but I also don't know what the parameter even is.

CHAIRMAN WALLIS: I think that the closest I could work it out was that it's P + rho v^2 because it's conserving momentum. An integral of this mysterious PT, this P + rho v^2.

MEMBER RANSOM: So it's kind of a --

CHAIRMAN WALLIS: And if you actually use that you can get more than the stagnation pressure.

MR. ARCHITZEL: I think the reason to look at this and why they did it that way is the application is standard. To my understanding, it's been used in licensing. It is putting impact on structures that are used for --

CHAIRMAN WALLIS: But the point I think of my observation --

MEMBER RANSOM: Well, P + rho v^2 is not a parameter that you could ever measure or anything else and the common parameter for use in aerodynamic drag lift and then forces on bodies is 1/2 rho v^2 which is called the dynamic pressure. That one, I think, would be an appropriate pressure to be looking at as far as damage walling is concerned.

MR. ARCHITZEL: But that is also the isentropic stagnation pressure.

MEMBER RANSOM: No, it's not. Only an incompressible flow would that P (static) + 1/2 rho v^2 is the isentropic stagnation pressure in an incompressible flow, not in a compressible jet.

CHAIRMAN WALLIS: We could spend forever on this bicker, but I think that the assumption is that around 10 or 11D in this figure the static pressure is all atmospheric.

MR. ARCHITZEL: Right.

CHAIRMAN WALLIS: The rest of it is all just philosophy.

MR. ARCHITZEL: Yes.

CHAIRMAN WALLIS: Whereas Vic Ransom has some very nice pictures of when in a real jet you get shock time and there's some stuff continuing out to 50 L/D. So whatever this is, it's certainly not a good description of reality.

MR. ARCHITZEL: The reason for displaying it in this presentation and the SE is to demonstrate how our application of the ANSI model identified some conservatency that licensees should apply in your field zone at 10 and less.

CHAIRMAN WALLIS: The criterion that I'm trying to use, I'm trying to base all my judgments on some sort of idea in my head of the criteria for judgment. The criteria I have for judgment is that physical models should have some relationship to reality and as much as possible they should relate it to some experiment and you have to be very careful at basing regulation on some sort of a fantasy in the head of the regulator about what happens which then becomes law and there is no real wee physical basis for it. Let's take this thing here. It looks like something conjured up a committee sitting in a room without any reference to what really happens.

MR. ARCHITZEL: I think I would like to address that by saying that we didn't mandate that the industry came in using this standard as the model for the ZOI. Certainly for BWRs, they used the CFD model which is more complex in modeling the zone.

We evaluated the CFD model and the BWR. That was compared to this and we did that in Appendix I looking at those type of numbers and noted that generally this created conservative volumes relative to the CFD that was used for the BWR. There are some boards to that effect in the Appendix and we would have certainly accepted the industry coming in with a CFD that did a better job modeling this zone of influence.

In the end when we translated, does it make much difference? I'm not really sure. It certainly doesn't address the 40 or 30 L/D type of question because we translated that into a different volume which if the reflections really happen in a current space, yes it's conservative. But we can't say it's definitely conservative for all cases like when we're talking about with the long distance situation.

CHAIRMAN WALLIS: Okay. If I understand what you're saying, that would be very helpful if instead of presenting what looks like the fantasy, you had said, "This is the regulation. This is the reality and here is our calculation which shows that it doesn't make much difference and here are some numbers" then that would help us. But if you just present the fantasy, we have no way of telling whether it has any relationship to anything.

MEMBER RANSOM: Well, part of the problem too is even in the standard, it never defines these pressure isobars in any terms that are meaningful for gastenomics (PH). And I find that amazing.

MR. ARCHITZEL: I'm not sure if we shouldn't invite the industry to make a comment on this because it is their proposal. Do you want to hear from them or not?

MR. CARUSO: If they want to make a comment, they are free to. Does someone want to make a comment on it?

MR. ADREYCHEK: This is Tim Adreychek, Westinghouse. One of the things about the 50 L/Ds is if you have a large break pipe break, you're looking at about on the order of about 116, 117 feet. The diameter of a containment is about 130 feet. This sphere, one of the other conceptions and reasons we use this sphere was it tends to encompass the entire or a major portion of a compartment that would contain equipment that would have insulation associated with it.

We recognize that a jet cannot expand

freely across the entire containment. The intent was to again develop a conservative volume that would conservatively predict the amount of debris that would be generated from a pipe break and this spherical approach seemed to be a reasonable way to do that.

I understand what you're saying, Dr. Ransom, regarding the expansion of the jet with a supersonic blast wave in front of it. That blast wave isn't going to go very far in most PWR containments because the compartmentalization of it.

The intent was to try to develop a model that would conservatively predict debris generation recognizing the limitations of the geometry that we had to work with in such a way that we would calculate debris, debris generation, that we could use to evaluate performance of the sump. That was the basis for one of the basis reasons that we used this spherical region.

CHAIRMAN WALLIS: The compartment sometimes is essentially the containment and you have steam generators in compartments.

MR. ADREYCHEK: Yes. Some of them are more open than others, but there are a variety of designs of containment out there.

CHAIRMAN WALLIS: You do have

compartments, it seems to me, more in an analysis that they use for fires. But you say that everything in this compartment gets destroyed rather than saying that this jet mysteriously goes through walls of compartments and damages something outside.

MR. ADREYCHEK: That's certainly one of the approaches that we identified in the guidelines that you can conservatively assume all insulation in a compartment becomes debris. So we did identify that and going on to a spherical zone of influence was the next approach.

CHAIRMAN WALLIS: But the spherical zone of influence cuts through the walls of compartments? Does it?

MR. ADREYCHEK: No, it does not. No.

CHAIRMAN WALLIS: Does not?

MR. ADREYCHEK: No.

CHAIRMAN WALLIS: It stops where there happens to be a wall of the compartment then.

MR. LATELLIER: I have a discussion on that.

CHAIRMAN WALLIS: You have a spherical zone calculated and then you cut it off where there are walls.

MR. ARCHITZEL: There's a discussion but it's not conservative but we are accepting that.

That's part of their guidelines though.

CHAIRMAN WALLIS: Within a very open containment where the steam generator is standing up there, you could have a sort of influence which is almost as big as the containment.

MR. ADREYCHEK: Very close.

MR. ARCHITZEL: Let me go back to slide 4. I would like to just note that in addition to the spherical zone boundary, we also are accepting - This is in section 6. You'll hear from Mark Kowal later this afternoon. We are additionally accepting within the baseline a hemispherical assumption for a non double-ended guillotine break which has been proposed by industry. That's not either physically bounded, but we are accepting hemispherical for those partial breaks in the RCS.

MR. CARUSO: How does the licensee determine whether it's a doubled ended?

MR. ARCHITZEL: Well, they're allowed to take, if they are using the alternative pressure, perhaps let Mark talk about it later, but we're talking about we have a risk informed or alternative approach. I'm not sure.

MR. CARUSO: That's the alternative resolution issue.

MR. ARCHITZEL: Right. Allows non full breaks. It's an option. It's not a requirement. So later, we will discuss it, but when that partial break is taken.

MR. CARUSO: But you can only do that in this alternative methodology. That's not a requirement.

MR. ARCHITZEL: Yes, because in the baseline without that, everything is double-ended break.

MR. CARUSO: Right. So in the baseline you assume the full sphere, but in the alternative --

MR. ARCHITZEL: The alternative has baseline aspects and non-baseline. That's what I'm saying. In the baseline, we would allow hemispherical, the baseline portions of the alternative.

MR. CARUSO: The baseline portions of the alternate, but the baseline baseline.

MR. ARCHITZEL: Right.

MR. KOWAL: This is Mark Kowal. Just to address that in the alternate evaluation section 6 that I'll talk about later, this comes up for breaks in the main RCS loop piping only which are partial breaks equal to the debris generation break size that we'll be talking about.

CHAIRMAN WALLIS: Why does this account for pipe whip?

MEMBER SIEBER: Doesn't.

CHAIRMAN WALLIS: No. The problem I had is that if I have a destruction that goes out 50 L/D and I let the pipe whip, then it sweeps out. It's like a guy with a machine gun sweeping around that area and that enables your damage at 50 L/D to be spread around all over the place.

MR. ARCHITZEL: Well, it wouldn't be spread around. It would go with --

CHAIRMAN WALLIS: Well, the pipe whips fast. This jet goes all over the place. It sweeps the wall and it's like a fire hose sweeping along a wall. It's very different from giving an equivalent sphere.

MR. LATELLIER: But you might also argue that that transient sweep gives you less damage than you might get under --

CHAIRMAN WALLIS: I don't know.

MR. LATELLIER: I don't know either, but you might.

CHAIRMAN WALLIS: If you have a destruction pressure and the only criterion is pressure, it could be reached for a millisecond and it damages it because that's the only measure you have.

MR. LATELLIER: Dr. Ransom has proposed and I would like to discuss this further but he's mentioned that the primary damage mechanism is the shock.

CHAIRMAN WALLIS: It might well be.

MR. LATELLIER: Shock loading. I would propose that that is very important to breaching any kind of cladding material, any kind of aluminum or stainless steel structure. Once that's been breached then the erosion becomes much more important. In the transient of a pipe whip phenomena, you don't have either of those effects dominating in quite the same way.

MR. ARCHITZEL: I guess one point I would like to make on that is, again I'll go back to BWR because I was reading that trying to understand what was done in the past, and when I read the two phase limited, they called it the recirc line breaks in the BWR. This dismissed the two phase type breaks as being less significant because it would blow off the RMI insulation intact.

Whereas the steam breaks would open the cover and destroy the included RMI to make a debris concern when that was evaluated there. So it is a little bit, "Yes, you'll blow it off. You'll open it" but if it's so much you can blast it off there, you won't necessarily have to damage that's of concern for some blockage. So it's a reason that we, in the BWR situation, discounted the recirc, the two phase breaks.

CHAIRMAN WALLIS: Can I go back to Bruce's statement he just made that the blast wave can do damage? I understand the blast wave isn't considered at all in the guidance and yet it seems to be that it actually can do significant damage. Maybe you should get the guidance rewritten to include the blast wave.

MR. LATELLIER: I don't think, we say we don't really know what it is and we've done this empirically with these measured pressures as a method.

CHAIRMAN WALLIS: That didn't include the blast wave. The damaged pressures, I think, were just -

MEMBER RANSOM: Again this is something that's going to have to be done by transient CFD analysis to find out what does that blast wave actually look like in this kind of situation and certainly, it seems to be a factor in the tests that were made. The other thing that has to be done is if you want to see how it decays, there is no simple way to actually look at a spherical expanding wave and how much the pressure differential across that decays with radius.

CFD again would be a good way of looking at that. I think I pointed out a hole you can find in other places, a simplified models that could be used to estimate that at least. But the thing that's kind of appalling is nothing was done.

MR. ARCHITZEL: But I like your conclusions in the end which I'm not sure given where we are is it worth pursuing this because it's just such a complicated problem that the tools are available anyway. Going to your conclusions about putting in gates up above and trapping debris and solving the problem on this model which isn't precise or exact, I did appreciate those. My problem is spending the time and effort understanding to try and understand the shock wave and what it really does and getting an alternative approach.

CHAIRMAN WALLIS: Ralph, can we go back to your experiments? You showed us some experiments. Is it the blast wave or is it erosion by the jet that causes the damage in those experiments or is it a combination of the two?

MR. ARCHITZEL: I would defer to Bruce, but I believe it's a combination.

MR. LATELLIER: I do not believe that it was ever separated, those effects were ever separated, in these integrated tests.

CHAIRMAN WALLIS: If it's the blast wave and you correlate it using the jet pressure, it seems to me you're explaining A by B where B is quite different from A. That's not the scientific method and if the blast wave caused the damage, you have to model the blast wave, not the jet.

MR. LATELLIER: Well, let's remind ourselves of the empirical method here. In the air jet tests which have been the most comprehensive to date, the freely expanding jet isobars were mapped to some resolution with stagnation pressure gauges in place.

CHAIRMAN WALLIS: There was no blast wave in the air jets?

MR. LATELLIER: You're saying that as a fact?

CHAIRMAN WALLIS: No, I'm just asking a question. Was there or was there not a blast wave? I suspect that if the jet was turned on slowly, there wasn't a blast wave at all.

MR. ARCHITZEL: I think it ruptured.

CHAIRMAN WALLIS: The ruptured disks were used to be typical of the opening blast.

CHAIRMAN WALLIS: So there's a big bang when it is.

MR. LATELLIER: Yes, there should have been. So that effect was present in the measurement and also in the characterization of the spacial volume. The second step was to put target material --

CHAIRMAN WALLIS: Then the blast wave should have damaged stuff that was over here and not in the direction of the jet at all.

MR. LATELLIER: In fact, I think Ralph has an example where that might be true.

CHAIRMAN WALLIS: So maybe the blast wave is very important.

MEMBER RANSOM: Well particularly in a test, I think, you get a blast wave out in front of the jet which is driven actually. You know the more gas you pour out, you continue to drive the blast wave so it can stay strong for quite a long distance. Whereas if it were just an initial radially expanding jet, it just dissipate fairly rapidly. But nevertheless, if it is a blast wave effect, a break opening up is going to cause considerable damage downstream.

MR. LATELLIER: If I could finish my point about the empirical process, the isobars of the jet were mapped by measurement first and then the test objects were put in place and the damage pressures that we've been using that define either the onset or the degree of destruction were empirically correlated to the those free jet measurements.

MEMBER RANSOM: You're talking about the ANSI jet wall.

MR. LATELLIER: No, I'm talking about the experimentally determined.

MEMBER RANSOM: Where is that data?

MR. ARCHITZEL: That's in the SECY tests that were done in the BWR. We have it. I could provide that to you. I have it right here actually.

MEMBER RANSOM: I mean because what would be interesting is to know what you mean by pressure there too of course.

MR. ARCHITZEL: Well, that was measured.

MEMBER RANSOM: And what do they look like.

CHAIRMAN WALLIS: Well, presumably it's a stagnation probe that measures that.

MR. SCHAEFER: Here is the testing in that section.

CHAIRMAN WALLIS: Is it a stagnation probe that measures the pressure?

MR. ARCHITZEL: I don't think it's described.

MEMBER RANSOM: But even so, stagnation probe in supersonic flow measures only the pressure downstream of a normal shock.

MR. LATELLIER: I assume that they would.

MEMBER RANSOM: And it's considerably less.

MR. ARCHITZEL: But the other point to also remember is that that was in the SECY tests. The other tests with OPG a lot of times we did use to back calculate what stagnation pressures would have existed. Sometimes it was an instrument of the same way.

MR. LATELLIER: But that's exactly the distinction I would like to make. We have to understand what the measurements tell you about the damage, the degree of damage and then you can discuss the translation to any predicted model and spacial volumes. Dr. Wallis, I assume that the pressure measurements were done with a perpendicular transducer plate rather than a static probe.

CHAIRMAN WALLIS: That was on the plate and then you measure the static pressure.

MR. ARCHITZEL: The pipe was drilled as I remember and discussed. The pipe was drilled on the transducer and put inside that hole.

CHAIRMAN WALLIS: So it's very much like a stagnation.

MEMBER RANSOM: Is it described in there?

MR. ARCHITZEL: Yes, it is.

MR. LATELLIER: But my hope was the opposite.

MEMBER RANSOM: We can talk about that a little later.

MR. LATELLIER: But my hope was that the experimental measurement was closer to a surrogate target than that so that you were measuring something physically related to the damage process.

MR. TRAIFOROS: Now, on the shock waves, finally I would like to make a statement that certainly Dr. Ransom's and Dr. Wallis's observations are correct. We are talking about the importance of the shock wave in the introductory paragraph of the GR. However, we are not addressing it any further. The closest that I found on NRC documents addressing PWR was the CR 67.62 which is the parametric evaluation which states that the debris generation resulting from blast effects would be confined to a small region surrounding the break location and that the major contributor to the debris generation is jet impingement which is basically the position that the GR is taking but it appears that it may not be adequately documented.

MR. ARCHITZEL: Documented or justified?

MR. TRAIFOROS: Both.

CHAIRMAN WALLIS: Well, is it true or not? I mean if it's true, then maybe we can forget about the blast wave. But you can't make it go away just by talking about it. If there was an analysis or something, some numbers, I can say this a thousand times. I have a little button I press here which says that same thing every time. Show us some numbers and some analysis. But then maybe the blast wave is a red herring. I don't know.

MR. ARCHITZEL: Well, I just want to say from that perspective that the work in the BWR, there's a tab in the BWR document that dismisses the blast wave because of the opening times and perhaps do we need to do that work again, I guess? I thought it was a more significant problem here, but it has been done.

It may not be that tab. There is a discussion of the shock and here it is, "Evaluation for Existence of Blast Wave Following." So what was done with the boilers, now that's different pressures and different conditions, but there is an evaluation that says there is no shock waves that was done for the BWR. I can't vouch for the --

MEMBER RANSOM: Is that computational flow dynamics? I was looking at this.

MR. ARCHITZEL: So I don't know. I guess we have to look at that and say, "Is it valid?" I had a hard time looking at that and then looking at the pictures we had with the shocks inside, but I think it was Dr. Wallis. I guess the point is that if that works not sufficient because it's different conditions would we have to redo it and I don't know that we could do it any time soon. That's the problem we're at.

CHAIRMAN WALLIS: I don't know. I'm just looking for some expert who knows.

MR. ARCHITZEL: Not me.

CHAIRMAN WALLIS: Maybe we have to move on. We've said the blast wave might be something that needs to be resolved but we're not quite sure if it's important or not.

MR. ARCHITZEL: We'll get somebody that understands it to look it over next week or something.

CHAIRMAN WALLIS: All right.

MR. ARCHITZEL: If I can go to slide, I think I'm on four.

CHAIRMAN WALLIS: Yes.

MEMBER SIEBER: I think this would be a good time.

CHAIRMAN WALLIS: We asked you about pipe width and then we discussed this business about spherical volume, conservative, energy loss. I'm not sure that's true either.

MR. ARCHITZEL: Well it retains the volume.

CHAIRMAN WALLIS: Because multiple reflections can actually help to refocus the energy rather then to dissipate it.

MR. ARCHITZEL: But it could damage more if you happen to have congested areas of containment as much material --

CHAIRMAN WALLIS: The best thing is really to let it expand very freely and then have a shock that knocks down the pressure to a very low value. That's the best thing is to have it unimpeded than to have shock wave. If you refocus it with multiple reflections, you can actually behind the shock wave which then results in a higher pressure.

But I don't know this is of importance. It's just when you make a general statement like that, it just is based on some kind of nonscientific basis. We have to be careful about general statements that seem to make sense, but may not be so true.

If you look at the SANDIA analysis for those classical things, they had an expansion to extraordinarily high MOX numbers and very low pressures, subatmospheric pressures, and then shocks back to a pressure which is surprisingly low. So even though it's gone to this enormous 2,000 or 3,000 feet a second velocity, it comes back and behind the shock the pressure is remarkably low. That's a wonderful way to dissipate energy.

MR. ARCHITZEL: But if that was the case, wouldn't you accept that in that audience --

CHAIRMAN WALLIS: If you put things in the way, it might make it worse.

MR. ARCHITZEL: But could it affect the entire volume is that point, the maximum volume.

CHAIRMAN WALLIS: Let's move on here.

MEMBER SIEBER: Would this make a good time to break for lunch?

CHAIRMAN WALLIS: No, I'm going to wait until 12:30 p.m. You're right. We should break for lunch very shortly here. Can you say something in five minutes?

MR. ARCHITZEL: Okay. Let me go faster. Let me go to slide 5, the Size of Zone of Influence. We've discussed this already. The GR 421 recommends using the ANSI 58.2 standard and the appendices that determine this. We agree that the 58.2 is --

CHAIRMAN WALLIS: So all the points that's very easy for the ACRS to make about errors in the ANSI standards of using the stagnation enthalpy to determine that conditions when the jet is moving at high velocity, all those sorts of things are irrelevant.

MR. ARCHITZEL: They are not irrelevant. I guess we have ways to --

CHAIRMAN WALLIS: We may bring them up in our letter but there are definitely some very peculiar things about this standard, but you're accepting it anyway.

MR. ARCHITZEL: We accept the use of it. That's correct.

CHAIRMAN WALLIS: Okay. Nothing we say about it is going to make any difference.

MR. ARCHITZEL: That's right.

CHAIRMAN WALLIS: You just accept it. Right?

MR. ARCHITZEL: I'm not sure that's --

CHAIRMAN WALLIS: What would we have to say to make you change your mind?

MR. ARCHITZEL: I'd have to take it back and discuss it with management. It might be things like we should use a CFD code or something like that.

CHAIRMAN WALLIS: Yes.

MR. ARCHITZEL: We should have licensees say that it's not acceptable to use what's been done or to look at the shock wave effect there. I don't know the answer to that at this meeting.

MR. LATELLIER: If I could interject to temper the discussion perhaps, at our last public meeting, the ACRS committee asked the question, "What can we do to help the Staff?" And I would like to thank both Dr. Wallis and Dr. Ransom for providing the insights and the write-ups. This is useful and useable information that we can help to judge the acceptability of our approach. I can't, as a contractor, promise what action will be taken but it will be duly considered.

MR. ARCHITZEL: To move on to the next

point.

CHAIRMAN WALLIS: Thank you. I think you're considering it very well. I just wonder what the Staff needs to consider. I mean how bad does the standard have to be before you say do something different? What's the criterion here? Is it just the easy way to do it or is there some logical criterion that you're using?

MR. ARCHITZEL: I'm sorry. I can't answer that.

CHAIRMAN WALLIS: That's another one of my things I say all the time. Are you just saying it or do you have a basis for it? That's all. I think that has to be asked of everything really.

MEMBER RANSOM: Well, one thing I think would be fairly simple is to clear up this definition of pressures that are used and simply define them and see if you can reach any kind of consensus on what you mean by them because in reading these documents it's never been defined in ordinary gastenomic terms. So if it's some kind of fictitious thing that's new, that needs to be understood. But I would sure encourage that to be done at a very minimum.

CHAIRMAN WALLIS: So maybe we should go to lunch with you put up slide 7. We can go to lunch with that on our minds as the one piece of good figure.

MR. ARCHITZEL: I was hoping I could be all done. I'm going to be here at the end. Okay. I don't know if I have many more points to make though other than the 40 percent.

CHAIRMAN WALLIS: I think you're being very helpful and we're just trying to ask questions in order to figure enough information to decide what we should recommend. That's all we're trying to do. And if you have anything else that you think of that you forgot to say this morning that you can discover and bring with you after lunch, please do or even tomorrow. With that, we will break and can we take less than an hour for lunch? Is that reasonable?

MR. ARCHITZEL: Sure.

CHAIRMAN WALLIS: Suppose we take 45 minutes for lunch and meet at 1:15 p.m. Okay? We will then do that. Our lunch break is to 1:15 p.m. Off the record.

(Whereupon, at 12:30 p.m., the above-entitled matter recessed to reconvene at 1:17 p.m. the same day.)

CHAIRMAN WALLIS: Let's come back into session. We'll resume where we broke off for lunch.

MR. ARCHITZEL: Should I continue?

CHAIRMAN WALLIS: We're ready. Yes, please.

MR. ARCHITZEL: First before we start, we talked at the end of the break, and Rob Elliott would like to -- we talked to Rob Elliott. He can express a little bit better some of our positions.

MR. ELLIOTT: A lot of the discussion - this is Rob Elliott from the Staff - a lot of the discussion that we had before the break talked a lot about what we don't know, and I'd like to remind the Committee about some of the things that we do know. The Air Jet testing that we conducted in Colorado, that were conducted by the industry in Colorado for the BWRs did simulate an instantaneous pipe break with a ruptured disk, so we did have the blast wave considered in the experiments.

We can't tell you from those experiments whether or not the jet impingement or glass wave created the debris, but we do know from those experiments that regardless of which created the debris, we did get some important insights about debris generation. One of the important insights that we got out of this test, for instance, is that for jacketed material, if the seam of the jacketing were not oriented in a direction towards the jet, you got no debris generation at all. You got a dented jacket is what you got. And the amount of debris generation you got would be maximized if that seam were at about a 45 degree angle relative to the break in the direction of the break.

CHAIRMAN WALLIS: Is there something in the guidance?

MR. ELLIOTT: This is not something in the guidance. I'm expressing -- what I'm trying to express here are some of the things that I see would be conservatism in using the spherical zone of influence. So given that, the spherical zone of influence assumes that everything in the zone of influence becomes debris. Okay. So that's significant when you think about what we saw in the experiments which said that if the jacketing were not oriented in a direction that contributes to debris generation, you might get no debris from that jacketing at all.

CHAIRMAN WALLIS: So this damage pressure is defined on the basis of the worst possible orientation of the seam?

MR. ELLIOTT: Absolutely.

CHAIRMAN WALLIS: Okay. Thank you.

MR. ELLIOTT: Okay. The second thing --

MEMBER RANSOM: Didn't we see some photos of damage where most of the damage was on the downstream side. Were those those kind of --

MR. ELLIOTT: But the seam of that jacketing started out in the direction out front.

MEMBER RANSOM: Out front and then it was rotated around.

MR. ELLIOTT: And then blew out the back side.

MEMBER RANSOM: Okay.

MR. ELLIOTT: The second thing I'd like to point out is that the spherical zone of influence will completely neglect any benefit from shadowing, structures or piping that would minimize, or protect or shield possible debris sources, that's completely neglected in the spherical zone of influence.

CHAIRMAN WALLIS: But there's something though in the guidance about behind a substantial object or something.

MR. ELLIOTT: Yes, Ralph will probably talk a little more about that.

CHAIRMAN WALLIS: What's the difference between shadowing and being behind --

MR. ELLIOTT: They're talking about

significant barriers like walls, or something like that, as opposed to piping or structural components.

CHAIRMAN WALLIS: Something like a steam generator is a barrier.

MR. ELLIOTT: Something that big, yes, would be.

CHAIRMAN WALLIS: Or a pressurizer or something.

MR. ELLIOTT: Something that would be a robust barrier.

CHAIRMAN WALLIS: So a 36 inch pipe is not?

MR. ARCHITZEL: No. It's large components.

MR. ELLIOTT: And then if you combine that with what Tim Andreychek was telling us a little bit earlier about the size of the zone of influence relative to the size of the containment, it's our judgment that we think that there's a lot of conservatism built into the spherical zone of influence as far as debris generation goes. And so I just wanted to point that out, that we do have insights and we can share with you from the URG the testing that was done at CZ in Colorado.

CHAIRMAN WALLIS: So your judgment that

there's a lot of conservatism is something which has evidence or has some sort of rationale explicable?

MR. ELLIOTT: It has evidence in what I'm telling you from what we've seen in debris generation. Not knowing -- it's empirical and not knowing what causes the debris generation, but we have seen in testing that regardless of whether it's the blast effect or the jet that there are attributes that are necessary in order to maximize debris generation. And we consider the maximum or worst case when we're assuming how much debris is generated.

CHAIRMAN WALLIS: Is there some way that between now and tomorrow you can actually have some data that we can look at, where you say here's the data and this is why our approach is conservative in the light of the data. Is there something we can look at like that by tomorrow?

MR. ARCHITZEL: We do have the CZ test results in that document we gave Ralph, but that's --

CHAIRMAN WALLIS: We can't read everything. We need to be pointed to it. If you can put it on a slide or something so it's very clear and explain it to us.

MR. ELLIOTT: We'll do our best.

CHAIRMAN WALLIS: Because that's much

better than just talking about it.

MR. ELLIOTT: Okay. I'll see if I can put something.

MEMBER RANSOM: Can you also show us what was measured in those tests?

MR. ELLIOTT: Sure.

MEMBER RANSOM: In terms of pressure, flow rates, that kind of thing.

MR. ELLIOTT: Sure.

MR. ARCHITZEL: Let me continue on with the last bullet I'd like to go over on this slide. And this is one point in the GR that we're not accepting, or actually telling the industry we're not accepting. Some plants had in their licensing basis that there's no damage beyond 10 diameter limits, and we don't accept that for debris generation, so we made it clear in the GR. That's all that point is at the bottom. The methodology is as has been discussed on damage pressures, et cetera.

On 6, I think I'll just quickly say that we -- I don't know that --

CHAIRMAN WALLIS: Why don't you go into a little detail on that.

MR. ARCHITZEL: It's just basically the calculation or procedure for calculating that equivalent volume, and then doubling it, and then coming up with a spherical volume, so I'll go on to -- we've already done 7, and let me go to 8 then.

We noted earlier today that the GR in Section 3.42.2 recommends that for the baseline case the zone of influence is selected based on the potential effect on insulation in site containment with a minimum destruction pressure, so it doesn't matter even it's in the zone. That's what the GR says. And then this zone is applied to all insulation types across the board.

We are accepting this position, but we also know, and it is one of the refinements that a well-characterized destruction pressure is valid to be spread over the spectrum or separate ZOI centered on that same break. And actually, even in the sample problem, NEI did use a different destruction pressure for one of these, I think the coating.

The next point I'd like to make is that the -- what we've been discussing about before is on Table 3.1 in the -- no, we weren't discussing this one. There is a table in the GR. It does match experimentally determined damage pressures versus calculated values, and that we did check this independently. This was in Appendix I, and we did note fairly good agreement, although where there was non-conservatism on the other chart in the near field, NEI actually chose values that bounded those, so it didn't make any difference, and we accept those values in that table. Even though we're not accepting one for the coatings, the point is that the determination was acceptable in the table in the GR. Go to slide 9, please.

Damage pressure considerations, I guess we have to work on what the right nomenclature is. But as Bruce mentioned earlier, the damage pressure does require an understanding of limits of the jet model and the experimental data. And I think we've discussed this already, how the jet model predicts impingement pressures in the downstream direction. And the point would be made that it can under-estimate the radial extent, the shears, et cetera, going that radially in that jet.

Another problem with the ANSI jet model is that if you take it to very low pressures, it is unbounded, so it gives unrealistically large zones of influence for low destruction pressures. And that is evidenced in some of the graphs of the CFD done for the boilers versus this. You get down towards the low pressures, it goes up quite a bit in volume, and that's probably not realistic.

The next point is that the data used in the guidance report is dominated by tests that were developed using high pressure air, as we discussed before. And we have concerns about whether this air jet testing was appropriate or not, so we did sponsor or pay some money, went in together with OPG, and did some limited amount of two-phase testing in a joint program with OPG. This is, I think, around 1991 time frame, around in there. But there was only one test of low density fiberglass. And as you noticed, there was a significant amount of damage, like over 50 percent of the insulation was blown out, really a large amount of damage.

IN addition to that, there was quite a bit of damage to aluminum clad Calcium Silicate, where as in the BWR testing it was like 160 pounds destruction pressure determined, and the OPG testing similar type of offsets on the seams it was around 60, so there's like a factor of 66 percent, quite a bit of reduction in pressure for damage on the Calcium Silicate insulation.

In addition, we talked earlier about plausible or possible damage mechanisms associated with two-phase versus air jet tests in general, so the idea that you can have those water droplets penetrating the article and causing additional damage - we have the uncertainties of the model. Considering all those uncertainties and the limited amount of data, we're proposing that the damage pressure for materials that have been tested only with air be reduced by 40 percent.

CHAIRMAN WALLIS: I think the numbers you said were 160 in test and it was 60 in the other. Was that right?

MR. LATELLIER: I'd like to correct that. I think it was more like 190 reduced to 24. It was almost a factor of 5.

CHAIRMAN WALLIS: So why are you only reducing by 40 percent when you got a reduction of a factor of 5?

MR. ARCHITZEL: Well, that was Cal-Sil. Okay.

CHAIRMAN WALLIS: But still, I mean, it indicates that there's a great deal of uncertainty in these tests. One test gives you 190 and one gives you 25 --

MR. ARCHITZEL: It is an unknown. There was a thought that we wouldn't -- some of us thought we shouldn't go as much as that.

CHAIRMAN WALLIS: This is a theme that sort of runs through, I think, my assessment of all of this work. Everything seems to be based on a few tests. It's difficult to get consistency between tests, so there's a huge amount of uncertainty involved.

MR. ARCHITZEL: On the next graph, go to the next page, please. What I would like to do is say that with the 40 percent reduction, what we have done effectively that's tripling the zone of influence, so what we have --

CHAIRMAN WALLIS: But the tests differed by a factor much bigger than 40 percent.

MR. ARCHITZEL: It's 125 times. You would say it's 5 in the Cal-Sil.

CHAIRMAN WALLIS: So what is the -- was the 190 overly high or something?

MR. ARCHITZEL: Well, they weren't necessarily the same construction either. There's definite uncertainties associated with the way OPG puts together a Cal-Sil test and --

CHAIRMAN WALLIS: Okay. What would you calculate if one was measured to be 190 and one was measured to be 25, what do you calculate?

MR. ARCHITZEL: We have no capability for calculating a damage pressure from first principles. That's a property of the test material.

CHAIRMAN WALLIS: You have to -- what does the 40 percent do then, changes the size of the --

MR. ARCHITZEL: It changes the size of the zone of influence --

CHAIRMAN WALLIS: By changing the damage pressure.

MR. ARCHITZEL: Yes, by changing the damage pressure, it's an incentive to go out and get --

CHAIRMAN WALLIS: So it is a calculated damage. It was a recommended damage pressure or something.

MR. LATELLIER: I misunderstood your question. You're asking about the size of the corresponding damage --

CHAIRMAN WALLIS: I'm saying that you made a measurement of 190, and another measurement of 25 is the damage pressure. What do you assume it to be, or what do you calculate it to be? What do you predict it to be? What's your theoretical value, or your accepted value, or whatever, to compare with these tests?

MR. ARCHITZEL: You mean for the non-tested material?

CHAIRMAN WALLIS: This test here, what do you predict for those tests?

MR. ARCHITZEL: We used the OPG, so did the industry. The industry, if you look at the Cal-Sil line there, the industry is using the testing from the OPG data. They're using the 24.

CHAIRMAN WALLIS: So they're using the 24.

MR. ARCHITZEL: And we're approving use of 24.

CHAIRMAN WALLIS: And you're reducing that by --

MR. ARCHITZEL: No, because that's two-phase testing. They didn't try to use the 190 for the Cal-Sil. They came in and they used the --

CHAIRMAN WALLIS: So we're talking about two different things here where you're reducing something by 40 percent.

MR. ARCHITZEL: All the remainder of the material that was not tested with two-phase is being reduced by 40 percent.

CHAIRMAN WALLIS: So in that graph, Cal-Sil is the only one that was in fact tested in the two-phase.

MR. ARCHITZEL: With well-characterized

tests. I mean, I'm going to show you some other two-phase testing that was done in Germany. There's a very limited amount --

CHAIRMAN WALLIS: But you see what my problem is when you've got two things that differ by a factor of 5 or whatever, it seems a lot bigger than 40 percent.

MR. LATELLIER: That's true. It's even worse than that, Dr. Wallis. There are some tests available that show a lower degree of pressure under two-phase conditions.

CHAIRMAN WALLIS: That's right.

MR. LATELLIER: And so this, as we explained this morning, there are plausible mechanisms that can be discussed for reasons for which two-phase conditions may enhance the damage mechanism. None of them, with the exception of perhaps Cal-Sil, they have not been thoroughly investigated, so we felt it prudent to acknowledge the potential for that to occur, and perhaps to encourage further testing to be done.

Now I can give you the historical benefit of why we chose the number of 40 percent. Earlier this morning we talked about what is the definition of the damage pressure, and it was mentioned that there is a certain amount of subjectivity in how you define the degree of damage. If you choose an onset, you may be looking for penetration of a blanket or exposure of the internal material to water.

On the other hand, if you were worried about some substantial damage that exposes material to transport and degradation, that could give you an alternative definition of damage. Historically, from the BWR testing, the difference between the onset of damage and definition of substantial damage was the reduction between 6 PSI for the threshold, and 10 PSI for the substantial damage criteria that would lead to a vulnerability. That reduction of 40 percent is one possible rationale for our reduction of 40 percent.

MR. ARCHITZEL: I'll say there's also some evidence to the contrary, so we've had some work done that shows that the two-phase velocities out of two-phase breaks is much lower. One of the rationales for not evaluating in the BWRs the recirc line breaks, like I mentioned earlier, is that they weren't considered bounding compared to steam line breaks. And air was considered above and beyond the steam line breaks. We had some people from research trying to help out and give them the answers. Over this next couple of days they addressed this question, and what they basically came up with is a two-phrase break by the physics of it is going to give you higher velocity - excuse me - lower velocities and lower velocities to water, but that doesn't tell you the damage part of it. It just tells you the volume of the isobars will be smaller in a two-phase jet, so when the metric is actually what is this impingement pressure being measured, that's where there's a little bit of a discontinuity in the result.

But I'd like to point out one other place, and that's the BWR did test -- there was a limited two-phase test of insulation, I mentioned before, and they saw very little -- much more damage with a steam than they did with the equivalent two-phase blow-down, so there are some -- that's that issue about blowing it off and not damaging it though, but there is some countervening thought process that it may not be quite as bad as 5 times, so there's an incentive to test. There's a big penalty if you have the air jet test right now, and that's in our GR.

CHAIRMAN WALLIS: Forty percent came from the difference between the pressure it takes to begin damage, and the pressure it takes to achieve --

MR. LATELLIER: As determined by air jet testing in the fiberglass.

CHAIRMAN WALLIS: What I thought we were talking about was the spread that you do on tests. In one place you get 24, in another place you get 190, so there's an uncertainty, which you're now fixing up with a 40 percent, which seemed to be coming from some different thing all together. It doesn't fix up uncertainty by fixing up by the fact that -- which makes a difference between the onset of destruction and total destruction. It doesn't accommodate the uncertainty. You see what I mean?

MR. ARCHITZEL: That doesn't totally address the uncertainty, and we could actually -- you're correct.

CHAIRMAN WALLIS: So again, it looks as though you grasp as a straw. You've got something that's available, but you sort of applied it in a context which is somewhat different.

MR. LATELLIER: Let's return to one of Ralph's earlier slides on the OPG test, the single fiberglass test conducted at OPG. In that slide, the orifice is about 3 inches in diameter, target is placed 10 diameters down-range. This one. The target is 48 inches wide placed at about 30 inches down-range.

You can see that there is damage clear out to the ends of the fiberglass mat. Despite its potential deficiencies, if we would superimpose the ANSI jet model envelope on this target under these conditions, the envelope of ambient pressure is only about 32 inches wide, so according to that model you would not expect to see damage beyond that range, and yet it exists.

CHAIRMAN WALLIS: Well, I think what you're telling me is that it's not a local phenomenon. You can open up the lining, the cover at one place, and you can rip it off, just like undoing plastic from a CD or something, which is impossible for me. Once you get it started, you can rip it off, so if you get it started in one place, you can rip it off all the way along the pipe. That's what you're telling me, I think.

MR. LATELLIER: Well, I'm not sure that's true, because if you notice the banding, the steel bands are placed at about 8 inch intervals, and those were not broken or displaced.

CHAIRMAN WALLIS: Those are still there. I see that.

MR. LATELLIER: And so you need some shear force along the entire --

CHAIRMAN WALLIS: But you see what I'm

getting at, that here you say it's just a local pressure that does it. It may be that once it opens up in one place it's much easier to get whatever it is in there that pulls it off somewhere else.

MR. LATELLIER: Clearly, that's true.

CHAIRMAN WALLIS: There's penetration by the liquid itself, which then travels along and comes out again.

COMMITTEE MEMBER: And originally the seam was at 45 degrees upstream.

MR. ARCHITZEL: Well, the OPG testing did orient the seams in a vulnerable direction compared to the testing that was done for the BWRs, where it wasn't in as vulnerable direction, so that's a factor that you might say is not quite times 5, but also a factor that says perhaps when the BWRs were tested, they didn't have the most challenging seam location.

CHAIRMAN WALLIS: What are we looking at there? Are we looking at here that all the covering has gone and we're just looking at Cal-Sil?

MR. ARCHITZEL: Well, the front and the back, and this is Cal-Sil. The next one will be fiberglass.

CHAIRMAN WALLIS: The right-hand slide we're just looking at Cal-Sil?

MR. ARCHITZEL: This is the same. It's just the front and the back.

CHAIRMAN WALLIS: The front is still crooked?

MR. ARCHITZEL: Exactly.

CHAIRMAN WALLIS: So eventually ripped the covering --

MR. ARCHITZEL: From the back.

CHAIRMAN WALLIS: Eventually torn it off.

MR. ARCHITZEL: Yes. Go the next slide too, you see the same. And actually, what OPG did by the way, so it's actually -- you see the nozzle there. If you go back to the Cal-Sil, but they did find -- one of our recommendations, one of our comments in here as you're doing this type testing is that they looking at this then turned around and double-banded with offset seams the jacketing.

MR. ELLIOTT: Double-jacketed.

MR. ARCHITZEL: Double-jacketed with bands, and the destruction pressures went to like 300, and they couldn't destroy anything, so that as a solution, as a way to minimize, and that's in one of the things that NEI has proposed as ways to address this problem - if you double jacket and band properly, this material, you'll get tremendous -- even if you rip off the one, the other is still there, so there was no damage up to the maximum they had.

CHAIRMAN WALLIS: And the place where it's hanging on still is in the middle. That is where it's supposed to be worse.

MR. LATELLIER: Well, it's largely a function of where the seams are placed. And there were a couple of orientations, I'm not sure which this one was, where the seams were placed near the center or off-set from the center.

MEMBER RANSOM: The statement is though the seam was at 45 degrees. That's facing upstream, right?

MR. LATELLIER: Yes. If the jet is here, the longitudinal seam, it's running this way. It was rotated at 45 degrees from vertical.

MEMBER RANSOM: So in this picture it's been rotated back.

MR. LATELLIER: It's been ripped, not rotated, but actually torn.

MR. ARCHITZEL: Maybe it does actually look more like zero degrees on this one, I guess was the point being made.

MEMBER FORD: It seems to me these tests are telling you something. Could you go back, because surely you just caught it just like a weather vane. You've caught the seam, you whipped it around. The seam is at 45 degrees.

MR. LATELLIER: I'm not positive on that.

MEMBER FORD: Well, because there's no constraint on the fiberglass from just turning. But, in fact, there would be a constraint.

MR. LATELLIER: I'm not sure it turned. I guess I'd have to look that up.

MEMBER FORD: You say it could have been damaged in the front and then the whole thing turned around.

CHAIRMAN WALLIS: The thing ripped around just like a sail on a boat.

MR. LATELLIER: It's certainly something to confirm.

CHAIRMAN WALLIS: That is quite possible, unless someone really observed it.

MR. ARCHITZEL: I think you're right, Peter, in that there's nothing stopping it from turning on the pipe. Generally the blankets, I forget, are like 4 foot long section so yes, there's not a lot of friction there to hold it in place.

CHAIRMAN WALLIS: So it could have ripped on the front and just been turned around?

MR. LATELLIER: One of the, I guess disappointments about this single test, it's all the data that we have, is that it doesn't discriminate the threshold of damage; where in a complete test, you would have placed this target at increasing distances to help judge the degree of damage. We have only this one case.

CHAIRMAN WALLIS: What bothers me is you have here a hypothesis. You have this plume it looks like a flame, an ANSI standard. And it hypothesizes that these damage pressures around it. And you want to do a test to test the hypothesis in some thorough way. I don't think you do it by just sort of casually doing one test here and one test there. You do a systematic matrix of tests.

MR. LATELLIER: And, of course, that was our --

CHAIRMAN WALLIS: Always to check things out, and this seems to be so casual. You've got one test here and one test there, and you're not quite sure what they show, and each of them shows something a little bit peculiar. What do you conclude?

MR. ARCHITZEL: Well, this is a limited test program. We didn't do these type tests for the PWR resolution.

MR. ELLIOTT: Actually, there's a reason why this test was cut short, and that's because they started blowing stuff out into the parking lots, and they were concerned about worker safety, so they discontinued these tests for a reason, not because they wanted to do just two tests.

MR. LATELLIER: Indeed, we had a more systematic matrix planned for investigation of fiberglass damage.

CHAIRMAN WALLIS: Would that convince me it was a better test because they stopped it because it blew into the parking lot? That's an excuse for why they stopped it, but it doesn't mean that it was any way a better test. The worst test because they only did two. How many would have been required to really thoroughly investigate the ANSI standard?

MR. LATELLIER: I think we had something between five and eight tests planned for this investigation.

CHAIRMAN WALLIS: And you managed to do two.

MR. LATELLIER: No, one.

CHAIRMAN WALLIS: One. You did one.

MR. LATELLIER: This is the --

CHAIRMAN WALLIS: This is like the Cal-

Sil. Well, one test worked out of so many.

MR. LATELLIER: For Calcium Silicate it was very thoroughly investigated. That was their primary insulation application, and they did arrive at the information that they needed.

CHAIRMAN WALLIS: So when you quoted 190 and 24, that's a mean five --

MR. ARCHITZEL: No, that's different test programs. Twenty-four is OPG, 190 is the BWR OG test program, different test program.

MEMBER FORD: How many data points were used to come up with the 24 number?

MR. ARCHITZEL: I think seven or eight tests, something like that. I got the report.

MR. LATELLIER: But don't misunderstand, it's not the mean of replicated conditions. It's a set of five to eight tests with the target placed at different locations so that the onset of damage could be bounded.

CHAIRMAN WALLIS: Tell me about that. I'm sorry. I'm really curious now, because we have this ANSI jet model which says that there's a pressure of so much at different places, and you put these things at different places. And does this correlate then that the damage occurs wherever ANSI says it's going to be 25, or is it 50 in one place, and 25 in M-15, and someone takes an average?

MR. ARCHITZEL: This wasn't the measured test. That was the BWR, so this is the one we back calculated.

CHAIRMAN WALLIS: No, we've got a test with five different position, and this then lets me begin to test the hypothesis. What can I conclude from those five tests?

MR. LATELLIER: As I tried to explain, I was very careful to keep separate the empirical study from the modeling effort. And as I explained, the free jet expansion was measured. The pressures at various locations was pre-determined, and the damage pressures were correlated to those measurements, not to the model.

CHAIRMAN WALLIS: So you measured - what was it called - you measured what a stagnation probe would measure, and then you correlated with that. But you didn't go back and say what does this tell me about the ANSI jet model.

MR. LATELLIER: I believe that comparison was made, but I did not participate in it. We have made some effort, as I shared a paper with Ralph Caruso. We made some effort to search the literature for experimental measurements of centerline pressure, and to do that comparison that you suggest. What we find is that the ANSI jet under-estimates the centerline pressure, and its decay behavior. However, because of the manner in which it preserves the forward thrust, it exaggerates the spread. Essentially, the pressure profile is much flatter than that observed in experiments. The question of just what the definitions of measurement and model are still relevant, and we're working on that.

CHAIRMAN WALLIS: ANSI jet pressure model is a cone.

MR. LATELLIER: Simple linear variations.

CHAIRMAN WALLIS: You're saying it's flatter than in -- do the experiments even point to the more pointed?

MR. LATELLIER: Yes.

CHAIRMAN WALLIS: Okay.

MR. LATELLIER: But we do need to determine the basis of the pressure definition and what was measured.

CHAIRMAN WALLIS: I thought in Vice Ransom's reference it was flatter than the cone.

MEMBER RANSOM: Which part are you talking about, the limit?

CHAIRMAN WALLIS: The pressure distribution across the -- the radial pressure distribution in the core in the region where you had these shockwaves and things. It was fairly uniform. Well, we can't spend forever on this. But again, it seems to me that you're evolving an understanding, which is good.

MR. TRAIFOROS: I would like to make an observation, if I may. The steady state thrust coefficient for dry steam is 1.26 based on the ANSI methodology. For air, it's approximately 1.27, so if we compare air and dry steam, it would seem to me that based on the ANSI methodology would calculate the same thrust. And if we take the damage as being caused by the same thrust, we would expect the same damage. But again, as you indicated, there are some other things that are going on in there regarding what causes the damage to the insulation.

Now what is interesting is that for liquid, the system peak was to 2.08, for dry steam is 1.26, for air is 1.27. I was wondering whether you used these, and also you are talking about 40 percent reduction. So if you have a high mix of steam that has low quality and you reduce by 40 percent, air and low quality steam, we have a difference of 40 percent.

I was wondering whether you had looked into this.

MR. LATELLIER: We certainly have calculated the difference in the thrust coefficient as a function of quality, as a function of upstream stagnation conditions. We did not use that as a basis for the 40 percent reduction. That may be a useful thing for us to examine.

MR. ARCHITZEL: I don't know that I need to really focus on this. This just demonstrates the 40 percent reduction and the resulting change in the ZOI from the GR to the Staff SER. So we go on to 11 then.

CHAIRMAN WALLIS: Well, let me see what it says.

MR. ARCHITZEL: The first column, this is basically a modified table out of the GR. The first column is the destruction prefaces that were proposed by NEI.

CHAIRMAN WALLIS: So where does the jet go? Suppose you have it directed at a plate, it's a robust barrier, and it squirts out sideways, how do you take account of the fact that it's squirting out sideways and not going straight?

MR. ARCHITZEL: A robust barrier, that's a couple of slides later on.

CHAIRMAN WALLIS: It protects anything behind it, but then it makes it worse for whatever is on the side.

MR. ARCHITZEL: Well, we've accepted the position that there is no expansion of the spherical ZOI. It's a little bit of a compromise with the tripling of the volume of the ZOI for the 40 percent. We're accepting that --

CHAIRMAN WALLIS: Did you look at this archetypical Sandia report where they analyze a jet impinging on a large plate, impinged on the plate. Actually at the nozzle it opened up in a front expansion and squirted sideways, and they got velocities going sideways of two or three thousand feet a second, because the plate is there. So the plate is protecting what's behind it, but it's diverting the jet to squirt out sideways, so I just want to be sure that when you're allowing to protect things with a barrier, you're taking account of the fact that the barrier itself like a turbine bucket is turning things in a different direction and directing it at something else.

MR. ARCHITZEL: Well, that's still within the zone it would be incorporated, but if it's outside --

CHAIRMAN WALLIS: Just think of your garden hose. I mean, it's impinging on something - you don't want to stand too close because the jet gets diverted sideways. It's not as if -- I mean, it protects what's behind the obstruction, but it makes the stuff that's beside the obstruction mobile.

MR. ANDREYCHEK: May I make a comment regarding that, Dr. Wallis? Tim Andreychek, Westinghouse. The high energy piping is of concern, our typically not located directly against a wall where that particular phenomenon would be observed, so for a primary system piece of piping going to say the reactor vessel off to the steam generator, you're in typically a more open area. You're not going to see that immediate plate or obstacle just in front of the jet. And, there, I don't think that the phenomenon is as prevalent as you might expect if you put a garden hose right in front of a plate, in which case you would see a redirection of energy --

CHAIRMAN WALLIS: You could do the experiment in your hotel sink. I mean, just direct the jet from the faucet into the sink with the plug in it, and stand there.

MR. ANDREYCHEK: We agree.

CHAIRMAN WALLIS: Turn it on fully. It's

pretty clear the jet turns around and comes back at you. So the floor may be protected from the jet, but you aren't. It's just seems to be another one of these things where the naive assumption is made that you have a barrier to protect something, forget about all the other effects. That seems to permeate this.

MR. CARUSO: Can I ask a question about a practical example. If you have a steam generator with say a cold leg break, how far around the steam generator do you assume that any insulation is stripped off the steam generator?

MR. ARCHITZEL: I was just going to say that I was unclear on that point, whether that's considered in the shadow or is it beyond a component because of the pipe, so I'm not clear, and I was almost going to revise the SC to say in that situation you should consider that traveling along the vessel, and not being in the shadow. But we haven't written that explicitly, and I'm not sure what industry's point is. We're accepting the - there's a slide later on - we're accepting the truncation but that does not mean necessarily we're accepting that there's no damage on the back side of a component, which is the question you're asking.

CHAIRMAN WALLIS: What are you going to do when they come in with a submittal which says that the steam generator --

MR. ANDREYCHEK: Well, I was going to do right here - I just made a note to myself before Ralph had said something about ZOI and the shadow on component, and so I was going to try and change the SE, but I would like to get if industry has a position on what their interpretation was of in the shadow.

CHAIRMAN WALLIS: But you're accepting their position on the shadow, aren't you?

MR. ARCHITZEL: But I'm saying, modifying it for that aspect. It wasn't clear. It's not clear in the GR what the position is. It says "large components, items behind large components and walls are considered in the shadow." We're accepting that, but what I'm saying, it's not clear how you treat that component and the insulation on the back side of the component. You don't necessarily need to consider that in the shadow, and that's how I was going to think about revising the SE.

MR. CARUSO: Could I make a suggestion, it would be a good idea to incorporate examples like that into the guidance report or into the SER, that --

MR. ARCHITZEL: Well, this is being reviewed by management, so I've got to take it back to management. I've got to think about it.

MR. CARUSO: I understand that.

MR. ARCHITZEL: Probably have to consult with industry, so I shouldn't make these comments here.

CHAIRMAN WALLIS: Well, let me -- you're familiar with how planes slow down when you land, and then there's a loud noise as the jet is directed backwards so that it slows down the plane. So the jet that was going backwards now has some things which come out and direct it forward, reverse thrust, whatever they call it. So that doesn't kill the jet, it just goes in a different direction. Although someone standing behind it is protected by a robust barrier, but then it goes the other way, so some kind of naive assumption that if you put something in the way of a jet, it stops. Now that seem to be a primitive idea.

MR. ARCHITZEL: Okay. The initial -- we did debate whether we should accept that position or not accept that position. There was a precedent for accepting it on the BWRs, and I'm not talking about the reverse side of components now. I'm talking about the fundamental position. And considering all the other conservatisms that exist in the ZOI, we made a decision to accept this non-conservative aspect of not resizing the spherical ZOI, which in reality should be resized because it's not a spherical ZOI no matter what. It has to hit things to become spherical in the first place, so just because you hit something to not resize for the volume is not necessarily conservative. So I hear you, but we did make that decision to accept that position.

MR. JOHNSON: Yes, Ralph's right. This is Mike Johnson. We pressed on the staff looking at conservatisms here and there, and there were several instances of which we were looking at coming back with positions that were not accepting, not going to be accepting what was in the guideline, and we said understand that our fundamental position would be that we ought to worry about what the industry is proposing with respect to robust structures, for example, ZOI. Given the overall conservatism in what we believe is the spherical ZOI, isn't it okay. And I think the position that we ended up with, and it's on the slide that Ralph hasn't yet gotten to. I guess we are on it, Ralph. Which says that we had those conservatisms, but in considering the overall conservatisms, we think that it's okay.

CHAIRMAN WALLIS: So again, this is the

kind of thing where you think and believe, but we don't see any kind of comparison with any theory or experiment. Just somebody thinks and believes that this barrier -- we can assume that the barrier kills the jet. That's the basis of the decision. They're just trying to determine what the basis of the decision is, not saying this is a good or bad way to make decisions.

MR. ARCHITZEL: And it's balanced with the loss of energy off the reflections or elsewhere.

CHAIRMAN WALLIS: Which is also based on thinking that maybe it happens, not based on an analysis or an experiment.

MR. JOHNSON: It's based on judgment. This one was based on judgment.

MR. ARCHITZEL: And precedent. I mean, there was a precedent that said this approach was acceptable, so we had that.

CHAIRMAN WALLIS: So someone else had some judgment before.

MR. ARCHITZEL: And I think I've addressed the points that --

CHAIRMAN WALLIS: Well, let me -- when the jet engine is tested for noise against a wall at the back end of the runway, people are presumably advised not to stand beside the wall in front of it because the jet is directed sideways. I'm sorry. I'm trying to give you some images which tell you that it's not quite the way it seems to be thought to be.

MEMBER RANSOM: Well, another image you might take a look at is a shuttle launch or any rocket launch, and look at the flames going downstream of the jet deflector. You'll see hundreds of diameters, if not thousands of diameters that that jet persists. It's not easily mitigated, and the mixing with the surrounding atmosphere is about the only vehicle for reducing the mass average velocity of the jet.

CHAIRMAN WALLIS: That's a very good image. That's a good one.

MEMBER RANSOM: And, in fact, the shocks and all don't really dissipate the momentum. They only change kinetic energy to thermal energy which heats up the jet somewhat. And so it's -- I wouldn't dismiss this too easily.

CHAIRMAN WALLIS: Well, it's again a question of you make a judgment call and you say we believe, but if you had asked the guy who is familiar with shuttle launches, he might say my experience is quite different.

MR. ARCHITZEL: This is actually not quite that situation. We did -- the initial position was not to accept this resizing, so it was a deliberate decision to then in balance with the 40 percent aspect of the ZOI and other conservatisms of the ZOI and the precedent not to pursue it.

CHAIRMAN WALLIS: See, I think one of the ways we can help the staff is to make it clearer and help them to make it clearer what the basis of decisions is, and what the rationale is. And there's far too much, it seems to me, we thought that probably, or we believe that it should be or something like that. And that's the kind of thing that we try to get out of the educational system all together amongst students, because what you think might happen, unless you have technical might often be quite wrong. I don't want to harp on this. It just seems to me that one way the ACRS can help the staff is to make sure that it has a good basis for decisions which are defensible.

MR. ARCHITZEL: I guess in this context you would think this is not a defensible position. Though it's an arbitrary one, we --

CHAIRMAN WALLIS: Well, I'm trying to dig for what it is that you would give as reasons which might be then taken as being a defensible position.

I'm a little uneasy if it's all words.

MEMBER RANSOM: Mr. Chairman, I'm wondering if this isn't a possible application for the non-parametric statistical approach where there's high uncertainty in many aspects of this thing, and that approach gives you a way of placing a confidence on the overall result.

CHAIRMAN WALLIS: What would you take then for -- you'd still have to have a model to put your uncertainties into.

MEMBER RANSOM: That's true, but they do have a model. How much effect is in different parts, it's just a matter of what is the uncertainty.

CHAIRMAN WALLIS: You have to put some pretty big uncertainties in there.

MEMBER RANSOM: Right.

CHAIRMAN WALLIS: Thank you.

MR. ARCHITZEL: I think I'm winding down here. Hopefully there are not too many more. There are several -- there is one simplifying determination for a ZOI which is basically you can envelope an entire compartment, and we accept that. We do have a caveat to look immediately outside like a doorway, et cetera, if you're taking this approach to make sure there's not vulnerable insulation materials immediately outside the vent path. But beyond that, we're accepting that.

CHAIRMAN WALLIS: So the picture I have is there's a jet in a space, let's say a room, and the room confines the damage to the room. And outside there nothing significant happens.

MR. ARCHITZEL: Except you look immediately outside for the vent path to make sure that --

CHAIRMAN WALLIS: There is event path, so you look to that.

MR. ARCHITZEL: That's correct. And it could be bigger than the ZOI that would be calculated, but it's simple enough to just determine it that way, and you don't have to do an analysis of it.

CHAIRMAN WALLIS: Does it do significant damage to the room itself? Does it blow off doors and things like that?

MR. ARCHITZEL: Well, that's different. We're not doing that analysis.

CHAIRMAN WALLIS: No, but I mean it's --

MR. ARCHITZEL: That should have already been done. Subcompartment analyses should have already been done on these rooms, so we're just discussing the debris generation part.

If we go to slide 13, there are two refinements, and I think I've mentioned the first one, and that is we do accept that you can take insulation damage pressures unique to the particular material provided it's well-characterized and it's acceptable to do those in some to arrive at a total debris source term.

We do note on this one that additionally we'r still requiring the 40 percent reduction for materials not tested on two-phase conditions.

CHAIRMAN WALLIS: I don't understand why they wouldn't always do this. There's no real benefit to being terribly conservative.

MR. ARCHITZEL: Simplistic, I assume. I'm not sure why the recommendation came in, other than if you can simply go through it and you don't need to do a lot of work, you're done.

CHAIRMAN WALLIS: Right.

MR. ARCHITZEL: You don't need to calculate different spherical zones if you don't have a problem. You're done, and you're finished. Although if industry has a different point on that, I'm not sure.

Anyway, next slide is 14.

CHAIRMAN WALLIS: What does this mean,

it's less than the decreased measure for Calcium Silicate?

MR. ARCHITZEL: That was what we talked about before, the five to one versus the 40 percent is not the --

CHAIRMAN WALLIS: Is it bigger than 40 percent effect for Calcium Silicate?

MR. ARCHITZEL: That was the five to one we discussed.

CHAIRMAN WALLIS: Yes, so what are you going to do about that?

MR. LATELLIER: They're using the lower value. The industry is using the lower value determined for Calcium --

MR. ARCHITZEL: But we're saying for materials not tested air jet G is 40 percent. It's th same discussion we had earlier.

CHAIRMAN WALLIS: Now I'm understanding. Thank you.

MR. ARCHITZEL: And 14, the next refinement that's offered by the industry is that they talk about instead of resizing, just using directly the models in the back of the ANSI standard to freely expanding jet offset, the ones if it's restrained, et cetera. And we are improving that. In other words, you can take along the axis of the pipe and you do offset, and you get the big plumes as opposed to the sphere. We're not exactly sure that gains the industry that much, because if this was prior analyses that were done, we are saying you still have to do it in the most vulnerable locations. It can't be just the high stress locations. That's a different position, but we're accepting that you can alternately calculate direct jet impingement.

COMMITTEE MEMBER: Why did they want to use that?

MR. ARCHITZEL: I was speculating why, and I thought it was because the analyses were already done and some plants are licensed to the MEB-31 and those unique break locations that have the analyses done and in place. I may be wrong. I offer industry if I'm wrong. I don't know if somebody wants to say anything. That's my speculation.

This is a summary slide I presented in the beginning. I'd ask if there's any questions. If we do, I guess we've got some take-backs here in terms of being -- we believe it's acceptable. We have to provide some additional material to justify our positions. In addition to the SER providing some additional clarification, I guess we still need some more for even using the approach is the point that we made. And we plan to include the 40 percent factor to address the two-phase uncertainties. I did have, and I don't know if you wanted -- somebody said something earlier about these other models and backups. We don't need to go into them if they're not spherical.

CHAIRMAN WALLIS: Unless you have something that helps -- this is something that really helps clarify what we discussed earlier?

MR. ARCHITZEL: No, I don't think so.

CHAIRMAN WALLIS: Just raise new questions?

MR. ARCHITZEL: It just shows what was done earlier in different resolutions of this, like the three-phase zone.

CHAIRMAN WALLIS: Well, is there anything which is -- we asked for data or anything you have that's based on quantitative material? Thank you, Ralph.

MR. TRAIFOROS: I would like to ask another question. In terms of refinements, I was wondering whether you looked at possibly considering system depressurization and friction of the fluid in the pipe in terms of determining, if you will, a steady state thrust coefficient.

MR. LATELLIER: Actually, the industry did not propose that as a refinement, but the staff actually offered that as a potential reduction of effective pressure at the outlet. We felt that the development of internal friction loss is sufficiently robust to make that determination for specific break locations, and so the industry may find that there are particular scenarios that are driving their safety decisions where that refinement would be appropriate and might be advantageous.

MR. TRAIFOROS: I was reading the update of your SER and I didn't see a reference to this in terms of refinements.

MR. ARCHITZEL: I know it's in there because I've read it also. I forget what section, but those exact words are in the -- I think it is called additional refinements that can be used.

CHAIRMAN WALLIS: Is this the sort of place where we should talk about Appendix A or whatever it is?

MR. ARCHITZEL: The only time to talk about Appendix I is now, not A.

CHAIRMAN WALLIS: Oh, I. The only time you get a chance to talk about Appendix I is now?

MR. ARCHITZEL: This is the time.

CHAIRMAN WALLIS: Well, there's a figure in there, 1.4, 1-4, which I didn't understand. And, in fact, I thought it was -- well, I won't say what I thought it was. I just want to understand it.

MR. ARCHITZEL: What's the number again?

CHAIRMAN WALLIS: Figure 1.4. It's related to my colleague, Vic Ransom's question about what you mean by impact pressure or damage pressure, or pressure on all these various things. I think the intent of Figure 1.4 was to explain what was meant by some of these things, so that that seemed to be a key figure.

MR. LATELLIER: I think you're referring to the control volume force balance on a rigid plate?

CHAIRMAN WALLIS: Yes. Can you talk about that, or should we wait until you have a slide you can put up and we can talk about it?

MR. LATELLIER: As long as we all understand which figure is being referred to.

CHAIRMAN WALLIS: Well, can we -- I don't know. I need to look at it. I don't have it. Is it in the --

COMMITTEE MEMBER: This one here. Right?

CHAIRMAN WALLIS: That's not -- no, that's not the one I had in mind. Oh, maybe this is the one.

This is the one. Okay. You seem to have a picture where the pressure on the target was PA everywhere at asymptotic plane, or what is going on in this thing called an asymptotic plane in that figure? It says DA PA Row A VA, and yet it looks as it the jet is going sideways. What's happening there?

MR. LATELLIER: I haven't seen this figure in its present form, and there's clearly a graphics problem. As we had originally illustrated it, the jet impingement was flared outward in a convex manner more similar to the --

CHAIRMAN WALLIS: So it is an impinging jet on a plate.

MR. LATELLIER: It is intended to be a jet impinging on a plate.

CHAIRMAN WALLIS: And the pressure is almost spherical over the plate.

MR. LATELLIER: By assumption of the ANSI jet model, that's --

CHAIRMAN WALLIS: That makes absolutely no sense whatsoever.

MR. LATELLIER: This figure was offered as a rationale for deriving the form of the ANSI jet --

CHAIRMAN WALLIS: But you can't put something like that in a published document which is authoritative. It doesn't make any sense whatsoever.

MR. LATELLIER: This is purely development of the ANSI jet equations.

CHAIRMAN WALLIS: But you see there's a confusion, as in the ANSI jet and through all of this, there's a confusion between what happens in a free jet and what happens when a jet hits something. And the pressures and all that are different depending upon the circumstances, and it seems to be all mixed up in this figure in a way which really makes me think that there's a lot more mixed up than there ought to be about these analyses. You can't put a figure like that in a document that's going to go out in the public domain.

MR. LATELLIER: I see absolutely no reason not to. This is a justification of the ANSI jet equations. Now I certainly accept the deficiencies.

CHAIRMAN WALLIS: The ANSI jet is a free jet. There's no big target in the ANSI jet --

MR. LATELLIER: I'm sorry, but that's incorrect.

CHAIRMAN WALLIS: No.

MR. LATELLIER: The ANSI jet and the Sandia wagon model are very similar in concept.

CHAIRMAN WALLIS: But they're completely

different problem. The ANSI jet is a free jet, and they're talking about the pressure which you would get locally if you put a probe there. When you put a big plate in a jet, the pressure distribution changes completely.

MR. LATELLIER: The intent of the ANSI model is to calculate thrust loadings on large objects. And, in fact, it alludes to comparisons of pressure data collected in just that manner.

CHAIRMAN WALLIS: Okay. Well, if the pressure were atmospherical over the plate, there would be no thrust on the plate whatsoever. So what do you do about all the momentum coming in out of the jet? It just makes absolutely no sense. I mean, this is something that would get a zero on a homework problem in a first course in mechanics. You cannot put this in a published document, which is supposed to establish the NRC knows what it's doing. And I'm sorry to be so severe, but I just would not -- if you're going to put that in, I would not accept any of this stuff, if that kind of stuff is going to go into your SER. And that's the first time I've said anything so forceful today, but I really feel that you have to be told that.

MR. ARCHITZEL: Why don't we take this

back and we'll talk about it tomorrow.

MR. CARUSO: Like at page 6, the claim there is that it can be even higher than the stagnation pressure.

MR. LATELLIER: There are clearly some graphics problems with this figure, and we'll bring you the original and see if we can discuss an acceptable revision.

CHAIRMAN WALLIS: Would you do that tomorrow?

MR. LATELLIER: I believe that we can, yes.

CHAIRMAN WALLIS: And if you flunk your exam, I don't quite know what the consequences ought to be. I'm sure you won't.

MR. LATELLIER: At least I know what I'll be doing this evening.

CHAIRMAN WALLIS: Thank you. This is something I can't say enough of, and when we find what look like basic errors in what are supposed to be authoritative documents, it tends to demolish the credibility of the entire document. I have to say that again and again to you guys. And it is something you should avoid like the plague. Okay. So you're going to sort it all out for us, and I'm going to be --

tomorrow I'll be able to say I'm very sorry, I misunderstood you, and what I said was as a result of a misunderstanding. That's what I'd love to be able to say tomorrow.

MR. LATELLIER: I hope so.

CHAIRMAN WALLIS: Okay. So can we move on. The next topic is what?

MR. HAFERA: The next topic is latent debris. I'm Tom Hafera from the Plant Systems Branch.

CHAIRMAN WALLIS: We really ought to have to have Jack Sieber here. I guess he's not here. He's our expert on latent debris. Well, go ahead. I hope he'll be back.

MR. HAFERA: Okay. Let's proceed. Latent debris is basically miscellaneous items found in most PWR containments. It's a slightly different concept than was used in the BWRs. Miscellaneous dirt, fiber, foreign materials can also include things like tape, tags, filters, rags, rope, signs, whatever. The key to latent debris is it has to be defined both from a characteristic standpoint and total inventory, and the characteristics being whether it should be considered fiber or particulate. And that will become evident as I go on later, and basically, that deals with what kind of bed you build up on the screen.

For plants that are all RMI, all Reflective Metal Insulation, latent debris may be the dominant contributor for their head loss, and for the bed on their sump screen.

NEI proposed a method for evaluating latent debris. It's a five-step approach. We consider that to be generally acceptable. The guidance and sample methods proposed by NEI and the industry we feel could be more refined. We will be providing some of that information.

CHAIRMAN WALLIS: Remember Jack Sieber saying no one is going to climb on top of the steam generator.

MR. HAFERA: No one is going to climb on top of the steam generator.

CHAIRMAN WALLIS: And yet it's a big horizontal surface where stuff has been accumulating for some time.

MR. HAFERA: That's correct. Right. We also feel that some additional and detailed information is needed in terms of realistic estimates for debris, some special factors that will enhance debris loads on certain surfaces, and how to deal with fail tags taking placards, that type of information is not really clear in the NEI document.

CHAIRMAN WALLIS: What do they do about large surfaces like the top of the steam generator where you have not cleaned and you have not measured? What do they assume about the amount of debris that's up there?

MR. HAFERA: Well, we're going to -- basically, if you don't mind, let me proceed and I'll tell you what approach we're going to recommend. How is that?

CHAIRMAN WALLIS: Okay. That's fine.

MR. HAFERA: NEI proposed, as I mentioned, NEI proposed a five-step approach. Their first step is you estimate horizontal and vertical surfaces in containment. You go out and do a statistical sample or survey, containment survey to evaluate resident debris build-up. You define those debris characteristic. You need to recognize, as I mentioned, what type of debris you have in terms of is it fibrous or is it particulate, and some other type of characteristics that feed transport and head loss. You need to determine what fraction of your surface area is susceptible. We want to give plants and licensees the ability to credit programmatic and documented cleanliness programs. And then last, you calculate the total quantity that would be involved in fulfilling the debris bed that might fill up, form on an ECCS. So again, those are the five steps that NEI has proposed. We consider in general from an upper level perspective those five steps to be acceptable.

Some of the details that we feel are inappropriate or don't have sufficient technical basis, NEI proposed a method for sampling debris. Their method was to have someone go out and try to physically measure the thickness of the debris. We feel that's not really practical, and it's not -- it leads to some subjectivity and inaccuracy. A much better way is to go wipe it off and weigh it.

CHAIRMAN WALLIS: If you can measure it's thickness, there's far too much of it.

MR. HAFERA: Right. So we didn't feel that was a practical and realistic way, so we provide an alternate. They mentioned a number of things in their surveys, and they refer to NEI-02, which was basically a document that was meant to survey a containment for insulation and other things. But they don't account for a number of surfaces, things like steam generators, pressurizers, pressurizer relief tanks, some of the other larger components that are in containment. It wasn't necessarily covered real well, the details weren't really laid out real well in the

NEI document, so we've provided some extra guidance in the SER.

NEI document did not -- it provided some general guidance for tags, tape and placards. We felt in some cases it was maybe a little bit overly conservative, found that it was not consistent with Reg Guide 1.82, and it didn't mention anything to do with a tag or tape, or placard that would be affected in a way that it would be destroyed, so we provide a recommendation for that.

And now one thing about NEI is they did recommend some -- they provide some parameters for fiber density, particle density, and a few other parameters that are used in head loss calculation. And they recognize that there was ongoing testing by LANL and research that was going on where new numbers might be provided, so we're providing updated numbers.

Your first-step estimate, horizontal and vertical surfaces. They provide rationale for guidance for flat surfaces, round surfaces, vertical surfaces. Each one should be dealt with slightly different because a flat surface will collect debris easier than a vertical surface, and a round surface will only collect debris on the upper side.

They provide some guidance for surface

area calculations and estimation of dimensions which we found to be very reasonable. We don't need to cover every square inch of your containment. You can just make a reasonable estimation.

As I mentioned previously, their guidance did not include a lot of major components that would be inside a normal PWR containment, and we provided that. Another one that comes out is structural members; things like I-beams, structural supports, basically any surface that would be where you really need to be -- as we mentioned in the SER, you need to consider any surface that's subjected to containment spray washdown, because containment spray washdown could potentially transport the debris into the pool.

We mentioned that some special consideration is needed to be added in case there's -- I'm sure there's plants out there that have oil leaks, places where surfaces will collect extra debris. Those surfaces and surface areas have to be dealt with on a case-by-case basis. You can't just say well, there's not going to be anything there.

CHAIRMAN WALLIS: Can we talk about oil leaks?

MR. HAFERA: Sure.

CHAIRMAN WALLIS: If you have Cal-Sil on

a filter or a screen, it's on a filter essentially, because if it's on the Nukon and you get oil in it, the oil fills up the pores, makes it much more difficult to get water through it.

MR. HAFERA: Okay.

CHAIRMAN WALLIS: So oil in the Cal-Sil will affect its ability to allow water to flow through it.

MR. HAFERA: Yes.

CHAIRMAN WALLIS: That doesn't seem to be in any of the correlations or anything.

MR. HAFERA: I don't understand how that's relevant to latent debris. Cal-Sil is --

CHAIRMAN WALLIS: Well, it's very relevant to head loss on the filter if you get oil in the filter materials. It tends to bind it or clog it, or stick it, or whatever you want to say. I mean, greasy material is just the last thing you want on a filter.

MR. LATELLIER: I agree with that statement. However, I think we're willing to give them credit for not having significant quantities of oil spilled on a surface.

CHAIRMAN WALLIS: But we don't know how much oil is spilled, do we?

MR. LATELLIER: That's a fact. We're

simply drawing their attention to the potential for accumulating dust and dirt on --

CHAIRMAN WALLIS: I mean, if there were large amounts of oil --

MR. HAFERA: If you had a significant oil leak, that would show up in your power plant in a different area. In other words, if you had a significant reactor coolant pump oil leak, your reactor coolant pump would leak oil, lose oil. If you had an oil leak out of a hydraulic snubber, the snubber would become inoperable. So you can't have significant oil leaks in containment; otherwise, they affect your --

CHAIRMAN WALLIS: I'm just wondering how much is significant. If a cubic foot of stuff is enough to clog a filter, then maybe if you add half a pint of oil to that, it makes a tremendous difference.

MR. CARUSO: Can a break damage a reactor coolant pump lube oil reservoir, or in some way cause damage to a reactor coolant pump lube oil system to cause that lube oil to be mixed in with the debris from the break?

MR. HAFERA: That would be a plant-specific item. That would be an item that -- it would depend on the physical location, and design, and construction of their reactor coolant pump oil system. I'm familiar with the Westinghouse pumps. I think the Westinghouse pumps --

MR. CARUSO: Is it something that should be considered?

MR. HAFERA: I would defer to the plant designer, but I would suspect that a Westinghouse design, the reactor coolant pump oil systems are pretty much up out of the lube areas, and would probably not be in the zone of influence for a LOCA. But that, again --

MR. CARUSO: The pumps have to have --

MR. HAFERA: That may be plant-specific.

MR. CARUSO: The pumps have to have oil collection systems for fire protection reasons. Right?

MR. HAFERA: Correct.

MR. LATELLIER: I don't know the extent of the analyses, but there are loading calculations done for safety critical equipment. I'm just not familiar in what level of detail, whether it assesses the oil lines or reservoirs.

CHAIRMAN WALLIS: It seems to me we're considering possible chemical effects and things in the sump which may not happen at all. But we certainly know that if there were an oil leak that it would probably have some effect on the -- the globules of oil going through the filter might well affect the ability for it to allow water to go through. Probably that should be a concern.

MEMBER KRESS: Is there any evidence that moisture build-up and oil leaks actually enhance dust build-up? And if there is some evidence, do you have a way to quantify that build-up? I don't know what you mean by special considerations is what I'm getting.

MR. LATELLIER: We were simply trying to draw the licensee's attention to special considerations other than the flat large surface areas that they might more naturally look for. Another special consideration may be air filters in general for inlet air. If there are large concentrations of dust and dirt that are there by intent, by filtration mechanism, we need to ensure that it's not vulnerable to --

MEMBER KRESS: Yes, I would have been more happier if that one had been called out instead of oil leaks and moisture build-up.

MR. LATELLIER: This was a brainstorming exercise to just think of alternative mechanisms.

MEMBER KRESS: You're asking them to think of things, but you're giving them a couple of examples.

MR. HAFERA: Well, we're asking them to think of things that were not included in either the NEI guidance report or in NEI 02-01.

MEMBER KRESS: I'd be hardpressed to quantify the enhanced dust build-up due an oil leak or a moisture build-up, but on a filter I could probably get some quantified.

MR. LATELLIER: I think if these conditions were found, the incentive would be simply to rectify it, just to clean it up and remove it from consideration.

MEMBER KRESS: I see.

MR. HAFERA: Or to sample it and include that extra debris as a stand-alone item. But your comment, HVAC inlet filters, that is specifically culled out --

MEMBER KRESS: That is culled out.

MR. HAFERA: -- in other documents, so that's why we didn't consider it as a specific item for this.

MEMBER KRESS: Okay. As long as it's culled out.

CHAIRMAN WALLIS: If you have a lot of hot water around, it may well free the oil from the dust, and the oil will float to the surface, and you'll get an oil slick from the surface of the sump rather than oil going into the filter. But again, that's just my guess about what would happen.

MR. HAFERA: Right. But thermodynamically the containment pool is typically peaks at about 250 degrees, so it doesn't really get that hot.

CHAIRMAN WALLIS: I have water coming into my basement. It floats the oil -- the guy puts cat litter underneath the oil filter because there are a few drips of oil that come out, and the cat litter float down into the screen of the sump pump or whatever, but the oil seems to come off and fill the whole -- cover the whole pool with an oil slick, even the tiniest little bit of it.

MR. HAFERA: So the bottom line is, we culled out oil leaks because they were a condition that a licensee should at least pay attention to, and consider as an extra item for latent debris. Now as far as considering oil in terms of debris generation, transport, sump clogging, I'm not sure we've covered that. And I'm not --

CHAIRMAN WALLIS: What does it do to the

chemical reactions in the pool to have the oil there.

MR. HAFERA: That's a whole different issue. We can take that --

CHAIRMAN WALLIS: This is sort of aside, but we're going to get to chemical reaction somewhere, and it seems to me that a lot of the experiments seem to focus on a few things; whereas, there's a real hodge-podge of stuff that can get involved in this chemical reaction, including things like the half pint of oil which leaked and was never cleaned up, and what it does to the formation of something or other. It's there.

MEMBER KRESS: What's the meaning of the fourth bullet?

MR. HAFERA: Well, okay. The fourth bullet is - I was ready to say if everybody is ready to go on. For vertical surfaces we've provided a realistic conservative assumption that you could assume 30 pounds for all the vertical surfaces in the containment, and that's based on the five samples that LANL received from the industry in terms of study.

MR. LATELLIER: Now I have to correct that statement. The samples that we did receive were collected over a variety of surfaces, and we gained a lot of information about the composition, the fractions of fiber, et cetera, and their properties related to head loss. But the estimate for vertical surfaces was based on a single volunteer plant study of their own surfaces as sampled in a manner that we agree with, in an appropriate manner, swiping and weighing, pre and post test swipe measurements. And we added some reasonable conservatism to their estimate to account for the variations between plants, the variations both of plant cleanliness and also in the plant areas.

CHAIRMAN WALLIS: Could I ask Jack Sieber, who has just come in, if it's reasonable to assume that the dust that builds up on vertical surfaces in containment is limited to 30 pounds.

M Not much builds up on vertical surfaces, but I'm not sure how much.

CHAIRMAN WALLIS: But you did find an awful lot of latent debris, which was presumably on horizontal surfaces.

MR. HAFERA: Yes.

MR. LATELLIER: For reference, the volunteer plant estimated about 6 pounds on all of the vertical surfaces in containment.

MR. HAFERA: Okay. Second step, NEI recommended evaluation of resident debris build-up.

NEI says the first thing you should do is plan your containment surveys breaking the containment into zones where you would expect higher debris loads, lower debris loads. We agree with that. That's an acceptable method. You should try to sample as many different zones as you can. We also indicate that you might want to pay more attention to where these zones are in relation to spray and washdown, and in the pool area.

As I mentioned before, NEI guidance for measurement of debris is to go out and try to measure the thickness. We don't consider that to be practical. We think it's much more practical to collect debris in sample areas using a swipe or a vacuum that you can then weigh and determine its mass. And the guidance provided for tags, tape, and placards, NEI doesn't provide any guidance in terms of any plant labels or anything that would be destroyed. Our recommendation is if it's going to be destroyed consider it as fiber and evaluate it for transport in terms of the transport analysis.

CHAIRMAN WALLIS: How about inaccessible areas, like underneath things and so on, can they take any samples or try to take samples? Just thinking about my house, that when you move a piece of furniture, there always seems to be an extraordinary amount of debris underneath it. Yet it's not typical of the whole house. Why does it get there? Does it get transported preferentially there? Does it get blown there as people walk by or something?

MR. HAFERA: Well, when we say plan your survey, again you plan your survey based on your transport analysis, your evaluation of debris generation, and everything else. An area like underneath the reactor vessel, you wouldn't need to go do a survey there because it's essentially going to be a quiescent pool and none of that debris is going to be transported to the sump screen. That's what planning the surveys is all about.

CHAIRMAN WALLIS: And then the vertical surfaces, if you have a radiator in your house, it always gets covered with stuff because there of the thermal currents and things that deposit on it.

MR. HAFERA: Correct.

CHAIRMAN WALLIS: There are certain places that preferentially collect it, and do you have any guidance about that?

MR. LATELLIER: That is specifically mentioned as one of these alternative sources that should be examined.

CHAIRMAN WALLIS: Is there anything you learned from your containment pool or filter, something about the debris amount that's in there? Because presumably, if you're cleaning the filters every month, this is a measure of how much debris you're generating.

M That's true.

CHAIRMAN WALLIS: That could be used as a measure.

MEMBER KRESS: It's not a good measure. It's competing with deposition on all surfaces, so it's hard to --

CHAIRMAN WALLIS: It's actually sucking air through it, so it's extracting it.

MEMBER KRESS: Sucking it out of the air, but that's competing with the stuff falling out and depositing. It's hard to extract the number you're looking for.

CHAIRMAN WALLIS: I'm just thinking of Davis-Besse, that they were cleaning the filters quite frequently. So that was an indication of how debris was being generated.

M That's an unusual case.

CHAIRMAN WALLIS: Oh, yes. We think so.

M We're hoping.

MR. HAFERA: Okay. The third step defined by NEI is to define debris characteristics. NEI correctly indicates that the key factor is fiber particulate mix. That's what will determine how debris is transported, how it will make up the --

CHAIRMAN WALLIS: Can you give us some numbers? I asked for this earlier today, how much debris do you expect to find in a typical plant of this type.

MR. HAFERA: I think we mentioned 30 pounds on vertical surfaces. Cleanliness programs are greatly different between plants, size.

CHAIRMAN WALLIS: Are we talking about 100 pounds or 1,000 pounds, or what?

MR. LATELLIER: Some industry estimates have estimated somewhat above 100 pounds, 150 pounds. I guess it is our judgment that might be a representative value, but not necessarily a bounding value.

CHAIRMAN WALLIS: So how much do you need to make one of these thin layers we were talking about?

M It depends on the screen size.

MR. HAFERA: It depends on way too many factors. It depends on what's the fiber of the particulate mix, what's the transport analysis.

CHAIRMAN WALLIS: The particles, so let's assume that there's enough fiber to hold them there and then they deposit. How much debris do we need, given that we've already got something to hold it there to make a thin layer which can be significant in terms of head loss.

MR. HAFERA: That may not be a valid assumption either.

MR. LATELLIER: It does depend on the screen area. I'm searching for some typical values.

CHAIRMAN WALLIS: Do you want to do the calculation on 12 square feet, and 100 pounds --

MEMBER KRESS: You've got to add the density.

CHAIRMAN WALLIS: Yes. Do you want me to do it?

MR. LATELLIER: I'd prefer that than to make a guess.

MEMBER KRESS: Tell us what the density of --

CHAIRMAN WALLIS: It seems to me it might make a layer which would be significant.

MR. LATELLIER: It doesn't take a great deal of --

CHAIRMAN WALLIS: Thinking about a pile of 100 pounds, I mean you said that one square foot of fibers or something, a cubic foot of dirt weighs 100 pounds or something like that. So talking about cubic feet of dirt, we know a cubic foot from previous testimony can significantly affect a screen, so this is a significant thing.

MR. SHAFFER: Dr. Wallis, to form a thin bed, well, first you have to have sufficient fiber for filtration. Okay. That's one thing. Not counting Cal-Sil but normal stuff. And then aside from that, you need sufficient particulate for the bed to start behaving like it's just a layer of particulate, so that the porosity then starts going towards the porosity of just a packed bed of particulates. Okay. So you have kind of an inter-stage of going from fiber particulate behavior to pure particulate behavior. You have a little bit of --

CHAIRMAN WALLIS: It's kind of self-controlling, because the particulates go through when there's no fibers, and then recirculate and come back again. And by the time there's enough fibers, then they can build up.

MR. SHAFFER: They can do that. Now as far as the mass of particulates it takes, well, it depends on the type of particulate. You have to work in the densities of the solid particle and the sludge density.

CHAIRMAN WALLIS: So you're prevaricating I'd say. You're saying it all depends on all these things, but I just want an estimate of how much -- how significant it is, not all the things it might depend on, but is it important.

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: Could you make a thin bed with these sorts of hundreds of pounds of dust and latent debris?

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: You could.

MR. SHAFFER: Yes. We tested a surrogate latent sample and we created thin beds with reasonable mass ratios, and we've encountered these thin beds operationally too. They are a real --

CHAIRMAN WALLIS: So it's very important, even in the best possible plant that's all metal insulation and everything to do a good job on this latent debris.

MR. SHAFFER: Exactly. Especially with an old MRI plant --

CHAIRMAN WALLIS: This is a bit of a

problem because you're depending upon the programs of the licensee. This isn't something physical that you say you've now got Cal-Sil; therefore, you must do something, or you've got corrective metal; therefore, you must do something. You're saying you've got to have housekeeping which every year does the right job

MR. SHAFFER: Absolutely. Absolutely.

MR. HAFERA: Okay. I already mentioned that NEI also recognized that their values for fiber density, particulate density, and particle diameter might be revised, and we provided the updated guidance for that.

Step four provided by NEI was determine the fraction of containment susceptible. They provided some general considerations to allow licensees to credit housekeeping activities, and I think we just had that discussion. It has to be evaluated on a plant-by-plant basis. Our only consideration is if you're going to rely on housekeeping it has to be documented, and it has to be programmatic.

CHAIRMAN WALLIS: So we're going to have inspectors going around rubbing their finger on surfaces and looking at it.

MR. HAFERA: We have inspectors evaluate

FME programs all over the country, so I don't know why they --

CHAIRMAN WALLIS: So there is an understanding. Do they take samples or anything, or they just look at the program?

MR. HAFERA: Usually they look at the program, and then they walk around the plant and do observations, and talk to people.

CHAIRMAN WALLIS: It doesn't have to be -- I mean, you can take a swipe with a cloth or something. You get a pretty good idea if it looks black, that you've got a certain amount of debris. You can correlate that with so much debris per unit area, and you could figure it.

M Generally, the inspectors don't do that, and in union plants they aren't allowed by contract to do physical work, so you send technicians out to take the samples. The inspectors check on their work.

MR. JOHNSON: I understood the question to mean NRC inspectors. Is that not what you meant? Did you mean licensee inspectors, or did you mean NRC inspectors?

CHAIRMAN WALLIS: Well, presumably NRC inspectors have to satisfy themselves that the plant is being kept clean enough.

MR. JOHNSON: Absolutely.

CHAIRMAN WALLIS: All right. And I'm not quite sure how they do it.

MR. JOHNSON: There's actually -- Mark can talk more about that. Mark is actually a senior resident at Calvin Cliffs, Mark Kowal who started out the presentation, but there's a containment close-out inspection that gets done, and inspectors are well aware of the cleanliness, how well the licensees are implementing that program.

CHAIRMAN WALLIS: So is there some kind of criterion now, if you establish that as a result of the analysis these plants are going to do, they're going to establish that our plant is going to be okay, meets all the requirements of 50.46, as long as it does not have more than 150 pounds of latent debris. They could make that analysis, right? So now they have a number to shoot at. Every time they do their housekeeping, they have to prove that they're within some margin away from this 150 pounds of debris, which could clog the screen. Is that the way you're going to do it, quantitatively like that?

MR. JOHNSON: Well, I mean having thought about this for as long as you were asking the question

- I mean, there could be aspects of the evaluation that licensee -- where licensees in terms of the assumptions of the evaluation say things that are, in fact, commitments, that need to be implemented through programs or programmatic activities to ensure that the assumptions of the evaluation are true. And yes, we would expect that licensees would live with those, and we would expect that we could verify them.

CHAIRMAN WALLIS: Is this something that should be in the guidance as the quality of the program, what you expect as far as the output from the programs. Is it already there?

MR. ARCHITZEL: There is a -- in the admin control section later in 5, there's a writeup on that, with the expectation that -- we added the expectation that there are procedures in place to justify these --

CHAIRMAN WALLIS: So there's a follow-up. It's not just a one-shot thing.

MR. ARCHITZEL: Not a lot of information in 5, but it has those type words in it.

MR. HAFERA: Yes, this is an ongoing thing. This will not be a once and done deal. And as you mentioned, Dr. Wallis, that would be -- I know from my perspective I would say that would be a perfect way to do audit a plant, where you could say okay, show me your samples that you took this outage, and show me how that fit into your past, and how did that compare to your previous samples, and how did that fit into ECCS sump clogging calculation. That would be a perfect way to do audit that.

MR. ELLIOTT: I'd suggest one other thing. They could do similar to what the BWRs did with sludge, which was to determine the generation rate by measuring the amount of sludge that accumulated in the pool over multiple outages, determining what the rate of generation was, and then depending upon how much you assumed for your strainer design, you could then decide how often you need to go and clean.

CHAIRMAN WALLIS: So there's a precedent of doing something like this with BWRs.

MR. ELLIOTT: That's correct.

MR. HAFERA: Step five from NEI was calculate the quantity and composition of debris. Basically, that would be just your survey data from your break-up of your containment zones and areas. You would sum those together to come up with a complete quantity in containment.

NEI does not provide any guidance for categorization of the debris, and so we provided that. And again, we emphasize that you need to separate fiber from particulates, because depending upon -- fibers transport differently, particulates transport differently. Heavy particulates will sink, small particulates float.

MEMBER KRESS: And you do that with your sampling method that you're proposing. You're going to swipe this stuff and then scrape it off and weigh it.

MR. HAFERA: Yes, you do a statistical sample. You weigh it or you put it under a microscope.

MEMBER KRESS: Put it under a microscope first and see what the fiber versus particulate --

MR. LATELLIER: Unless the plants do a careful and thorough survey of their plant debris, it is manually tedious to separate fiber and particulates.

MEMBER KRESS: That's why I asked.

MR. LATELLIER: Of course, that's the exercise that we did at LANL using the five volunteer plant samples that were sent to us. We also have provided generic recommendations of the fiber to particulate ratio that were observed.

MEMBER KRESS: Because they could use generic --

MR. LATELLIER: They could. And for your information, the default recommendation is about 15 percent of the total mass estimate should be considered in fibrous form.

MEMBER KRESS: Makes me feel better.

MR. LATELLIER: So in summary, the general five steps provided by NEI are considered acceptable. We think that we need to substitute the guidance that we provide for sample methods and the new assumed values for debris characteristics that we've discussed here. We also provide some additional clarification for containment surveys, how they should be done, enhanced areas that should be looked at, how to deal with failed tape and tags, placards, and miscellaneous other things, and realistic estimates of debris loads. And that should provide an acceptable method for licensees to evaluate latent debris. Any other questions? Okay. Thank you.

CHAIRMAN WALLIS: Are we ready to move on? Thank you very much. I've already told the people who have asked me, that what we'll try to do - we knew we were going to get behind, is we will just keep going and we'll try to finish at a reasonable time, but it may well be an hour or two after the time we originally planned to finish today.

MR. WAGAGE: Good afternoon. My name is Hanry Wagage. I'm going to present to you the staff evaluation of NEI guidance, Section 3.6 debris transport. First, I'll give the summary of my presentation. NEI's debris transport methodology, the baseline methodologies are generally acceptable. NEI provided analytical refinements on pool recirculated transport, two methods. They are acceptable to the staff.

The staff gave supplemental guidance in the body of the safety evaluation and we had appendices to give additional guidance. Using NEI's baseline methodology and the staff's supplemental guidance, and the restrictions that are force, one can predict the amount of debris being transported to the sump screen.

CHAIRMAN WALLIS: Are you going to prove to us or demonstrate with rational arguments why your method produces a conservative mass of debris?

MR. WAGAGE: Yes, I'm going to try to do that.

CHAIRMAN WALLIS: Okay. Thank you.

MR. WAGAGE: In the morning and this afternoon, you heard about selection of the break, and generation of material because of the break, and characterization of debris, what sizes it breaks to. And the latent debris which is already available in the containment.

The purpose of debris transport is to find out how this debris is going to end up on the pump screen. This transport involves several mechanisms. Blow-down needs the movement of debris because of the brick. Wash-down transport is movement of debris because of the break flow as well as the containment spray flow, when the containment sprays come up later during that series. Pool transport is the transport of debris into the pool, especially the concern here that there are some areas of the containment which does not participate in recirculation. The water stays stagnant. That means whatever the debris in that region just stay there without being transported onto the sump screen.

Another transport mechanism is sump pool transport. Once the debris ends up in the sump pool, when the recirculation pumps start, because of the recirculation of water it adds debris onto the screen and it gets settled there on the screen.

This is a complex problem because debris is generated at the break location, and it's all over the containment, to find out how it moves to the sump screen. To do it realistically is a difficult problem, a complex problem.

The baseline methodology NEI proposed is based on NUREG/CR-6762 on logic tree, which I'm going to show you in the next slide. This you know as a file computer, the debris size is coming from the previous sections, has two sizes, small and large. As you see from this logic tree, the large debris does not transport. It just -- only the -- the large debris would not go through this transport mechanism. The baseline guidance assumed that these large debris formed by falling and lying on that side, would get stuck at the flow drains, radiological products and things, and glass racks because the smallest opening of those is 4 inch by 4 inches. That is assumed that large debris would not transport because of that.

We took exception to that because the large debris may be 3 by 6 inch, because it can relocate. It can orient itself and pass through the glass rack and end up in the sump pool. It would not cause problems unless this large debris would pass on to the sump screen.

MR. CARUSO: What about the possibility of the large pieces becoming smaller pieces in the transport stream?

MR. WAGAGE: That's another reason we took exception to this large debris would not transport. When large debris -- I did that later, but because the question came, I can --

MR. CARUSO: Do it later.

MR. WAGAGE: Okay. As you see, all the four transport mechanism I mentioned are here. The important question is to find the strict fashion, how would it go through different parts. For example, how debris will end up in the containment, how much would end up in the lower containment.

CHAIRMAN WALLIS: Now active and inactive pools presumably are the ones which have flow through them or not. They're either stagnant or they have flow through them. Is that what you mean by active or inactive?

MR. WAGAGE: Active is that pool participate in recirculation, recirculation of water.

CHAIRMAN WALLIS: There's a flow through it. It's not just a stagnant pool.

MR. WAGAGE: Flow through it. Inactive means water stay stagnant.

CHAIRMAN WALLIS: Okay.

MR. WAGAGE: As I mentioned, to do this right, you take some -- the NEI proposed was to use conservative assumptions. Assume this steep grid fashion so that we get most of the debris ending up on the sump screen.

CHAIRMAN WALLIS: Well, I'm trying to think about inactive pools. I'm sorry. The purpose of recirculation is to recapture the water and recirculate it through the reactor to cool it. And as you do that, you hope that you get most of it back again. It doesn't get hung up in active region. So I assume that after a while there are not really many regions which are any longer inactive. Is that true?

MR. SHAFFER: There's one primary inactive pool region, that's the reactor cavity.

CHAIRMAN WALLIS: There are certain places which --

MR. SHAFFER: Assuming there isn't a drain pass through it.

CHAIRMAN WALLIS: -- really do stay inactive forever.

MR. SHAFFER: Exactly.

CHAIRMAN WALLIS: Right. There are other ones that may start out inactive, and then as you get going in the recirculation and so on, they could become more active as you spread more liquid into them.

MR. SHAFFER: There's plenty of areas in the pool that that are fairly quiescent; in other words, they don't move a lot, but they're still moving. Those are not what's considered here. We want pools that are kind of dead-ended some place, and significant and large enough.

CHAIRMAN WALLIS: But they could change from inactive to active as the --

M Well, even the reactor cavity will do that because the board instrumentation area is down there, and there's a fence door that gives you access to the main containment, so you have to - once the water level builds up enough in there, it will run out the door. But it takes a lot of water to do that

MR. SHAFFER: Well, the idea here is that if there's a water drain like the sprays drain directly into that pool so that it fills and then flows out the door, then it's not an inactive pool.

M Any more.

MR. SHAFFER: But if the water drains to the sump floor and flows into that door and down, that's the only way in, then the pool might be considered inactive.

M Well, that's the only one I can think of that's inactive.

MR. SHAFFER: That's the only one of significant size. There may be some smaller ones.

MEMBER SIEBER: Okay.

MR. WAGAGE: As I said, the idea is to conservatively find split fraction for the loading peak. This methodology assumes three types of containments, mostly compartmentalized containment that during the break assume the 25 percent small fine debris would end up there in that containment. We got this fraction by comparing the volume of the upper containment and the total containment.

MEMBER FORD: Remind you. You started off by saying you were going to use conservative assumptions which are going towards the bottom to the sump screen. Where did these figures come from? In previous presentations we've seen various models of mass transport flow and things of this nature. What data are there to support those assumptions, which is your word? What data are there? And what's the basis for saying 15 percent conservative value from going towards the bottom. Where do these numbers come from?

MR. WAGAGE: Actually, you're talking about the inactive pool transport.

MEMBER FORD: It doesn't matter. In this whole event tree scenario, you've got numbers. Where do the numbers come from?

MR. WAGAGE: Number, as I was explaining, one is that transporting of debris onto the upper containment, 25 percent.

MEMBER FORD: Yes.

MR. WAGAGE: That we calculated by comparing typical containment, what is upper containment and lower containment, detailed analyses for --

MEMBER FORD: Just the volume. It's got nothing at all to do with the actual transportation mechanism. You just indicated a difference of 25 percent to 75 percent.

MR. WAGAGE: This is only for one type of containment, mostly compartmentalized containment, a significant fraction can go in the upwards direction. But even if that lays there in the containment space, come up for small size of fiber and particulate, all that come down to this sump pool. They assume that later that washdown transport is 100 percent for that. Only difference it makes is for RMI. RMI debris, small fines, it goes to the upper containment. RMI debris which goes to the upper containment, they assumed that the velocities will not be sufficient to take it down to the sump pool.

MEMBER FORD: But my question is, remind me as to why are you choosing these specific values? Is it based on modeling, is it based on substantial data? Where do these numbers come from?

MR. WAGAGE: This is judgment based on volumes, and it's conservative - assume it's not 100 percent, that some part goes to the upper containment

MR. SHAFFER: If I may jump in here a little bit, let's -- we're looking at it in two ways. The baseline methodology versus reality. Okay. The baseline has been broken up into real course steps, and generally they've assumed numbers that are just highly conservative, like 100 percent washdown transport. Okay. Can't argue with that. When they say only 25 percent goes up, 75 to the floor - well, based on our volunteer plant analysis, that's very conservative.

MEMBER FORD: But that was data.

MR. SHAFFER: Right. Now when we go to the volunteer plant analysis that we've done - we've done this and broke it up into many, many more steps. And to quantify, some of those steps we actually have data for from the BWR debris transport studies we've done. For instance, we have blown up insulation blankets, fiberglass, and transported the debris down-range to a test chamber with gratings and different structures, and measured the fraction that gets impacted inertially and captured. So when we look at the plant, if you break this upper containment down into nodes, then there's some steps where you can actually apply real and experimental data. There's other steps where we just don't have the answer. We have to take the ultimate conservative approach worst case. Or in some cases we can actually put a little judgment in there, but we break it into these many, many, many steps, quantify the steps, and then just come down with a transport number that is conservative, but a little more realistic than just taking this baseline where we're just assuming 100 percent transport. It's analysis, it's very interactable, but yet we can still get to a better answer than assuming 100 percent transport.

MEMBER FORD: If I read this Figure 3.3, which is for the new plant, analyzing it, about 42 percent, 43 percent of your total weight of Nukon debris was created will end up in the pump. That was based, the way I'm hearing you on engineering judgment as to which way it jumps as you go down this event tree. How dependent are you -- if it was not 43 percent, but 49 percent, what impact would that have on your MPSE data? How accurate do you have to be?

MR. WAGAGE: That 43 percent is originally for during the break we assume that 60 percent is small fines, out of that 43 percent can end up on sump screen. That mean that only 17 percent did not end up in the sump screen.

MEMBER FORD: Maybe I'm not asking the question very clearly.

MR. SHAFFER: If you examine that chart a little bit more, you'll find that -- okay. You start out and you say 60 percent is fine. Forty percent doesn't transport. Then of the 60 percent, the only part that doesn't get to the screen is what went into the inactive pool. They assumed everything that went up came back down again.

Now we've looked at that and tried to decide well, is that conservative. And yes, we believe it is, because we did some transport analysis on a volunteer plant. We applied the baseline methodology to the volunteer plant and compared the two results, and for the volunteer plant, the baseline methodology was conservative. The baseline doesn't have a lot of mechanistic analysis in it. It was designed to circumvent all the complexities.

MEMBER FORD: The reason for my question

is that I understand there's not been a lot of experiments. Some of these judgments are based on these few experiments, and there's a big uncertainty of the values. My question essentially is, does it matter? And I haven't heard the answer to that part of the question. Does it matter whether it's 43 percent or whether it's 49 percent?

MR. CARUSO: Do you have idea how much margin is available as a result of all this uncertainty

MR. SHAFFER: Well, the idea of these models is to do it in a matter where you believe you've got a conservative amount transported. You've bounded it. Okay. We actually believe it would be less than the bounding number.

MEMBER FORD: Why do you say that?

MR. SHAFFER: Because we look at the steps and try to make each step conservative. In the baseline, there are two steps that are not conservative, so we studied it and tried to decide whether the over-conservatism in some steps and the two that are not conservative still resulted in a conservative package. That was the purpose of some of this confirmatory research we did.

CHAIRMAN WALLIS: Conservative relative to what?

MR. SHAFFER: Conservative relative to a more realistic analysis that we did for the volunteer plant.

CHAIRMAN WALLIS: And the realistic analysis is based on what, transporting a lot of things by fluid mechanics and all that sort of stuff?

MR. SHAFFER: It has elements of that, elements of experiments, and in parts where we just don't know, then we take for that part a very conservative assumption.

CHAIRMAN WALLIS: Very conservative would always be at 100 percent of the worst.

MR. SHAFFER: In some steps --

CHAIRMAN WALLIS: It depends on where you've cut it off and why.

MR. SHAFFER: Okay. You decompose the problem into many, many steps - the ones you can solve, you solve. The ones you can't solve, you take the worst case condition or something close to it, and then you quantify the overall transport chart, something a whole lot more complex than this guide. And that's the analysis we've done. It's in one of the confirmation appendices that you've got. But it's that that we're using to compare to the baseline to make a judgment call.

CHAIRMAN WALLIS: Well, reply to his question, does it matter whether it's 45 or 49 percent?

MR. SHAFFER: Forty-nine percent will get you a higher head loss.

CHAIRMAN WALLIS: Presumably it could, because if you NPSH is up to the borderline where your pump isn't going to work, then a few more percent --

MR. SHAFFER: Could put it over.

CHAIRMAN WALLIS: -- puts you over that borderline, so these little bits of percent could make quite a difference.

MR. SHAFFER: Yes. But the idea is that hopefully we've confirmed the 43 percent is conservative.

CHAIRMAN WALLIS: When you say hopefully, at what point do we raise a red flag?

MR. ARCHITZEL: Ralph Architzel. You're going to hear a discussion of MPSH later, but basically the margin is in the MPSH calculation, and so you can go up against that margin. It's got its own inherent conservatisms in the baseline and we're relaxing some of those in the others. You can go up to that limit, but if you go beyond that limit, you've got to change something. So basically you bounce up against the MPSH calculation.

MR. ELLIOTT: This is Ralph Elliott. I'd like to add one other thing. These calculations are driven to the most bounding break, so from a risk perspective most of your other breaks are actually going to be less detriment to your sump screen. So if you size it for the worst case scenario, which is typically going to be one of the very largest types in the plant, at least that's the way it worked out with the BWRs was typically the larger pipes, then the much smaller pipes, much more likely pipes to break had much less debris, and were bounded by the more limiting break. So if you argue a percent or two here at the upper bound of the design of the sump, I could see where it could potentially make a difference. But overall, as far as the overall impact on the plant, it may not be --

MEMBER FORD: Well, that's encouraging because throughout the presentation so far I've been hearing there's uncertainty here, uncertainty there. And my feeling is well, so what? And it comes down to well, what's the risk do you have? And this is the first time I've heard someone say we've done a risk analysis of these uncertainties.

MR. ELLIOTT: I'm giving you a qualitative analysis rather than a numeric analysis.

MEMBER FORD: Okay.

MR. ELLIOTT: But we do know from our experience with the BWRs that the design was driven by a very limiting break, and that most of the other breaks did not end up coming up anywhere near that level of debris, and were not as significant a challenge to the sump or in that case a strainer. And in the PWRs I would imagine you would probably find the same thing. We're driving the problem to take us to the most limiting scenario we can, and which probably means that most of the other breaks would be bounded by that. And that's what we're hoping to have, a high confidence that we would be bounding the problem. So I just offer that as a little thought.

MEMBER FORD: Can I try another line of the same sort of thing? I mean, you've got these analyses where 75 percent of the debris was assumed directly deposited on the sump floor and 25 percent in the upper containment and washdown, 30 percent of each case sequestered in inactive pools. This is the example that NEI worked out. Presumably, this is all plant-specific, all these percentages depend upon the shapes of things, and where the break is, and what's the design of the floor and the walls, and all that. It depends on a lot of things. You can't just take this number 75 percent and use it. You have to calculate it from a lot of things, isn't that right? It's not just a magical thing pulled out from somewhere.

MR. SHAFFER: In reality, that's absolutely true. But what they're trying to do is take numbers that are so far conservative that you can point them to all the plants blanket-wide.

MEMBER FORD: As to all the plants?

MR. SHAFFER: All the plants.

MEMBER FORD: And all break sizes in all places

MR. SHAFFER: They do have a couple of numbers in here that they've split into three containment categories, and have a little different numbers for each of the three containment categories. But besides that, they're going to apply the same very simple baseline methodology across the board.

MR. WAGAGE: Dr. Wallis, that 75 percent settling on the floor and 25 percent going into the upper containment, that is only for the mostly compartmentalized containment. There is a chance possibly that some of the flow would go upward. And for the other types of the containment, mostly uncompartmentalized, all the debris settled down on the floor, 100 percent settles down on the floor, nothing goes to the upper containment.

CHAIRMAN WALLIS: So they have to decide whether they're compartmentalized or not. There's some sort of a decision made there.

MR. WAGAGE: Yes, that's right. The guidance was not clear on finding that in that case we gave additional guidance that when it is not clear to put into which category, always assume mostly uncompartmentalized, assume 100 percent of small debris settle down on the floor.

MEMBER FORD: If you look at Section 3.64 in their document, calculate transport factors, there's four lines. It just says calculate. It doesn't say how to calculate. It's on page 3.51.

MR. SHAFFER: They're referring to this logic chart that was just put up. Okay. You put the distributions on it. You just multiply the numbers across.

MEMBER FORD: Yes, I recognize that.

MR. SHAFFER: Yes.

MEMBER FORD: It says, "Calculate the transport factors for each type of debris." I'm wondering, it is a guidance document so where do -- how do you do that calculation?

MR. WAGAGE: There is information in this document and we summarized those numbers in the table in the Safety Evaluation.

MEMBER FORD: Okay.

MR. WAGAGE: That lists all the numbers important for these debris transport --

CHAIRMAN WALLIS: But does it say use equation so-and-so in some way, or does it just say assume some percentage?

MR. WAGAGE: There is no equation for --

CHAIRMAN WALLIS: No equations at all. They're supposed to calculate something? There's nothing mechanistic. It's all some kind of --

MEMBER FORD: It comes down to Professor Wallis' earlier question as to you're going to be deluged with a whole lot of different calculation methods if they're going to come up with anything less than 100 percent being deposited on the sump screen. You're going to have different calculation methods if you go down that event tree.

MR. WAGAGE: Only for that mostly uncompartmentalized. Most compartmentalized containment there is a possibility of some fraction can go upward.

MR. SHAFFER: It may not be adequately explained, but the chart itself is a calculational method. And we've been using these for a while, and maybe because we're used to it, we forgot to say how you quantify the chart, but it is a calculational method.

CHAIRMAN WALLIS: So are there equations used in the calculational method?

MR. SHAFFER: Well, you could write an equation off that chart.

CHAIRMAN WALLIS: Don't say well, I could do it. There are equations which you guys use.

MR. SHAFFER: No, we just used the chart. I mean, the --

CHAIRMAN WALLIS: Then you haven't calculated anything.

MR. SHAFFER: It's just a matter of multiplying numbers across to the other end.

CHAIRMAN WALLIS: That's what bothers me, there's no mechanics, there's no equations. It's just sort of putting in some numbers into a chart where somebody has already decided what the percents are

MR. SHAFFER: Okay. Valid criticism. They should have actually explained that.

CHAIRMAN WALLIS: Well, see, that gets back to the whole question of what's the basis of these magical percentages. I didn't really spend time on this part of it, but if it's all just magical percentages and someone's judgment, then I have a little difficulty knowing whether to say it's any good or not. It may be very reasonable, but I usually like to see some basis. And there probably is, where you actually refer to some experiment or some mechanism, or something that's calculable.

MR. SHAFFER: Well, for the volunteer plant, this was examined in some detail. For instance, the amount of fibrous debris blown upwards was over 90 percent in that analysis, so when they come along and say well, we're only going to blow 25 percent up, it's fairly easy to sit back and say yes, we think that number is going to be conservative in all cases.

MR. LATELLIER: The greatest value - excuse me, Bruce Latellier - the greatest value of this approach is that it's systematic and it's documentable. It's traceable so that the assumptions and the basis for each of those branch fractions can be examined and re-examined. The reason that it was offered as an appendix is two-fold. It offers something short of maximum conservatism for the benefit of the licensees. It also provides the staff an evaluation methodology for assessing the reasonableness of plant submittals.

CHAIRMAN WALLIS: I'm just worried that your reviewer is going to get in front of the plant and say oh, in this branch we have 61 percent, in this branch we have 49 and so on. It's going to be difficult to figure out just where these numbers came from.

MR. SHAFFER: Well, the idea is that when they apply the baseline, they're going to take the numbers recommended in the guidance and put them in their respective trees and get an answer. If the baseline doesn't result in acceptable MPSH availability, then they have to go analytical refinements. And when they go there, then it's a whole new ball game. If they want to reduce transport, then they've got to come up with a much better analysis.

CHAIRMAN WALLIS: Well, the basis -- I guess you probably have said it, but the basis of the baseline is that everything is conservative, everything.

MR. SHAFFER: Everything in the baseline

--

CHAIRMAN WALLIS: Based on experiments or something.

MR. SHAFFER: Well, the idea of the baseline was to make everything in there so conservative that you basically couldn't argue with it.

CHAIRMAN WALLIS: But conservatism cannot be based on judgment. I don't think I will accept someone's judgment that something is conservative. That's not an argument. It's got to be based on something quantitative that's deducible or measurable

MR. SHAFFER: Our acceptance of the baseline is based on our confirmatory analyses that we've done where we've done a much more thorough job of it.

CHAIRMAN WALLIS: It was based on physics and something.

MR. SHAFFER: Yes, there is physics in there. And another way to look at this work we did on the volunteer plant is to start off assuming 100 percent transport, and then go into the containment and look for specific locations where you can demonstrate that some debris is going to get trapped and stay there, and then start reducing your 100 percent down. But what we've done is work out a systematic approach for doing that, so that if -- and some of that approach I did, I used thermohydraulics from the MELCOR code to get flow splits. I used inertial impact, captured fractions that we got from test data, and in some places I just simply had to assume the worst case. I believe that the result is still conservative by a good margin, but it is somewhat better than saying 100 percent transport. That's the concept.

CHAIRMAN WALLIS: Is there any benchmark for this, like Barciback or something that actually happened that made the news, see if it works?

MR. SHAFFER: No.

MR. LATELLIER: Some of the transport fractions were determined by integrated tank testing in the NRC research programs. Clint has separated the problem into the primary physical means of transport; that being the blow-down when its initially distributed, the washdown when it returns under spray impingement, and finally the pool transport within the sump pool. And we do have some limited information on each of those phases, and it's been applied to the best extent possible.

CHAIRMAN WALLIS: Shall we move along?

MR. WAGAGE: These are baseline methodology. Only small fine debris assumed to transport on to the sump screen. This methodology does assume 100 percent of debris would settle on the sump screen, whatever comes down up there. This is conservative for head loss analysis but, however, for downstream effect it's not -- downstream effects are done separately.

CHAIRMAN WALLIS: But it might --

MR. WAGAGE: Assume that all the debris settles on the concrete for this area. Next one, please.

As I mentioned, the baseline did not assume large debris transport. Because as an artifact of these assumptions, no pool turbulence need to be calculated because that debris is assumed will end up on the sump pool. And all the small debris which came on to the active pool transported on to the sump screen.

No debris size distribution within the group. There were two groups, small fines, and large debris. In small fines, the debris was assumed to be its basic constituents, particulate and fiber. No different size distribution.

MEMBER RANSOM: If some of the transport

returns to the pools through gratings and things like that, do you assume that the gratings then screen out any debris larger than the openings in the gratings?

MR. WAGAGE: Yes. That's the assumption for not transporting large debris, assume that the largest opening of grating was 4 inch by 4 inch. Large debris size is larger than that.

MEMBER RANSOM: Point by point meaning point by point in a containment?

MR. WAGAGE: Yes. In the containment, and also the debris that gets in to the largest opening, assumed to be 4 inch by inch. Debris larger than that is not transported. This methodology assumed that debris is uniformly distributed and uniformly mixed with water, and because of that there was no intense locations need to be predicted.

The methodology did not address transient debris transport, that means at any time it did not calculate how much debris was on the sump screen, but conservatively assumed all the debris transported on the sump screen.

CHAIRMAN WALLIS: So it's a homogenous mixture on the sump screen?

MR. WAGAGE: Yes.

CHAIRMAN WALLIS: And that gets you into

trouble with this thin bed business.

MR. WAGAGE: Yes, I have thin bed -- I plan to address that thin bed in Head Loss Section --

CHAIRMAN WALLIS: It seems to me what's going to happen is do all this stuff, but since the thin bed could be worse, do you have to do that. That's going to govern everything.

MR. WAGAGE: Actually, to address your concern properly, I need to define what thin bed means, and then address that question. I'm prepared to show some tests, show some effect of thin bed, and how the baseline guidance asks the licensees to address the check of thin bed. I have prepared that report second part. If you like, I can do it now.

CHAIRMAN WALLIS: No, you're going to get to it later. That's fine.

MR. WAGAGE: Okay. Next one, please. This Section 3.6, debris transport, has analytical refinements proposed for pool recirculation transport of debris. The one method is nodal network where the sump pool is divided into several open channels, and flow is assumed to uniform across the open channels. The one draw-back in this method was that there was no debris of transport model to give you how to calculate that velocity.

The second methodology proposed was to use computation of fluid dynamics method. Some calculations were shown. That debris transport model was shown that was when the velocity became higher than transport velocity, in that particular region of the flow, debris was assumed to transport onto the sump screen. When the velocity is lower in that region, the assumption was that the debris would not transport. We had concerns in that because in that case now we need to know where the debris enters, because when debris enters at high velocity it can be directed to the sump screen.

MEMBER RANSOM: Well, were there entrainment models incorporated that would predict what velocity you needed to actually entrain the debris?

MR. SHAFFER: From our experiments -- well, first you have to break the debris down into size groups that characterize the different transport mechanisms. That was another criticism we had of the analytical refinements, is in the baseline they had two size groups which matched the simplistic models for transport. But when they go to analytical, they need to prepare a size distribution that matches realistic transport mechanism.

First of all, you have like the individual fibers. Those will stay suspended in the water. They don't settle at full turbulences that we see. The next group, and I'm talking --

MEMBER RANSOM: And those are transported --

MR. SHAFFER: Yes. Okay. I'm talking like low density fiberglass now. Okay. The next size you tend to get are you might think of them as cottonball size. And then there's the bigger portions, like what you see in the bag over there. You might have entire blankets, but these bigger ones and the cottonballs, when they are introduced into hot water, tend to saturate rather rapidly, and then they sink to the floor of the pool. So the transport then is how fast a velocity would it take to get them to roll or slide, you know, a big piece like --

MEMBER RANSOM: Just drag on the --

MR. SHAFFER: Yes, slip along.

MEMBER RANSOM: Okay.

MR. SHAFFER: Okay. Now we have some tests where we've gone in and in a flume measured the velocity it takes to just start these things moving, so their idea is to calculate the fraction of the floor velocity that is less than a transport velocity and then say okay, if it's less it doesn't move, if it's greater it does move.

A criticism we have is they don't say how the debris enters the pool. They leave it -- when you read it, you get the idea they're assuming uniform distribution. Well, that's not reality. Reality is at the end of blow-down you'll have more of it near where the break was than away from the break of initially deposited debris. Debris that's blown upwards in the containment comes down with the sprays. That means they're going to enter the locations where the spray drainage enters the pool, which happens to be more active parts of the pool.

So our criticism is you need to introduce a model that shows where the debris enters the pool, and then when you do the transport in the pool, take that into account. But those are the methods they offered, those are our criticisms. And in confirmatory analysis, we've demonstrated a more realistic approach for guidance.

MEMBER FORD: Could I ask a question? Going back to the baseline case where you talk about Nukon and you were saying 43 percent, your baseline case would be deposited in the sump.

MR. SHAFFER: Yes.

MEMBER FORD: What would the situation be - could you have a containment design and a rupture event sequence. By using this refined technique you'd come up with more than 43 percent for Nukon on the pump screen? And if so, what would you do?

MR. SHAFFER: Yes. Now we found two of the assumptions in the baseline model that we do not consider conservative; and hence, there's a couple of limitations in there to the baseline. One of those was that they assumed large debris did not transport.

MEMBER FORD: Right.

MR. SHAFFER: Okay. Now not all large debris is going to be located some place where it's going to be stopped by a grating or something. So if a containment sump is characterized as fast-flowing, then we believe they need to large debris transport in their baseline. If it's a kind of a pool where the velocities are very low, then you're down in a range where large debris doesn't move, and we accept it.

The other thing was the inactive pool fraction. Their method of calculating that is to take the volume of the inactive pool versus the total volume of water and use that fraction. But in reality, the debris is not going to be uniformly distributed in the water. In fact, a lot of it is going to get blown upwards, and it's going to come down at some time later. It takes a while to work its way down. By that time the inactive pool may already be filled, so we felt that we needed to cap that number somehow, because in their sample problem, say at 30 percent. We felt that's too high. And also, we haven't seen any surveys of the plants to know just how big that number can be, so we capped it at 15 percent. It's somewhat of a judgment call, but the judgment call came from the volunteer plant analysis. Fifteen percent on the volunteer plant was okay, even though I calculated like 3 percent going in the inactive pool. If I still allowed them to do 15 percent, we would have -- the baseline was still conservative, so we capped it artificially.

MEMBER FORD: So there will be a check --

MR. SHAFFER: Yes.

MEMBER FORD: -- that when they do the baseline calculation to assure that they right, and go through the MPSH calculation. You would be doing a double check to make sure that that baseline is conservative

MR. SHAFFER: Yes. So we believe that if they follow the limitations and the baseline package together, we believe they're going to be okay.

There's some judgment call in there, but we believe that to be the case.

MEMBER FORD: Will you be doing all these alternative things for all 69 plants?

MR. SHAFFER: Well, if they do the baseline with the limitations and they're not okay, then they have to go to analytical refinements.

MEMBER FORD: Sure.

MR. SHAFFER: Okay. Now that's a whole new ball game. If they go to analytical refinements, then they need to address the non-conservative assumptions in the baseline, as well.

MEMBER FORD: I understand that.

MR. SHAFFER: Yes.

MEMBER FORD: My question really was, is are you going to be doing these independently, are you going to be doing these analytical refinements to double check that their baseline calculations are, in fact, conservative?

MR. SHAFFER: Well that's a question for somebody at the NRC to answer.

MR. JOHNSON: No, we're not going to -- I don't want you to leave with the impression that we're going to be double-checking every one of these evaluations that are done in the course of the baseline to second-guess whether the baseline methodology is being conservative. We're going to -- we're looking for the methodology to be conservative so that if plants comply with that we can be comfortable that they're okay. And we're going to audit some plants.

MEMBER FORD: Okay.

MR. WAGAGE: We accepted the calculations because that's the preferred method for calculating pool transport as given in Reg Guide 1.82, Revision 3. Reg Guide 1.82, Revision 3 also allows licensees to come up with alternate methods, but in that case licensees have to confirm the validity of those methods with experiments so we accept it as an alternate method. We want the licensees to prove that their method is correct.

Staff gave supplemental guidance in the main body of safety evaluation. We talk about these appendices pool transport, debris transport comparison. Those are the plants, the volunteer plant analyses that we did to improve on that baseline calculation.

Staff major restrictions and limitations. Actually, Clint talked about the first two, relocation of debris into active pools and set the limit of 15 percent. And transport of large debris, we want the licensees to calculate the transport of large debris in case the pool velocities are sufficient to transport the --

CHAIRMAN WALLIS: How big is that? Do you specify what you mean by high velocity?

MR. WAGAGE: Velocity, the transport velocities, I think they're given in a table.

CHAIRMAN WALLIS: They're given in a table. Okay.

MR. WAGAGE: The last restriction is for uniform distribution of debris on the sump pool floor. That did effect that in case licensees come up with refinement for inactive pool transport in case they want to get the credit for more than 15 percent. Then as part of that assumption, the licensee assumes that -- the guidance assumes that the debris is uniformly distributed. Now the licensees have to revisit the uniform distribution of debris in case 10 to 15 percent is going to be throughout.

To conclude my part, using NEI's calculation or methodology, and staff's supplemental guidance, and restrictions and limitations, one can calculate conservatively mass of debris being transported onto the sump screen.

The next one is a summary of my presentation.

CHAIRMAN WALLIS: Now you have another presentation to follow this one.

MR. WAGAGE: Yes.

CHAIRMAN WALLIS: Is this a good time to take a break? It's a good time to take a break now. And then when we come back, we will go as long as we think is reasonable. We do have some flexibility tomorrow. I think tomorrow we have matters which are not really being considered very much, so they shouldn't take very long. I mean, there's nothing much to say on guidance about chemical precipitation because there isn't any guidance, and there's not much to say about downstream effects because there isn't any guidance. So maybe we can move along quickly tomorrow, and perhaps something -- if we're too late today, we may have to put off the very last item, but we'll try not to. So let's take a break for 15 minutes. We'll come back at five minutes to 4. Thank you for your presentation, and we'll see you after the break.

MR. WAGAGE: Thank you.

(Whereupon, the proceedings in the above-entitled matter went off the record at 3:39 p.m. and went back on the record at 3:58 p.m.)

CHAIRMAN WALLIS: We are looking forward to finishing our day on a high note.

(Laughter.)

MR. WAGAGE: Good afternoon again. My name is Hanry Wagage. I also reviewed NEI Guidance 70.7 on head loss. Clint Schaffer helped me with this section.

This is the summary of my presentation. NEI's guidance on head loss is generally acceptable to the staff. NEI did not propose any analytical refinements for head loss.

Staff provided supplemental guidance in the main body of the evaluation and one in one appendix. And also staff gave some restrictions, one restriction or limitation for this capability.

Using the NEI methodology on head loss evaluation, one can reasonably predict the head loss across sump screen.

In my previous presentation, I discussed about transporting most of the debris onto the sump screen. Now next in this section, it is to evaluate the head loss across the debris bed.

The purpose of evaluating the head loss across the debris bed is that it is in the sump performance criteria. I'm going to show you in the next review graph this is -- compared to the previous problem of transporting debris from various places to the sump screen, this is not that complicated if you look at it. But that means that what you need to calculate is the head loss across a debris bed formed on the sump screen.

But this is a complex problem because the structure of the debris circling on the sump screen and how the flow goes through these various tortuous parts in the debris bed. It depends on all these effects the head loss across the debris bed.

Next please. Sump performance criteria is for fully submerged sump screen. Sump is assumed to fail when head loss across the debris bed is greater than the implicit modeling.

Implicit modeling is the difference between implicit available and implicit required. Implicit required is given by the pump manufacturer and implicit available is calculated according to the plant's licensing basis.

For partially submerged sump screen, in addition to that head loss across the sump screen greater than implicit modeling, there is another failure criteria which is when the head loss is greater than half of the submerged height, sump is assumed to fail. These are given in Reg Guide 1.82.3.

Next please. Baseline methodology calculates head loss across fiber bed. There are various kinds of fiber beds. One is debris bed. One is fiber and particulate.

To form a debris bed with particulate, particulate being a smaller size, they can pass through the sump screen. That means there has to be something to hold the debris of particulates. In this analysis, the baseline assumes --

CHAIRMAN WALLIS: I'm sorry. I'm thinking about the partially submerged sump screen.

MR. WAGAGE: Yes?

CHAIRMAN WALLIS: I'm not sure when you want to talk about it but this is one where you have a screen and you have a pool. And the fluid flows through the screen and there is a lower level on the downstream side.

MR. WAGAGE: Yes.

CHAIRMAN WALLIS: And so presumably the head loss varies with height on the screen --

MR. WAGAGE: Head loss --

CHAIRMAN WALLIS: -- because at the top there's no head loss because there's no driving force.

Then as you get more and more depth in the water, you get more driving force so it's a varying --

MR. WAGAGE: Actually the head -- we're talking about the head loss across the debris bed.

CHAIRMAN WALLIS: That's right.

MR. WAGAGE: And that increases because the flow rate increases.

CHAIRMAN WALLIS: But where the debris bed is on the surface, the free surface, there's no head loss because there's no flow.

MR. LATELLIER: That's correct, Dr. Wallace.

CHAIRMAN WALLIS: So there's something -- how do you --

MR. LATELLIER: The point is --

CHAIRMAN WALLIS: -- define head loss when it varies with depth?

MR. LATELLIER: The point of this is that the static head of the pool on the upstream side of the screen is the only driving force available.

CHAIRMAN WALLIS: But that's why you have this -- half the submerged screen?

MR. LATELLIER: Yes.

CHAIRMAN WALLIS: What do you mean by head loss when it varies from top to bottom of the screen?

It's not uniform so how is it defined? Is it clear how it's defined?

MR. LATELLIER: The calculations that we've done looking at debris bed formulation under the static pressure gradient lead us to suggest that the average static head is what provides adequate flow.

CHAIRMAN WALLIS: So what's meant here is the average head loss?

MR. LATELLIER: Essentially that's correct.

CHAIRMAN WALLIS: And that's clear?

MR. LATELLIER: And if the total head loss is greater than that, then you will not provide adequate flow.

CHAIRMAN WALLIS: Yes, I think that's right. It's just the definition of head loss has to be clear. Otherwise it may be computed in some way that's inconsistent with the criteria.

MR. WAGAGE: As I was talking, to form a debris bed with particulate, there has to be fiber to hold the particulates. For RMI, it can form a debris bed without any other debris.

CHAIRMAN WALLIS: Can -- Cal-Sil can form a debris bed without anything else?

CHAIRMAN WALLIS: Yes. Cal-Sil is an

exception for particulate.

CHAIRMAN WALLIS: There's not overwhelming evidence but there's still evidence?

MR. WAGAGE: Actually I noticed that part of overwhelming evidence because for first people to experiment that maybe one may be overwhelming evidence. But when I noticed that it was not overwhelming evidence, I took that part off. I said there is experimental evidence that Cal-Sil without fiber would form a debris bed.

CHAIRMAN WALLIS: But still they have to -- if they don't have fibers -- if they have Cal-Sil, they have to assume it can form a debris bed?

MR. WAGAGE: Yes, we gave that guidance when we addressed Cal-Sil in the safety evaluation.

CHAIRMAN WALLIS: And there's no need to have an eighth of an inch of fiber or anything like that? They just have Cal-Sil.

MR. WAGAGE: For Cal-Sil, yes.

CHAIRMAN WALLIS: Okay.

MR. WAGAGE: And the other kind is mixed debris bed, any combination of these debris.

Thermal-hydraulic parameters considered were water level, the guidance asks licensees to assume minimum water level in the pool but that would not effect --

CHAIRMAN WALLIS: Excuse me, to go back to the statement that one square foot of material could clog a small screen, that's one cubic foot of Cal-Sil would clog a screen if it were the small one.

And that's not much if I look -- you've said this before, it's about a couple feet length of pipe or something is enough, if transported to the screen, to clog the smallest screens that are in existence. That puts some perspective on the nature of the problem then.

And I noticed the NRC contractors nodding. Does that give consent what I just said?

MR. SHAFFER: It makes sense to me. We don't have -- actually determined what minimum layer of Cal-Sil that it takes but it probably is not a very thick layer.

CHAIRMAN WALLIS: That's a pretty -- it sounds a pretty dramatic conclusion to me that this small amount can have that big an effect.

MR. SHAFFER: Yes. For these small sump screens, it doesn't take a lot of debris to block them.

MR. WAGAGE: The guidance is to assume maximum flow rate across the maximum pump flow rate so that it would give the maximum flow rate across the debris bed which would be a higher head loss.

Guidance gave three options to use the temperature of the pool water. Of the three options, we recommended -- we accepted using minimum water temperature for calculating head loss across the debris bed.

MEMBER RANSOM: Is there any provision for sumps that -- with, I guess they're partially submerged screens, but as you fill up the one side of the sump, you simply raise the water level. And, of course, it pours over and begins plugging up progressively.

And taking that -- is there any way to take that into account? Or are there any designs that that would even be a factor with?

MR. SHAFFER: The water level up against the sump depends on the water inventory primarily. And the -- it depends on how much water is being held up in various places in the containment. So these kinds of calculations have already been done by the plants.

Once the water levels start dropping behind the screen, it won't drop too much if it's working normally. If we start to get a real head loss, that water level behind the screen is going to drop real rapidly. And in that case, you're not going to have time for water to build up on the other side.

CHAIRMAN WALLIS: Yes, you reach essentials like critical flow through the screen and you dry it out on the downstream side. You just suck away all the water there and then you start ingesting air.

MR. WAGAGE: The kind of debris considered were fibers insulation, RMI coatings, concrete, dust, dirt, and Cal-Sil.

MEMBER RANSOM: Along that line, though, what do you normally do? If there's hold up somewhere else and the sump goes dry, what happens?

MR. SHAFFER: Well, if the sump goes dry, your pump is going to cavitate.

MEMBER RANSOM: Sure.

MEMBER SIEBER: Ruining the pump.

MEMBER RANSOM: Well, do you shut it off? Or --

MEMBER SIEBER: It will shut itself off if you don't shut it off.

MEMBER RANSOM: Okay. You're saying it will shut itself? Well, of course, if it cavitates, it destroys itself.

(Laughter.)

MEMBER RANSOM: I'm assuming you could have water hold up for other reason that there's no return. And --

MR. SHAFFER: Well, water will hold up in any number of places under normal operating conditions. For instance there's some water on the floors. It takes a certain water level just to overflow the drains. There's water flowing in with the sprays. There's film flows. Water in the pipes.

And when they do these minimum water level calcs for the sump pool, they include all of these factors.

Okay, the one thing that might not be included there is something that will come up on upstream effects, I mean what happens if you block the drain holes and all of a sudden you get more water held up due to debris blockage than had been previously calculated? That is a subject for the upstream effects.

MEMBER RANSOM: So I'm surprised it's not like my basement sump pump, you know, it goes dry, well it just shuts off and waits for the water to build up and you turn it back on.

MR. SHAFFER: That's a question for

somebody that --

MR. HAFERA: This is Tom Hafera, you understand how the LOCA event progresses. By the time you get on the recirc mode, you have about three to six feet of water on the containment floor. The sump is not going to go dry. The sump is maybe 10 feet by -- or 12 feet by 12 feet. The containment is 130 feet in diameter.

MEMBER RANSOM: Well, what you're saying there counters what you just said.

MR. HAFERA: The little bit of center is going to be full of water all the time. What will happen is you lose suction, you'll cavitate the pump because it will saturate and get air bubbles, you know, you'll get water bubbles, right?

MEMBER SIEBER: Well, you get steam pockets.

MR. HAFERA: You get steam pockets, yes.

MEMBER RANSOM: Well, my original question was whether or not you could hold an entrance to the sump and the water levels simply build up and spill over and continue until, I guess, in time, of course, you could plug the entire thing.

MR. HAFERA: It's not going to be dry during a recirculation phase or event. That's why your refueling water storage tank has 300,000 gallons of water.

MEMBER RANSOM: Yes, but we were talking about the partially submerged sump screens. And I guess there are sump designs like that.

MR. SHAFFER: Well, most of the water is going to -- the majority of the water is probably going to be in the sump pool already. They design the drains to try to minimize water hold up.

So you're saying if you just wait a little bit, some of this water will come down and your water level will come back. And that, if it exists, is not something you can rely on.

MEMBER SIEBER: Well, the difficulty is do you ruin the pump during the period of cavitation? Do you break the shaft? You're taking chunks out of the impeller as the steam is collapsing up against the blades? So a pump with a lot of horsepower will not run very long in that condition without major mechanical problems.

Most of these pumps are deep draft pumps which are subject to vibration. And so you have a tendency to either break a coupling or smash a bearing or something like that just from the vibration. So cavitation is serious.

And the operator can tell whether it's cavitating or not because the flow meter will go up and down. And also the pump current will do that, too.

PARTICIPANT: And it will make a lot of noise, too.

MEMBER SIEBER: Yes, but there's nobody to listen to it. I mean --

(Laughter.)

MEMBER SIEBER: -- if you know what I mean.

PARTICIPANT: Yes, I know what you mean.

CHAIRMAN WALLIS: Well, there's two things. There's not enough positive suction head, which gives you this cavitation with a submerged screen. But if you have a partially submerged screen, you can get to the point where the pump is trying to pump more water than can run into the --

MEMBER SIEBER: Yes, and then --

CHAIRMAN WALLIS: -- sump well.

MEMBER SIEBER: -- then the downstream side of the level goes down --

CHAIRMAN WALLIS: Then it has to suck in air or whatever is there because it's --

MEMBER SIEBER: Well, it will cavitate

before it gets to air.

CHAIRMAN WALLIS: -- trying to pump out more water than it can get.

MEMBER SIEBER: It will cavitate before it gets into sucking air.

CHAIRMAN WALLIS: Okay.

MR. WAGAGE: Well, the failure criteria does say that when the head loss causes the water level to drop to half, it is assumed to fail.

MEMBER SIEBER: Yes. And the height on the upstream side of a vertical screen is determined by how big the RWST is. You know you could have one that's 50 feet high. But the level will only equal the volume in the RWST fit into the volume of containment. So that's five, six, seven feet. And so that's the limit.

CHAIRMAN WALLIS: And that's one of the principles we all agreed to which is conservation of mass.

MEMBER SIEBER: Yes. It's one of the things --

CHAIRMAN WALLIS: We have a little more trouble with --

MEMBER SIEBER: -- that works every time.

MR. WAGAGE: Baseline guidance is to use

head loss correlations when available and do testing when it is not available. Head loss correlations are equations which fit test data.

CHAIRMAN WALLIS: You can have some sort of a mechanistic basis probably.

MR. WAGAGE: Mechanistic basis for what? I'm sorry.

CHAIRMAN WALLIS: These head loss correlations have some basis in physics. They do. They're not just fitting data. They do have a basis in terms of the history of development of logical pressure drop models for flow through things. There's a long history of that. It's not just correlation that's pulled out of the air.

MR. WAGAGE: Yes, I agree that when there is more physics involved, that it's better to go to a region where it does not have data.

CHAIRMAN WALLIS: And you guys are pleased with this correlation? And it satisfies all your criteria for validity?

MR. WAGAGE: I was going to address that later. This correlation, we received your comments and concerns on the validity of this correlation for PWR sump performance. The Office of Research is addressing that in parallel. This correlation has been tested for different materials.

And we have two parts to address here your concerns. One is Office of Research confirmed these correlations for test data. After my presentation, a staff member from Office of Research is going to present the comparison of correlations to data.

The second part is that we asked licensees to validate the correlations to their application. We added a significant amount of guidance in our Appendix 5, Staff Supplemental Guidance.

One, one on using the correlation application methodologies. Second one from open literature we found for what conditions, what parameters, and what debris this correlation has been validated.

Where licensees can find the application fits within those ranges, the licensees can use it. If not, the licensees have to validate for their applications.

In the baseline guidance, NEI also recognized that this correlation would not -- has not been tested for all the available debris. In that case, NEI guidance asks the licensees to confirm that.

CHAIRMAN WALLIS: Now this head loss correlation has three pieces. There's a formula for head loss. There's a formula for the compression under the head loss or whatever it is. And there's another formula for the maximum density which is achievable, the sludge density. And this is for mixed beds.

But if you have Cal-Sil by itself, it's not supposed to compress. So you know its density. So that part, that doesn't matter. And the compression is known. It doesn't compress.

So you only need to use Cal-Sil alone -- if you have the thin bed or the bad thing is the sandwich which has a layer of Cal-Sil alone in it. For that piece, you only need to worry about the head loss part of the correlation, is that -- am I correct here?

You don't have to worry about compression? It's already at the sludge limit so you don't need to worry about that. You don't need to worry about compression because you assume that Cal-Sil doesn't compress. Are those true statements?

MR. SHAFFER: The 6224 correlation requires some fiber to be in it. It's the way it was constructed. And you could apply it to the Cal-Sil only bed by tricking it into thinking -- just putting in some tiny, tiny quantity.

But when you were to operate the correlation as a package, the limitation equation would be what would control, not the compression equation.

CHAIRMAN WALLIS: Well, there is no compression with Cal-Sil.

MR. SHAFFER: Right, right. So --

CHAIRMAN WALLIS: So it's already at the limit.

MR. SHAFFER: Right. So if you put in some tiny, tiny amount of fiber, it would all fall out.

CHAIRMAN WALLIS: You don't need to put any fibers in. I mean the correlation doesn't know what you're putting in. You're just putting an SV into the correlation and calculate, go ahead and calculate.

MR. SHAFFER: I believe that -- well, the correlation has a mass ratio. It has the particulate to fiber mass ratio.

CHAIRMAN WALLIS: That can be zero, though. The fiber -- the correlation that has an SV, it doesn't have any ratio. It just has an SV.

MR. SHAFFER: Well, to use the correlation as it is written and as it is programmed in the blockage code, you would -- if you put in zero for fiber, it would say it was dividing by zero.

CHAIRMAN WALLIS: That's a silly way to calculate SV because if you've got Cal-Sil, you know what SV is. You don't need to --

MR. SHAFFER: Right.

CHAIRMAN WALLIS: -- postulate something which doesn't exist and then divide by it. You don't need to do that.

MR. SHAFFER: Well, you do have to modify the correlation a little bit for Cal-Sil alone.

CHAIRMAN WALLIS: You do?

MR. SHAFFER: Well, I mean you can't give it a mass ratio that's infinity.

CHAIRMAN WALLIS: But you don't -- the equation for the head loss only has SV in it. And if you know SV for Cal-Sil, you just put it in there, right?

MR. SHAFFER: Right. Okay.

CHAIRMAN WALLIS: But the other thing is the peculiarity of your code.

MR. SHAFFER: Right.

CHAIRMAN WALLIS: Okay. So we've established that.

MR. SHAFFER: Okay.

CHAIRMAN WALLIS: If you know SV, you can use the correlation.

MR. SHAFFER: Right.

CHAIRMAN WALLIS: Right. And for Cal-Sil, you don't need to worry about compression or anything. So the only question is what SV do you use for Cal-Sil, right?

MR. SHAFFER: Right.

CHAIRMAN WALLIS: That's the only question that survives is what SV shall I use for the worst case, which is a piece of the sandwich, which is only Cal-Sil?

MR. SHAFFER: Right.

CHAIRMAN WALLIS: That's the only question we have left if we accept that as the worst case. Forget about all this other stuff. The only thing that matters is we've got a piece of the sandwich which is only Cal-Sil. What do we use for its specific surface area?

MR. SHAFFER: We have not derived a specific surface area for Cal-Sil alone.

CHAIRMAN WALLIS: But that's the limiting case that everyone is worried about. That's the thin bed. And that is the issue, isn't it?

MR. SHAFFER: We have not done it yet.

CHAIRMAN WALLIS: Well, we can talk about it later on if you like. But isn't that it? Well, who is going to tell me what a thin bed is? I think we're going to find out it's a bed which is stuffed with Cal-Sil to the gills, right?

MR. SHAFFER: Okay.

CHAIRMAN WALLIS: And it's essentially all Cal-Sil in the worst case.

MR. SHAFFER: But your comment is valid. We have not done that little piece of the puzzle yet.

CHAIRMAN WALLIS: But this is the key thing, right?

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: And if I look -- I don't know if I want to keep making this speech but if I look at the one experiment where you've got Cal-Sil, you had a specific surface area of 270,000 where you've managed to get Cal-Sil alone to make a bed.

When you had a thin bed later on, which gave an unexpectedly high pressure drop, this became 880,000.

MR. SHAFFER: When I evaluated those tests and I looked at the photos of the debris beds, I felt that there was considerable flow bypass. I do not trust that 270,000 number. And we're not recommending

--

CHAIRMAN WALLIS: So you think it's bigger than that?

MR. SHAFFER: Yes, our recommended numbers are much bigger than that.

CHAIRMAN WALLIS: All right. I understand that. So we should forget about the 270,000?

MR. SHAFFER: Forget about it, yes.

CHAIRMAN WALLIS: Forget about it. Okay. That's good. So I don't have to worry about that. All I have to focus on is the 880,000 or whatever it is that --

MR. SHAFFER: Is that sufficient? That's your question. And I cannot answer that.

CHAIRMAN WALLIS: So all we need to worry about, it seems to me, is since most of these plants are going to have a thin bed, is put all that Cal-Sil that's in the thin bed, use 880,000 and calculate it. That's all you have to do.

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: None of this other stuff matters, compression and all that matters at all.

MR. SHAFFER: Yes.

MR. KROTIUK: That is the intent of our experimental program.

CHAIRMAN WALLIS: So why don't we just say that in the guidance and forget about all this other stuff which is very controversial about compression and all this stuff which --

MR. ELLIOTT: Dr. Wallace?

CHAIRMAN WALLIS: Yes.

MR. ELLIOTT: Not everybody has Cal-Sil. It's not --

CHAIRMAN WALLIS: Well, I know, but if they do --

MR. ELLIOTT: -- a given that every plant has that problem.

CHAIRMAN WALLIS: -- those who have the thin bed effect, can calculate --

MR. ELLIOTT: Well, those that do --

CHAIRMAN WALLIS: -- a very simple thing without worrying about all this other stuff.

MR. ELLIOTT: Right. But the other stuff would still apply to the plants without Cal-Sil.

CHAIRMAN WALLIS: Oh, yes, I agree with that. I agree with that. But those that have this so-called thin bed effect, which maybe needs a different name, have a very simple calculation to make, it seems to me. That's all they need to do.

And I think that might help us a lot in

figuring out which parts of this correlation we need to worry about. But if they don't have the thin bed effect, then maybe we do have to worry about whether you've calculated the compression right and all that sort of thing.

MR. SHAFFER: I agree. There is a step there that we should add in there someplace that specifically says what you just said.

CHAIRMAN WALLIS: Are you going to rewrite the SER in terms of these sorts of things that come up in these meetings?

MR. SHAFFER: I would anticipate maybe we would add a little --

CHAIRMAN WALLIS: See, this subcommittee, I'm not sure what it's going to decide but we might, as we have often done in the past, say you guys are not ready to go to the full committee because there are so many things you need to fix up that you have to go away and do your homework and come back.

That's what we do about many things. We say you're not ready to go to the full committee. But it seems to me that you're probably going to say we can't do that. We're driven by schedule. We have to go to the full committee with what we've got even if we can't defend it. That's not a very good state to be in because if you may then --

MR. JOHNSON: Dr. Wallis?

CHAIRMAN WALLIS: -- you may then get a very critical letter from us which I don't want to write.

MR. JOHNSON: We do feel we have to go to the full committee. We are taking notes and we have talked about a number of areas where we expect to make some changes to the SE based on the input that we've gotten today. I hope --

CHAIRMAN WALLIS: So what are we going to be reviewing at the full committee then if you're coming to that? Is this going to be something different?

MR. JOHNSON: Well, I think we'd like to do it the way we did the generic letter where we come to the full committee with an addition to what we've already given you sort of a red-line, strike out, if you will, you know, here are some of the things that we've done in response to the direction or the input that we've gotten from the subcommittee. That's the way I would try to approach that.

CHAIRMAN WALLIS: But the thing is if this changes -- see if it changes significantly, we may not have time to evaluate the changes.

MR. JOHNSON: Yes, absolutely. I mean that is true if it changes significantly. I'm hoping we don't have to make significant changes to the SE.

MR. LU: Dr. Wallis? This is Shanlai Lu from Plant System. I just wanted to add one point there. And when we calculate the Cal-Sil, the head loss across the Cal-Sil bed, actually the Cal-Sil itself has a certain percentage of fiber inside that.

It simply reaches the sludge limit for the cases we observed from the tests. So it still has to rely on the compression -- compressibility but you reach the sludge limit.

CHAIRMAN WALLIS: It doesn't compress. It's already at the sludge limit.

MR. LU: Yes, once you reach sludge limit, you use sludge limit. Yes, you are right.

CHAIRMAN WALLIS: Yes but it's never not at the sludge limit. Cal-Sil alone is always at the sludge limit as I understand it. The fact that it has fibers in it is irrelevant. Your assumption is that it's always at the sludge limit. You never let the Cal-Sil swell up to a bigger size than the sludge limit.

You only bring in the sludge limit when you start to add Cal-Sil to something like Nukon, which is compressible as I understand it. So I don't think you need to worry about -- and how many fibers are in Cal-Sil is completely irrelevant. The only thing you care about is what's its SV that you put in the equation.

MR. HSIA: This is Tony Hsia from Research. I would like industry to help me validate that.

But I thought most of so-called Cal-Sil plants, like Rob Elliot said, it's not -- a few of them are Cal-Sil plants. But those who are Cal-Sil plants, those also have fibers. So I don't know of any plant in industry, please correct me if I'm wrong, that's 100 percent Cal-Sil and nothing else.

CHAIRMAN WALLIS: No, listen. I just agreed, I think, with Bruce that the worst case is a sandwich where a layer is pure Cal-Sil. I don't care about anything else because that's my limiting case. So I'll calculate that.

MR. HSIA: My question is, you know, let's find out if that's realistically the case.

CHAIRMAN WALLIS: But it doesn't matter if you've got some fibers and things.

Well, I thought we'd already established that in the discussion today. I think that's -- if the sandwich has a layer which is pure Cal-Sil, that's what you got to calculate right.

MR. LU: Dr. Wallis, if you do have a sandwich-type phenomena, if you think about the debris transport itself, when you have the Cal-Sil debris coming in, you never can guarantee you have one layer of pure 100 percent Cal-Sil inside one layer of a sandwich.

CHAIRMAN WALLIS: You'll never have a thin bed effect if you don't have this compression to the sludge limit.

MR. LU: That's the --

CHAIRMAN WALLIS: You won't have a sludge limit unless you have enough Cal-Sil there.

MR. LU: That's right. Well, we are going to talk about a sludge limit for the thin bed.

CHAIRMAN WALLIS: Well, maybe we'll talk about that again. But I thought I was trying to help you. And I thought that I saw your contractors nodding and agreeing when I said something about all you have to do with the Cal-Sil plants is assume a thin bed and calculate it and it's all Cal-Sil.

MR. LATELLIER: If I could add two observations to this discussion, first of all, the fiber fraction inherent to calcium silicate is part of what enables it to form a layer on an existing screen. It's part of its internal support mechanism that lets the bed form.

The second point is that if Cal-Sil is present in combination with fiberglass debris, you should not forget, you should not neglect the contribution to head loss of the presence of that fiber.

And perhaps that's part of the disagreement here of Dr. Wallis is simply saying that the head loss is dominated by the presence of the particulate bed and that the sludge limit is -- the characteristics of the sludge limit need to be accurately characterized. They need to be quantified so that we can properly address head loss across both.

MEMBER SIEBER: But if you limit yourself to the Cal-Sil, that's non-conservative because there are other components.

MR. LATELLIER: That's correct. You cannot forget about the other constituents of a mixed bed.

CHAIRMAN WALLIS: But then you have the question of how is the sandwich made up. Do you put one layer down first and then another? All right?

And I think we agreed that if you have a layer which is the sludge limit, that tends to dominate everything no matter where it is. If it's on top, or the bottom, or inside the bed, it dominates everything.

MR. LATELLIER: What happens is if -- in the case that you postulate where the calcium silicate is on top, that drives the underlying fiber to its maximum compression. But the presence of that fiber induces an additional pressure drop. Now how it compares to the contribution due to Cal-Sil is a matter of quantity.

MEMBER SIEBER: But those head losses are added.

MR. LATELLIER: In that scenario, certainly they are.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: But the thin bed pressure drop is not greater than it would be for Cal-Sil alone, is it?

MR. LATELLIER: I don't think that there's any distinguishing features between the two cases.

CHAIRMAN WALLIS: Because we don't know what it is for Cal-Sil alone because we haven't answered for that. But I think, if I follow your logic about the thin bed effect, although this isn't proven, your hypothesis is that it's due to a compression to essentially the sludge limit, which is what you'd get if you had Cal-Sil alone.

MR. LATELLIER: That's correct. And that's the intent of our recent series of experiments.

CHAIRMAN WALLIS: Although we don't know what the SV is. We do know what it is in what appear to be these thin bed tests. And that's this 880,000. And the inference is that if we could do the tests, this might be the value for Cal-Sil alone.

MR. LATELLIER: That's correct.

CHAIRMAN WALLIS: All right. So I think we're getting there until the staff indicates something else. Well -- so this head loss correlation, if it does have some statements in it which are wrong, shouldn't you say so?

MR. WAGAGE: If it is wrong, that's true. But this has been used for BWR sump performance and there has --

CHAIRMAN WALLIS: Well, let me give you an example --

MR. WAGAGE: Yes.

CHAIRMAN WALLIS: -- that if you take the equation for the sludge limit -- and I'm being very specific in the formula given in NUREG/CR 6224 and you say let's take the case where we've got some Cal-Sil but there's so little that it's not going to have any effect at all. The equation predicts that the fiberglass compresses to the sludge limit, which is the density of Cal-Sil.

Well, how can fiberglass compress to the density of something which isn't even there? It just makes no sense whatsoever. And if compresses -- and if you have to say well, we've got some sludge from a BWR, which is 65 pounds per cubic foot sludge limit, but it isn't really there, your equation says that your fiberglass compresses to 65 pounds.

So something which isn't there at all in the equation limits the compressibility of the fiberglass itself. Now this is so absurd. And if you derive the proper formulation for the sludge limit, this doesn't happen, you know?

This is something so obvious that it would seem to me it ought to be noted and maybe stated that it's incorrect. Otherwise other people may use this for some other purpose or even in this application, the equation for the sludge limit as it appears in this NUREG may be used and may give absurd results.

And that is not something we really want to see. And so I'm -- there must be a way to say look, this equation cannot be right. It may be usable over some range because it may give a reasonable approximation or something. We recognize that it's not correct. And in future editions of guidance, maybe we can correct it or something.

But I don't think you can ignore, once it's been pointed out, that an equation is not physically correct. And in some limits, gives some absurd results. That's something that once it's been pointed out, I don't see how you could ignore it.

MR. HSIA: Dr. Wallis, this is Tony Hsia from Research again. I would like to put this whole thing in perspective.

We can present, I think Clint will be ready to present the applicability range of this correlation. This correlation cannot be applied, I'll be the first one to admit, that anything under the sun you want to apply it to. That would be totally wrong.

So what we need to do is clearly define how this correlation is developed, what are the range of parameters that should be used. And leave it as that.

You may be right. I'm not even -- you may be right to some extreme cases which may or may not happen. In real world, with 100 percent Cal-Sil and nothing else plant that may not work very well. But if you put in perspective, I would like to say that based on what I have seen, this correlation is good for a lot of applications. We just have to know how and when to apply it.

CHAIRMAN WALLIS: This correlation is being used for predicting what happens on a sump screen when it's all Cal-Sil, when there's no Cal-Sil, and everything in between. It's not, as I understand it, being used for only one particular ratio of particulates to fiber.

MR. HSIA: I agree.

CHAIRMAN WALLIS: It can be used for everything, right?

MR. HSIA: Well, that was --

CHAIRMAN WALLIS: So if it gives absurd results in part of this range, that has to be pointed out.

MR. HSIA: I agree with that. That's why I'm saying in reality, if there's no plant with 100 percent Cal-Sil everything else, we don't have to worry about it.

If, indeed, we have identified plants with 100 percent Cal-Sil and everything goes to the screen and if we can -- if we realize or identify the case which this correlation does not apply, we certainly will identify that and make it very clear to the user.

CHAIRMAN WALLIS: This is going to be in the revised SER?

MR. HSIA: I would recommend that to the NRR to do that if that is the case.

MR. LU: This is Shanlai Lu, Plant System. Yes, that's the case and we define the application range and the limit, and then anything beyond the test of the data, the range we defined, then the industry has to validate the current use of NUREG/CR 6224 against test data.

CHAIRMAN WALLIS: So you are going to qualify the use of this correlation rather than just blindly accepting it?

MR. SOLORIO: Yes, Dr. Wallis, this is Dave Solorio. In the version of the safety evaluation you have, we don't have that data in there yet so we need to share it with Ralph so he can share it with the rest of you.

CHAIRMAN WALLIS: I just think that how you qualify it will probably require some more research because there are some not insignificant questions about it.

And the database on which it's validated is a very sparse database with only certain ranges of particulate to fiber proportions with certain ways of laying down the fibers and the Cal-Sil in various orders. And it's not something that really covers the range of interest for LOCA analysis.

So if you start to get into this -- if you agree to start getting into this business of if finding the range over which you can use the correlation for what, you're going to get into a research program of a year. And that's not, I think, what you want to do.

So you maybe agreeing to do something that you cannot do without more knowledge. I don't know how you do that.

MR. LATELLIER: Could you clarify your comment about it hasn't been tested in the range of applicability for LOCA conditions?

CHAIRMAN WALLIS: Well, as I understand it, you did a lot of tests. The only tests which seem to be consistent enough to be correlatable were those in which you had one part of fiber and half a part of Cal-Sil.

You had one part of Cal-Sil to two parts of fiber, did some experiments. There were some anomalies in some of the experiments but there were some experiments.

Now if someone comes along and says I want to apply this to a situation where I have five parts of Cal-Sil to one part of fiber, which could occur, they're just going to use the correlation.

There's no validation of that for that particular ratio. They're just going to use it even though it may be that some of the equations, for reasons which may be valid or not, have been questioned over that range.

So I don't see you have the base for these very small number of tests to extrapolate to all the conditions that are going to happen in a plant and say the correlation is valid.

Now if you had a wider matrix or something and if correlation always worked with no fudging of the coefficients, no adjustment of anything, you might say -- you might have a better argument. I just don't see how you can say that you have good enough technical base to know --

MR. LATELLIER: We actually --

CHAIRMAN WALLIS: -- the bounds of this correlation as it applies to a plant. You do know that you have to fix it up for certain situations even in the experiments you've already done.

MR. JOHNSON: This is Mike Johnson. We actually had research keyed up to give a presentation including, I guess Tony and other folks, about the experimental data.

CHAIRMAN WALLIS: Do you want to do that tomorrow morning?

MR. JOHNSON: And I wonder when there is a good time to do that.

CHAIRMAN WALLIS: I think tomorrow morning we probably could move along quickly because we have discussions of guidance where there's no guidance unless I'm mistaken.

MR. JOHNSON: Okay.

CHAIRMAN WALLIS: That should take a little time perhaps unless you have more to say about chemical effects.

MR. JOHNSON: Maybe we can --

CHAIRMAN WALLIS: Maybe you can do that tomorrow morning?

MR. JOHNSON: Okay.

CHAIRMAN WALLIS: Okay, go ahead.

MR. JOHNSON: Is that -- Tony, I'm looking around.

MR. HSIA: Okay.

MR. JOHNSON: Okay? Does that work?

CHAIRMAN WALLIS: Is that okay with the Committee if we hear more about that?

But this is data over a very limited range, right?

MR. HSIA: We can do a presentation of 6224 correlation --

CHAIRMAN WALLIS: For some of the experiments done by LANL?

MR. HSIA: That's right.

CHAIRMAN WALLIS: You can do that? That's good.

MR. HSIA: We can do it now.

CHAIRMAN WALLIS: You can do it now. That's okay.

MR. HSIA: We are prepared to present another table that shows the range of applicability of different parameters depending on the material you are faced with. That will give you --

CHAIRMAN WALLIS: Oh, that's a very different thing. The correlation -- having success with a few tests is very different from saying what's the basis for extrapolating it to a lot of other conditions.

MR. HSIA: From the test -- we have test data to be able to validate that correlation for these applications. And these are recommended guidance.

CHAIRMAN WALLIS: Okay, are you going to do that now?

MR. HSIA: If you would prefer.

CHAIRMAN WALLIS: That would be much better than just talking about it.

MR. LU: Okay. Let's do it now.

CHAIRMAN WALLIS: Okay.

MR. LU: We prepared this set of presentation in response to the ACRS comments. So we don't put it on the CT on this regular PC. We are setting it up now.

And one point I would make is that we do not anticipate that beyond the testing data range and then if you are comfortable, then anybody can extrapolate the correlation of the application range beyond the range we define. And that if they want to use it, they have to do additional validation tests.

But right now we have done so many tests so far, I think that's a good stepping stone for anybody to use this correlation for further application.

MEMBER SIEBER: I saw smoke coming out of that earlier.

MR. KROTIUK: While we are setting that

up, let me just introduce myself. I'm Bill Krotiuk. I'm in the Office of Research. And I'll just --before the slide comes up, I will just sort of introduce what I will be presenting.

What I basically did is that using the NUREG correlation for head loss, I used this correlation to show that the correlation, if used with appropriate properties for the materials, SV plus other properties, that it would match the test data, some of the test data I chose. some of the points, some of the test data would do that comparison well.

And then using the bounding conservative values for SV and other parameters, that it would bound the head loss that would be calculated with the correlation as compared to test data.

Okay, good. This is basically what I'm saying. And to do this also, I compared the existing correlation, the proposed NUREG correlation, to more theoretical forms of correlation and they're -- basically it's called the Ergun equation and it's listed in various books.

And made some adjustments with that to see how a more theoretical basis of form of equation would match the correlation that is proposed in the NUREG. So just to review what we have here, the head loss relationship, and it is doing the calculation using the NUREG methodology, so it includes the compressibility effect. And basically that's the two equations that are indicated there.

So what I'm going to show is comparisons to data for the NUREG correlation indicated there. And I should say that the correlation for head loss is broken up into two parts. There's a laminar part and a turbulent part.

So the first part on the left of the -- in the NUREG correlation on the left of the addition sign is the laminar part and the right side is the turbulent part.

I modified it a little bit to include the NUREG correlation for the laminar part and a form that is specified in the Ergun relationship. And the main difference is is that the porosity in the lower -- in the denominator is an EQ rather than a single porosity value.

Next one. And then using this same methodology, I compared it to the Ergun equation, which is again the theoretical basis of the equation, for a cylindrical-shaped debris and also for spherical-shaped debris.

These are the six tests that I chose just to compare it. Three of them are just with Nukon data and then three others are with the combinations of Nukon plus Cal-Sil at various temperatures.

Next. This is the first comparison. And I'll just show all the comparisons just to show what we have here.

The diamonds are the test data. The pink line with the square is the straight NUREG correlation for the best estimate, now this is using best estimate properties, to compare with the test data.

The green line is the NUREG correlation in the laminar regions and the Ergun correlation with the cylindrical debris. So I'll call that a modified NUREG.

And then the blue line on the bottom is the Ergun equation for the cylindrical debris geometry.

And the bottom line, which is sort of, I guess, purple, would be the Ergun equation using spherical-shaped debris. For this -- and, again, as I indicated up here, this is for Nukon. So the SV for that Nukon was 171,000 one over per foot.

And the data basically -- I mean the correlations for the two NUREG and the NUREG-modified version predict somewhat at least a range of the data.

CHAIRMAN WALLIS: I note the data are above the correlation.

MR. KROTIUK: That's right. There are points that the data are above the correlation.

CHAIRMAN WALLIS: They are all above the correlation.

MR. LU: But this case is the best estimate case.

CHAIRMAN WALLIS: Yes, this is -- I just note -- I'm just noting on this figure --

MR. KROTIUK: Right, right.

CHAIRMAN WALLIS: -- that the points --

MR. KROTIUK: Okay, yes.

CHAIRMAN WALLIS: -- are all above the correlation.

MR. KROTIUK: Okay, there are points, yes, agreed.

CHAIRMAN WALLIS: They are all above the --

MEMBER SIEBER: Not all of them.

MR. KROTIUK: Not all of them but some of them.

CHAIRMAN WALLIS: Well, where are the other ones?

MEMBER SIEBER: At the very end it looks

like.

CHAIRMAN WALLIS: Are those points, too, although I didn't quite understand that.

MR. KROTIUK: Those are points also.

CHAIRMAN WALLIS: Those are points also, okay. So something changes at eight feet a second or something.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Okay. I'm sorry. I didn't realize those were also points. I thought they were just defining the curve or something.

MR. KROTIUK: No, no --

CHAIRMAN WALLIS: Okay, okay.

MR. KROTIUK: -- those are points at the end. I apologize if it the blue diamonds are not totally visible.

CHAIRMAN WALLIS: No, that's okay, that's okay.

MR. KROTIUK: I must say one thing also is that the value for the SV that I used here was the best estimate value for SV that was recommended for Nukon as the result of the Los Alamos testing. So that's representative of that.

Okay, next one. This is another test.

CHAIRMAN WALLIS: This is a test with half as much fiber?

MR. KROTIUK: With half as much fiber. The other one was, if I remember correctly, 116 grams of Nukon.

CHAIRMAN WALLIS: Right. And this is exactly half of much.

MR. KROTIUK: Right. And, again, using the same value for the SV, the NUREG and the NUREG-modified correlation are predicting -- there may be two points that are above but I mean they're predicting the trends basically.

CHAIRMAN WALLIS: So if we compare one with the other --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: -- in the first one you have a group of points that are above the curve and some on it at higher velocity. And this one you have points on the curve and below it at higher velocity.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: So one could conclude that if you stack these two beds one on top of the other, that you would not be getting the right value because when you have twice as much you get a higher value than predicted, which could be due to the fact that the fatter bed compresses more than predicted?

MR. KROTIUK: That could be a function of the compression.

CHAIRMAN WALLIS: Because it seems to me that if you put one bed on top of another bed, the lower bed is subject to the pressure from the upper bed so it compresses more.

MR. KROTIUK: You could get a different compression.

CHAIRMAN WALLIS: So there is some indication that even though the correlation seems to work that the compression effect is underestimated in going from one curve to the other?

MR. KROTIUK: I have to think about that a little bit. It's not, you know --

CHAIRMAN WALLIS: Well, this is one of the contentions in the write up I gave you --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- is I had exactly the same -- that's the contention I had.

MR. KROTIUK: Right. Okay.

CHAIRMAN WALLIS: That yes, you might -- because, of course, it's based on data, you might do a reasonable job with a set of data but when you start saying did I get the compression effect right, you might start to --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: -- it raises a different question. And you get a different answer.

MR. KROTIUK: Right. And I -- let me just say right here that I in no way am offering any valid -- any -- how would I say the -- I'm in no way trying to validate the compression equation. I'm just showing how the methodology would compare to test data.

Within the Office of Research, we are independently looking at the compression relation and, you know, to try to determine its appropriateness although we haven't --

CHAIRMAN WALLIS: Well, for instance, if you look at --

MR. KROTIUK: -- finished that.

CHAIRMAN WALLIS: -- point six --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: -- you have a line which goes through five.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: And if you look at point six on the previous graph, it goes through ten, which says it's twice the pressure drop --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: -- whereas the data are up at 12 or 13 which is completely compatible with the prediction of the compressibility model that I described in my memo to you. So --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- I'm not discouraged by this. I just simply think that it may be that the effect is not important. But trends here showing seem to be compatible with my own feelings about the compressibility model -- well, not just feelings, my own deductions.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: So thank you. That's very useful. That's very good.

MR. KROTIUK: Okay.

CHAIRMAN WALLIS: Now you're going to show us some more?

MR. KROTIUK: Right. And in this one, the correlations, the NUREG and the NUREG-modified correlation is under predicting the --

CHAIRMAN WALLIS: The hotter it gets, the worse the under prediction it would appear.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: And you're going to use it for even hotter water?

MR. LU: Yes, within the temperature range we defined.

CHAIRMAN WALLIS: So there's no concern there that if you go from 70 to 125 that the correlation under predicts a bit more. And when you start going to 200, there may be --

MR. KROTIUK: Okay.

CHAIRMAN WALLIS: -- the under prediction might be by a factor of 2 or something like that?

MR. KROTIUK: Let me try to just put this in perspective a little bit is that these first graphs that I am showing is what I am terming as a best estimate calculation using the defined parameters that Los Alamos said would be representative of the fibers.

And in their report, they also state that they recommend conservative values. And after I present these best estimate, I will present results from a conservative calculation using upper bound values of SV --

CHAIRMAN WALLIS: So you're fixing up SV rather than fixing up the theory?

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: SV plus -- there's also densities, I mean, but there's a couple conservative parameters that would go along with that.

CHAIRMAN WALLIS: But the correlation is going to be used for water temperatures of 200 degrees or something, whatever, 190?

MR. KROTIUK: What's the range?

MR. LU: There will be a table in the next set of slides we're going to show you regarding exactly -- I think it's 75 to 125, something, that's what we tested at this point.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: Okay, let's go to the next one. Now this is a comparison for the combined Nukon/calcium silicate. And, again, it's 100 grams of Nukon, 55 grams of calcium silicate.

And in this case, the NUREG and the NUREG-modified correlation falls within the data.

CHAIRMAN WALLIS: The two sets of diamonds are for increasing and decreasing flow rate?

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: That is correct, yes. And on the upper righthand corner, I am indicating the values of SV that were used for both the fiber and the particle. And particle is the Cal-Sil at this point. The fiber is the Nukon.

This is for different masses of Nukon and calcium silicate. The NUREG correlation is following the basic trends and with, I guess -- in this case, it's pretty well within the data.

CHAIRMAN WALLIS: In fact your 523,000 is exactly what's in the Los Alamos report.

MR. KROTIUK: I'm sorry. Say again. The --

CHAIRMAN WALLIS: The SV particle that you have --

MR. KROTIUK: Right. The SV particles are exactly what's in the Los Alamos report.

CHAIRMAN WALLIS: So you've redone their calculation, okay.

MR. KROTIUK: Right. Okay. And then the last one is for, again, a different mass of Nukon and calcium silicate.

CHAIRMAN WALLIS: Right. And I guess we're concerned with things like Test H, where you needed to go to 880,000 or something, to get effect of the highest point.

MR. KROTIUK: Yes. And, unfortunately, I did not look at Test 6H and that's on my back burner. I will look at that one probably tomorrow unfortunately.

CHAIRMAN WALLIS: So I guess the message is that you get just about the same results as Los Alamos?

MR. KROTIUK: Yes. And that's what I wanted to show.

CHAIRMAN WALLIS: And that you use a different kind of equation like Ergun cylinder, it seems to give about the same results as the NUREG correlation?

MR. KROTIUK: For the Ergun cylinder it will give the same results about as the new calculation for some of the applications, but not for all of them. It's not straight across the board.

CHAIRMAN WALLIS: But it's still using a one minus epsilon to the 1.5 rather than squared.

MR. KROTIUK: For the laminar portion.

CHAIRMAN WALLIS: For the laminar portion.

MR. KROTIUK: For the laminar portion. Just for information, I did try to adjust that in actually the laminar portion that's with the 1.5 and actually came up with a little bit better result. And I guess that's illustrated somewhat by the cylindrical --

CHAIRMAN WALLIS: The pressure drop in the laminar region, which I guess this is mostly --

MR. KROTIUK: Right. Correct. It's parameter --

CHAIRMAN WALLIS: -- is proportional to the square of SV.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: So if SV goes from 171 to 880 and you have to square that --

MR. KROTIUK: Well, it's not 171.

CHAIRMAN WALLIS: Well, that's just is for fibers alone. I'm comparing --

MR. KROTIUK: Oh, okay. Right, I'm sorry.

CHAIRMAN WALLIS: -- fibers with pure Cal-Sil or --

MR. KROTIUK: Yes, yes, go ahead. Right.

CHAIRMAN WALLIS: -- the thin bed --

MR. KROTIUK: Okay, yes. Now I see.

CHAIRMAN WALLIS: -- that assumes to 880, so we're talking about a factor of -- I don't know --

MR. KROTIUK: A large factor.

CHAIRMAN WALLIS: -- 15 or 20 something --

MR. KROTIUK: Yes, it's a large factor.

CHAIRMAN WALLIS: -- between one and the other.

MR. KROTIUK: Right. It's a large factor.

CHAIRMAN WALLIS: So it's very important

to get that SV right if you're going to use it.

MR. KROTIUK: Absolutely, yes. Okay.

CHAIRMAN WALLIS: So I guess, like I said, the real focus is on what should you require people to use for SV if you're going to require they use this correlation.

MR. KROTIUK: That's right. And so you have to define a specific range of applicability.

CHAIRMAN WALLIS: Right.

MR. KROTIUK: Okay. I think I said this basically as I was going along. So let's go to the next one.

What I just now tried to indicate for three tests, a bounding calculation using -- since this is not the thin bed, this is a mixed fiber Cal-Sil case, the recommended value for SV in this case is 600,000 for the Cal-Sil.

And there are also some changes with regarding to densities. So this is just an illustration of the changes that were made now to the model to try to --

CHAIRMAN WALLIS: To try to capture --

MR. KROTIUK: -- show what --

CHAIRMAN WALLIS: -- to bound all the points, is that --

MR. KROTIUK: That's correct. To bound the points.

I looked at the three tests, 6B, 6E, and 6F, which are the combination of a Nukon and a Cal-Sil -- okay, let's go to the next one -- in this -- oh, yes, that's what I meant to say is that again I am looking at the NUREG correlation, the modified NUREG, the Ergun using the cylindrical-shaped geometry, and the Ergun using the spherical debris geometry.

The key thing is that the Ergun with the spherical, which spherical is always lower than the test data even using these bounding numbers, and the NUREG and the modified NUREG are all very close to each other -- are both very close to each other. They're almost indistinguishable. There's just slight differences.

But in this case, you can see that it is higher than the test data. The Ergun with the cylindrical shape, it seems to fall apart and doesn't follow the basic shape of what is going on, what the data is showing.

Okay, next. This is now for different gram weights of Nukon and Cal-Sil. Again, the NUREG and NUREG modified is definitely bounding the measurements. In fact, it is higher.

CHAIRMAN WALLIS: That's too high, yes.

MR. KROTIUK: Yes. And the Ergun cylindrical, again, the shape is just wrong. It doesn't seem to hold up. And the spherical Ergun equation is lower.

CHAIRMAN WALLIS: So what's different between these two tests that have different SVs?

MR. KROTIUK: Okay, I'm sorry. These -- no, there's an SV for the fiber and an SV for the particle.

CHAIRMAN WALLIS: Yes, I know that.

MR. KROTIUK: Okay. Then I misunderstood your question.

CHAIRMAN WALLIS: I say when you use 600,000, you're way above it, right?

MR. KROTIUK: Right.

CHAIRMAN WALLIS: But you showed another graph which was the same data which had an SV of --

MR. KROTIUK: Five or whatever.

CHAIRMAN WALLIS: -- five or 400,000.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: It went through the data.

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: So it seems, again, this brings out the point that it's very important --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- because it's very sensitive to what you use for SV.

MR. KROTIUK: It's very sensitive to what you use for SV and I have to admit that there were also some changes made to -- based on the recommended values, for some densities. But I think the most important parameter is the SV.

CHAIRMAN WALLIS: Because what seems to be happening in these tests is that in some of them, you know, there are these jumps --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- which seem to indicate that the SV itself is changing through the test. Now you have some tests here where it's smoother but there are other ones which have bigger jumps.

MR. KROTIUK: There are some of that nature.

CHAIRMAN WALLIS: Right.

MR. KROTIUK: And the --

CHAIRMAN WALLIS: Now that would concern me a bit to have an SV which is changing through the test.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: And then if you look at test 6H, it starts out as a very low SV. And then it leaps up to this very high value.

MR. KROTIUK: Yes, and that's, unfortunately, the one I didn't look at.

CHAIRMAN WALLIS: All right. Okay. Well, that's been very helpful.

MR. KROTIUK: Okay. And I think I -- was that the least one?

PARTICIPANT: Yes, that's the last one.

CHAIRMAN WALLIS: Thank you.

MR. CARUSO: The SV of 600,000 is the value that's recommended for use in the mixed bed configuration.

MR. KROTIUK: Correct.

MR. CARUSO: But it also says it is also important to note that the calcium silicate tested was obtained from only one manufacturer. And that these recommendations do not necessarily apply to all types of calcium silicate insulation debris.

You don't provide any guidance for individuals to determine whether their cal-sil is this cal-sil. And what they should do if they cannot determine that their Cal-Sil is this Cal-Sil.

MR. KROTIUK: Yes, that's a very valid question. And, unfortunately, I'm going to have to defer that response to someone else because I don't have an answer.

MR. SHAFFER: Well, that's why we build the conservatism in there. That 600,000 number was an enhanced number over the 500,000-type number which actually matched the data. So when we came down to recommending a number, we enhanced the number somewhat to take care of these types of uncertainties.

MR. CARUSO: Oh, so the 600,000 number is --

MR. SHAFFER: Has a built in safety factor.

MR. CARUSO: -- is intended to bound all different types of calcium silicate?

MR. SHAFFER: It has a safety factor to try to compensate for the unknown associated with the different types of Cal-Sil. But obviously we only tested one type of Cal-Sil.

CHAIRMAN WALLIS: Now this 600,000 applies to mixed beds here. But then -- when does a mixed bed become a thin bed and this number become 880,000 or whatever? How does one change into the other because they're both mixed beds aren't they?

MR. SHAFFER: They are but the mixed bed is typically a case where the particles are not generally interacting with one another. And the thin bed is the case where the particles are now in contact with each other.

CHAIRMAN WALLIS: But this is a hypothesis?

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: A hypothesis.

MR. CARUSO: How does somebody who is applying this know when they have a thin bed configuration or a mixed bed configuration?

MR. SHAFFER: The recommendation says that they should assume the thin bed unless they have justifiable reasons to say they can't get a thin bed.

CHAIRMAN WALLIS: So we're back to using 880,000 unless you can justify using something --

MR. SHAFFER: There are two possible ways they can justify not having a thin bed. If you go to the complex strainer designs that we used in BWR, it's like a stacked disk strainer, all the testing that was done there, they never achieved the thin bed.

And the general consensus was that you would not get a thin bed on those type of strainers for reasons that you couldn't get uniformity in deposition across the convoluted screens. Okay, that's one possibility.

The other possibility is if they come in and they say our highest velocity is so low that the existing test data indicates you will not actually get into these high regimes, they might -- I mean they're saying if you have existing test data and you can look at it and say we're within this part of the test data and you didn't get a thin bed, they might be able to some way say --

CHAIRMAN WALLIS: But you don't know that. You only have a thin bed for one condition really which is test H or a repeated test H. You only have one experiment which is sort of anomalous and gives you this very high value.

So how can you ever use one experiment as a basis for deciding what's the limit to some theory? One experiment doesn't have any limits. It's just one experiment. There's no limit.

MR. HSIA: This is Tony Shia. I'm sorry. This is Tony Hsia from Research. I would like to say we live in a world of limited resources. Unfortunately, they only used one type of Cal-Sil. Even if you buy Cal-Sil from the same manufacturer, different batches may give you some different -- come up with some different SVs. So --

CHAIRMAN WALLIS: But this is a different question.

MR. HSIA: -- after all, this is a guidance. The guidance is for the user to realize what is the strength, what is the weakness of this correlation.

CHAIRMAN WALLIS: Well, I was actually --

MR. HSIA: This is a range --

CHAIRMAN WALLIS: -- asking a different question.

MR. HSIA: -- of applications you can use to basically say if you have any doubt about your Cal-Sil whether it fits to 880 or 600,000, the user has to take some risk -- responsibility, I should say, to verify that.

CHAIRMAN WALLIS: So if he gets a mixed bed, which has proportions that are significantly different from two to one, he should do his own experiments?

MR. HSIA: I don't think experiment is the right term I would use. I think he should verify the SV.

CHAIRMAN WALLIS: Then he has to do experiments.

MR. HSIA: Well, I don't know whether the manufacturer will be able to give you the SV as one of the numbers they have.

CHAIRMAN WALLIS: And this 880,000 is based on one test? And yet you're using it to lever everything they do, everything they do has to -- instead of the possibility that they might have an SV given by one of your tests seems to be rather extraordinary.

MR. HSIA: That is extraordinary. And we try to focus our attention and our energy on the majority of the cases. And in all the plants we have surveyed, we realize that the most of the plants are not Cal-Sil.

CHAIRMAN WALLIS: But doesn't it trouble you? I'm astonished that you don't say -- you can't really say limited resources or something. This is an important problem. And if you only have time to do one test and it's not good enough, go back and do some more.

You cannot say that one test, you hang your hat on one test.

MR. SHAFFER: It's actually three tests.

CHAIRMAN WALLIS: Duplicating the same conditions?

MR. SHAFFER: Well, one's a duplicate. And there's one test that was done after the report was put out that is along the same lines as the one test you're talking about. And it happens to fall in agreement.

CHAIRMAN WALLIS: But that's the one that you showed me yesterday?

MR. SHAFFER: It's the one I mentioned that --

CHAIRMAN WALLIS: But if we look at that, we're going to get into another anomaly.

MR. SHAFFER: No, not that test.

CHAIRMAN WALLIS: Are we allowed to discuss --

MR. SHAFFER: It was not that test.

CHAIRMAN WALLIS: It was a different one.

MR. SHAFFER: The test I'm talking about was a demonstration test that we conducted at the International Workshop. And it was along the lines of 6H, just a higher mass ratio. So --

CHAIRMAN WALLIS: But you know what we sort of looked at yesterday. And this seems also to have some other message to it, right?

MR. SHAFFER: Granted. That's a recent test and we actually haven't analyzed it.

CHAIRMAN WALLIS: But it might have a different message? So if when you analyze that you might come back and say this shouldn't be 880,000. We found a test where it's something else.

MR. SHAFFER: That is a concern.

CHAIRMAN WALLIS: Well, it seems to me that's not really a very good way to make a decision. When you've got one test and someone goes away and does one more test and gets quite a different value --

PARTICIPANT: Two points determine a line.

CHAIRMAN WALLIS: Two points determine a line. But, you know, if you want to do a test for anything, the strength of steel or anything, you don't do one test.

MR. LU: That's the reason our position is this is just s stepping stone for the industry to use the experience and the procedure we developed.

CHAIRMAN WALLIS: I don't think it is. I think you're giving guidance. You're not saying here is a stepping stone. We're just beginning to understand it. You guys must go away and understand it much better.

I thought you were giving guidance about this is the way to calculate.

MR. LU: Yes, but we are giving guidance

for the testing range we covered at this point. Anything beyond that there is no -- nobody can extrapolate.

CHAIRMAN WALLIS: So, I see, if they have Cal-Sil in proportion to fibers not two to one but one to one, can they use your method?

MR. LU: Why don't we just get into that application procedure and the test data range we have already covered. That should at least address your concern.

MR. HSIA: I still would like to stress, Dr. Wallis, that number one, there are few plants with Cal-Sil. Number two, even the plants with Cal-Sil, a lot of them are in the secondary side. The fiber material is on the primary side. And we really don't know exactly what kind of proportion you're going have reaching at the screen.

So we're --

CHAIRMAN WALLIS: Yes.

MR. HSIA: -- doing the best we can trying to --

CHAIRMAN WALLIS: That's right. That's the whole point. You don't know what proportion you're going to have there.

MR. HSIA: That's correct.

CHAIRMAN WALLIS: And you're going to hang it all on one -- so let's just talk about Cal-Sil on this matrix of this new slide that you have here. You have 600,000 recommended, and 880,000 for a thin bed, although I still don't understand how you know whether or not you've got a thin bed, I'm still hung up on that.

That's based on one test, 880,000 comes from one test. Right?

MR. SHAFFER: As I said, it's one test. There's one reproducibility on that. And then there's another test that is near that same parameter that came out with about the same -- so it's not quite one test.

MEMBER FORD: I have a question. Rob, you mentioned that we keep pushing about the comparison between your theory and your observations. And you said that the surprising thing at Barsibeck was that the sump clogged in one hour in comparison to the calculated or expected value of eight hours.

Now with these new algorithms that you have, have you done the what if question of trying to determine what would have had to have been done in that particular operating experience to get sump blockage in one hour?

For instance, I thinking if you had different types of insulation coming down at different times, could that explain it?

MR. ELLIOTT: We haven't tried to go back and calculate Barsibeck, if that's what you're thinking.

MEMBER FORD: I would have thought that --

MR. ELLIOTT: It's not really prototypical of our plants. It's a primarily mineral wool plant. Mineral wool is not used in great quantities in domestic BWRs.

MEMBER FORD: But in terms of methodology it's important, isn't it?

MR. ELLIOTT: Yes, but I don't think we've actually -- unless Clint or Bruce remembers doing it. But I don't remember actually trying to run a calculation on Barsibeck, not specifically to reproduce that combination of debris. But that was the motivation for investigating high head loss with small amount of product.

MEMBER FORD: The reason why I go oh, when you said it's not relevant to our reactors, I seem to remember that we went exactly the same answer from licensees when we were asking about vessel head penetrations and it's relationship to the French experience. They said they're not the same as ours.

MR. ELLIOTT: Well, we also don't have --

MEMBER FORD: -- but the methodology is the same.

MR. ELLIOTT: I don't think we also have the kind of data you would need to be actually able to accurately reproduce it. We don't have measurements of how much debris was actually on the screens, you know, once they cleaned it of and that sort of thing.

MEMBER FORD: So why did you say you were surprised if you hadn't done the methodology.

MEMBER SIEBER: They were surprised.

MR. ELLIOTT: Oh, we weren't surprised. They were surprised.

MEMBER FORD: Oh, they were surprised?

MR. ELLIOTT: Their calculations were that they didn't expect to have to -- they actually had back-flush designed into their systems.

MEMBER FORD: Oh, okay.

MR. ELLIOTT: And their licensing basis is that they wouldn't have to back-flush for ten hours --

MEMBER FORD: Okay.

MR. ELLIOTT: -- in a LOCA, okay? And what they had here -- in a large break LOCA -- and here they had a small break LOCA essentially, you know, stuck open safety relief value, and they clogged the screens in an hour. That's what caught them by surprise.

MEMBER FORD: Okay.

MR. ELLIOTT: But we -- I mean we don't have enough details about -- even if we tried to reproduce that, I don't think we could because we don't have enough information.

And I don't think it was ever collected as to how much debris was generated, how much actually got down onto the screens because the one thing they did do is they turned right around and they blew it all off with back-flush.

MR. SHAFFER: As the result of recent comments and questions on head loss correlation, we have decided to add an additional subsection to one of our confirmatory research appendices, Appendix 5, on head loss. And in this appendix, I'm presenting procedures on how to apply the correlation, how to validate it.

And I've started a list of existing validation studies. Keep in mind when you look at this list that we've just started and it's not complete.

This first slide lists out the kinds of

parameters you have to look at to have a quality test for validating the correlations.

You first have to recognize the assumptions that went into the development of the correlation. First of all, it is a fibrous debris bed correlation with or without particulates. It assumed a uniform thickness so when you run a test, you need to obtain that uniform thickness.

It assumes homogeneity, single-phase flow, perpendicular approach philosophy. And the correlation is not a transient correlation, it's a steady state. So in the test, you need to achieve a quasi-steady state condition.

And I'd say nearly complete filtration. You dump a certain amount of particulate into the system. If you don't get near complete filtration, you won't know how much of the particulate is in the free bed. So when you run a test to validate the correlation, you need to address these kinds of things.

Next slide.

CHAIRMAN WALLIS: You're assuming a homogeneous bed so this filtration is not something where you lay down the fiber and then the particles arrive later and form a filter cake on top or something?

MR. SHAFFER: The correlation was not developed for a standard-type debris bed.

CHAIRMAN WALLIS: Right, yes.

MR. SHAFFER: Okay. This slide addresses how do you use experiments to make determinations of the input parameters are appropriate for the correlation? First of all, the velocity, temperature, and debris mass is a test parameter so you know those.

The densities you can obtain from some source, typically the manufacturer will provide densities. If not, you can do some simple lab bench-type tests, volume displacement, that sort of thing, to come up with densities.

The next thing that is starting to come out is the coefficient to the compression function. And in the previous work, in 6224, we had coefficients which were applicable to Nukon. They also seem to be pretty good for other low-density fiberglass.

But you may have other materials, fibrous materials, in which the coefficients may need to be torqued. And in the NEI guidance, they're recommending that you can adjust that lead coefficient.

Now if you have test data where you test fibrous insulation alone and you can also measure its thickness under various flows, you can then deduce what these coefficients are for a particular fiber. So we're recommending that in your new tests that you actually try to do that.

CHAIRMAN WALLIS: Is there a matrix of the test basis for picking alpha and gamma from thickness data? There's very little thickness data, isn't there?

MR. SHAFFER: There's very little but we're anticipating there's going to be new testing coming up. And --

CHAIRMAN WALLIS: But we can use the recent LANL report where they measured thickness?

MR. SHAFFER: That is one source of data.

CHAIRMAN WALLIS: Is there a better source of data which would perhaps validate alpha and gamma?

MR. SHAFFER: Not that I know of.

CHAIRMAN WALLIS: So the best we have is that LANL report?

MR. SHAFFER: The LANL report.

MR. LATELLIER: And I should emphasize that those tests were not designed for accurate thickness measurements.

CHAIRMAN WALLIS: Well, thickness was

recorded to a measurement -- to a recording of a sixteenth of an inch. Someone wrote down numbers --

MR. LATELLIER: That is correct --

CHAIRMAN WALLIS: -- within a sixteenth of an inch and has said that's my best estimate of what the thickness is.

MR. LATELLIER: But I would not like to endorse that method for accurate --

CHAIRMAN WALLIS: But somebody actually made measurements and wrote down numbers that he or she believed described what was seen or measured.

MR. LATELLIER: That is true. But I believe we could do better than that.

CHAIRMAN WALLIS: Of course, you can always do better.

MR. SHAFFER: Okay. Moving on. After you know this information, you know everything in the correlation except the specific surface area. So you take the head loss, you adjust the area until the correlation starts to replicate the head loss data. That gives you an idea what the specific surface area is.

And we're acknowledging here that there's other uncertainties in the correlation that automatically get subsumed into that specific surface area.

CHAIRMAN WALLIS: Now suppose we took a figure form the LANL report where they calculated to the compression and compared with experiment and it turned out that there was a large deviation. Would that have any influence on you at all? Or on the staff?

MR. SHAFFER: Well, the figure from that report seemed to show that the compression for the one test that was demonstrated --

CHAIRMAN WALLIS: Yes, well, let's recalculate that number and see if it still works.

MR. SHAFFER: Okay.

CHAIRMAN WALLIS: Because I'm just wondering, you know, if we're going to hang our hat on LANL data being the best for thickness, we'd better perhaps check it, all right?

MR. SHAFFER: We could do that.

Okay, if you're validating against test data or you do not have this thickness data to determine the coefficients for the compression function, it is possible to vary those coefficients and the specific surface area simultaneously until the correlation does a good job, it's deducing both of them simultaneously. It's a little disadvantage but it can be done.

Next slide please. Analytical determinations of specific surface area, there's quite a bit of discussion in the NEI guidance on doing this with simple formulas, four over diameter for the fiber, six over diameter for the particulates.

We have a good deal of concern about doing that. For the fibers, it's not so bad because fibers tend to be a lot more uniform. And certainly when this is done for Nukon, it did a very good job. If you do it for other low-density fiberglass diameters, it's probably pretty good.

But for some of these more exotic fibers, there's a -- we have some concerns there.

For our particulates, using six over diameter means that you've got a diameter. Now when you have a postulated particulate like 10 microns for the coatings debris, there's no problem. You've already picked a single diameter.

But now when you're talking about realistic distributions where the distribution may be in three or four size groups, you took a realistic particulate and you sifted it and you've got four size groups, well, what diameter do you put into the six over D?

If you use the mid-range diameter, you're going to underestimate some specific surface area. And we have --

CHAIRMAN WALLIS: Cal-Sil, as you say at the bottom, is anything but a sphere because Cal-Sil doesn't look like a lot of spheres.

MR. SHAFFER: Right. Cal-Sil is a different animal. I'm talking more the standard particulates, which are rock hard.

I can point out here that if you use the smallest diameter in each size group, you're going to be conservative. But your problem is is in the smallest size group where you don't know what the minimum diameter is.

CHAIRMAN WALLIS: The smallest size may be almost atomic.

MR. SHAFFER: Yes. But still you have to decide which size group is going to get through the filter bed. See, but when you get right down to it, there's no substitute for actual head loss testing.

CHAIRMAN WALLIS: I like that idea.

MR. SHAFFER: Okay. And the last bullet on there is a concern about Cal-Sil. Calcium silicate, the particles aren't rock hard like sand. They are made of this diatomaceous earth, calcium silicate, chemical reactions, all that stuff.

And when you look at them under a sim photo, they're kind of airy particles. And when you put pressure on them, it appears, in our testing, that they deform.

CHAIRMAN WALLIS: And yet they have the sludge limit -- is the sludge limit after they deform or before they deform?

MR. SHAFFER: Well, we have that sim photo that's in our Cal-Sil report. It's post test. And if you look at that, you can see that the Cal-Sil particles are jammed one against another and they're jammed tight. And that means that they have done some deforming.

CHAIRMAN WALLIS: So that density that is then greater than what you get from a settling test or something like that?

MR. SHAFFER: Our working theory, in my opinion, is that the sludge density is not a fixed number for Cal-Sil. It depends somewhat on how much pressure you put on it.

CHAIRMAN WALLIS: Oh, so Cal-Sil is compressible? There isn't this magical sludge density that it goes to?

MR. SHAFFER: With Cal-Sil, it has a

behavior that does not match up with the formulation 6224 because it was developed for particulates that are rock hard.

CHAIRMAN WALLIS: Yes.

MR. SHAFFER: Okay. So our guidance was aimed at trying to predict a bounding head loss, not in trying to predict everything that went on in between.

CHAIRMAN WALLIS: But there could be a yield stress for Cal-Sil if it's this friable sort of diatomaceous earth which is made up of the skeletons of small organisms living in the sea. And it seems to me it's very likely that it has a crushing sort of yield stress or something. It's not just elastic.

MR. LATELLIER: But let me interject that although that behavior may be true, we are only interested in a relative range of --

CHAIRMAN WALLIS: I know.

MR. LATELLIER: -- head loss which is induced by the --

CHAIRMAN WALLIS: I agree.

MR. LATELLIER: -- drag on the --

CHAIRMAN WALLIS: I agree.

MR. LATELLIER: -- particles.

CHAIRMAN WALLIS: I agree but I guess what

I'm learning here is that Cal-Sil, which was described as if it had a sludge density probably is still compressible so it doesn't really have a hard sludge density like rust. And there still needs to be perhaps some work done defining if and how Cal-Sil does deform if you're going to make calculations for Cal-Sil plants.

MR. LU: Even though the Cal-Sil might be compressible, but the total out of H or the maximum head loss, that's actually very interesting within the range of from zero to 25 feet head loss. So within that range, and then if we take an average, that should be sufficient for us to confirm that.

CHAIRMAN WALLIS: So you have faith that up to 25 psi is not enough to cause any significant deformation of the Cal-Sil?

MR. LU: Again, it will be based on test data.

CHAIRMAN WALLIS: It's based on test data? Have people actually measured the compressibility of Cal-Sil?

MR. LU: No, what I'm saying is that again based on test data for the head loss, I'm not saying that we have a measure for the compressibility of the calcium particle.

CHAIRMAN WALLIS: And the head loss -- well, the head loss that -- the claim that you've reached this sludge density is based on the intricacies of this head loss correlation and the compressibility and so on. And a predicted density of some sort. It's not something that's measured.

MR. LU: It's the limit of the compressibility.

CHAIRMAN WALLIS: This sludge density to me is something which is always deduced as a reason for something happening rather than actually measured by itself as happening in an experiment.

What is all this noise that keeps interfering? We still connected? Let's disconnect our phone. We've been on the phone. Someone's been listening in all along here.

MEMBER SIEBER: Apparently not.

CHAIRMAN WALLIS: Okay. So --

MR. SHAFFER: Should we move on?

CHAIRMAN WALLIS: Yes.

MR. SHAFFER: Next slide. Okay. This is the start of a list of validations that have been done. It's -- I'm sure a lot of you out there know that it's not complete but we're going to be working on completing it.

CHAIRMAN WALLIS: I think it's very good. Now if we look at what we were presented with half an hour ago, it appeared as if 125 degrees gave data which were significantly above the correlation.

So maybe for 125, the SB is 200,000 or something. I don't know what it is. But there seemed to be a trend with temperature which is not reflected in your table here. You simply say it's 171,000.

But if we looked at that experiment which was presented to us a little while ago, one might be led to fit it with a somewhat higher SV at 125 degrees.

MR. SHAFFER: For the Nukon, that's been tested in several test studies. So I wouldn't go to just that one test that we were looking at before but the breadth of the Nukon testing because this was done when we were doing the BWR work. And there was a lot of Nukon data.

CHAIRMAN WALLIS: Okay, well, I looked at 6224 and I noticed that Nukon processed different ways, chopped up different ways, and so on, seemed to give a significantly different pressure drop. And you're saying there's only SV that describes all of those things for Nukon?

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: But then what do I do with those curves which show that the way it's pre-treated changes this pressure drop?

MR. SHAFFER: Well, some of those curves may also have experimental errors into them --

CHAIRMAN WALLIS: Oh.

MR. SHAFFER: -- that need to be considered.

CHAIRMAN WALLIS: Suppose I look at French data on Nukon, do I get the same answer?

MR. SHAFFER: You should do. We haven't done that.

CHAIRMAN WALLIS: Should do? But they're concerned with the same problem.

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: And there is an international database I understand?

MR. SHAFFER: There's one referenced there.

CHAIRMAN WALLIS: It's been referenced, yes.

MR. LU: We have not heard of that yet.

CHAIRMAN WALLIS: Are you satisfied that this 171,000 is descriptive of Nukon in France?

MR. SHAFFER: Well, the 171,000 is pretty closely four divided by the diameter of those fibers. And in our testing, it works well.

CHAIRMAN WALLIS: Well, you see what I'm getting at? Just saying it's 171,000 doesn't tell me much about how it varies from place to place or preparation to preparation and so on.

And if it does, as I saw in 6224, I thought I saw different curves for different ways of preparing the fibers. Then the question is well, which one of these am I going to use for a LOCA?

MR. SHAFFER: You're going to use the 171,000 for all --

CHAIRMAN WALLIS: Okay.

MR. SHAFFER: -- types of Nukon.

CHAIRMAN WALLIS: Because that's the one that's been approved.

MR. SHAFFER: Now --

CHAIRMAN WALLIS: That's the wonder of regulation. You can legislate.

MR. SHAFFER: Yes. Now we have studied a number of these Nukon debris tests. And that 171,000 is a reliable number.

CHAIRMAN WALLIS: Is it conservative? Is that the idea? It's conservative?

MR. SHAFFER: It does a pretty job of

predicting the Nukon data, not necessarily bounded, but it goes right to the middle of the data.

CHAIRMAN WALLIS: Okay. Well, maybe this is -- you know, maybe we don't have time to do this. But if we had time, I would want to look at some of these curves in the classical NUREG report where they seem to give different results for different Nukons.

MR. SHAFFER: But the other point I would make is that we're not going to be seeing Nukon alone in the plants. There will always be particulates embedded in that Nukon. And the particulates --

CHAIRMAN WALLIS: So you're changing the conversation.

MR. SHAFFER: -- are going to drive the head loss.

CHAIRMAN WALLIS: Now we're talking about what the value is for Nukon alone. I thought that was what we were discussing. But I don't think we have time to go into all this. It's just one of those concerns that I had and I can't find in reading this NUREG that there seemed to be differences depending on how it was prepared. But you are sure that that doesn't matter?

MR. SHAFFER: I do not believe it matters.

CHAIRMAN WALLIS: Okay.

MR. SHAFFER: And I believe that number is one of our better known and validated numbers.

Now we have some validation on Koawool, Transco, and a little bit on Mineral Wool. Let's go to the next slide. Okay. Here are some of the particulates for which we have some validation. Obviously, iron oxide corrosion products was studied extensively in the BWR resolution. And we have 183,000 for that number and it's been validated pretty well.

MEMBER SIEBER: But that's not generally applicable to PWRs?

MR. SHAFFER: That's correct. But here we're going to try to list all of the validations.

MEMBER SIEBER: Okay.

MR. SHAFFER: Okay? Now we've been talking a lot about the calcium silicate studies. Now we've taken some criticisms, we've got a couple points to address, and -- but aside from that, I think we've got some pretty good validations here.

There's also one here called latent particulates. And this is another one that's turned out very well. And what happened here was that we had some plants volunteer to collect debris in the plants. And we sent that to Los Alamos where they have a lab that can handle radioactive debris.

It was characterized in terms of size groups, specific gravity, and so forth. Those characteristics we constituted a surrogate from sand and dirt in quantities that we could do head loss testing on.

And can we go to the next slide? Bounce on down a little ways. Keep going. There we go. Okay, this is a table of results from that study. We've got three sizes groups, 500 to 2,000 microns, 75 to 500, and less than 75. And this dirt has a pretty high fraction to release small stuff because there's a clay component in there that breaks down, okay?

And we have the mass fractions for each size group that came from the LANL study. So we have our recipe or our formula, okay?

In the head loss testing, we tested each one of these groups separately and we tested the recipe. And deduced a specific surface area over here in this column from the head loss data.

Then if you back out an effective diameter, it's over here in this final column, now if you compare that effective diameter with the size range, you can see that it fits in there pretty well. What's more, we can take the three groupings and recombine them using our formulas and get the recipe number pretty close.

Now this stuff is documented in our reports but it is an excellent validation of the NUREG 6224 correlation and it provides guidance to the licensees on how to address their own particulates.

Now if perhaps you've come up with the same sort of recipe and can use this 106,000 number or perhaps you do analytical refinements and you say okay, these two course sands are not going to get there, you're just going to have the less than 75 on the screen.

And that would give you some idea well, then you've got to back up and use this 285,000 number for your specific surface area.

So we have validated on a realistic and complicated approach here. And we provided guidance on light and debris at the same time.

CHAIRMAN WALLIS: I found the figure I was looking for. It's in this NUREG 6224. It says comparison of existing head correlations for pure Nukon. It gives four curves for fibers and shreds and air blasts and so on. You've probably looked at that.

And it gives different curves, which differ by factor of almost ten at the same velocity.

Head loss predicted differently for fibers and shreds and air blasts.

MR. SHAFFER: Which figure --

CHAIRMAN WALLIS: Now is this because the experiment was bad or that it was -- is it because the fibers are somehow different in the different tests or something?

What do I do with that sort of evidence when I see that there are four correlations -- there seem to be five correlations actually -- for these different conditions which differ by so much. What should I conclude there? And how does your 171,000 fit in there?

MR. SHAFFER: That's a question, I guess. I need to go back and review that in order to answer it. I haven't seen that document in a while.

But in any case, the debris bed formation that is going to give you the higher head losses should be the one that comes out the most uniform. And that ought to be the one that forms one fiber at a time. And that's the kind of debris bed we studied in the Cal-Sil study. And the 171,000 worked out pretty well there.

So maybe some of those debris beds where you've got large chunks coming in are not actually well formed and there may be some, you know, holes in the debris maybe. We can look at that and come up with an answer to that question.

CHAIRMAN WALLIS: Okay. Thank you.

MR. SHAFFER: But the answer is that the 171,000 should be conservative for the debris beds that are formed really uniform.

MR. DINGLOR: Could I ask one question?

CHAIRMAN WALLIS: Could you stand up and identify yourself please?

MR. DINGLOR: This is Mo Dinglor. I'd like to ask a clarifying question on one of the tables. It has the temperature range and the velocity range. Is the clarification if I'm 59 degrees, I can't use it? And if I'm 126, I can't use it for the iron oxide?

And if my velocity is less than .15, you're saying I can't use the correlation? Is that what this table tells me?

MR. SHAFFER: That tells you the range of parameters as they were tested.

MR. LATELLIER: I need to weigh in on the issue of determining limits of applicability. I think there's a desire, in fact a very critical need that our correlation be practical.

It has to have enough physics to capture the behavior of several variables, temperature, for example, the viscosity effect, the velocity effects, the thickness of the debris beds. And to maybe a greater or lesser extent, the composition as it varies in mixed beds. Those four things we have to have some confidence in its ability to extrapolate or interpellate between the test conditions.

Now as applied classically in recent years, the insulation type or the debris type that's in question, that drives the specific values of the free parameters in the model. And that's what we've always emphasized the need for test data for.

Now if there are anomalies in our test data that do not capture the trends in these four physical parameters, then we need to rectify that rather than trying to limit ourselves, as Mr. Dinglor points out, to a very narrow range of temperature because that's the only test that exists.

I don't think we've served the purpose of practicality if we try to do that. It would be to our much greater benefit if we resolved the disparities that we see with regard to these four variables.

MR. LU: Yes, the table released here is just for the test data we have collected so far. And in terms of application range that would be put into the SER and we'll consider just what Bruce just said and what exactly the range we can commit on.

But if anything beyond that range, once we issue SER and if you want to use the correlation, you have to validate that.

MR. CARUSO: So the answer to his question is if it's 59 degrees, the answer is you can't use it.

MR. LATELLIER: All the LOCAs are much beyond that so what's the point?

CHAIRMAN WALLIS: Well, it's never going to be 59 degrees but it might be 130.

MR. ARCHITZEL: They're all above the upper end. They're all 220 or something like that and stop at 190?

MEMBER SIEBER: Microphone?

CHAIRMAN WALLIS: Everything is beyond the range in the sump.

MR. DINGLOR: That's right.

CHAIRMAN WALLIS: So I don't really like this graph at all, this matrix at all.

MR. DINGLOR: I'll go back to Ralph's question. I'm a very simple guy, yes or no. Is this table going to be in the SER and then I can't use it if it's 59 or 126? I'm a simple man.

MR. SHAFFER: This is Tony Hsia from Research. Although I did not check all the data, but I -- based on some of the other evidence I've seen, I don't think we can categorically say you've got .14 velocity feet per second or .16, you cannot use this table. I don't think that's what the intent is.

CHAIRMAN WALLIS: Okay but there's still some range over which they cannot use it presumably. And they need to know what it is. I'm a simple man, too.

MR. HSIA: Yes. The intent --

CHAIRMAN WALLIS: I've been very simple all day.

(Laughter.)

MR. SHAFFER: Thank you very much. We're in the same arena.

CHAIRMAN WALLIS: Okay.

MR. SHAFFER: But the intent of this table is really to demonstrate that the staff and its consultants have done enough work to be able to generate its validity of this correlation to be able to demonstrate it. And I don't think we should be cutting --

CHAIRMAN WALLIS: You see that's another evidence that you may not be ready to make a decision because you may not have really thought out what you're going to accept for temperature ranges and things.

MR. DINGLOR: So are you going to put a table like this in the SER that has a temperature range limitation on it?

MR. HSIA: That is our current intent.

CHAIRMAN WALLIS: I think that would be fatal because they can't use any of this because most of their sumps are hotter than that.

MR. LATELLIER: Yes, I mean I think we've said a couple times that we're going to try to put -- we're going to put in the S-track, we're going to put in the SE limitations so that licensees know how to apply it. And you don't have it today.

My understanding is we've already started work on that. We've seen some of it. And we need to -- we're going to have that work wrapped up in the next few days, I guess, is what we're saying.

MR. SHAFFER: This is true with any guidance that the NRC gives. If it's too prescriptive, we get into the problem you just asked, what about .1 feet per second over? That is not the intent of this table.

If we don't have this table, the question becomes well, you have no idea what the range should be. So I'd like to still say I firmly think the staff and its consultants have done a credible job of presenting this information to the user. And --

CHAIRMAN WALLIS: It doesn't help. It doesn't help. It doesn't help. They may have done good work. But if it isn't usable by the industry, it is useless.

MEMBER SIEBER: Well, it sounds like the SE that has been sent to us for our review is not complete.

MR. CARUSO: And it looks like the data that they're about to put in is not useful.

MEMBER SIEBER: Well, it depends on what they put in. If we don't have it in front of us, we can't review it. And can't make a decision as to --

CHAIRMAN WALLIS: Right.

MEMBER SIEBER: -- whether it's good or not.

MR. CARUSO: Well, we have --

MEMBER SIEBER: But it's an essential piece --

MR. CARUSO: -- we have some numbers --

MEMBER SIEBER: -- to do the job.

MR. CARUSO: -- we have some numbers right here and references for them.

MEMBER SIEBER: No, that's not the numbers really. This is test data. The numbers that really need to be in the safety evaluation is the applicable range, whether it's based on the endpoints of the test data or not. They may be different. There may be some way to justify a greater range than the test data now support.

MR. CARUSO: Well, that's interesting because they -- I thought we just heard an argument that said they worked within the range of applicability, which is generally, from my experience, within the range of the --

MEMBER SIEBER: Of the data.

MR. CARUSO: -- test data. And if you try to go outside of the test data, then you have to make some sort of a bridge argument, which we have not heard so far, which says that it's good beyond 125 up to 250 degrees.

MEMBER SIEBER: So it's not useful to solve the practical problem in PWR sumps since they all run hotter than that.

MR. CARUSO: Yes.

MEMBER SIEBER: And so what are we doing now?

MR. CARUSO: That's a very good question.

MEMBER SIEBER: You know we have a safety evaluation that really can't be used.

CHAIRMAN WALLIS: This number 880,000 was evaluated at 110 degrees. Does that mean that it's only valid at exactly 110 degrees? Or is it valid over a range?

MR. LATELLIER: Who knows? But the basic physics equation is the correlation is formulated has implicit an understanding of the temperature effect through viscosity.

CHAIRMAN WALLIS: Not for one test with one anomalous result which you don't know -- it might be attributable to temperature. You don't know what it's due to.

MR. LATELLIER: And I acknowledge that those anomalies need to be resolved because we do need a correlation that's practical over the range of applicability.

MEMBER SIEBER: Yes. And an implicit statement of what that range is is not sufficient. It has to be explicit.

MEMBER RANSOM: I agree with Bruce. I think the staff would need to use these range of parameters to do a sensitivity study and see how sensitive they are. If they're not sensitive up to, for example, the temperature of 125, that may not be an issue.

If it is extremely sensitive, that's the place we need to highlight.

MEMBER SIEBER: I think that's an approach but that means the work is not complete.

MR. HSIA: No, we can do that analysis.

CHAIRMAN WALLIS: I think you're going to ask the -- you're actually asking the licensee to do tests if he has Cal-Sil at the temperature which he expects over a range of bed thicknesses and velocity in order to find out what this SV is.

So you're really putting all the burden on him because you don't know what it is for 200 degrees with different velocity and a different fiber to particulate mass ratio. You have no idea what it is.

So it's all a burden that's now on the licensee. That doesn't -- is that really your intent?

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: What kind of guidance is that?

MR. CARUSO: And if he makes enough experiments to determine that, he doesn't really need the correlation.

CHAIRMAN WALLIS: If he does all the experiments, he doesn't need the correlation anyway, that's right. Absolutely.

MEMBER SIEBER: Yes.

MR. LATELLIER: I think the staff has always emphasized that there is such a variety of insulation types that there will always be some uncertainty in the basic physical properties and how they're treated. And that the industry, in some cases it's appropriate for them to assume some burden for characterizing those unique types.

CHAIRMAN WALLIS: Well, I think we need to hear from NEI and the industry about their reaction to this SER in the form in which it finally takes.

MR. CARUSO: Unfortunately, it doesn't appear that it's final yet.

CHAIRMAN WALLIS: So --

MEMBER SIEBER: Right.

MR. CARUSO: It appears to be a work in progress.

MR. JOHNSON: Yes, we're looking at having Research provide some sensitivity information on this information I guess very quickly, right?

MR. SHAFFER: I think days.

MR. JOHNSON: Within days. I'm a little

bit troubled by the notion that because it's not all done we can't move forward. And maybe I can talk to that some in my closing. But I think that is a theme that I have heard throughout the day that really troubles me.

Recall that, you know, we're talking about an accident that is -- it's going -- an initiating event that is extremely low likelihood. We're talking about that, for example, the situation where most PWRs have been reviewed for leak before a break. And so we already know that for the biggest ruptures in the biggest pipes, we expect them to leak before they break.

We're looking at a situation where if the break is in the small pipe, we don't expect, in most cases, that even recirc will be required. For example, we're looking at a situation where in the analysis there is already a margin in the analysis specifically with respect to net positive suction head or containment back pressure, for example, in the calculation of net positive suction head.

And so we're looking at an issue that needs to be addressed. But we're looking at an issue that is of low likelihood. And we've made the case that, again, we need to get on with this but that it's of low likelihood.

CHAIRMAN WALLIS: What you say, Michael, is true. But it has no relevance whatever to the question of what does the licensee do when asked to demonstrate compliance or whatever with 5046 under the present rules? What calculations can he make? And what assumptions is he allowed to make?

MR. JOHNSON: Absolutely.

CHAIRMAN WALLIS: It has nothing to do with it being an unlikely accident.

MR. JOHNSON: Absolutely. I actually -- I wasn't really -- I had some more to my thought. And the thought goes to the point that you're making which is -- so then -- but we didn't stop with this fact that this accident is highly unlikely.

We said, well, you know, given that, we still need to come up with an evaluation methodology that has sufficient rigor, that has sufficient conservatism, and we've talked throughout the day about areas of the analysis, the evaluation that are conservative.

And, in fact, one of the things that I think impressed the Subcommittee in the June presentation by the industry was the areas of conservatism in the evaluation. And we talked about it and I know we had a lot of discussion about the break location and we picked the worst location. Of course 5046 requires that you pick the worst location for debris generation.

We talked about the zone of influence. And I know that there's a concern about whether the spherical zone of influence is, in fact, conservative. We believe that it is.

We've talked about transport and every case, and, in fact, there's a table in the SE that looks at the conservatisms in the analysis. We think that the way in which transport is handled, in fact, is appropriately conservative.

We talked about two phase, this two-phase jet. And I know there's some concern about the two-phase jet and the single jet. And there was a lot of push back, I think, in terms of why 40 percent -- whether 40 percent was the right number.

But in the end, we've approached this evaluation to add conservatism to be bounding not with rigor, perhaps not with a lot of -- in an amount that's overly precise.

But, again, I would make the argument that I don't know that we need to be able to be precise to develop a fix to the problem that exists with PWRs that moves us to a place that is better than we are today, that addresses the vulnerabilities.

And I just worry that -- I worry that we're losing sight of that as we dig in on each of these individual aspects of the analysis.

Now I don't -- having said that, I'm not making the point that we don't need to do more. We certainly need in the guidance to provide for the industry and provide for individual licensees the capability to not have to do extensive tests and, you know, I'm bothered by that as you are bothered by that. And we're looking to address that in the evaluation.

But having said that, I think, and hopefully, again, hopefully we've got another half a day to try to convince you. But I believe that we're coming out in a place that enables us to walk away from this with a product that can be taken by licensees and their contractors to look at how to evaluate their sumps to resolve the problem.

MEMBER KRESS: In order to do that, you're going to have to back off on this restriction that the correlation can only be used over the range of the test data. And I don't know how you're going to do that.

MEMBER SIEBER: You've got to say that in the safety evaluation.

MR. JOHNSON: Right.

MEMBER SIEBER: As opposed to letting people implicitly assume it.

You've got to say something. It's incomplete the way it is regardless of how rare and unlikely the accident is going to be. And how hard we've worked so far and, you know, we have this and we have that, we're still missing a piece.

MEMBER KRESS: And it doesn't have to recognize that there's a lot of conservatisms in there unless one can make use of that information in some way.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: And if you're going to extrapolate this correlation way beyond the range, and based on a few data points, then you're going to have to justify doing something like that.

MEMBER KRESS: Yes.

MEMBER SIEBER: I think there's merit in pursuing the sensitivity analysis suggestion.

MEMBER KRESS: It's difficult to do a good sensitivity analysis unless you have either --

MEMBER SIEBER: Yes, you need --

MEMBER KRESS: -- a very good model --

MEMBER SIEBER: You need a better --

MEMBER KRESS: -- or a lot of data.

MEMBER SIEBER: -- you need a better --

MEMBER KRESS: Yes.

MR. SHAFFER: We're appreciative of the Committee's input today on a variety of issues and, in particular, just now Dr. Wallis mentioned if we want to extrapolate, but from the limited data points there, maybe the Committee will help us in finding out a technical basis that's strong enough to be able to do that.

MEMBER SIEBER: Okay.

MR. LU: The last item.

MR. SHAFFER: We have one last slide. The NUREG correlation, of course, is built for a flat screen and the test data is usually cumulated that way. But as in the PWR resolution, we can anticipate the PWRs probably replace screens with these more advanced designs like a stacked disk strainer.

So how do you apply the correlation to that? This is just a brief summary on what was done in BWR resolution. It has been applied to the total screen area of these convoluted screens before you get significant debris. That's saying that initially you get uniform deposition. Okay, so it's applicable at that point.

And at a later time when it's fully engulfed in debris and all the crevasses are filled, we have been applying it by using the circumscribed screen area, okay, so the two endpoints.

Now people have done different things to fill in the points. Some, I believe, have actually done a linear extrapolation. But I know that we, in some of our research, have actually back calculated an effective screen area to fill in the points.

But the idea is if you take a prototypical or actual strainer, you test it, you get the test data, you back out this effective screen area, the function of debris loading, and then you have that piece of data that goes with that particular strainer.

MEMBER KRESS: Let me as you a question about that. If you had one of these convoluted filters, would it be possible to exclude considerations of the thin bed effect all together?

MR. SHAFFER: Okay. I believe the conclusion was that none of the tests with these convoluted screens ever achieved a thin bed. It was also never actually proven you couldn't get one.

But the judgment after the fact was that the debris accumulation on these convoluted screens is not uniform through most of the period. And because it has this non-uniformity where it might be thin bed in one place, it could be something else someplace else, is the reason we never got the thin bed.

But --

MEMBER KRESS: You could solve a lot of the problems and issues if you could exclude the thin bed effect.

CHAIRMAN WALLIS: Just listening to Dave, my impression is that analyzing this thing away in light of all these tremendous uncertainties is far less effective than saying we'll put in a fix and we'll show that it works. And it will take anything that's thrown at it within reason, you know.

Take all the conservative assumptions, throw all this stuff at it. It will never make a thin bed. It will always work. It will back flush or it will clean itself by scraping or something and we've shown that it works. And we'll put it in the plant. And we'll put the whole thing to rest forever.

MEMBER KRESS: That's right.

CHAIRMAN WALLIS: This trying to analyze it and then getting new data two years from now which says I'm sorry, it wasn't 88, it was 200 or it was two million or something is not going to be a very effective solution.

MR. JOHNSON: And I would only add to that that in addition, you know, you take care of the coatings problems we talked about. You make sure that you take care of your latent debris through effective cleanliness programs.

CHAIRMAN WALLIS: You can do those things.

MR. JOHNSON: It can be done.

CHAIRMAN WALLIS: You can do some of those things, yes, but --

MR. ELLIOTT: This is Rob. I was just going to mention that's, in fact, despite what we talked about the BWR/URG, in fact, in practice, what most the BWRs did was put the biggest strainer in they could --

CHAIRMAN WALLIS: That's right.

MR. ELLIOTT: -- and then went back and used the URG to define what their licensing basis would be for that strainer so that they could make sure that they had criteria to make sure that they didn't exceed the design basis of the strainer.

But, in general, that's the way they did it.

CHAIRMAN WALLIS: This is probably the

engineering solution that an engineer would take rather than a regulator is say let's put something in which we know will work. And forget about all this other stuff.

MR. ELLIOTT: It's the same concept.

CHAIRMAN WALLIS: Right. Is that too simple to be considered? Do you have to have this extraordinarily complex business of analyzing everything in sight? Or can you put in an engineering fix and not have to do all those things?

MR. JOHNSON: It's sort of a choice of the licensee, I would think, to some extent.

MEMBER SIEBER: Yes, it's the licensee that does that.

CHAIRMAN WALLIS: It's the licensee's choice. I see. Well, maybe that's what they have to do.

MR. ELLIOTT: In the end, I still don't think you get away from having to have a methodology because you're going to need something to demonstrate your compliance.

CHAIRMAN WALLIS: Well, you simply say we know it will -- we've shown that it will handle anything you throw at it.

MR. ELLIOTT: Well, but then -- yes, if

you have some other, some other basis --

CHAIRMAN WALLIS: Right.

MR. ELLIOTT: But -- I see John Butler getting up here. Is he going to say something?

(Laughter.)

MR. ELLIOTT: Dr. Wallis, I wanted to point out there were grimaces in the back of the room as I was speaking so I wanted John to come up and have the industry --

MR. BUTLER: John Butler, NEI. I can hold my remarks until tomorrow.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: Are you holding very tight here?

(Laughter.)

MR. BUTLER: I'm steaming at what Michael is talking about. But I'll withhold my remarks until tomorrow.

MR. ELLIOTT: Okay.

CHAIRMAN WALLIS: Okay.

MEMBER SIEBER: I think in any event, the licensee needs a methodology to decide whether he should modify the plant and say this is that methodology.

MR. ELLIOTT: Or if they do decide to

modify the plant, then there needs to be some criteria in which they can say we're done.

MEMBER SIEBER: Right.

MR. ELLIOTT: We're in compliance.

MEMBER SIEBER: Right.

MR. ELLIOTT: And that still leaves us with some kind of methodology.

CHAIRMAN WALLIS: So we should leave some time tomorrow for responses from industry, NEI, and others -- other people who want to speak tomorrow in the audience? We'll try to give you some time.

We can dispose of some of these other items for which there isn't that much substance, I think. We can perhaps have you speak at around ten or ten-thirty or something like that.

Thank you.

MR. LU: We're done.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: So are we finished on this?

MR. LU: Yes.

CHAIRMAN WALLIS: Where are we in the schedule?

MEMBER SIEBER: Right on time.

(Laughter.)

CHAIRMAN WALLIS: Have we finished head loss?

MR. SHAFFER: Yes.

CHAIRMAN WALLIS: We're tired of head loss by now?

MR. ELLIOTT: We're tired, yes.

CHAIRMAN WALLIS: How much time do we need to do this? Maybe we can do physical refinements? We can do one of the two things that are left tonight?

PARTICIPANT: Physical refinements should only take five or ten minutes.

CHAIRMAN WALLIS: Five minutes? And how about alternative evaluations?

PARTICIPANT: The alternative evaluation is longer.

CHAIRMAN WALLIS: Can we do that, too?

PARTICIPANT: I'm sorry.

CHAIRMAN WALLIS: So we will try to cover Items 9 and 10 tonight, assuming it is going to take five minutes and ten minutes for those two?

PARTICIPANT: Well, no. We can start that and finish tomorrow.

CHAIRMAN WALLIS: Start that and then resume tomorrow. Okay. Thank you.

We'll take a break until -- how long can we take? Until 20 past six?

PARTICIPANT: Yes.

CHAIRMAN WALLIS: All right. Thank you.

(Whereupon, the foregoing matter went off the record at 6:06 p.m. and went back on the record at 6:21 p.m.)

CHAIRMAN WALLIS: We're back on the record. And we're going to see if we can make any progress.

MR. KOWAL: My name is Mark Kowal again. With me is Ralph Architzel and Tom Hafera. And we're going to go quickly through Section 5 of the guidance report and the safety evaluation report.

And basically Section 5 provides guidance and considerations for physical refinements that licensees can implement toward resolving the GSI issue. There is not a significant amount of information in Section 5. And some of it we've already discussed throughout the day today. So we'll try to go through this quickly.

Basically there are three areas of physical refinements that were outlined in this section. Ralph is going to talk to the debris source term. Tom is going to speak to the debris transport obstructions. And I will cover screen modifications.

Next slide.

MR. ARCHITZEL: Slide 3 please. On the debris source term, basically five categories for design operational refinements are discussed in Section 5.1. One is housekeeping and FME programs. And basically, recognition that enhanced FME programs, housekeeping programs, may be required.

As I mentioned before, the comment we have is that procedures need to be in place to assure that these programs are, if they're credited, are carried through.

We agree with basically all these refinements. They're operational. They're not technical refinements in that sense.

Change out of insulation, we agree with it. You need to be careful about creating additional debris when you do remove the insulation so there should be some caveats about being careful about taking that one and adding insulation, challenges to the latent debris when action is taken.

The next slide please, on 4, I'd like to mention modification of existing insulation. An example was pointed out earlier. You could double cover Cal-Sil, as an example, and then you increase your damage pressures.

Modifying other equipment, preventing filter housings from accepting water intrusion so you don't get the filters disintegrating and adding to the debris source term.

And then the last item the industry is proposing is to modify or improve coatings programs and to basically qualify them so they don't have the latent unqualified source term. And that's all.

CHAIRMAN WALLIS: These seem to be very straightforward things to do.

MR. KOWAL: Right. We don't have any problems with them. It may be difficult to do a coatings qualification program but the idea is the right idea to get off.

MEMBER SIEBER: Well, replacing coatings would be tremendously expensive.

MR. KOWAL: No, we're talking about in situ qualification --

MEMBER SIEBER: Okay.

MR. KOWAL: -- and what you need to do to say you've not got qualified coatings versus unqualified. There was a similar type discussion on the BWRs. You can take an effort to determine how your coatings were made and are they qualified.

CHAIRMAN WALLIS: You don't build an autoclave around a pipe and test it?

MEMBER SIEBER: No.

MR. KOWAL: You can test in place and there's different things that can be done. But we'd have to interact with the staff when they're actually -- you know, they'd have to have some basis for how they actually upgraded their coatings. But it's an effort. It's not a freebie. But then you could do that.

MEMBER SIEBER: Even if you qualified the materials, a lot of unqualified coatings don't have specifications on, you know, what the primers are or how thick everything should be. And even if they do, if it's unqualified, you may not have the documentation that proves it. So it's not a simple thing.

MR. KOWAL: No, it's not simple. But the point is you just don't have to throw your hands up and say everything is unqualified.

MEMBER SIEBER: Right.

MR. KOWAL: You can take some steps to reduce that term.

MEMBER SIEBER: Okay.

MR. KOWAL: And we're amenable to thinking that's a good idea.

MEMBER SIEBER: Okay.

MR. HAFERA: Section 5.2 of the NEI guidance report provided guidance regarding use of obstructions and debris racks to prevent debris from reaching the containment sump.

That could be applied either in areas of containment where the break location might be or where there's robust barriers. Or it could be around the containment sump itself.

MEMBER SIEBER: These would be things like curb?

MR. HAFERA: Things like curbs, fences --

MEMBER SIEBER: Okay.

MR. HAFERA: -- whatever type other things. The guidance report basically says that these would have to be considered on a plant-specific basis depending upon the configuration --

MEMBER SIEBER: Right.

MR. HAFERA: -- specific design, and also on the debris type to that specific plant, the debris distribution. And the velocity profile of their containment sump pool.

MEMBER SIEBER: Right.

MR. HAFERA: We agree with that. There

doesn't seem to be anything much more to add so we think as long as they consider those factors -- and the guidance report mentions things like considering sliding velocities and tumbling velocities of debris, so it's really pretty good. And we think it's acceptable.

MEMBER SIEBER: Okay. Mark?

MR. KOWAL: Next slide. Section 5.3 of the guidance report provides considerations for new screen designs that licensees that might decide they want to try and implement or incorporate into their plants.

In general, the staff finds these considerations to be a useful and acceptable introduction to what would need to be done to pursue these sump modifications.

And we emphasize two performance objectives for new sump screens. The design should accommodate the maximum volume of debris predicted to arrive at the screen. And the design should account for the possibility of thin bed formation.

Now we talked a little bit about this with the BWRs chose to install large passive-type sump screens with complex geometries and debris traps and things to make it difficult to form a uniform bed on the screen.

CHAIRMAN WALLIS: And when you rewrite the guidance or your SER, you're going to make it really clear what you mean by this thin bed?

MR. KOWAL: Yes. We will do that.

CHAIRMAN WALLIS: And what the conditions are for it to form and that sort of thing so that we know what it is and have some clue as to how to predict whether or not it forms.

MR. KOWAL: Then basically three designs were discussed in this section, the passive strainer designs, backwash strainer designs, and active strainer designs. And really passive strainer designs require no movement to perform their intended functions.

The GR guidance report offers considerations concluding the design is straightforward. BWRs have incorporated this design. They can be modular. Because they're passive, they have a high reliability.

And really the primary design concept with these passive screens would be to maximize the strainer surface area while trying to minimize the total volume.

CHAIRMAN WALLIS: These are all

qualitative. The problem that the licensee faces is he wants to buy a strainer and he needs to calculate whether or not it will work adequately. And I'm not sure there's any guidance for these unusual-type strainers.

MR. ARCHITZEL: They typically were tested in the past.

CHAIRMAN WALLIS: Right so he has to do a lot of testing or something?

MR. ARCHITZEL: They already are a set that are tested and they'd have to do testing generically. And there are vendors out there to do that.

CHAIRMAN WALLIS: So he has to test a strainer which he hasn't yet bought and he has to do some sort of --

MR. ARCHITZEL: The vendor tests them.

MR. KOWAL: The BWR has been through this. I think there were three or four vendors that provided the strainers. And they were not --

CHAIRMAN WALLIS: So the rational thing would be --

MR. KOWAL: -- plant specific --

CHAIRMAN WALLIS: -- for the industry to get together and to support some studies of really good designs which will work and then prove them out and then install them. That might be the rational thing for the industry to do?

MR. KOWAL: Yes.

MR. ARCHITZEL: I believe they're doing that.

CHAIRMAN WALLIS: Otherwise they're going to be buying things not quite knowing what you're going to accept.

MR. KOWAL: That's right.

Next slide. Backwash strainer designs, there were some considerations offered. And those are really where you might use an air- or water-type active system to backwash the debris off of the screen.

This type of system would require instrumentation, power supplies. There might be surveillance testing required to ensure it's going to perform its function. They going to need to use reliable, some reliability of components.

One of the big considerations includes the resuspension and settling of the debris. After you actually backwash, the debris will re-accumulate on the screen at some point.

CHAIRMAN WALLIS: Now going back to a

point made by one of my colleagues much earlier today, there must be a big knowledge base say in the chemical industry that faces this stuff all the time of how, you know, they have different kinds of strainers that they put in different kinds of material. And they know how they work. Can't you use that?

MR. ARCHITZEL: Well, the power plants do this all the time also --

CHAIRMAN WALLIS: Right.

MR. ARCHITZEL: -- but they process the debris out of the path. And that's the difficulty. So it's the difficulty of sequestering debris that is collected. Certainly utilities --

CHAIRMAN WALLIS: We've seen things that come in from --

MR. ARCHITZEL: -- know about strainers.

CHAIRMAN WALLIS: -- when water comes into a power plant from a lake, there's all kinds of things.

MR. ARCHITZEL: Right. And they do it. But there's not a place to place the debris inside a container.

CHAIRMAN WALLIS: Oh, you can't get rid of the debris? You can't put it in one of these compartments somewhere?

MEMBER SIEBER: Put it in a land/sea box.

Question, will backwash or an active strainer be safety related? If so, to what extent or defense in depth redundancy and so forth going to required?

MR. KOWAL: Well, that's one of the things we'll talk about next in the alternate evaluation section is --

MEMBER SIEBER: All right.

MR. KOWAL: -- is the possibility of new designs, new screen designs maybe not being safety related.

MEMBER SIEBER: All right.

MR. KOWAL: Or single failure approved.

CHAIRMAN WALLIS: I see my friend here points out the problem may be that in order to put in the strainer you'd like to buy, you have to bust some concrete and you might not want to do that because there's some pretty large hunks of concrete there and it won't fit. You run out of space.

MEMBER SIEBER: It's either concrete or the liner, you know.

CHAIRMAN WALLIS: Yes.

MEMBER SIEBER: And the liner is the boundary for the container.

CHAIRMAN WALLIS: And you don't touch that.

MEMBER SIEBER: I would think twice before I would do that.

MR. KOWAL: Okay, next slide. Active strainer designs were also discussed. An active strainer design would be a system that would provide for continuous cleaning of the --

CHAIRMAN WALLIS: I can just see a story --

MR. KOWAL: -- sump screen.

CHAIRMAN WALLIS: -- down the road. Someone buys the perfect strainer and there's no way to get it into the plant.

(Laughter.)

MR. KOWAL: A good design engineer could --

MEMBER SIEBER: There's always a way.

MR. KOWAL: -- think of that before they bought it.

MEMBER SIEBER: That's right.

MR. KOWAL: But this type of design could use a brush or some kind of scraping mechanism that would be continuously cleaning --

CHAIRMAN WALLIS: Now all this is --

MR. KOWAL: -- the sump screen.

CHAIRMAN WALLIS: -- sort of hypothetical. These things might exist. They all have to be proven, though.

MR. KOWAL: Yes, there are no active strainer screens that I am aware of in operation at least today.

CHAIRMAN WALLIS: So what would help industry would be rather than describing what might work would be to say how you would evaluate it if they did put such a thing in. That would be useful to them, wouldn't that? What you would accept as testing and what would you accept as uncertainty limits and things like that? Whatever?

MR. KOWAL: Right. And certainly there would need to be some testing to demonstrate that these would function.

MEMBER RANSOM: Do you mean active strainers in this application?

MR. KOWAL: Yes, active.

MEMBER RANSOM: Certainly we went out to Cook, you know, and saw the strainers they're using for the inlet water, they're quite unique. Are you familiar with them?

MR. KOWAL: I'm not familiar with them.

MR. ARCHITZEL: There was an active strainer that GE proposed for BWRs and it had, you know, scraping, et cetera. And they're talking about a motor-driven one. They may come in. They may not come in. There's been some discussion.

I don't know if there are other vendors but there's been some discussion of active strainers for this situation. And I guess I was challenged earlier, perhaps the industry really isn't uniformly pursuing those strainers as I thought.

MR. KOWAL: Well, I guess there's issues of they would need surveillance testing, operability testing, design testing. Those types of things may not deem them to be the choice strainer.

CHAIRMAN WALLIS: Okay.

MR. KOWAL: I guess that's about all I had to say. There's a couple other bullets there.

MEMBER SIEBER: Well done.

MR. KOWAL: Okay, then we can move on to Section 6.

CHAIRMAN WALLIS: Is this the risk base? Or the --

MR. KOWAL: Well, this is an alternate approach that includes --

CHAIRMAN WALLIS: This is going to take

forever isn't it? I'm not sure we want to embark on -- maybe you could summarize it quickly and then we can take it up in the morning.

MR. KOWAL: Okay.

CHAIRMAN WALLIS: Because I think this is a major topic. It's the risk informed --

MEMBER SIEBER: It's got a lot of slides.

CHAIRMAN WALLIS: This is more important than some of the things we're thinking of doing tomorrow morning. This is a really significant topic.

If you could sketch it out for us and maybe we could be quiet, you could do it very quickly.

MR. KOWAL: Okay. I could actually --

CHAIRMAN WALLIS: And then we can come back and ask you all the questions tomorrow morning. This is a really important aspect of the whole problem.

MR. KOWAL: -- I could actually suggest that I can skip over a few of the slides --

CHAIRMAN WALLIS: If you could just give us something to think about as we're dreaming.

MR. KOWAL: Okay.

CHAIRMAN WALLIS: And then we can --

MR. KOWAL: All right. So --

CHAIRMAN WALLIS: -- be ready tomorrow.

MR. KOWAL: The alternate -- this is an alternate approach for resolution of the issue. Basically we began working on this approach in April, I believe, of this year. We've had three public meetings with industry and stakeholders and discussed how -- what this approach -- how to develop and how to define this type of an approach.

And it sort of evolved into an approach that includes elements that are both realistic and risk informed. And it's similar to the 5046 rulemaking effort to redefine the large break LOCA break size where they've selected a transition break size.

What we've done with GSI 191 is selected a debris generation break size and for break sizes below that debris generation break size, customary design basis analyses would apply similar to the Section 3 type of baseline analysis that we've gone through today.

And the debris generation break size is defined as all auxiliary piping attached to the RCS. And it includes a break size equivalent to a 14-inch, double-ended 14-inch break in the main loop RCS piping.

The basis for the break size -- so

anything below that break size would fall into what we're calling the Region 1 analysis, which is the customary design basis analysis. Anything larger than that would fall into the Region 2 analysis, which would allow for more realistic --

CHAIRMAN WALLIS: Now where is the realism?

MR. KOWAL: -- or risk informed --

CHAIRMAN WALLIS: Is the realism in the accident analysis? Or in the debris transport of the sump blockage --

MR. KOWAL: The realism comes in the MPSH calculations and those assumptions. In both the Region 1 and Region 2 analyses, for the most part, the other phases of the debris generation, the zone of influence, the debris transport --

CHAIRMAN WALLIS: Are all the same?

MR. KOWAL: -- are all the same as we've talked about --

CHAIRMAN WALLIS: So there's no change in --

MR. KOWAL: -- in the baseline.

CHAIRMAN WALLIS: -- any of the those things?

MR. KOWAL: There is a change for partial breaks because the Region 1 analyses include breaks that are up to the double-ended 14-inch equivalent area.

CHAIRMAN WALLIS: So there's no attempt to say that the recommendation of the 600,000 is conservative, therefore for these bigger breaks, you can assume 500,000 for your specific area --

MR. KOWAL: No.

CHAIRMAN WALLIS: -- for the Cal-Sil or something. You could still use all the same numbers?

MR. KOWAL: That's right.

CHAIRMAN WALLIS: So really there's no change as far as we're concerned. The only thing is in the accident analysis part where you're --

MR. KOWAL: Right.

CHAIRMAN WALLIS: -- not quite so conservative.

MR. KOWAL: You'll have time-dependent variables --

CHAIRMAN WALLIS: Okay.

MR. KOWAL: -- you could use. You'll have for the MPSH calculations, you'll probably use more realistic parameters, maybe containment pressure -- for containment over pressure --

CHAIRMAN WALLIS: It's the accident

grounds.

MR. KOWAL: -- service water, component cooling water temperatures, those types of things. Now because we're still in design basis phase with this, there may be exemptions that might be required if licensees in the realistic space want to go with a non-safety-related or non-single failure proof-type of design on the strainers.

CHAIRMAN WALLIS: But if they -- when they're analyzing say the double-ended guillotine break, which is this region where you don't need to be so exact, they still have to use the same zone of influence and the same -- all these things we discussed today are exactly the same?

MR. KOWAL: Well, that's what is suggested in the NEI guidance. And the reason for that is -- I guess there aren't any existing realistic-type of models. There isn't that much testing available. Like all the things we've talked about today.

CHAIRMAN WALLIS: So none of the models existing today are realistic?

MR. KOWAL: Well, I don't mean to say it that way. I guess it's difficult to know or to come up --

CHAIRMAN WALLIS: Well, you said there are no realistic models --

MR. KOWAL: Well --

CHAIRMAN WALLIS: -- in a different statement than what I said. Or did I misunderstand? Maybe I misunderstood.

MR. SOLORIO: Dr. Wallis, this is Dave Solorio, I think what he was trying to say is there's not a lot of testing to support a new model. So, therefore, we're going with what we've talked about today.

And to some extent, we've already investigated or thought about the analytic improvements to the baseline. Those have been exhausted to the extent that they're defensible.

I would mention, I think what we're doing is say industry isn't the one that didn't propose any refinements to that aspect of it. So we're not proposing on our own. So if they had, we may have considered it, but they did not.

CHAIRMAN WALLIS: So they don't buy very much do they?

MR. KOWAL: Maybe we can talk a little bit about the MPSH calculations and how much that might buy them?

MR. LOBEL: Well, this is Richard Lobel

from Containment Systems. I can't give you a numerical value of what it would buy them. But things like the sump water temperature are very significant for calculating the MPSH. And if they're going to do a more realistic calculation of that without -- with a more realistic decay heat without the two percent extra --

CHAIRMAN WALLIS: This would effect head loss. This would effect the head loss calculation if they have a more realistic sump water temperature.

MR. LOBEL: Right.

CHAIRMAN WALLIS: They might even get into a range where they're allowed to use the correlation.

MR. LOBEL: That's right.

(Laughter.)

MR. LOBEL: So it will buy them something. And we've also had some discussions about credit for containment pressure, if that's needed like --

CHAIRMAN WALLIS: So it does effect what we heard about today? It might effect the head loss because you've got a different sump temperature, different viscosity --

MR. LOBEL: Right.

CHAIRMAN WALLIS: -- maybe different SV or whatever is appropriate. Higher viscosity is not good.

MR. KOWAL: And also water depth.

CHAIRMAN WALLIS: Water depth is different.

MR. LOBEL: Yes.

MR. KOWAL: Right.

CHAIRMAN WALLIS: So there are some differences.

MR. LOBEL: Yes.

MR. KOWAL: Right. And those are the types of things that would be considered in that.

MR. LOBEL: Also another important thing --

CHAIRMAN WALLIS: Well, are the things that are conservative for LOCA analysis still conservative for this? Or does it go the other way? It may be that some of the things you're made to assume for a LOCA, when you remove those conservatisms, it's not clear to me that they make things better for sump blockage. They may change the temperature of the sump in some way that makes things worse. I don't know.

MEMBER SIEBER: No.

CHAIRMAN WALLIS: They always help?

MEMBER SIEBER: I think so. It's just a

milder action.

MR. LOBEL: Yes.

MEMBER SIEBER: It's less harsh.

MR. LOBEL: Another assumption that's made for the MPSH calculations, we haven't gotten the details from the PWRs but one significant conservatism that the BWRs uses is that the pumps are pumping at a very high flow rate. If you use a more realistic flow rate, you have less required MPSH. And that gives you more --

CHAIRMAN WALLIS: The operators have throttled back on something?

MEMBER SIEBER: Yes.

MR. LOBEL: Well, throttled back or not assumed that the sumps are pumping at run out or maximum design flow.

MEMBER SIEBER: You don't have to run every pump.

MEMBER KRESS: So that would probably give a lot of margin, too.

MEMBER KRESS: Can those be variable speed pumps? They're electric motors.

MR. LOBEL: Well, yes. They may not be able to do that for the pumps. There may be pumps where they can. The other thing that they can do is turn off pumps that they don't need.

In the calculations, you might assume that you have a lot of pumps running that you really don't need to satisfy the flow for a realistic calculation. And, therefore, you have less flow going into the sump screen. So that would cut back on the head loss.

MEMBER RANSOM: Are the pumps, though, in separate sumps?

MEMBER SIEBER: No.

MR. LOBEL: The pumps are outside the containment.

MEMBER RANSOM: All drawing from --

MEMBER SIEBER: They draw from the --

MR. LOBEL: They're drawing from the sump but the pumps are outside the containment.

MEMBER RANSOM: So they're more or less in parallel, I guess.

MEMBER SIEBER: They have their own deep wells but it's all one sump.

MR. KOWAL: There are some plants that have multiple pumps. But the majority has one.

MEMBER SIEBER: Right.

MR. KOWAL: And there is a risk informed piece that Donny can talk about as far as crediting for operator actions.

MR. HARRISON: Right.

MR. KOWAL: And I'm not sure how much interest there is in that part of it at this time.

CHAIRMAN WALLIS: Well, I think we heard --

MEMBER SIEBER: Tomorrow there might be.

CHAIRMAN WALLIS: -- that the initial calculation of the effect on core damage frequency of this problem was that it was a big thing.

And then when you decide to credit operator actions, it actually didn't look quite so significant. As I understand it, there are quite a few things the operators can do to mitigate this accident.

MR. HARRISON: Well, and that's probably true except for on the large break LOCA, you're limited by time and just the sheer volume of --

CHAIRMAN WALLIS: Maybe they're discouraged from doing anything in the large break LOCA.

MR. HARRISON: Well, again, the thing that comes up in the risk-informed aspect of this is the mitigation capability that is presented by the licensee needs to be able to demonstrate a certain reliability. And you can back-calculate using the Reg

Guide 1174 criteria, you can back-calculate to a reliability that you need --

MEMBER SIEBER: To satisfy that.

MR. HARRISON: -- to satisfy that guideline. So that's basically the simple approach. And that would include both plant modifications if they put in an active strainer or it would include operator actions, say they turned a tray of containment spray pumps off. And they credit that to achieve that success in the model.

Then what you'd have to do is show the reliability of those combined actions are acceptable. So, again, it just becomes a real liability issue.

MEMBER SIEBER: Yes, the issue of contained spray is different than the below head safety injection.

MR. HARRISON: It's --

MEMBER SIEBER: They may not be required at the same time.

MR. HARRISON: Right.

MEMBER SIEBER: It would be beyond the break site.

MR. HARRISON. But if they take credit for that to show --

MEMBER SIEBER: Right.

MR. HARRISON: -- acceptable net positive suction head for the other part of it, then --

MEMBER SIEBER: Yes.

MR. HARRISON: -- that part of it has to be a reliable action.

MEMBER SIEBER: You mean as a way to cool it down.

MR. HARRISON: Right.

MEMBER SIEBER: Okay. Got it.

MR. HARRISON: So in a simple way, that's basically the approach. There was one aspect where we talked about passive failures. If they were to design the screen such that by design the screen functions and they meet their environmental conditions and all, then there wouldn't need to be a risk-informed aspect to that.

MEMBER SIEBER: Right.

MR. HARRISON: So it's only if they're actually taking credit for something or some plant modification beyond a passive screen design.

CHAIRMAN WALLIS: Now why would a plant ever want not to do this? Presumably if they pass a simple baseline, then they don't have to do anything. It's easy.

MR. HARRISON: I think that would be --

CHAIRMAN WALLIS: But you can always gain something by using this alternative approach.

MR. HARRISON: Right.

CHAIRMAN WALLIS: So it may well be that all plants, since almost all of them will not pass the really conservative baseline, will almost all want to select this option.

MEMBER SIEBER: I would think that licensees would want to explore this approach to learn how much margin they have and to give more flexibility to their operating the CMGs, for example, where you would be. You know?

You already have the programs in place and the people employed to do the work, so, you know, it's not like it would be a big additional expense. There's always something to learn from insights.

MR. KOWAL: So as an overview, that's what the alternate approach involves.

CHAIRMAN WALLIS: And how do you measure this mitigating capability that you're wanting to achieve? Is there some criterion for minimum mitigation that's acceptable or something?

MR. KOWAL: Yes. And, again, what we've tried to do is calculate a target reliability working backwards. So its mitigative capability has to have a 98 percent reliability.

CHAIRMAN WALLIS: So that's someone's choice of numbers that --

MR. HARRISON: Well, and here's the -- the bases are the two sub bullets there. One is we start off with Reg Guide 1174 guideline of 10 to minus 5. And then we use what I characterize as the highest large break LOCA frequency that's been published, which is the NUREG 1150 large break LOCA, and that's 5E to minus 4.

And we went there because we have an expert solicitation process going on. We don't have results from that yet -- final results.

CHAIRMAN WALLIS: But you've seen the preliminary ones which would that give you a lower frequency?

MR. HARRISON: Lower frequency. So this would bound that condition. So we know we're being conservative when we go this path. And, again, even being conservative, you really just have to demonstrate a 98 percent reliability or a failure probability on demand of, you know, two percent. So -- which you may be able to achieve with a single train.

And that brings us back to Mark's question of --

CHAIRMAN WALLIS: Assuming that all these conservative assumptions that we heard about for sump blockage and so on are within that 98th percentile of certainty? Is that --

MR. HARRISON: No, this is not a certainty calculation. This is just a strictly mean --

CHAIRMAN WALLIS: But isn't that also tied in with this? If you're looking for such a high reliability, then doesn't that also tie in with how sure you are about the conservative nature of your other assumptions?

MR. HARRISON: Well, I guess from a purist's standpoint, I would look at that as -- the modeling I do of the current condition and the modeling I do of the post condition are going to have the same issues with them.

If you can determine it's acceptable currently, you're going to carry that uncertainty. And from my perspective of trying to come up with what the mitigation system reliability needs to be, it's a pass fail.

You have to either demonstrate that you don't clog or you do clog. And the uncertainties that go with that are going to be there no matter what.

CHAIRMAN WALLIS: So someone calculates that the pressure drop across the screen is 25 feet of water. And he says, gee whiz, well I can just squeeze out enough water to cool this thing even with that.

Is that -- doesn't that raise the question of uncertainties in this 25 feet? If it were 27 he might be in terrible trouble. And if it were 23, he might not be. Little changes when you're near the margin make a big difference.

MR. HARRISON: And, again, maybe this is a --

CHAIRMAN WALLIS: We don't have much confidence in those numbers at that degree of accuracy.

MR. HARRISON: What I would say, though, is this is an uncertainty that's in the deterministic side of it.

MEMBER SIEBER: Right.

MR. HARRISON: Okay.

CHAIRMAN WALLIS: So you just forget that?

MR. HARRISON: Well, once I move over to this side --

CHAIRMAN WALLIS: I know.

MR. HARRISON: -- it either passed or it didn't pass that side.

CHAIRMAN WALLIS: That's right. It's just that I right then have a suspicion that you're focusing on the wrong thing. That you're looking for this 98 percent reliability whereas being 95 percent sure that you calculated the sump head loss correctly might have a much bigger effect on the answer.

MR. HARRISON: Right. No, I would agree with you there.

MEMBER SIEBER: Well --

MR. HARRISON: But, again, that's a different uncertainty piece you're looking at.

CHAIRMAN WALLIS: The problem of mixing deterministic with --

MR. HARRISON: The reliability part of it, yes.

MEMBER SIEBER: Well, one of the problems is you that don't have a way to verify that you're within the risk range that you want because you can't surveil that accident condition, so to speak. Or only once you can do that.

MR. HARRISON: You only get it once.

MEMBER SIEBER: Yes, right. So the testing that you would do to establish the sump won't clog is impractical.

MR. HARRISON: But, I mean theoretically

you could take the deterministic side and its uncertainty and carry it forward.

MEMBER SIEBER: You could.

MR. HARRISON: But that would be a complicated modeling. This is a very simplistic approach.

CHAIRMAN WALLIS: So this is something new that almost is worth almost half a day by itself if you really dug into it.

MEMBER SIEBER: I'm sure --

CHAIRMAN WALLIS: I'm not sure --

MEMBER SIEBER: -- we could do it.

CHAIRMAN WALLIS: -- we won't have that time. It seems to me this is a new step in the way you approach this issue.

MR. HARRISON: Well, again, I'm not sure if it's that new from a risk-informed standpoint. It's really just kind of working the problem backwards.

If I know what the answer is that I need to achieve, which is the 10 to minus 5 per year number for CDF, delta CDF, then I can kind of work backwards to figure out what reliability minimum do I have to have to get that.

So, I mean, from a strictly risk-informed

--

CHAIRMAN WALLIS: Do you have to change the 5046 in some way to achieve that?

MR. HARRISON: That's the question about if there needs to be a license amendment for this part.

MR. KOWAL: If 5046 rulemaking was completed already, we wouldn't need to use this approach.

CHAIRMAN WALLIS: You wouldn't need to do this?

MR. KOWAL: Right.

CHAIRMAN WALLIS: Because it would already been incorporated in that.

MR. KOWAL: That's right.

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: So this is sort of a --

MR. KOWAL: This is in advance of --

CHAIRMAN WALLIS: -- stopgap thing that --

MR. KOWAL: Right. And that's why we may need exemption requests.

MEMBER BLUM: So anticipatory regulation like anticipatory research sort of.

MR. HARRISON: I think that takes you back to where Mark was before of -- he had a slide on here somewhere I saw that talked about there might be a need for exemptions or license amendments as part of this approach method. So that's --

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: Go back to that --

MR. HARRISON: Uh-oh, see I shouldn't have --

CHAIRMAN WALLIS: -- you have all kinds of stuff there.

MEMBER SIEBER: Yes, you shouldn't have done that. You should have turned it off.

(Laughter.)

CHAIRMAN WALLIS: Are you going to turn it off or are you going to go all through this?

MR. HARRISON: No, no.

MEMBER SIEBER: We're going to do that tomorrow.

CHAIRMAN WALLIS: Are you going to go through all this tomorrow?

MR. HARRISON: Do you need to go --

MR. KOWAL: As much as you want, we can go through it tomorrow. We were prepared to go through it.

CHAIRMAN WALLIS: Well, the thing that interested me was, as you flipped it by, I saw the statement staff has no technical basis for accepting a translation to a sphere, talking about ZOI. No basis to judge that this is conservative, non-conservative, or realistic. Well, that sounds like the ACRS question this morning.

Are you now questioning the spherical zone of influence?

MR. KOWAL: This has to do with the application of the zone of influence for the partial breaks --

CHAIRMAN WALLIS: Right.

MR. KOWAL: -- in the main loop piping for debris generation --

CHAIRMAN WALLIS: But staff has no technical basis --

MR. KOWAL: -- break size.

CHAIRMAN WALLIS: -- for accepting a translation to a sphere.

MR. KOWAL: Right.

CHAIRMAN WALLIS: That's a pretty strong statement. And we were asking you if you had a technical basis. And now we've got our answer.

MR. KOWAL: Well, the guidance report --

CHAIRMAN WALLIS: I don't think you want to say that, do you?

MR. KOWAL: Well, the guidance report talks about two options here for how to handle the zone of influence for the partial breaks in the main RCS loop piping. One is to -- because it's directionally dependent, it's on the side of the pipe, I guess, the guidance report suggests either use of a hemisphere --

CHAIRMAN WALLIS: That's okay.

MR. KOWAL: -- or translating that hemisphere volume into an equivalent spherical volume. And using the sphere.

And what we're saying here is that we have no technical basis for knowing whether that translation from the hemisphere to a smaller sphere would be conservative or non-conservative.

And this is what Ralph had mentioned earlier this afternoon when he was going through the zone of influence.

CHAIRMAN WALLIS: Can you explain what a partial break is?

MR. KOWAL: Well, the partial break would be a break size equivalent to the area of a double-ended 14-inch --

CHAIRMAN WALLIS: But in a bigger pipe?

MR. KOWAL: -- but in a bigger -- in the

main loop piping.

CHAIRMAN WALLIS: Oh, that's a real problem because that might be a long fish-mouthed sort of thing which doesn't, at some point --

MR. KOWAL: Right. That's what we've been talking about a spherical zone of influence for double-ended breaks. And that's --

CHAIRMAN WALLIS: And the hemisphere is based on the idea perhaps that the fish mouth might spew out in several directions --

MR. KOWAL: Right.

CHAIRMAN WALLIS: -- but not behind itself, is that it?

MR. KOWAL: Right. Or it offers an alternative of using an equivalent volume sphere.

CHAIRMAN WALLIS: Well, is there anything else you can say as sort of an overview of this this evening?

CHAIRMAN WALLIS: And we can get into the details tomorrow?

MEMBER SIEBER: Say no.

CHAIRMAN WALLIS: Because maybe once we accept -- if we accept the idea of risk informing and of a critical break size where you do things a little bit different for the analysis of the accident as you would for the proposed change of 5046, if that is acceptable, maybe the rest of it follows, does it? We don't need to go into all the details?

MR. KOWAL: I agree.

CHAIRMAN WALLIS: Is that true?

MR. KOWAL: I don't think there's anything. We did issue -- write a SECY paper to inform the Commission of this approach in --

PARTICIPANT: Do you have copies of that?

CHAIRMAN WALLIS: Yes, we have. We have visited this before to some extent.

MR. KOWAL: -- in August. I think I've mentioned all the key points.

MEMBER SIEBER: Good.

CHAIRMAN WALLIS: So the main problem might be to convince the public that what looks like a relaxation based on risk information is okay.

MEMBER SIEBER: This responds to the recommendation in our letter.

CHAIRMAN WALLIS: Yes, I mean I think the ACRS --

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: -- likes the idea of risk informed.

MR. KOWAL: Yes, that is true.

CHAIRMAN WALLIS: We said you ought to pursue a risk-informed approach.

MR. KOWAL: Right. That is true.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: We said pursue. We didn't necessarily say recommend.

(Laughter.)

CHAIRMAN WALLIS: Pursue this fleeting -- do we need to do anything else?

PARTICIPANT: No, not tonight.

CHAIRMAN WALLIS: Do you have any sort of profound wisdom for us before we go to dinner so we can sleep on it?

MR. HARRISON: Well, I was just going to ask is there any material that we need to present tomorrow that or --

CHAIRMAN WALLIS: I think we might come back to this because this is a key thing, isn't it? This sort of risk informing, something you haven't risk informed before. And when we're a bit more alert, perhaps? Okay?

Anybody else wish to say anything before seven o'clock? One minute?

MEMBER SIEBER: No.

CHAIRMAN WALLIS: Anybody from the floor

can't contain your eagerness to say something now?

PARTICIPANT: We'll wait to tomorrow.

CHAIRMAN WALLIS: Wait until tomorrow, okay. So we will meet together for a really joyful occasion tomorrow at eight-thirty in the morning.

Thank you very much for everything that you contributed today.

(Whereupon, the above-entitled meeting was concluded at 7:00 p.m.)