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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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THURSDAY,

September 23, 2004

+ + + + +

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

MEMBERS PRESENT:

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

ACRS STAFF PRESENT:

Ralph Caruso	ACRS
Spyros Traiforos	ACRS Consultant

C O N T E N T S

P R O C E E D I N G S

(8:31 a.m.)

CHAIRMAN WALLIS: This is the second day of the meeting of the Thermal Hydraulics Subcommittee, and we were looking at the Alternate Evaluation yesterday. Does anyone have anything to say on that?

(No response.)

CHAIRMAN WALLIS: Okay. Mark Kowal.

MR. KOWAL: I am Mark Kowal from Containment Section and Donny Harrison from the Probabilistic Safety Assessment Branch.

We did spend some time last night going through an overview. We would like to go through our slides. Because we have discussed some of this already, we'll just try to skip through things we've discussed unless there are questions.

CHAIRMAN WALLIS: Maybe we can go a bit faster than we would otherwise.

MR. KOWAL: I'll try.

In summary, staff finds that this is an acceptable approach that can be used and involves both realistic and risk informed.

Next slide.

This just lists the points we'll discuss. I covered these in general yesterday. Milestones, as

I mentioned, there were three public meetings on this. The Section 6 of the guidance report was submitted in July, and staff issued a second information paper in August of this year.

Next slide.

We talked a little bit about the motivation for this. You know, it goes back to the NRC policy statement on CRA and the Commission's request to implement an aggressive and realistic approach to resolving GSI-191.

And as I mentioned, the ongoing rulemaking for 5046 and the effort to redefine the large break LOCA and, you know, a comparable approach for that. We think GSI-191 space is defining a regeneration break size.

Just to put things in perspective, as Dr. Wallis mentioned, this alternate approach is Option B in the guidance report. Much of this we covered yesterday. The alternate approach defines a debris generation break size that would distinguish between customary and more realistic analyses.

Next slide, please.

And because, you know, there may be exemptions that would be required in order to implement this approach, there might be license amendment requests required to implement this approach.

Plant specific exemptions could be submitted in accordance with 10 CFR 50.12, and licensees would assess the need for license amendments in this area using the 50.59 process.

Next slide.

Staff review and approval of any exemptions or license amendment request would be consistent with or consider the requirements in Reg. Guide 1174, standard review plan, Section 19, and also reviewing design basis analysis for compliance with 5046 for both the Region 1 and the Region 2 break sizes.

Next slide.

Okay. I mentioned yesterday what the debris generation break size was, how that was defined. Again, I'll go through this. All ASME Code Class 1 attached to auxiliary piping to the RCS. Design basis rules would still apply, and the basis for this really is double ended breaks in these types of pipes cannot completely be ruled out.

MEMBER RANSOM: The plant specific exemption, do you mean to be able to use this?

MR. KOWAL: No, not to be able to use

this. That may rise in the situation where a new strainer design might not be safety related. Plants would need to -- or single failure proof, I guess -- the plants would need to request an exemption for that from requirements of 50.46.

MR. LOBEL: I have a few examples of where there might have to be exemptions.

MR. KOWAL: Do you want us to get into that now?

MEMBER RANSOM: No, go ahead.

MR. KOWAL: Okay. Also, as I mentioned yesterday, the break size and the main loop piping would be a break equivalent to double ended rupture of the 14 inch pipe, which is approximately 197 square inches, and design basis rules will continue to apply in that space also.

For breaks larger than that break size, the regeneration break size, licensees would need to demonstrate mitigated capability to insure that they could mitigate the events.

In determining the break size, the staff considered ongoing research, expert elicitation work that's still in progress, and also the regeneration break sizes. It's consistent with the current 50.46 rulemaking transition break size.

One thing I would like to note. We are not redefining the design basis break size with what we're doing in GSI-191 in advance of the 50.46 rulemaking.

Next slide.

With respect to the 50.46 rulemaking, the staff does agree that licensees would be able to reperform their analyses using a break size consistent with a new size that would come about from the rulemaking. Based on the current status, staff doesn't expect that the break size would be larger than the debris generation break size defined here.

There is some guidance in the NEI document on consideration of single versus double ended auxiliary pipe ruptures. Basically there's a criterion given such that if a break occurs within a certain number of diameters from a normally closed isolation valve only a single ended break would need to be considered. The staff finds this to be acceptable based on the amount of energy available in inventory and volume available on such a break.

For example, for a ten inch or for a one foot diameter pipe break, this criteria would imply that there would be an isolation valve within ten pipe diameters.

CHAIRMAN WALLIS: Oh, that's what it refers to. The ten pipe diameters refers to the location of the valve?

MR. KOWAL: Yes, from the break. So it's a relatively small volume on the isolated side compared to the volume you'd have on the primary side.

CHAIRMAN WALLIS: That's assuming that someone has closed the valve?

MR. KOWAL: That's right. That's assuming the valve is --

CHAIRMAN WALLIS: Does someone know that the break is going to happen so they close the valve ahead of time?

MR. KOWAL: Normally a closed isolation valve.

CHAIRMAN WALLIS: Well, it's normally closed.

MR. KOWAL: If it's normally closed, not if it's normally open.

CHAIRMAN WALLIS: Oh, okay. I see. I'm still waking up here.

MR. KOWAL: So am I.

Next slide.

Some of the details in the Region 1 analysis. As I mentioned, this would be applicable to breaks smaller than the regeneration break size, which would include all auxiliary attached piping and RCS main loop piping up to the 14 inch equivalent break. Also any secondary side piping would be included in this Region 1 analysis space.

As we mentioned yesterday, many of the Region 1 analyses would apply. The baseline methods discussed in Sections 3 and 4 of the guidance report, including debris generation, transport, and accumulation on the sump screen. A full range of break locations would be assessed, as we discussed yesterday morning. Branch technical position, MEB-31 would not be applied.

Piping restraints and supports may be credited to limit pipe movement if analytically justified. However, the staff would note that these may not produce the limiting locations for debris generation.

Next slide.

This we mentioned yesterday, the zone of influence for partial breaks. I don't know if we need to go through this again, Dr. Wallis. This was the slide that caught your eye yesterday.

CHAIRMAN WALLIS: Just go through it quickly the way you've been doing it.

MR. KOWAL: Okay. Typically in the baseline methodologies, spherical zone of influence is applied for double-ended breaks. The guidance report offers a proposal that because you're going to have partial breaks in the main loop piping, they suggest the use of a hemisphere zone of influence or alternately translating that hemisphere into an equivalent spherical volume.

And the staff does not find this acceptable. We really have no basis --

CHAIRMAN WALLIS: You really have a choice of both. They could sort of do both and see which one looks best for them. That's not a very good rule or a very good guidance. Let them play around and see which one looks the best. You only have one or the other. Wouldn't that be better?

MR. KOWAL: Right. Well, our feeling is that you should use a hemisphere. Now the reason to use a sphere is to simplify the analysis because, you know, the hemisphere is directionally dependent. So we had no problem using a spherical --

CHAIRMAN WALLIS: So the hemisphere. You don't know how the break is going to be. So you'd have to rotate this hemisphere to find the worst place or something, wouldn't you?

MR. KOWAL: Right. Now, they could use a spherical zone of influence with the same radius as the hemisphere.

PARTICIPANT: The same or equivalent?

CHAIRMAN WALLIS: Equivalent you mean, equivalent volume?

MR. KOWAL: Equivalent, with an equivalent radius.

CHAIRMAN WALLIS: Equivalent volume. The same volume as the --

MR. KOWAL: No, no, no, no. A spherical volume -- a sphere with the same -- basically two times the hemisphere.

CHAIRMAN WALLIS: Two times?

DR. TRAIFOROS: And the hemisphere is defined? The diameter of the hemisphere is defined as what?

MR. KOWAL: That was not specifically described or discussed in the guidance. I'm not sure what or if they would fall back to the baseline methods for determining that, is what I expect them to be.

CHAIRMAN WALLIS: There must be a described method for calculating the radius of this thing. You can't just leave it up in the air.

MR. LATELLIER: God morning, Dr. Wallis. Bruce Latellier.

We've assumed that they would be computing the size of that hemisphere in the same manner that we've described for the spherical ZOI based on equivalent volume.

CHAIRMAN WALLIS: Equivalent volume, right. I thought that was what we said.

DR. TRAIFOROS: So it's two times the diameter basically, if you will. The equivalent volume, but this equivalent volume will be defined based on the trajectory of the jet. This is the starting point, but then we get into situations where we don't have, indeed, a double-ended break. We have a slot break, and then we don't have -- ANSI does not provide the guidance for this.

MR. LATELLIER: In fact, the ANSI jet model does have suggestions for a single ended break or a fish-mouth opening from the sidewall of a pipe.

DR. TRAIFOROS: You are right.

MR. LATELLIER: And that equivalent volume beneath a damage contour could be remapped into a hemisphere in much the same manner we described yesterday for a double ended guillotine break being mapped into a sphere.

DR. TRAIFOROS: You have to decide though if this is a double ended or a single ended, I mean, in terms of what the diameter is.

MR. LATELLIER: You're correct, and in this case, we're talking about tears in the sidewall. So they are single opening breaks.

DR. TRAIFOROS: Okay.

CHAIRMAN WALLIS: But the area is equivalent to the double ended 14 inch pipe.

MR. KOWAL: Yes.

CHAIRMAN WALLIS: It's a pretty big area.

MR. KOWAL: Right. There's guidance considers impacts of the break size on event timings and thermal hydraulic conditions, crediting operator actions. It can be done consistent with the current design basis considerations, and the acceptance criteria continues to be core and containment cooling based on adequate NPSH.

CHAIRMAN WALLIS: Now, that's where I think we need some discussion. I mean, is it clear? The SER says that the GR doesn't specify what is meant by adequate core cooling. That's the whole purpose of this whole exercise, is to cool the core adequately. How do you define that?

MR. LOBEL: We define that for Region 1.

It would be the same as the existing regulations and criteria of the SRP. For core cooling it would be 10 CFR 50.46, and for containment cooling it would be satisfying the design pressure and temperature of the containment.

So for Region 1 analysis there wouldn't be any change in the criterion.

CHAIRMAN WALLIS: Why isn't it the same in Region 2?

MR. LOBEL: Because that's a realistic analysis, and the decision has been made to use criteria that are more compatible with risks.

CHAIRMAN WALLIS: Do we know yet what adequate cold cooling means in the risk informed space? I'm not sure we know that yet.

MR. LOBEL: Well, I'm going to get to that. I can answer now or wait until we get to the Region 2 discussion.

CHAIRMAN WALLIS: II think we need to know that.

MR. LOBEL: Okay.

CHAIRMAN WALLIS: You can answer it when you get to it, but we need to get an answer to that question.

MR. LOBEL: For Region 1 for the NPSH

considerations, for the ECCS pumps and the containment spray pumps, for the calculation of containment conditions, the containment pressure and temperature, the methods used for calculating NPSH would be similar to the methods used for calculating the minimum pressure for LOCA, design basis LOCA calculation for reflood.

That is, you would assume conditions that would give you a minimum pressure, except that what you really want also and what's really more important is you want to maximize the sump temperature.

So we're not only minimizing the pressure, but maximizing the sump temperature since the temperature of the pump water has a significant effect on the NPSH.

Minimizing the containment pressure isn't important unless you are going to take credit for containment pressure in calculating NPSH. In this slide I've shown a few examples of some parameters and the way they might be biased for a conservative calculation for this type of NPSH calculation, Region 1 calculation where you're still doing things in terms of design basis.

And since it is a Region 1 calculation, it would have the types of conservatisms that are included now in a design basis calculation.

CHAIRMAN WALLIS: Yes, there really isn't much to say about Region 1. It's just the same as usual.

MR. LOBEL: Yes.

MR. KOWAL: The next slide.

The staff also offered some additional considerations in the SER regarding the Region 1 analyses, things such as the guidance report doesn't specifically identify which phenomena might receive time dependent treatment. This we should expect to be documented in the analyses that are performed.

CHAIRMAN WALLIS: What do you have in mind to be important here? What kind of time dependent phenomena are important?

MR. KOWAL: I would imagine pressures, temperatures. Anything else? I don't know. We haven't really seen what this might be actually.

CHAIRMAN WALLIS: I presume you're asking for it because it matters.

MR. KOWAL: I would think so, yes. We really haven't had any discussions with the industry about the details of how these calculations would be done, and we have had some talks among ourselves, but we haven't really defined in detail how the calculation would be done. There isn't any sort of standard review plan.

CHAIRMAN WALLIS: But it has been open ended if you're asking them to document information on all time dependent matters. It just seems a bit open ended.

MR. KOWAL: I would think that would be part of the analyses that are performed. I mean, whenever you do a calculation --

CHAIRMAN WALLIS: So you think it's going to be there anyway?

MR. KOWAL: I think it's going to be there anyway, yeah.

The next point here actually was a point that was raised that, you know, much of the data that has been developed for the regeneration or some of these things is based on conditions that might be indicative of double-ended breaks.

For example, the jet blow-down times in some of this debris generation testing may have been on the order of ten, 20, 30 seconds, and now if you have a partial break in the main loop piping, you might have blow-downs longer than that.

CHAIRMAN WALLIS: Right.

MR. KOWAL: That may effect, you know, jet erosion.

CHAIRMAN WALLIS: That's one of the problems with defining a jet pressure, that this cal-sil actually wear away if you direct a jet at it, and over a period of time, and that doesn't appear to be in any of the methods.

MR. KOWAL: Right. That's a --

MR. ELLIOTT: Well, there's a problem with residence time. What we found in the experimentation is that you didn't get a significant difference by extending the time of blow-down because generally the insulation was blown off the pipe and out of the immediate zone of influence down the test facility. So it wasn't sitting there trapped to be --

CHAIRMAN WALLIS: And then what happened to it? It was just lying around and nothing happened to it?

MR. ELLIOTT: Yes.

CHAIRMAN WALLIS: Well, if it is not blown off the pipe though, if it is an erosion, if there's something left there, then it might wear away, particularly if you band it all and sort of try and constrain it some more. Then it may erode rather than breaking off.

MR. ELLIOTT: That would be true.

CHAIRMAN WALLIS: And, again, I don't know that you have any basis for deciding how rapidly it erodes.

MR. ELLIOTT: That's true.

CHAIRMAN WALLIS: That's true? You said that's true?

MR. ELLIOTT: I think so. I mean, I don't see any fault with the logic on whether or not it would erode or not.

MR. KOWAL: Another consideration included that, you know, it is difficult to judge when maximum head loss might occur and how operator actions may impact, you know, and maximum head loss might not correspond with the, you know, minimum NPSH margins, depending on what's going on during the accident.

Also, if credit is taken for containment over --

CHAIRMAN WALLIS: Well, again, this is analyses to consider that it is difficult to judge. So that's a very strange way to say it. You should say analyses should evaluate when the maximum head loss does occur or something like that. One should consider that it's difficult to judge.

MR. KOWAL: Okay.

CHAIRMAN WALLIS: What are you supposed to do, throw up your hands and scratch your hair or something?

MR. KOWAL: That's a good point. I'm not sure what the wording in the SER is.

MR. LOBEL: What's usually done for the BWR is they divide their accident into a short-term and a long-term event, and they debris generation at the end of the short term and use that for the whole short term, and then for the long term they use the maximum debris loading at the time. So you want the maximum debris loading and apply it at the time of the maximum suppression pool temperature.

So they only have to calculate a debris loading once, and they use it once for the short term, once for the long term, and they apply it at the worst condition.

MR. KOWAL: And also if credit is taken for a containment over pressure, analyses should conform with the current guidance in Reg. Guide 182.

Next slide.

Okay. The Region 2 analysis basis is it mentions applicable for breaks larger than the regeneration break size. These are only in the RCS main loop piping breaks. Again, much of the Section 3 and 4 baseline analyses apply to this region. The full range of break locations would be assessed, and again, as in Region 1, piping restraints and supports could be credited.

Next slide.

Now we'll get into some of the NPSH and risk informed considerations in the Region 2 space.

CHAIRMAN WALLIS: So this is where we get into adequate core cooling?

MR. LOBEL: Right. The acceptance criteria proposed by the industry for Region 2 analyses are adequate core cooling and adequate containment cooling so that the containment boundary remain intact, and these weren't further defined. So in our SER, we applied definitions, and the definition we used is the definition of the scope of the emergency operator procedures rather than the severe accident management guidelines.

So the definitions correspond to the applicability of the EOPs, and adequate core cooling in these terms is the significant clad oxidation and loss of coolable geometry have not occurred.

CHAIRMAN WALLIS: Now, how do you define "significant"?

MEMBER FORD: Seventeen percent.

MR. LOBEL: Well, probably, yeah, 17

percent.

CHAIRMAN WALLIS: Oh, so that's much better, say, 17 percent. I know what you're talking about. Significant, some people might think it's one percent.

MR. LOBEL: Well, this is an area where an exemption might apply, where the regulation 50.46 is still in effect. So the limit would be 17 percent by the regulation, but an exemption might be asked for to go beyond the 17 percent because it's justified.

CHAIRMAN WALLIS: Now, this is for the point of view of hydrogen production. Is that why that's in there?

MR. LOBEL: The 17 percent?

CHAIRMAN WALLIS: Yeah.

MR. LOBEL: The 17 percent is in there really to maintain coolable geometry.

CHAIRMAN WALLIS: But it definitely depends where the 17 percent is?

MR. LOBEL: Well --

CHAIRMAN WALLIS: If you completely oxidize 17 percent of the cladding on those rods and not anywhere else, then --

MR. LOBEL: No, there's two criteria in 50.46.

CHAIRMAN WALLIS: There's a maximum.

MR. LOBEL: There's a maximum on the hot rod.

CHAIRMAN WALLIS: Why isn't that in here, too, or is it just supposed to be in here, too?

MR. LOBEL: It really is in there because they have to comply with 50.46.

CHAIRMAN WALLIS: Yeah, I thought they did. So what are you changing?

MR. LOBEL: Well, the only change is that we would consider going past that if there was an adequate argument made.

CHAIRMAN WALLIS: I think you make it into a jungle if you try to start defining loss of coolable geometry. I mean, Three Mile Island cooled, but not particularly effectively in the way that you'd like it to, but it did cool.

MR. LOBEL: Well, it wasn't really a coolable geometry.

CHAIRMAN WALLIS: Well, it cooled.

MEMBER KRESS: Isn't that your temperature limit on your hot rod, the plant's coolable geometry?

CHAIRMAN WALLIS: Coolable geometry is not the right term. It's really coolable without fission product or emission or something. It has got to be intact coolable geometry because you can cool anything.

MR. LOBEL: Right, and that's the purpose of the 2,200 degrees and the 17 percent oxidation.

CHAIRMAN WALLIS: Okay.

MEMBER KRESS: -- standard definitions.

CHAIRMAN WALLIS: I know, I know, but if you start allowing exemptions and people come in with some other description of coolable geometry and you start saying, well, maybe we should allow that.

MR. JOHNSON: This is Mike Johnson.

I really believe that we are not anticipating that we would go beyond 50.46, 17 percent, 2,200 in coolable geometry, whatever the words are in 50.46. We're entertaining, what we're looking at 50.46, risk informed 50.46 in terms of where the staff might go on that, I think in advance of that we would not be inclined to go beyond that for this issue.

CHAIRMAN WALLIS: Did you want to say that definitely in this?

MR. JOHNSON: Well, I think actually the words are okay. I just think we need to be clear that what is intended by adequate core cooling is adequate core cooling as provided for in accordance with the requirements of 50.46 at this time.

CHAIRMAN WALLIS: Yeah, I think what you mean is you're going to go with the existing 50.46, and if it changes, the you'll go without.

MR. JOHNSON: Right.

CHAIRMAN WALLIS: You'll go with whatever is in 50.46.

MR. JOHNSON: Right.

CHAIRMAN WALLIS: Why don't you just say that?

MEMBER SIEBER: There may be exemptions.

MR. JOHNSON: Well, again, I mean,m licensees can request exemptions at any time. I don't think at this time we'd be entertaining going beyond, and Rich and I haven't talked on this, but where we are today is I don't believe in light of where we're going on 50.46 and the discussions that we're having about how far we go and how fast we go, I don't think in sump space we would be entertaining changes to 2,200.

CHAIRMAN WALLIS: Well, Michael, I'm sure you're being sincere, but I've learned from experience that I cannot trust -- I won't put it that way -- that verbal statements by the staff really are not good enough because quite often you find the document changes because of some other consideration which may make a lot of sense.

MR. JOHNSON: I'm agreeing with you. I'm agreeing with you. We should sharpen up the words that we care about.

CHAIRMAN WALLIS: It has got to be in the document, and just your assurance at a meeting is not really --

MR. JOHNSON: Yes.

CHAIRMAN WALLIS: -- good enough, although I appreciate your statement.

MR. JOHNSON: Absolutely.

MR. LOBEL: Well, let me point out that as long as there's adequate NPSH for the ECCS pumps, there shouldn't be a problem.

CHAIRMAN WALLIS: There shouldn't be. I agree.

MR. LOBEL: And that's even though we're doing a more realistic analysis.

CHAIRMAN WALLIS: Well, then you've got this minimum number of pumps, yeah. Okay. Well, maybe we can move on.

MR. LOBEL: Okay.

CHAIRMAN WALLIS: We have looked at this one.

MR. LOBEL: Well, moving on, we also used the definition from the EOPs for the containment cooling, adequate containment cooling. As the containment boundary remains intact, and that's from the EOPs, and that insures according to the words in the EOP that the containment is in a safe and stable state and preventing fission product release. The containment boundary remains intact.

MR. CARUSO: What is the LA parameter.

MR. LOBEL: Yeah, I was going to mention that. The L sub A with this definition could be exceeded. L sub A is defined in Appendix J for containment leak testing, and it's the allowable leak rate that's in every plant's technical specifications, and it's the value that's used for dose calculations, the value of containment leakage that's used for dose calculations.

So this is another place where if we were going to go with a more liberal definition, there might have to be an exemption.

CHAIRMAN WALLIS: We asked you yesterday what industry buys by all of this. I mean, you have all of this regulation, and it's not clear how we can evaluate it without having some idea of is consequences. Is it going to result in significant changes to the plant or is it going to result in minor changes in operation or what?

MR. LOBEL: I'm not the one to answer that question.

CHAIRMAN WALLIS: I think it will be interesting to found out. I would always want to know the consequences of an action before I embarked on it. I don't know if the staff has the same attitude.

MR. JOHNSON: Well, we have, in fact, talked about the fact that it would be beneficial to see how if you ran through all of the baseline refinements for a plant X, let's say, just to see where that would take you in terms of what the debris, what the positive suction head would be, and what kinds of things you would need to do in terms of fixes, that's certainly something that we think would be worthwhile doing.

With respect to what benefit could be provided by, you know, this relaxation, I guess you would call it this Region 2 analysis and more realistic --

CHAIRMAN WALLIS: Well, I'd like to say I'm sort of surprised here because when we visited this a few years ago, it came as a proposal from industry as I recall, and industry promised us that they would look at this business, and you know, this was really for the large break LOCA. This is really a 50.46 question, but it's interwoven here; that they would look at it, and they would supply an argument for why there should be some relaxation, and they would talk about the consequences of it.

Now it seems to be turned on its head, and the agency is changing the rules without any awareness of what the consequences might be, which seems rather peculiar to me just personally, I mean, speaking as a member of the public rather than as a member of this committee. It seems the thing is backwards.

MR. JOHNSON: Actually I thought your question was what benefits might be achieved. You were asking about what consequences?

CHAIRMAN WALLIS: Yeah, what are the consequences of this and how can we make a decision about doing something without having some awareness of what's going to happen when we do it?

MR. JOHNSON: Well, the consequences, Donny can talk and talked about the consequences in risk space with respect to this alternative approach, and we think it's okay to go with that because the consequences are acceptable from a Reg. Guide 1174 approach. We said the.

The industry has articulated, and I know they can today talk about what benefits they think we accrue from more realistic analyses in this Region 2 areas, and in fact, Rich has given some ideas, gave some ideas yesterday afternoon, I think, about some of the things that could be done in the Region 2 analysis.

CHAIRMAN WALLIS: Well, is the rationale for doing this to try to solve GSI-191 more effectively? I mean, if it is so, then tell us why it solves it more effectively. Is the rationale for this to shrink risk space in line with risk informed regulation until it's more efficient and effective because that's a principle of the agency? Is that why it's being done?

What is the gain? What is the motivation for doing this? What is the justification for doing this?

It may look all right, but surely there's some argument, cogent, where you can say we're doing this because of A, B, C. It helps us to resolve GSI-something because blah, blah, blah, or something.

MR. JOHNSON: Yeah, there is, and we tried the -- I mean, the words in the SC in the start of the section that Mark is responsible for that tries to articulate why we think it's the right thing to do, and they sort of touch on the things that you've mentioned, Dr. Wallis. We think from a risk informed perspective it's the right thing to do.

I talked a little bit yesterday about, you know, why from a risk perspective it's the right things to do. We know that there's conservatism as Rich has indicated in the analysis, particularly in that positive section here, but also in other areas where it makes sense to go away from a conservative approach to a more realistic approach, and as long as we are sure that the requirements are 50.46 are met with respect to the things that we've talked about, 2,200 degrees --

CHAIRMAN WALLIS: So the reason you're doing this is to be virtuous, that going from conservative to realistic is somehow virtuous?

MR. JOHNSON: Well, and what it does for licensees is it enables them to put in place modifications with sufficient margin, with not overly demanding designs based on some over-conservatisms, I'll say. I'll use that word, over-conservatisms based on having to analyze for for the double ended break of the largest pipe in the RCS, for example.

So I think it benefits the industry, and it moves in the direction of being more risk informed. It's all those things that you've mentioned that we --

CHAIRMAN WALLIS: So you've given me one specific. You're sort of talking at a philosophical, hypothetical level, and I don't want to pursue this very much longer, but it would really help if someone would say if we do this, these are the sort of things that could happen, and then give a ten list and say that, well, these first seven make sense, but if we let them do eight, nine, and ten, which they could do with this, then we're going to run up against some other regulation, and gee whiz, you'd better think about that or something.

Has anybody thought about the consequences of letting industry do this? Does it impinge on other regulations? Are there some ways that go through the system or would things be applied for which you would then have to say, "Oh, sorry. We didn't really mean that," or something?

I mean, has anybody thought about these things?

MR. JOHNSON: Yes. Yes, we have.

CHAIRMAN WALLIS: But you haven't given me any examples of anything.

MR. JOHNSON: Well, you know, we are

thinking this is sort of bounded by fixes in the area of the sump, and we're dealing with, you know, some changes in long-term cooling.

Of course, the question that you ask is a really good question for 50.46, and of course, we're spending a lot of effort thinking about what are the tentacles of risk informing the break size. You know, what else does it affect in the regulation or what other plain changes might result from that, and are those acceptable?

I think we were able to draw a box around the changes or this area because it deals specifically with the sump, and we're able to look in terms of insuring that when you step back and you look from a risk perspective in a Reg. Guide 1174 approach and not just looks at delta CDF, but also looks at defense in depth and all those other things, that approach, we think that it's okay, and where it's not okay, Mark has indicated those areas in terms of the evaluation in this section, where we think even applying this approach we can't go further.

MR. SOLORIO: I would just like to add to what Mark said, Dr. Wallis. I'll get with Mr. Caruso and point out to him the words in the SECY that we put to talk about other issues. In coming up with this method, we involved research. We involved other parts of NRR so that we could make sure we were considering the impact of other regulatory requirements and Ill get quickly with Mr. Caruso to get him that.

CHAIRMAN WALLIS: Well, let me ask you something. With this option instead of having n plants that had to make major adaptations or modifications or buy stuff, would there be n over two plants or would there be n minus one plant? Would there be zero plants? And does this have a big effect on the resolution of GSI-191?

MR. JOHNSON: Can I ask -- I don't know -- Tony Petrangelo? I don't know. I don't know the answer. Maybe the industry has thought about what --

CHAIRMAN WALLIS: If it has no effect at all, we'll just forget it and we won't even talk about it. It's not worth it.

DR. PETRANGELO: Dr. Wallis.

CHAIRMAN WALLIS: Are you talking about resolving GSI-191? Does it have any effect or not?

DR. PETRANGELO: Dr. Wallis.

CHAIRMAN WALLIS: Yes.

DR. PETRANGELO: It's Tony Petrangelo from NEI.

The truth is we don't know.

CHAIRMAN WALLIS: You don't know.

DR. PETRANGELO: No.

CHAIRMAN WALLIS: You have no idea?

DR. PETRANGELO: No.

CHAIRMAN WALLIS: You can't give me any speculation?

DR. PETRANGELO: None whatsoever.

CHAIRMAN WALLIS: So we're taking a step in the dark, complete in the dark.

DR. PETRANGELO: To a large extent it's in the dark.

CHAIRMAN WALLIS: Thank you.

MR. JOHNSON: Can I just --

(Laughter.)

MR. JOHNSON: Tony, you really disappointed me.

Dr. Wallis, can I just add even though we don't know, I still think it's the right step. I don't think it makes sense to end up with an approach to the sump resolution that is blind to the direction that we're moving in 50.46, blind to the direction that we know we're going to take and with respect to 50.46 based on what the Commission --

CHAIRMAN WALLIS: No, no, no. You're doing exactly what the ACRS asked you to do.

MR. JOHNSON: Exactly.

CHAIRMAN WALLIS: And I'm just saying do you have any idea of the consequences? And it appears that nobody has, and that I find somewhat surprising. Now, maybe I'm just somewhat unusual, but before I do something, I like to know what the consequences are. That's all.

MR. HARRISON: Dr. Wallis, if I can just add one maybe little perspective though is in Section 5, there's multiple approaches to resolving the sump by design. You can design a passive sump that's way bigger than the one you've got. You can put in a sump that's got passive features or active features. From a risk informed decision making part of that, a licensee could go through the process and say there's pros and cons to each approach, and there's going to be costs associated with whatever approach you put in.

So there's going to be, I would assume, some licensees going through saying what's the best approach for my plant and how do I make that decision?

This process will give them that option.

CHAIRMAN WALLIS: I guess I'm disappointed because we're trying to resolve something that has been around for decades, and it would be very good, very nice; I'd be pleased, but maybe it's inappropriate. I would be pleased if you guys could say, "We have found the key to resolving it, and it's to risk inform it," and when you risk inform it, lots of the problems which we had with it go away, and we get a much more effective, quicker solution. That's what I'd like to hear.

DR. PETRANEGLO: Dr. Wallis, Tony Petrangelo again.

It's true that we don't know exactly what individual plants are going to do with Section 6 and how much of a difference that's going to make and the ultimate resolution of the issue at their specific plant. Does it mean a smaller screen or not enlarging the screen and all? The truth is we don't know.

But the reason we're doing this is to try to get to a solution that is at least driven to some degree more by what's risk significant than what's not risk significant, and that's usually a good thing to do.

And we do that with a belief that, you know, trying to focus more on things that are more likely and more realistic and not necessarily add conservatism on top of conservatism is the right thing to do, and in this case while we don't know what the specific consequences are for each plant, it's a step in the right direction.

I can think of a bunch of other things we could do that we didn't have time to do that are much more risk informed. This is baby step risk informed. This is a little bit of realistic conservatism on the lower likelihood of the spectrum of breaks.

That's about as far as we could go at this point. So this is a baby step in the right direction. I think to call this risk informed at this point, and we had this debate with the staff as we were discussing the guidance, it's more realistically conservative than risk informed at this point.

But with more time than perhaps this 50.46 rulemaking evolves and some potential modifications come out of that, I think we'll have a direct benefit to this particular issue, but the truth is we don't know what the impact is on specific plants in applying the methodology at this point.

DR. HARRISON: And from a practical standpoint of if a licensee were to want to come in with an active system, mitigation feature on his sump, but he only wants to have a single train, he could come in for an exemption using the risk informed path to get justification for that.

So, I mean, there is some practical

solutions to GSI-191 that are being supported through that that you wouldn't be able to do if you didn't have a -- again, I agree with Tony. It's really not as much risk informed as it is more just trying to be realistic in the application.

CHAIRMAN WALLIS: Okay. So you're being virtuous.

Okay. That was useful. Thank you.

MR. KOWAL: Rich, did you want to continue?

CHAIRMAN WALLIS: Yes, go on, unless the committee -- unless any of my colleagues wish to step in, let's move on.

MR. LOBEL: The staff has previously allowed credit for pump operation where the available NPSH, less than required NPSH for a limited amount of time, where that was supported by data for that pump.

And we would propose to allow the same thing if necessary in this case.

The realistic parameters used for the Region 2 analyses to calculate containment conditions for NPSH probably will preclude the request for over pressure. The experience with the BWR seems to indicate that slightly less conservative analyses than that is normally done usually eliminate the need for over pressure.

The guidance report proposed that if a nominal parameters defined as one with a realistic value used in these analyses is exceeded during plant operation, an operability assessment in accordance with generic letter 9118 would not be necessary as long as the situation lasted less than 30 days.

And the staff didn't agree with this proposal since the realistic analysis is still a design basis analysis, and Regulatory Guidance generic letter 9118 in this case should still apply.

And finally the -- almost finally -- the guidance report proposed exceeding the nominal EQ envelope, and it wasn't clear from the guidance report what was exactly meant by a nominal EQ envelope, and we assumed that this was an environmental qualification envelope determined by a realistic analysis, and using a more realistic environmental qualification envelope would be acceptable for the Region 2 analyses.

However, the equipment in question would still have to comply with 10 CFR 50.49. So if a piece of equipment exceeded this nominal EQ envelope, we think that would still require an exemption.

Environmental qualification isn't my area.

I talked to somebody who works in this area, and their reaction was that the staff doesn't typically give exemptions for environmental qualification; that if a piece of equipment is need, it's needed and it should be qualified, and if it is not needed, then it shouldn't be in the program.

So it's not really clear what the industry meant by this. Maybe we're misinterpreting what was meant, but it's not clear, and if this comes up during the plant specific reviews, we'll have to resolve it there.

And finally, the guidance report talks about crediting operator action, and the staff has no objection to crediting operator action for things that are reasonable. We do this now with the NPSH analyses that we have accepted, and in fact, there's a license amendment in house now from a PWR that proposes that the operator would turn off a train of containment sprays under certain conditions to minimize debris transport to the sump.

DR. TRAIFOROS: How do you address this equipment nominal EQ envelope in your SER? Are you commenting on this?

Because you indicated that --

MR. LOBEL: You used about the same words that I said here. It just wasn't clear what the industry meant. Maybe the industry can clarify it.

DR. TRAIFOROS: Yes, certainly they will, but it would appear that if you start changing the equipment qualification envelope, you may be deviating from the design basis approach, in which case you have to say that and say how far you are from this envelope and invoke some other justification for being able to do that versus deciding that you don't need an extra pump. Therefore, your EQ envelope is different because then you are redefining your EQ envelope.

MR. LOBEL: Well, it would still be design basis. It would just be defined with a different envelope.

Let me give you one scenario where I think this might be useful to the industry, and I'm making this up. I haven't discussed this with anybody in the industry, but support that a licensee had a piece of equipment that had gone through the Appendix B process and had been environmentally qualified, and that piece of equipment could no longer be purchased from the vendor as an Appendix B piece of equipment anymore.

So the licensee goes out to Radio Shack and buys a piece of equipment that will do the same job and then has to go through a dedication process, which licensees do for equipment. Most licensees, if not all, have a dedication program for that kind of thing, for taking commercial equipment and qualifying it for safety functions.

In that case, the piece of equipment may not be qualified for the conservative environmental qualification envelope, but it may be qualified for the nominal environmental qualification. In that case there wouldn't be an issue, but then suppose the piece of equipment wasn't qualified for the conservative envelope and also wasn't qualified for the nominal envelope.

The way I understood the proposal was that the piece of equipment could be outside the nominal envelope, but not outside the conservative envelope, if you follow that. That's my own scenario, and what we're saying is we wouldn't approve of that kind of thing, but that would have to be within the nominal envelope.

MR. CARUSO: Rich, I'm confused. On the one hand, I thought I heard you say that they could excess L sub A, which seemed to imply that they could allow the containment pressure to go higher than it would be allowed to go under the current licensing basis, but at the same time you're saying that the EQ envelope would provide -- they would be allowed to do realistic sort of containment analyses, which seem to imply that the EQ envelope would be less severe than the currently licensing basis.

MR. LOBEL: Right.

MR. CARUSO: How can the containment pressure be higher, but the EQ envelope be lower?

MR. LOBEL: Well, the EQ envelope is -- I'm not sure I understand. You mean using a realistic --

MR. CARUSO: Yeah. That's a good question. I would say we haven't thought this through to that level, and if the question comes up in a license application, then it will have to be discussed.

All I'm saying now, and I probably said too much, is we were trying to understand what was in the guidance report, and I may have interpreted what was meant incorrectly.

CHAIRMAN WALLIS: Is it likely you might rewrite part of the SER on the basis of this discussion?

MR. LOBEL: The SER says -- rewrite the SER?

CHAIRMAN WALLIS: No, just the part of it that refers to EQ envelopes. The other part is somebody else's job.

MR. HARRISON: Rich, it was just noted here though that L sub A here is not pressure. It's the actual leakage term.

MR. LOBEL: Right, but if the pressure is greater --

MR. JOHNSON: But the idea being that the pressure would be greater than what would normally be allowed.

MR. CARUSO: The L sub A could possibly be greater.

MR. JOHNSON: Right.

MR. CARUSO: Then the leakage could possibly be greater than the L sub A.

MR. JOHNSON: Right. That's what I thought the logic was there.

MR. CARUSO: Yeah, right, right.

MR. SOLORIO: But, Rich, correct me if I'm wrong. I don't think I saw anything in the SE that would be impacted by a discussion that we just had, and the SE sort of says -- well, you tell me. What does the SE say again?

MR. LOBEL: The SE says the staff will assess the application of EQ envelopes as part of the generic letter, response, reviews, and closeout process.

MR. SOLORIO: And licensee should consider whether exemption is needed.

MR. LOBEL: Right.

MR. CARUSO: It sounds like it's just saying that you will consider this on a case-by-case basis.

MR. LOBEL: Yes.

MR. CARUSO: So there is no detailed guidance other than do what you think you can and we will consider it.

MR. LOBEL: Right.

MR. CARUSO: It's just that I heard these words here about allowing higher containment pressures, but at the same time allowing equipment to be qualified to a lower EQ envelope, and that wasn't consistent to me.

MR. LOBEL: Yeah, you're right.

CHAIRMAN WALLIS: Well, what about this business of allowing the containment pressure to be greater than the design pressure? Is this a new thing you're allowing?

MR. LOBEL: Well, if we're going to do the same thing we're doing with 50.46, then we're not going to allow it. I guess we'll change the SER.

CHAIRMAN WALLIS: So that was a misunderstanding that you might be allowing containment pressures higher than design pressure?

MR. SOLORIO: No.

MR. LOBEL: No.

MR. SOLORIO: I don't think the SER says that. I think that was his example.

CHAIRMAN WALLIS: But it was said here.

MR. SOLORIO: And it's not in writing as you hinted earlier.

CHAIRMAN WALLIS: It's in the SER?

MR. LOBEL: It's in the SER.

CHAIRMAN WALLIS: Well, what would the public think of that?

MR. LOBEL: What would the public think?

CHAIRMAN WALLIS: If you skip this thing. NRC now allows containment pressure to be greater than design pressure. How do you explain that one?

MR. LOBEL: I'd rather not be the one to explain it.

MR. CARUSO: The containment design pressure and the containment design temperature may be exceeded for analysis of breaks above the DGBS as stated in this section of the GR.

CHAIRMAN WALLIS: How much by how much? Because of this LA? Is the LA determining criterion or something?

MR. FEIST: Excuse me. Can I give some comments on this?

I'm Chuck Feist from Comanche Peak.

All of your peak containment pressures and temperatures happen 30 minutes before you're on recirc and you're in the sump issue. So I take it that what this is saying -- and you can take credit for operator action -- is if your sump clogs, I'll use my plant as an example. I have a partially submerged sump. So what we would do is we'd stop the pumps. That has always been in procedures, and we would add more water containment until we would submerge the sump.

During that time we would exceed our design EQ envelope, but if we used realistic analyses such as used for PRA, we would still be able to show it was below our equipment qualification envelope. I think that's what's intended.

CHAIRMAN WALLIS: Well, this disk containment pressure, presumably if you lose some of your long-term cooling, you build up pressure in the containment, don't you?

MR. FEIST: Yes.

CHAIRMAN WALLIS: And eventually you have to worry about how big that gets.

MR. FEIST: Yes, and if you used design analysis, it would exceed the qualification envelope for the equipment, but if you use realistic analysis it wouldn't.

CHAIRMAN WALLIS: Does that mean exceeding the design pressure for the containment?

MR. FEIST: No. Just EQ envelope. You would never exceed --

CHAIRMAN WALLIS: I don't know what equipment you're talking about when you just talk about equipment in general.

MR. FEIST: Yes.

CHAIRMAN WALLIS: Electrical and stuff like that.

MR. FEIST: Yes.

CHAIRMAN WALLIS: Well, shall we move on? Because I want to ask a question on the next one, and then we perhaps need to wrap this up and move on.

MR. HARRISON: Okay, and that's fine. I think we discussed this yesterday evening anyway, the way the risk informed aspects of the SE are consistent with what the guidance report says also. They back calculate a target reliability for the sump mitigation capability using the acceptance guidelines for delta CDF and the Reg. Guide 1174 and the NUREG 1150, the large break LOCA frequency.

CHAIRMAN WALLIS: Does this sort of indirectly address the question I wanted to ask, which is what is the effect of taking this step on the level of safety of these plants? It's the sort of question the public would ask, and I think you ought to make an attempt to answer it.

MR. HARRISON: From a risk perspective, it would, but --

CHAIRMAN WALLIS: It would. What effect would it have in some sort of meaningful terms?

MR. HARRISON: What you do is you say we're meeting Reg. Guide 1174 with the mitigation capability that --

CHAIRMAN WALLIS: But the public doesn't know. So what would you say to the public? Some member of the public says, "What is the effect of this step on the safety level of nuclear plants?" A perfectly reasonable question.

MR. HARRISON: The results is the mitigation as approved by the staff would result in an at most small change or small increase in risk, acceptably small.

CHAIRMAN WALLIS: How big is that risk? Can you put it in some terms the public might understand?

MR. HARRISON: In reality if we were to go there, I would say if you were to use a more realistic frequency for a large break LOCA, it would be down in the noise. You would --

CHAIRMAN WALLIS: That doesn't mean anything to the public. Noise doesn't mean anything to the public.

MR. HARRISON: But neither do the numbers.

CHAIRMAN WALLIS: One, one, seven, four doesn't mean anything. If you could say that the risk has changed by one part in a million or something, I mean, that might mean something to the public.

MR. HARRISON: By one in 100,000, right?

CHAIRMAN WALLIS: Okay. Something like that is a good answer.

MR. HARRISON: And that's the high. That's the max.

CHAIRMAN WALLIS: -- in the risk or that's the change in the risk?

MR. HARRISON: That's the change in risk.

CHAIRMAN WALLIS: But it's not the fractional change in the risk.

MR. HARRISON: Well, if you take a plant that's already around --

CHAIRMAN WALLIS: What's the fractional change? The risk before was something, afterwards something else.

MR. HARRISON: Right.

CHAIRMAN WALLIS: How much has it fractionally changed?

MR. HARRISON: Again, it --

CHAIRMAN WALLIS: Has it gone up by one percent or ten percent or .001 percent?

MR. HARRISON: If I used the conservative approach here, I would argue that it is probably -- and they meet the target reliability just barely so that they're at the 98 percent, they're going to have a percentile increase that's probably less than ten percent. So it's more like five percent.

CHAIRMAN WALLIS: Okay. So you're allowing maybe a five percent increase in risk?

MR. HARRISON: Using a conservative, simplified approach. In reality, if you were to use a large break LOCA --

CHAIRMAN WALLIS: And this is justified because the risk is so small in the first place; is that right?

MR. HARRISON: If you were to use a realistic large break LOCA frequency and not the conservative one we're using here, it would be down below one percent.

MR. CARUSO: And that's per plant.

MR. HARRISON: Correct. We do the analysis on a per plant basis. We regulate on a per plant basis.

CHAIRMAN WALLIS: I think you ought to be able to give answers like that. It would be helpful to someone who reads the transcript or wants to know.

Thank you.

MEMBER SIEBER: On the other hand, there are some plants that don't meet the current safety goal. So they can't use this approach at all.

MR. HARRISON: Well, what we wrote in the safety evaluation was that if a plant was -- within Reg. Guide 1174, there is a requirement that if you're above -- not a requirement, but guidance that says that if you're above one in ten minus four that you really should be focused on reducing your risk at your plant, and for those plants the goal is to come down in risk, and they should be taking those steps.

MEMBER SIEBER: So what I said is correct.

MR. HARRISON: If that were correct, then

I would expect that the mitigation capability required under this would need to be greater and/or the plant would have to come in with a technical justification for why they wouldn't need to do that, part of which could be the conservatism in the current model.

In other words, if I'm -- most plants in internal events, I can't imagine anyone being above ten to the minus four from internal events PRA. What's going to drive them there would be some consideration of a modeling that's conservatively done that's got them over ten to the minus four.

In that case what they could do is come in with a technical justification arguing why the conservative modeling has caused that and how a more realistic model would keep them below the ten to the minus four baseline value.

MEMBER SIEBER: Well, the only risk numbers that are part of NRC's records are the ones that were submitted years ago, and there are seven or eight plants in that --

MR. HARRISON: Collectively, right, but since that time with risk informed applications, we've received risk informed applications from almost all the plants, and I would be surprised if from internal events there would be a plant above ten to the minus four. That would surprise me today.

CHAIRMAN WALLIS: I don't think you give yourselves enough credit. I think that what you ought to be saying is that by resolving GSI-191, we're actually reducing the risks of the plant.

MR. HARRISON: Well, you're reducing it from the current conditions.

CHAIRMAN WALLIS: And the effect of this risk informed perturbation is less than the gain we get by resolving GSI-191. That's what I'd like to hear. That would give a really good argument. I mean, get on with this thing; resolving this thing that's floating around. No one quite knows how risky it is, and we don't get good measures for what the risk is. It changes depending on what you credit.

If you could really show that before you did this the risk was so much and after it was something else, and everybody was better off, that would be a very happy ending.

MR. HARRISON: Dr. Wallis, that's exactly the point.

CHAIRMAN WALLIS: Why can't you say that?

MR. HARRISON: We can say that.

CHAIRMAN WALLIS: But why can't you give me some numbers then?

MR. HARRISON: I mean, maybe that's my fault. What I've done in this part of the presentation is looked at the delta risk from an ideal situation.

CHAIRMAN WALLIS: Okay. What's the gain then in resolving GSI-191?

MR. HARRISON: You will basically do away with a failure mode that's currently there.

CHAIRMAN WALLIS: And what's the risk benefit?

MR. HARRISON: Numerically I think the arguments between industry, if you go back to the LANL (phonetic) report, the numbers were fairly high.

CHAIRMAN WALLIS: They were surprisingly high in the first report, yeah.

MR. HARRISON: Right, and again, dealing with large break LOCAs even within the second report, you gain some benefit from recovery actions, but you don't gain that much. So the real benefit is to fix the sump, and to make it functional and put away the problem. You are correct.

CHAIRMAN WALLIS: Do you have some numbers on there or is it speculation that you're actually gaining something?

MR. HARRISON: Well, you would clearly

gain because you're basically taking a condition where we believe there's a vulnerability. That's not the right word for it, but --

CHAIRMAN WALLIS: You believe, but do you have a number?

MR. JOHNSON: Yes. Donny, we've given numbers before, haven't we with respect to --

MR. HARRISON: They're in the technical assessment.

MR. JOHNSON: They're in the technical assessment report.

MR. HARRISON: Yes.

MR. JOHNSON: Do you recall what the number is?

MR. ARCHITZEL: Ralph Architzel.

There was a problem with the frequency on average for all the plants.

CHAIRMAN WALLIS: This is much more significant than this small perturbation by 1174.

MR. ARCHITZEL: The bottom line number was double the average core damage frequency for the feeder plant. That was the bottom line. There were other numbers that were --

CHAIRMAN WALLIS: Well, that's a significant achievement, and it would be really important to do it, to do it right.

MR. JOHNSON: Yes, and we can indicate that number in other documents on other occasions.

CHAIRMAN WALLIS: Well, I think this is what you need to tell the world, that you're solving a problem that has this value to the public. And your proposed solution will actually solve the problem. Because that's what we're here for, is this particular GSI-191, you know.

MR. JOHNSON: Yes.

CHAIRMAN WALLIS: Which has been around a long time.

MR. HARRISON: You're totally correct, Dr. Wallis. The benefit is that you have the improvement. From the risk informed standpoint if a licensee comes in for an exemption, the risk part of this is not taking the plant to a perfect condition. Idealized condition is what I refer to it as, where you design the sump to mitigate the condition with no mitigation capability at all.

That's what this delta risk calculation is going to, the ideal plant from the fix that you're proposing. That's the delta. But you are right. The actual fix will be as Ralph was saying. It's a factor of two improvement in this part of the plant.

So with that we'll just move on to the next slide. Again, just recognizing where this was done actually we can run through this real quick because this just tells you how we simplified the calculation. It's being done for the whole break thing. Even though this is a Region 2, we did the LOCA for the whole spectrum of a break. So just using the new Reg. Guide 50 number.

The base conditions assumes there's no clogging potential. This is the idea case. If I were to be fixing the sump perfectly, you would do away with a failure mode of sump clogging.

The next one is that in a mitigated case where I'm taking credit for some type of mitigation, I'm assuming that you will always clog the sump if the mitigation fails, and that's not necessarily always true.

CHAIRMAN WALLIS: So you're allowing some probability of sump clogging essentially?

MR. HARRISON: What I'm saying is if I have an active mitigation system that I'm relying on and that mitigation fails, or if I have an operator action to, say, throttle back core sprays, containment sprays, and he fails to take that action, I immediately assume I get the sump clogged.

MEMBER KRESS: Which is the same as assuming you get core damage.

MR. HARRISON: And the next bullet. If I get the sump clogged, I assume I go to core damage. So there's no recovery from that condition.

CHAIRMAN WALLIS: So just tell us the effectiveness of a design which allow you not to clog the sump.

MR. HARRISON: Right.

CHAIRMAN WALLIS: Or a design including action by operators or whatever.

MR. HARRISON: Right, right. And that's what the delta risk calculation we're doing actually is doing.

CHAIRMAN WALLIS: This gets to my question: what's the value of doing this? A non-clogging sump is worth so much in risk space.

MR. HARRISON: Right. Okay, and again, just the bottom line is that the approach is consistent with Reg. Guide 1174. We did add a requirement consistent with Reg. Guide 1174 that you'd have a performance monitoring. That's the fifth principle in that reg. guide, and so we established that there needed to be a performance monitoring program for it to assure whatever capability that you took credit for, that that maintains that reliability.

This one we talked about yesterday was just dealing with what do you do with passive components. The first one is let's say someone designs a big sump that takes care of the problem up front. Well, you don't need to come up with a failure probability for that because you've shown by design that it's functional. You've met. You've done the regional. It's essentially if you were doing a Region 1 analysis all the way through, you could walk away from this or we also gave the option that given that there's going to probably be credit for operator actions that are more likely going to be, you know, ten to the minus three range, we didn't think it was necessary to look at failure modes that were below ten to the minus five or so.

So if you've got a passive component, we don't expect you to go off and figure out its contribution if it's in the ten to the minus five, ten to the minus six range.

However, there's a caveat on that. It says if you can actually determine the reliability for that component, you probably ought to include it. So if you can measure it and inspect it, then include that piece of it.

That's it.

CHAIRMAN WALLIS: Thank you.

MR. KOWAL: I guess that's the end of our presentation.

CHAIRMAN WALLIS: Does anybody else wish to say anything at this time? Comments from the members of the public that are coming in later?

Shall we move on? We've got a few items that probably won't take very long. Thank you very much. I think this was an important aspect of the whole question.

What is this? This is something else?

MR. JOHNSON: Dr. Wallis, we had items, sump structural analysis and upstream effects that we can touch on very quickly.

CHAIRMAN WALLIS: I think, yes, it's probably not a very significant item. Presumably someone has the wit to make a sump which won't collapse.

(Laughter.)

CHAIRMAN WALLIS: Do you need to regulate that? Maybe you do.

MR. HAFERA: I'm Tom Hafera, Plant Systems Branch.

I reviewed Section 71, sump structural

analysis at the NEI document, and I think as we basically said, the NEI document says the sump must be capable of withstanding structural loads based on maximum to reload, rated flow rate plus hydrodynamic loads from a seismic event. That's what NEI recommends.

We looked at that. We agree with that. You can't provide any real specifics. It's going to be plant specific based on all of the variabilities and factors of sump design and what have you. So that's about all the guidance that really can be provided.

We agree with those four items that they provided and did clarify that, yes, Reg. Guide 182, Subsection 1 --

CHAIRMAN WALLIS: Are there any dynamic effects when a sump clogs and a pump is struggling? Do you get flow fluctuations which could put fluctuating loads on the screen?

MR. HAFERA: Yes.

CHAIRMAN WALLIS: And is this taken into consideration?

MR. HAFERA: Well, NEI, and we agreed, mentioned hydrodynamic loads.

CHAIRMAN WALLIS: Do we know how to

analyze that problem?

MR. HAFERA: The boilers had to do it.

CHAIRMAN WALLIS: Yes, I should think they should. I mean, this is always something you have to consider, but the science of it is not always that well developed. The fluctuating load question is when you sought to push things to flow rates near some limit, there's always something you have to think about, and the methods for handling it are not always very well established as far as I can figure out.

MR. HAFERA: This approach is consistent with the BWR URG.

CHAIRMAN WALLIS: It's consistent with what the BWRs did.

MR. CARUSO: Do you know if there are any plants that have sump screens that are located within the zone of influence?

MR. HAFERA: That, the whole question of sump screens being within the zone of influence, that was brought up, and it is identified in the SER that there's a GDC. That's a requirement. Bruce Latellier is familiar with that.

Bruce, what did we require for jet impingement on the sump screen that's --

MR. ARCHITZEL: The sump screen is not

treated differently than other components there.

MR. HAFERA: Yeah.

MR. ARCHITZEL: You can show the GDC-4 requirement. You're allowed to exempt it for the pipe whip and all of those type requirements. It's not treated differently that way

MR. CARUSO: Well, wait a minute, wait a minute, wait a minute. Don't go so fast. Does that mean that they don't have to consider the effect of jet impingement on the sump screen?

MR. ARCHITZEL: It means they follow different rules.

MR. CARUSO: So that means if the jet impinges on the sump screen and destroys it --

MR. HAFERA: Ralph, it means the rules are different. We're not going there in terms of GDC-4 with this analysis. And then you do get into the type of pipe you've got.

MR. ARCHITZEL: You have to recognize that whole issue is outside the issue of clogging a sump screen. Clogging a sump screen occurs 20 minutes after your LOCA. When you go on to research after it is already under five feet of water, you can't get a jet on a sump screen that's under five feet of water.

CHAIRMAN WALLIS: No, but the jet happens before. The jet happens at the LOCA.

MR. HAFERA: Right. The jet would happen at the LOCA, which, again, there's already written requirements for how to analyze that.

MR. CARUSO: And those written requirements state that they're not required to consider the impact loads of the jet on the sump screen.

MR. ARCHITZEL: There is guidance for making sure that the sump is not generally in the path of a high energy jet and things like that. That's in the reg. guide. So they're design requirements.

MR. CARUSO: Are there any plants that have sump screens that are located in a zone of influence?

MR. ARCHITZEL: Well, they would have had to look at that when they do their zone of influence.

MR. CARUSO: Does anyone know?

PARTICIPANT: The answer is yes.

MR. ARCHITZEL: Okay, but again, that's not part of this presentation.

CHAIRMAN WALLIS: Does anyone have experience of operating pumps with clogged screens, partially clogged screens? And do they shake? Is this all a theoretic --

MR. ARCHITZEL: There's lots of operating experiences that show sump screens clog and thin bed effects do occur, and, yes, there's all kinds of operating --

CHAIRMAN WALLIS: In real systems?

MR. ARCHITZEL: Real world, yes.

CHAIRMAN WALLIS: I mean in real nuclear plants?

MR. ARCHITZEL: Real nuclear plants.

CHAIRMAN WALLIS: And there's no problem with surging at the pump or fluctuating flows?

MR. HAFERA: Dr. Wallis, this comment was really more towards the earthquake effect over 30 days.

CHAIRMAN WALLIS: No, but I'm just wondering. I mean, in view of -- I don't understand why, but certainly in the LANL experiments there were quite unexplained fluctuations in the flow. It may be something to do with their system, nothing to do with reactor systems, but I mean, one always worries a bit about fluid structure interaction, particularly when things are reaching some sort of limit of operation.

MR. HAFERA: Pump cavitations are pretty well understood.

CHAIRMAN WALLIS: No, that's all right.

I'm talking about the fluctuating flow and the effect on the screen. I don't know if my materials colleagues said they're happy with that or not, but they have experience with fluctuating loads on things that sometimes break them. I just wondered if you shouldn't have a position on this. That's all.

There's no staff position on fluctuating loads?

MR. HAFERA: I could revisit that and clarify it.

CHAIRMAN WALLIS: Are you going to revisit it?

MR. HAFERA: Yes.

MEMBER RANSOM: Well, is a factor of safety applied to the design load?

CHAIRMAN WALLIS: I can't imagine that it isn't, but there seems to be a great silence.

MEMBER RANSOM: What is the typical factor that's used in this kind of design?

MR. UNIKEWICZ: Excuse me. This is Steven Unikewicz, Mechanical Branch.

The answer to some of those questions are that there are plants that are in the direct line. They're underneath steam generators, and there are a lot of impact loads from insulation coming off. When we evaluate those types of sumps, we look at sloshing loads. We look at the standard methods we've had before with containment moving around, with water impinging up against the screens.

That's sort of Design Engineering 101. Those are part of the design of license based things that you do look at. It's a normal part of an evaluation of a piece of component on the lower levels of containment. It does look at steam loads. It looks at impact loads. It looks at a couple of places.

In some cases, to protect those, there have been shields put in place and things of that nature. So all of these things you're talking about are normal design considerations.

CHAIRMAN WALLIS: That answered the question of whether the ZOI affected the screen.

MR. UNIKEWICZ: It can potentially, and I've seen where it possibly does.

CHAIRMAN WALLIS: Can you answer the question of fluctuating loads during the pump operation?

MR. UNIKEWICZ: Yes, I can. There have been many instances of fluctuating loads during pump operation. The problem then becomes that flow drops.

There is a lot of industry experience both within the nuclear industry, within the chemical industry, within the process industries of pumps starting to cavitate, fluctuating loads due to cavitation, fluctuating loads due to air ingestion within the components.

That, again, is part of when you do a system evaluation. You take that into account. You're looking at your basic design parameters. What do I have? What is going in? What are the primary fluid properties going in? What is my NPSH requirements, and so on and so forth?

Depending on the style of pump, the manufacturer of pump, depending on how many stages there are, whether it's a single stage pump, whether it's a multi-stage pump, it can, it may or may not have an adverse effect.

CHAIRMAN WALLIS: Okay. So all this would be is what you'd expect the licensees to analyze, and you'd be able to review it okay.

MR. UNIKEWICZ: Any reasonably competent design engineer, this is a normal part of their job.

CHAIRMAN WALLIS: That's something I think we need that sort of assurance.

MEMBER SIEBER: Well, I think one way to look at it is from a continuity standpoint, you know, as the flow fluctuates maybe from zero to full flow or maybe even exceeding that, that's in a suction pipe or discharge pipe from the sump or from the pump.

If you look at the sump itself though, it's so much larger in area that the change in level is very small compared to and also the velocities are very small compared to the change in velocity in the pump and in its lines.

CHAIRMAN WALLIS: The pump and the flow of the --

MEMBER SIEBER: So the forces have to be very small.

CHAIRMAN WALLIS: I think the flow through the screen is very much less. The fluctuation in the flow through the screen is very much less than the flow through the pump. This would be true, I think, if you had --

MEMBER SIEBER: The velocity.

CHAIRMAN WALLIS: It opens the velocity.

MR. UNIKEWICZ: Absolutely. That's absolutely correct. The flow velocity through the screen is going to be probably generally an order of magnitude less than the flows.

CHAIRMAN WALLIS: Well, it doesn't really make a difference whether the sump is submerged or not, it seems to me, whether it's being submerged or not.

MR. UNIKEWICZ: You're looking at, you know, a foot or two maybe coming through the screens. The typical velocities coming through some of those inlet pipes are going to be on the order of anywhere between seven to ten to 12 to 14 feet per second coming through the pipe, depending on --

CHAIRMAN WALLIS: Oh, the velocity is less. I was thinking of a capicitant if you have an open surface. If you had it on non-flooded screen, then you actually have the capacity of the liquid can go up and down so that you don't have the fluctuation transmitted to the screen.

If it's solid with water, then you're going to have the --

MR. UNIKEWICZ: I understand, and by the time you get to --

MEMBER SIEBER: Well, it's flooded when the pumps are on.

MR. UNIKEWICZ: -- that hopefully that level within containment is relatively stable so that you're not going to see a lot of fluctuation in levels at the point that you should be going out to research.

CHAIRMAN WALLIS: So you're assuring me

you've got it all under control.

MR. UNIKEWICZ: I've done this before, sir.

CHAIRMAN WALLIS: Thank you.

MEMBER SIEBER: That would appear to be the case.

DR. TRAIFOROS: I would like to make an additional comment if I may. It seems that it's worth reiterating that it is important to evaluate the effect of jets on screens. We understood your point. These types of analyses have been done. It's not quite certain whether everybody has done these types of analysis. The assumption is probably they have, but the point is they should be looked at again to make sure that this aspect is being addressed.

And a word of caution on the zone of influence. Since the weight has been calculated, it results in a smaller range to the jet. One has to consider the plain, old approach of a direct jet hitting the important areas.

CHAIRMAN WALLIS: Because by choosing a spherical zone of influence, you have artificially limited the distance at which things can be affected., and if there's something really key, really vital you don't want to damage, that may not be the right approach because it's still a probability, realistic probability of damaging it if there's something really vital.

Now, if it's stuff like insulation, maybe it doesn't make much difference because it's all over the place, but if there's some vital component you could damage, you may have to --

MR. UNIKEWICZ: Your point is well taken in that it should be part of the normal evaluation. You look at critical components within the path.

CHAIRMAN WALLIS: Right, right.

MR. UNIKEWICZ: Certainly if there is the potential for this screen to be within the path, then the expectation definitely would be you would look at it from an impingement standpoint.

MR. ARCHITZEL: Let me just make it clear. I made it before.

Ralph Architzel.

I just want to make clear that we're not redoing those analyses for this resolution. Those are licensing based analyses that have been done. They're in place, and we're not asking the licensee.

What we're talking about here is the structural analysis across the sump, but not a jet impingement analysis. So that's different analyses, and we don't plan to do it with --

CHAIRMAN WALLIS: But you're saying it's already done somewhere else in the regulation?

MR. ARCHITZEL: On different ground rules that --

CHAIRMAN WALLIS: That's okay. That's okay. Then you're assuring us that it's being looked it. The zone of influence is not artificially restricting consideration of jet damage.

MR. ARCHITZEL: Right, but it's not being revisited. There may be some problematic plants, as has been pointed out, but they've been analyzed and reviewed by the staff under the ground rules that we have.

CHAIRMAN WALLIS: But you're not so conveniently into the side. You're really taking it into consideration. Yes?

MEMBER SIEBER: On the other hand, you have to redo the calculations that the licensee replaces or enlarges the sump screen.

MR. ELLIOTT: Plants may be in a situation where they weren't in the zone of influence before, but when they enlarged the screen, they could run into a situation where now the screen is in the zone of influence, and they would have to evaluate it.

MEMBER SIEBER: That's right.

MR. HAFERA: Okay. So now that we've revisited zone of influence, in fact, the structural analysis is in summary. The NEI document is acceptable. We agree with the items that they have provided. They asked for a clarification on the application of Reg. Guide 182, Subsection 1118, and we provided that.

As far as some structural analysis, that's all I have. Any other questions?

CHAIRMAN WALLIS: Any other questions? Can we go on or take a break? Are you ready to take a break?

Thank you. We'll take a break until ten after ten.

(Whereupon, the foregoing matter went off the record at 9:56 a.m. and went back on the record at 10:12 a.m.)

CHAIRMAN WALLIS: Let's come back into session, please, and we'll continue with the staff's presentations.

MR. GOLLA: Okay. Good morning. My name is Joe Golla. I'm an engineer in the Plant Systems Section, and to my left is Steve Unikewicz. He's in the Division of Engineering. I am going to speak about upstream effects and downstream effects. If we could have the slide, please.

To summarize, we agreed basically with this section and added a few amplifying remarks. If we could go to Slide 5, please.

Basically the guidance report advises that NEI-0201 should be utilized in this review, and we agreed with that basically and just added a few amplifying remarks to it.

NEI-0201 is the containment condition assessment guideline, and it directs licensees on how to or provides guidance on how to assess the condition of the containment regarding locations of possible debris sources.

CHAIRMAN WALLIS: What are some of these unique geometric features, just reading your slide here? What sorts of features do we have in mind?

MR. GOLLA: That would be for licensees to inspect for --

CHAIRMAN WALLIS: Well, are these sills and stairwells and various changes of level and barriers to flow and that sort of thing?

Is there some assessment of the ability of licensees? If there are lots of unique features, this is going to give you a lot of plant specific analyses.

Is there some assessment of the ability of licensees and the staff to understand these enough to make any sort of sensible analysis?

MR. GOLLA: I don't understand. Could you repeat that?

CHAIRMAN WALLIS: All of these things that ask licensees to do something must be made -- these requests must have some sort of implication that the licensees are capable of doing it.

MR. GOLLA: Certain.

CHAIRMAN WALLIS: And without guidance from you, they may do all sorts of things.

MR. GOLLA: The assumption is that they're capable of inspecting the containment.

CHAIRMAN WALLIS: For possible hold-up to evaluate containment. This implies that they're going to make some analysis, or this can look at them and say, "Gee, whiz, this could happen." They've got to reach some conclusion from it presumably.

MR. GOLLA: Again, the assumption is that they're capable of doing that.

CHAIRMAN WALLIS: And the assumption is that you guys are capable of evaluating what they do. It's all assumptions.

It's okay. I'm just probing, you know.

MR. GOLLA: Yes.

CHAIRMAN WALLIS: Because I can see you faced with a whole lot of decisions to make and the licensees have a lot of decisions to make.

MR. GOLLA: Right, and that's sort of a bridge that gets crossed when you come to it.

CHAIRMAN WALLIS: Again, we've taken this step in the dark that we talked about earlier, are we?

MR. LATELLIER: If I could add, the unique features that we're talking about are the same features that affect the transport fractions that we discussed yesterday. It's somewhat an engineering judgment about where the containment water return paths are, and in relation to our testing database, we do have evidence of hold-up behind curves.

All of the features of a sump screen, for example, small orifice openings, all of those attributes are also applicable to the drain water return paths, and we have evidence of collection of various sizes on various gradings.

One particular unique feature is a designed drainage path that is designed to return water to the sump and any kind of coverings or gradings that are in place would be potential locations for water hold-up.

CHAIRMAN WALLIS: I think, Bruce, you're going to have to turn yourself into ten people because you seem to be the only person that is able to answer these questions, and when all of these 69 different applications come in you're going to be very busy.

MR. ARCHITZEL: What we're talking about is in the appendix. There's a firewall transport appendix for the volunteer plant. So it's not limited to the panel's knowledge. We did provide that analysis in the appendix of the SE.

MR. GOLLA: These locations that Bruce spoke of are basically called out in the guidance document rather as typical locations where water might be held up, not as unique design features.

CHAIRMAN WALLIS: So is there a formulation? You mentioned that this all relates to the fractionating of how the debris is put into the transport event tree, if you like. Is there a formulation that tells you how those fractions will change depending on these upstream effects, these barriers and things to the judgment?

MR. LATELLIER: There's not a single equation that can be evaluated. In the manner that we discussed yesterday, we look at the transport pathways and make judgments about the fraction that's retained or passed at each step.

CHAIRMAN WALLIS: Okay. So it's just a qualitative judgment.

MR. LATELLIER: As Mr. Shaffer explained yesterday, we appeal to the data where it's available, and where it's not, we apply conservative assumption. But the value of outlining in detail the transport path, that minimizes the impact of our conservative assumption, and it maximizes our use of defensible information.

CHAIRMAN WALLIS: Okay.

MEMBER SIEBER: The transport tree is unique to the containment configuration. So it's possible that each plant could have a unique tree, even though containment designs are pretty simple, and most of the obstructions and holdup points are designed in for that purpose, as opposed to just being there. So it's sort of obvious when you walk around containment where the holdup points are and what these unique features are and why they're there.

MR. GOLLA: That's why I mentioned in particular the designed containment water return paths. If there's a drainage system that's intended to perform that function, then it should be examined as a unique feature.

I think the criteria for applying judgment versus database information is a little bit different in this application where we're looking for a choke point, a critical pathway. We would need to be more confident in our information because it's just one critical step in the entire transport tree.

CHAIRMAN WALLIS: It seems to me that there's a lot of room here for the licensee and the staff to have different judgment. The licensee says, "I think these are the places where we might get holdup."

And the staff says, "Well, I look at it and my judgment says maybe these other places."

So you might well have some discussions with the licensees about what's reasonable and what's not.

MR. GOLLA: Sure. You know, we typically engage in those kinds of -- that kind of discourse whenever we do, whenever we review a license amendment request. One thing that does appear in here is a remark about if anything is added in terms of curbs or debris racks, to also evaluate their possible effect on the holdup of water.

CHAIRMAN WALLIS: I'm hoping we can get through this quite quickly.

MR. GOLLA: Are you done? Any more questions?

Okay. Let's move on to seven, please.

Okay. In summary, the staff has included additional specific items that licensees should include in their evaluation of downstream effects.

Next slide, please. The purpose --

MEMBER SIEBER: Is one of the downstream effects the effect of debris ingestion into the pumps themselves?

MR. UNIKEWICZ: Absolutely, and in fact, that is a significant piece of the evaluation, and within the guidance in Section 7.3, we added that those types of things need to be evaluated, and they are very pump specific. They are very equipment specific evaluations. They are very material specific evaluations. It's going to be a very -- that piece of this evaluation will be a bit of work.

MEMBER SIEBER: Well, it seems to be my recollection that when GSI first started out, the pump wasn't included; is that correct? And now it is, right?

MR. UNIKEWICZ: That's correct, and part of that is because it will say lessons learned from Davis-Besse.

MEMBER SIEBER: I saw the pump.

MR. UNIKEWICZ: And ACRS certainly has had numerous presentations on the issues of the ECI pump at Davis-Besse. Because of those, we felt as a staff it was very important to include it. It would be unwise of us not to discuss those types of downstream effects that we have seen.

MEMBER SIEBER: Maybe you could give me an estimate. I know of plants that have vertical shaft heat draft pumps, and also plants that have horizontal pumps. Could you give me some feeling as to how many plants have vertical pumps and how many plants have horizontal pumps?

MR. GOLLA: I don't have those numbers off the top of my head. What I can tell you is from a susceptibility standpoint, multi-stage pumps are certainly going to be much more susceptible to this type of effect than the single stage pump. So if you have a deep draft pump with not a lot of stages, maybe it's a single state; maybe it's a LPSI pump; it will be less susceptible to this type of damage than a multi-stage pump will.

And part of that comes from an aerodynamics standpoint and vibrations and leakage and things of that nature.

MEMBER SIEBER: Yes. On the other hand, one of the good things about a vertical staff pump (phonetic) is that you can put clean water into the bearings to lubricate them. They're rubber or elastomer type bearings.

MR. UNIKEWICZ: If you have clean water available.

MEMBER SIEBER: If you have a source, as opposed to just pumping all of the garbage back down in there, which is usually fatal.

MR. UNIKEWICZ: You're correct, and in a lot of cases what these pumps will do is they'll recirc the water. In fact, they'll take some of the inlet water and recirc it around and use it as cooling for those types of seals. Again, those are types of things that are going to have to be evaluated.

MEMBER SIEBER: Be reevaluated.

MR. UNIKEWICZ: And, again, it's a very specific evaluation. It's going to be very unique to each type. The thing to consider is long-term and short term operation. If you recall, the Davis-Besse issue wasn't a short-term operation issue. It was a long-term, more after precipitation issue, which is part of -- and I'm skipping ahead in the presentation. I apologize -- is to consider what the mission time is for each of the bits and pieces and parts within your ECCS system.

MEMBER SIEBER: Well, recirculation goes on though in some scenarios for days. So the mission time is going to be days. If it's going to fail, it will fail in that contract.

MR. UNIKEWICZ: And in some cases the mission time is a matter of hours, and again, it's going to be very unique to each situation. I can think of plants that have mission times on the order of hours. I can think of other plants that have mission times on the order of days and weeks, with different components, with different plant line-ups.

And one of the bits of guidance we provided was that you need to consider all of your plant line-ups. You need to consider how you're responding to your accident. Look at the lineup. Look at the modes of operation that you're using. Consider the flow effects. Consider how you're actually operating the plant during these accidents and these accident analyses and look at what's going to happen.

You may be okay. You may not be okay. It will depend. Again, that's part of I'll call it a standard design engineering evaluation. These are the types of things that you look at: material properties, fluid properties, fluid flows, and so on and so forth.

MEMBER SIEBER: Well, as far as vulnerability is concerned, I think that I personally worry as much about the pump as I do about screen blockage.

MR. UNIKEWICZ: I share your concern.

MEMBER SIEBER: And so I'm glad that it's in the guidance document, and I'm glad you're addressing it.

CHAIRMAN WALLIS: I have some experience with pumps, and many pumps will quite happily for a while pump mixtures of cal-sil, powdery stuff, granular stuff, fibrous stuff.

But when pumps jammed, usually the kind of pumps I have found is that when they jam, you take them apart and you find there's a piece of metal or something tough which is jammed between the rotating part and the part static part.

MR. UNIKEWICZ: That's correct.

CHAIRMAN WALLIS: And it doesn't take much of a piece of metal, you know, that's pulled in there and jams to stop the pump completely.

MR. UNIKEWICZ: And truly that was part of the Davis-Besse experience, and one of the things that we found was that the matting of debris, latent debris and those real hard particles from sand, dirt, and dust, and truly --

CHAIRMAN WALLIS: So what you care about is the covers of the insulation, the metal parts, sort of the reflective metal insulation, the pieces which are odd enough, tough enough that they get in there and they don't get ground up by the pump. They don't pass through the pump. They get stuck between the rotating and the static part, and the thing grinds to a halt.

MR. UNIKEWICZ: Or what it does is it wears the surfaces such that --

CHAIRMAN WALLIS: It can do that, too.

MR. UNIKEWICZ: -- you're now putting the pump into a vibrating mode --

CHAIRMAN WALLIS: It can do that, too.

MR. UNIKEWICZ: -- that you didn't want to have it do before.

The other thing it may do is depending on the internal clearances, you're looking at what are the flow characteristics of this pump. You know, was this support to put out 437 gallons a minute? And now because I've increased all of the internal tolerances, I'm only running at 350 gallons a minute and those type of effects.

They may not always show up in a long-term vibration analysis, but what you'll then see is you'll see the pump itself, the pump efficiency --

CHAIRMAN WALLIS: To put it into perspective, the kitchen disposal will eat all kinds of stuff, but you get a little piece of wire in there, and it --

MR. UNIKEWICZ: And it jams it rather quickly. That's correct, sir.

MEMBER SIEBER: So you should install magnets on your screen.

MR. UNIKEWICZ: Well, it's truly much more than the metals. A lot of it has to do with I'll say hard particles. The silicas that you may find in dust and dirt and the blasting of containment pieces that are hard, that will start to wear away after --

CHAIRMAN WALLIS: Does mica get from the concrete and stuff like that? I mean, in some of the tests --

MR. UNIKEWICZ: Absolutely.

CHAIRMAN WALLIS: -- jet impingement went way back. I forget when, 20 years ago or something. They actually had concrete spalled and broken off the wall and so on.

MR. UNIKEWICZ: That's correct, and those are the types of things you need to consider from a downstream standpoint. Now, you very well may be --

CHAIRMAN WALLIS: All we need here is some kind of assurance that they can be considered properly because obviously you've got to consider all of this stuff. Where is the assurance that they will be considered properly?

MR. UNIKEWICZ: Well, as the paragraphs go along, it talks about things you need to consider in your evaluation.

CHAIRMAN WALLIS: Well, that doesn't help me. I've been telling you you've got to to consider something gives me no assurance that it will be considered properly.

MR. UNIKEWICZ: Well, your design control manual at your plant, and if you're following your design control manual and if you're going your design evaluation, it is expected that any design engineer will look at the fluid properties. He will look at the abrasiveness of the fluid. He will then compare it against and look at --

CHAIRMAN WALLIS: Okay. So does he have guidance on a supposed three-by-six piece of stainless steel sheeting that comes through, broken off some piece of insulation material? Does he have guidance about how the pump handles that?

MR. UNIKEWICZ: In many cases there are. There's a lot of industry publications not only in the nuclear industry. Typically you find them more or less in the process industry.

CHAIRMAN WALLIS: So that he can assess the possibility that it will pass through the pump? And what's the probability it then passes through the reactor?

MEMBER SIEBER: Well, the probability that it gets to the pump is zero because nothing can get through the --

CHAIRMAN WALLIS: But it can get through sideways.

MEMBER SIEBER: The screen is bigger than the whole --

MR. UNIKEWICZ: That is not a true statement. We have found things in an experiment that things larger than a screen do because their aspect ratios do pass through the screen.

MEMBER SIEBER: Well, some long and skinny can.

MR. UNIKEWICZ: That is correct. So there are things that do.

MEMBER SIEBER: But that's not what I said.

MR. UNIKEWICZ: And the other case, say, I'm talking to a Gould pump, okay, and I'm going to pass a piece of stainless steel into this. My bearing materials are this. My bypass rings and all of this other kind of stuff; you look at the effects of that and, depending on pump manufacture, depending on configuration, depending on the design modifications you made have made as a licensee over the years because over time, depending on what you've done, you may have changed bearing materials. You may have changed wear ring materials. Okay?

You would have to go back through and assess is stainless steel harder than -- I have Stellite 6; I have Stellite 12; I have bronze; I have brass. Whatever it may be, in that case, make that sort of evaluation.

I need to look at clearances. What are I running clearances? Are my running clearances 10 mLs? Are they 15 mLs? Are they 7 mLs? What is the size of the screen?

Again, a normal part of an engineering -- a component level engineering evaluation, and on a system level evaluation.

CHAIRMAN WALLIS: So if we look at your Slides 8 and 9, there's a whole list of things that licensees should determine to consider and so on, and it's all up to them to do it, and we have to have some faith that they know how to do it and they can convince you that whatever they've done is appropriate. That's really where we are, isn't it?

MR. UNIKEWICZ: That's correct.

CHAIRMAN WALLIS: Can we go any further than that?

MR. GOLLA: We could given more time to work on this project. Sure, we could.

CHAIRMAN WALLIS: I mean today. Can we go out any further than just realizing that you're asking them to consider a whole lot of things? That seems to be the bottom line.

MR. UNIKEWICZ: That truly is the bottom line, and there are a myriad of things to consider. We have given guidance on saying these are things you absolutely must; these are things that we would expect that you are going to submit upon. These are things we would expect.

Now, granted most of these things are almost motherhood and apple pie from a design and control standpoint and from a component evaluation standpoint, but since they weren't in the GR, we felt it was needed to add those specific items.

And this certainly is not an all inclusive list. As somebody goes through, somebody may find other things to consider and --

CHAIRMAN WALLIS: How they consider these things might be cause for the staff to take exception and say they don't pass because they didn't consider it appropriately? They don't just go through some ritual of saying they've considered it. They actually do some analysis which has been assessed?

MR. UNIKEWICZ: We do not design and evaluate by checklist, and designing and evaluating by checklist is extraordinarily bad practice. We do not engineer by checklist.

CHAIRMAN WALLIS: Okay. But at least you've told us there are a myriad of things they have to consider above all the other things that we've heard about in the last week or so.

MR. UNIKEWICZ: And there will be -- I mean, this is an engineering problem. We expect them to do the engineering.

MEMBER KRESS: Are there any plans to do any confirmatory research, sending various debris combinations on a simulated loop?

MR. UNIKEWICZ: There is some movement afoot, especially from the perspective o looking at the throttle valves and looking at materials going through standard sized generic throttle valves and throttle valve clogging, and that is ongoing.

And as we have stated, if there is something that screams at us as that progresses, we'll certainly let everybody know.

MEMBER SIEBER: There's a wealth of experience pumping fluids that contain --

MR. UNIKEWICZ: Low level fluids.

MEMBER SIEBER: Yeah, process.

MR. UNIKEWICZ: That's right.

MEMBER SIEBER: They pump coal all over the place. They pump ashes.

MR. UNIKEWICZ: Ask ponds, and everything from dewatering systems.

MEMBER SIEBER: And the abrasive content is usually pretty high, you know. A coal slurry pipeline will run 50 or 60 percent coal, 40 percent water.

MEMBER KRESS: However, they use special pumps though, aren't they?

MR. UNIKEWICZ: Yes, they are.

MEMBER SIEBER: You can buy deep draft pumps that will pump slurries.

MR. UNIKEWICZ: Part of the concern is one minute these pumps were specified and installed, they were specified and installed to clean service. We're now asking them to -- that initial consideration was missed as people designed them early on. They should have considered; they should have looked at the TEMA standards. They should have looked at the NEI standards. They should have determined something better.

In some cases they'll be okay. Other cases, they may have to make some modifications. You may find that if my mission time is two weeks and my pump will last for six weeks, I'm okay. You may not. Again, it will depend on a lot of the things previously done which have determined from your fluid property standpoint.

CHAIRMAN WALLIS: Well, the only problem, I think that most of the debris is going to be perfectly happily going through and going through the reactor and coming back to the screen again and may be being caught, but I'm concerned about metal pieces, and I don't quite know that the licensees are going to be able to determine how many pieces of what shape they're going to sneak through the screen and what effect they might have downstream. That's the only concern I have.

MR. UNIKEWICZ: That is some --

MR. GOLLA: We do have some flow experiments that we're doing at Los Alamos, and we are looking into that.

CHAIRMAN WALLIS: So it's still a research topic then?

MR. GOLLA: Downstream effects, we know that there are downstream effects. We know that as material passes through the system that it has the potential to disable pumps important to the safety of our plants. We know that the harder materials will mat up, and that they will cause damage depending on type of material, depending on lots of other different things.

So can we do research? You can always do research.

Do we know this to be a real problem? The answer to that is yes.

Is there information out there and data out there not only within the nuclear industry, but within the process industry, within the rest of the industries? The answer to that is yes.

So is there information available to make those evaluations?

CHAIRMAN WALLIS: I strike all of that. The real question is is it adequate. Is this good enough? Because we just have to say does this make sense, but you know, is it a good enough statement in terms of the capability of analysis, which is now there?

MR. UNIKEWICZ: Well, part of it as we do the inspections and as we look at the submittals, some of it has to do with the strength of their design and evaluation programs, and again, we found some strengths and we found some weaknesses within many different programs.

I suspect that as we go through this, we will find some licensees do an extraordinarily thorough job. I suspect, on the other end, we're going to find licensees that don't do an extraordinarily thorough job, and we may need to talk to them a little more, and as we share design experience and operating experience, that's how we all learn.

CHAIRMAN WALLIS: Yeah. We're going to move on here, but they may not know what criteria you're going to use for this thorough job, and you're going to --

MR. UNIKEWICZ: The bulk of our theories --

CHAIRMAN WALLIS: They don't quite know how they're going to be graded.

MR. UNIKEWICZ: Being in compliance with design basis and license basis requirements. They need to meet the design basis and license basis requirements.

MEMBER SIEBER: You know, that's not as simple as it would first appear. You know, if you go to a pump manufacturer and say, "I want to pump to perform this kind of service," he will pull out his catalogue and say you need a double casing deep draft pump with fresh water bearing injection.

If you go to the same pump manufacturer and say, "I bought this pump 20 years ago for clean water service. Now I want to pump cement through it. How long will it last?" he probably won't know.

MR. UNIKEWICZ: I agree, and we've never made the statement that this evaluation was going to be easy; have never made the statement that somebody competent in looking at pumps and internals and understanding how a pump operates, never said this was going to be an easy job.

There will take some thinking. There will take some considered thought. There will take some engineering involvement, absolutely. There's no doubt in my mind.

MEMBER SIEBER: The pumps aren't cheap. So there is an expense associated with making them meet a severe service like that.

MR. UNIKEWICZ: Right, and the --

MEMBER SIEBER: That's the way it goes, right?

MR. UNIKEWICZ: There is an experience base, and there is expertise out both internally and external that I've seen that has capability to make these types of evaluations. So there's no doubt in my mind that these evaluations can be done. I've seen them done.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: Can we move on now? Jack, are you satisfied?

MEMBER SIEBER: Yes. I think there's problems there, but the problem isn't with the guidance through the safety evaluation. The problem is with the difficulty at solving the problem.

MR. UNIKEWICZ: that's the fun part of engineering, I guess.

MEMBER SIEBER: Yeah, it is.

CHAIRMAN WALLIS: Can we move on?

MR. GOLLA: We have basically covered everything that we had prepared.

CHAIRMAN WALLIS: Thank you very much.

I can we move from the macro to the micro or talk about chemical effects? Is that where we are or is there someone else first?

MR. JOHNSON: Yes. We have done ours. We promised yesterday that we would talk a little bit about spherical --

CHAIRMAN WALLIS: Sure. That's fine. Thank you, yes.

MR. ELLIOTT: Okay. Well, good morning. Yesterday morning, if you recall, I think I made a statement that sphericals under the influence was conservative, and I gave you three reasons why I believe that to be true.

One of the reasons was that I pointed out that the regeneration tests had been conducted to maximize debris regeneration such that -- and the zone of influence assumed that maximum debris regeneration throughout the zone of influence regardless of the orientation of the insulation seams to the break.

The other two reasons I listed were that the zone of influence neglected any shadowing effects of structures or big piping or equipment that may protect debris sources from direct impingement from the jet.

And then the third thing I pointed out is that the industry had pointed out yesterday, and it's my understanding they'll show you a picture later this morning of how big the zone of influence is relative to the size of the containment.

So what I did after you asked me to show you a little bit of data, I went back last night and tried to resurrect some information from the BWR air jet impact tests, which formulate a lot of the baseline knowledge that we have regarding debris generation.

Next slide, please.

This is the facility that they use to conduct these tests. It's basically a wind tunnel. They have a compressed air tank that pipes through a manifold and then to a nozzle located in the wind tunnel. It's a three inch nozzle. It has a rupture disk on it, and what they would do is they would set up a target pipe at a distance from the nozzle, set up the insulation, and then turn on the air through the manifold to the rupture disks. It would burst at approximately 1,100 psi, and then it would blow down for about six seconds and then --

CHAIRMAN WALLIS: Did they do any schlieren observation of the jet or anything like that so that we --

MR. ELLIOTT: Sorry?

CHAIRMAN WALLIS: Did they do any schlieren observation of the jet or have any idea of what the jet structure was?

MR. ELLIOTT: I'm unfamiliar with the term. What did you say?

CHAIRMAN WALLIS: Were there any attempts to visualize the shock pattern in the jet?

MR. ELLIOTT: No, I do not believe they attempted to do that. They did study the pressure downstream of the nozzle. They conducted four tests without insulation where they put a pressure transducer down the line and measured the --

MEMBER RANSOM: Do you have any detail on that?

MR. ELLIOTT: On the transducers?

MEMBER RANSOM: The location of the pressure taps and what they looked like.

MR. ELLIOTT: Well, the next slide.

MEMBER RANSOM: Before you leave that one though there are a couple of questions.

MR. ELLIOTT: Okay, okay.

MEMBER RANSOM: Do you mean the gases that you impinge on this pipe recirculated back out the transducer?

MR. ELLIOTT: Oh, I'm sorry. Yes, good point.

No. They did not. Well, there was some recirculation, I believe, but the end of the wind tunnel is open. It's an open grating.

MEMBER RANSOM: This end that you're showing closed?

MR. ELLIOTT: It's shown closed, but if you actually see, there's a dimension there of about 86 inches. That's telling you the height of the actual screen.

CHAIRMAN WALLIS: That's a hole?

MR. ELLIOTT: It's a screen.

CHAIRMAN WALLIS: Oh, it's a screen.

MR. ELLIOTT: And actually I have a picture of that in one of the follow-on slides. But there was some recirculation because we did see debris end up behind the nozzle after the test, and in fact, you see they have a video camera mounted there. The video camera ended up not really being a useful thing because once the debris was hit and you had a cloud of fiberglass all over the place. It was very difficult to see, at least for the fiberglass test.

MEMBER RANSOM: And so on this there were two measurements, one a static tap at the nozzle?

MR. ELLIOTT: One at the nozzle and one in the --

MEMBER RANSOM: One in the plenum?

MR. ELLIOTT: -- one in the plenum, and then they had four tests where they put a differential pressure transmitter on the pipe itself with no insulation.

MEMBER RANSOM: Differential?

MR. ELLIOTT: Yeah.

MEMBER RANSOM: Pipe to the atmosphere?

MR. ELLIOTT: Yes. Actually, no. They had, if I recall the test report correctly, they put the high pressure side on the pipe facing the nozzle. They put the low pressure I thought they said behind the nozzle, but I may remember incorrectly on that.

PARTICIPANT: But that's in the report.

MR. ELLIOTT: That's in the URG, yeah. This information is all in the URG report.

CHAIRMAN WALLIS: So this is a report that we have?

MEMBER RANSOM: No.

CHAIRMAN WALLIS: And we can get one?

MR. ELLIOTT: Yes.

CHAIRMAN WALLIS: Good. Thank you.

MR. ELLIOTT: And then the report is much more detailed than what I'm going to go over here. I'm really looking to give you some insight about the specific statement I said about the orientation of --

CHAIRMAN WALLIS: Did they check the ANSI jet model in any way?

MR. ELLIOTT: They did some comparisons to CFD calculations that were run by Dr. Belandin (phonetic) continuing --

CHAIRMAN WALLIS: You have calculations of this jet?

MR. ELLIOTT: He ran some. They're shown in the report. They're not in my presentation.

CHAIRMAN WALLIS: Do they have shock waves and things like that in them?

MR. ELLIOTT: I do not believe that he was modeling shock waves. He was modeling the pressure a certain distance from --

CHAIRMAN WALLIS: But that has to take account of the structure of the jet, which is almost inevitably fully of waves and shocks.

MR. ELLIOTT: Yes, I would believe so, but again, I didn't study that in detail for the purpose of this presentation.

CHAIRMAN WALLIS: So what is the purpose of the presentation?

MR. ELLIOTT: The purpose of the presentation is I'm trying to make a case that I told you yesterday, that the orientation of the protective jacketing on an insulation or the seams in the insulation for RMI cassettes makes a significant difference in how much debris is generated, and when they give you a destruction pressure for the debris, they did that -- forget that about destruction pressure. That's out, not really important.

What I'm trying to say is that the target orientation relative to the break makes a huge difference in how much debris can be generated off that particular target. Okay? And I'll give you the gauge point --

CHAIRMAN WALLIS: It seems to me they tested lots of things at different L over Ds.

MR. ELLIOTT: Right.

CHAIRMAN WALLIS: And then there's some in the report which says what happened? There's nothing --

MR. ELLIOTT: Well, in the report itself, they did -- during the pretesting they call it or shakedown testing, that is when they did testing to see which orientations they should use for the jacketing, and of course, they don't provide that information in the report other than the conclusions that they drew.

But what I was able to find were two tests that show you how significant this impact can be, and that's what I was going to present to you.

CHAIRMAN WALLIS: Okay. So that's --

MEMBER RANSOM: What pressure were these tested at?

MR. ELLIOTT: About 1,100 psi, I think, if you go --

MEMBER RANSOM: That's the stagnation pressure?

MR. ELLIOTT: That's the stagnation pressure. That's correct.

MEMBER KRESS: Why did they do these in a wind tunnel?

MR. ELLIOTT: All I can tell you is it just was a facility that's available that could be modified quickly to produce what they needed. We similarly took over the facility after the owner's group was done and modified it to do some of our transport studies testing.

MR. LATELLIER: It also provides a confinement volume for picking up the pieces. These types of tests are very messy when you're talking about recoverable fractions and determining size distributions.

MEMBER KRESS: Yes, and it also may affect the jet dynamics of those.

CHAIRMAN WALLIS: It is fairly big compared with the nozzle.

MEMBER KRESS: Yeah, it does look like it's big.

MR. ELLIOTT: Yeah, it's a ten foot diameter.

MEMBER KRESS: Yeah, it could be an infinite size basically.

MR. ELLIOTT: The table here just gives what we've already discussed, what type of pressure measurements they took.

CHAIRMAN WALLIS: They take pressure measurements on the target?

MR. UNIKEWICZ: They did pressure measurements on the target without insulation and then used CFD code.

CHAIRMAN WALLIS: Okay.

MR. UNIKEWICZ: Used that to independently verify that their CFD codes predicted the pressure.

CHAIRMAN WALLIS: They had a hole in the pipe or something?

MR. UNIKEWICZ: That's correct.

And then the last instrument obviously is the scale that they used to measure before the test the amount of insulation and then afterwards the amount of debris, and they broke that debris up into small fines and large pieces and give independent masses for that.

And you'll see that in almost every test they wee unable to recover all of it. As I noted, there's a screen at the end of the facility, and so they assumed that it went out the screen and was fines, and they added it, that missing mass, to the fines.

The next four slides, I'm not going to go into them in great detail. I just wanted to show you that they conducted 77 tests.

CHAIRMAN WALLIS: Lots of tests, yeah.

MR. UNIKEWICZ: All different kinds of insulation. TPI is Transco Products, Incorporated. You have NUKON. You have various types of fiberglass, Temp-Mat, K-Wool, and calcium-silicate.

CHAIRMAN WALLIS: So the seam was on the back side. They gave the best --

MR. UNIKEWICZ: Well, it depends. What they found in their pretesting was that, depending upon the type of insulation that you're using, the orientation could -- yeah, go back one more slide -- the orientation that creates the most debris generation is different.

For fiberglass, which is typically insulated by a single jacket with one seam that wraps all the way around the insulation and the pipe and has one seam in it, they found that orienting the insulation at the nine o'clock position, which is 180 degrees away from the nozzle --

CHAIRMAN WALLIS: That's the back side.

MR. UNIKEWICZ: The back side.

CHAIRMAN WALLIS: Three o'clock is the stagnation --

MR. UNIKEWICZ: Three o'clock is facing the nozzle.

-- gave them the maximum debris generation. Okay. So that's contrary to what I told you yesterday. I got it backwards yesterday. I think Bruce corrected me.

MEMBER RANSOM: Where was the seam again?

MR. UNIKEWICZ: So the seam was on the opposite side of the pipe from --

MEMBER RANSOM: The jet?

MR. UNIKEWICZ: -- from the jet.

MEMBER RANSOM: On the back side?

MR. UNIKEWICZ: On the back side. Okay? But for reflective metallic insulation, which is typically two crescent pieces of half pieces that are joined together so that there's two seams in it, they found they got the greatest generation when they oriented the seam in the plane of the jet at the three and nine o'clock positions.

CHAIRMAN WALLIS: So it's front and back.

MR. UNIKEWICZ: That's correct.

CHAIRMAN WALLIS: So you can't tell which one's the actor?

MR. UNIKEWICZ: Not really.

CHAIRMAN WALLIS: But you've got two seams?

MR. UNIKEWICZ: Yeah, there's two seams because there's two -- clam shell is what they call it. So there's a hinge, and then a latch mechanism on the front side to tie it together.

So what I chose is looked for two tests

that could help me demonstrate the point of how much of an effect this can have, and the first test that I'd like to show you is Test 31-1, which was conducted at a test of seven and a half L over D with NUKON insulation, steel jacketing, banded by a standard NUKON steel jacket which had its own latch mechanisms built in.

Then in addition to that, they put on nine heavy duty stainless steel bands. So they were intentionally trying to show or demonstrate that banding could make a significant difference in improving the amount of debris generation or reducing the amount of debris generation.

The standard seams on the jacketing, the PCI jacketing for this test were at the 12 and six o'clock positions, if I have this highlighted correct.

CHAIRMAN WALLIS: Okay. Well, let's --

MR. UNIKEWICZ: Okay.

CHAIRMAN WALLIS: And they found out what happened.

MR. UNIKEWICZ: Okay, and then they blasted it, and you can see that at the table at the bottom of the page there , they got 21.7 percent of fines, eight and a half percent of large, and then the remaining that was just a big blanket was about 69.8

percent.

The next slide shows you the pretest set-up.

CHAIRMAN WALLIS: What L over D? This is seven over D. Okay.

MR. UNIKEWICZ: Okay. And then the following slide shows you the post test.

CHAIRMAN WALLIS: A different test.

MR. UNIKEWICZ: Okay.

CHAIRMAN WALLIS: It's a different test.

MR. UNIKEWICZ: No, that's a typo on my part. No, 33.1, that's right, isn't it? Oh, 31.1. It's a typo on my part. I apologize for that.

That is post test for this test.

CHAIRMAN WALLIS: So it blew off something on the sides, not in the middle?

MR. UNIKEWICZ: Oh, it blew off the middle and what's on the sides was kind of shoved down to the side.

CHAIRMAN WALLIS: What's left.

MR. UNIKEWICZ: Yeah, what's left.

And if you go to the next page --

CHAIRMAN WALLIS: Well, it indicates there might be big flaps of metal coming off.

MR. UNIKEWICZ: Yeah, and you'll see that this is the screen down at the end of the wind tunnel, and you can see the amount of small debris. It doesn't show as well there as it does on the handout, I think, but you can see it all on the floor of the wind tunnel, and that was like that pretty much all the way down the floor.

The second test they conducted, 31.3, was conducted at 5L over D. So this was actually 2L over D closer than the previous test like I showed you, but in this case, the bands were at the nine o'clock position.

I highlighted the wrong line, but if you look at the jacketing was installed with two inch overlap. The jacket lap strikes at the nine o'clock position. Okay? So this is closer where you would expect there to be more debris generation, but in reality the only thing that was different is where those latches and strikes were, and in fact, it was closer, and you only got 5.4 percent debris generation and no large pieces at all.

And if you'll look at the picture on the next page, well, the next page is pretest and then --

CHAIRMAN WALLIS: Well, this is all very interesting and it shows the different results depending on various things, but why does this support your conclusion?

MR. UNIKEWICZ: Well, the primary difference between these two tests is the orientation of the jacketing.

CHAIRMAN WALLIS: So how you orient the latches makes a difference.

MR. UNIKEWICZ: That's the point I'm trying to make.

CHAIRMAN WALLIS: What has that got to do with the ZOI being conservative?

MR. UNIKEWICZ: Because the ZOI assumes that you're getting -- that all of the insulation is oriented in the worst case situation regardless of which way it really is oriented.

CHAIRMAN WALLIS: Yes, but that has nothing to do with turning a jet into a sphere.

MR. UNIKEWICZ: Well, there's three pieces. All right. Okay. What I'm answering and what you're asking me to answer were not he questions -- is not what I went back to research las night. I didn't go back to show you data that it would be a sphere.

CHAIRMAN WALLIS: What you're showing me is that there has been a substantial amount of work done, and that quite a few things influence the answer, and presumably it is all being pulled together into something that gives guidance somewhere, in some chart, but I don't quite understand why it is relevant to your conclusion.

I thought the question we were asking is is it reasonable to turn a directional jet into a sphere.

MR. UNIKEWICZ: Okay. That's not what I understood, and that was not what I was talking about when I made my statement. So I apologize for wasting your time.

MEMBER RANSOM: Well, I have a couple of questions. Was there any attempt to account for the fact that these were 1,000 psi instead of 2,200?

MR. UNIKEWICZ: Well, 1,000 psi is representative of the BWR, and they did do some --

MEMBER RANSOM: So this would be applied to BWR, but not necessarily PWRs?

MR. UNIKEWICZ: That's correct.

MEMBER RANSOM: And the other thing is as it's related to the ANSI jet model.

MR. UNIKEWICZ: It wasn't related to the ANSI jet model specifically. As I said, Continuum Dynamics ran their own CFD calculations about what the pressurizer bars would be in the wind tunnel.

MEMBER RANSOM: And there were no measurements on the target itself, I assume.

MR. UNIKEWICZ: Not with insulation on it. Okay? The insulation, I think, created problems. It's my recollection that having the pressure transducer and all of the insulation there --

MEMBER RANSOM: Even without the insulation, what did they do?

MR. UNIKEWICZ: Without the insulation, they took four measures at four different distances and used those to confirm CFD calculation predictions.

MEMBER RANSOM: Were they under --

MR. UNIKEWICZ: They're on the front of the pipe.

MEMBER RANSOM: Static tap on the front of the pipe, I guess.

MR. UNIKEWICZ: Well, I thought it was a differential pressure gauge.

MEMBER RANSOM: Well, it may have been differential pressure from the front of the pipe to the atmosphere, I guess.

MR. UNIKEWICZ: Yes. Okay, and as I said, they used that to confirm the predictions that they made in CFD calculations.

MEMBER RANSOM: All that data in the

reports.

MR. UNIKEWICZ: The report gives, basically because they were interested in debris generation, the report gives a lot of detail about the debris generation aspect of it. It gives examples of the CFD calculations and a description of what they were doing.

MR. ARCHITZEL: Those test are in there, Rob. Those four tests are in that report also.

MR. UNIKEWICZ: Yeah, the four tests without insulation where they actually predicted what the pressure would be, yes, those tests are in there. The results of those tests are in there.

CHAIRMAN WALLIS: Well, I think we have got to move on. We were glancing through this. We don't see -- my colleague, Vic Ransom, has very nice pictures of the calculations of the jet, and we're looking for something similar, but we haven't seen it, but we have time to go into that.

MR. UNIKEWICZ: Okay. I do want to point out in the URG though there are separate calculations that the owner's group did show. There are CFD calculations to show what the zone of influence would really look like.

CHAIRMAN WALLIS: Okay. I think we've got to move on. Thank you very much.

MEMBER RANSOM: Who did this work?

MR. UNIKEWICZ: Alan Boanin (phonetic) from Continuum Dynamics.

CHAIRMAN WALLIS: We've got to move on.

MEMBER RANSOM: And did they do the CFD work also?

MR. UNIKEWICZ: They did the CFD work also. He was the --

MEMBER RANSOM: Do you know what code they used?

MR. UNIKEWICZ: NPARC.

MEMBER RANSOM: NPAR?

MR. LATELLIER: NPARC. One final statement is that in comparison to the ANSI jet model we have compared the equivalent spherical volumes obtained from NPARC to those obtained from the ANSI jet model, and the ANSI jet is very conservative, especially for low --

MEMBER RANSOM: Where is that documented?

PARTICIPANT: Appendix I.

MR. LATELLIER: No, actually the best documentation is in Volume 3 of the supplement to the parametric evaluation.

PARTICIPANT: The reporter number 6367.

MEMBER RANSOM: Is that a NUREG report?

MR. LATELLIER: Yes, it is, and I have a copy I'll share with you.

CHAIRMAN WALLIS: Rick, are you going to look at these before before we meet again? It would be very helpful if you could give us some comment about what they show you and how they are related to some of your concerns.

So the next topic is an interesting one if Ralph is still here and willing to talk.

MR. ARCHITZEL: I was going to talk over here and not talk about it. Actually I'll try and save time or recover time for you if you want.

CHAIRMAN WALLIS: It depends on how many interesting things you have to say.

MR. ARCHITZEL: My name is Ralph Architzel and with me is Paul Klein from the Chemical Engineering Branch, the technical lead for this topic.

If I can go to the summary slide one more time, I'll try and do this. Guidance report, Section 7.4 does introduce the chemical effects topic, but it does defer guidance until testing is complete. The test results are needed to provide a technical basis for the resolution of this issue. The safety valuation indicates that licensees should address plant specific chemical effects if they're not covered by plant testing.

The licensees also should add conservatisms -- this is in the SE -- if sump modifications are engineered prior to the knowledge of the chemical effect test results. Those are statements that are in the safety valuation.

Go to the next slide, please.

The guidance report introduces the potential problems of chemical reactions in the post LOCA environment. These can contribute to the blockage of ECCS pump screens and increase the associated head loss across the screens.

The concern was raised by the ACRS that an adequate technical basis should be developed to resolve the issues related to chemical reactions. This was in your letter of September 30th, 2003.

The foundation of this concern was an observation of gelatinous material that had been observed and a water sample taken from the Three Mile containment following the accident in 1979.

As a result of that concern, Los Alamos did do a limited scope study to evaluate the potential chemical effects. Now, the committee knows this background. Basically it did demonstrate under sort of artificial conditions that you could induce precipitation of metal salts and results would indicate a gelatinous form, and there was associated high head loss associated with creating those products on those samples.

CHAIRMAN WALLIS: But there was no link between the actual corrosion tests and the gelatinous precipitant. The gelatinous precipitant was kind of artificially made with --

MR. ARCHITZEL: That's the point at the bottom. There was no integrated testing to say if you could form it would it transport, and the reason was --

CHAIRMAN WALLIS: Now, have you looked at the result of these tests, Ralph?

MR. ARCHITZEL: Am I going into them now?

CHAIRMAN WALLIS: Have you looked at the results?

MR. ARCHITZEL: I've looked at the results. They were very high --

CHAIRMAN WALLIS: You realize that they're very inclusive. The results are all over the place. They're rather like the preliminary tests of head loss, which are very difficult to explain, and so I'm a bit concerned about going into a test plan which is too locked in just to testing without thinking about what you're doing and maybe doing supplementary work to figure out what's happening and things like that.

MR. ARCHITZEL: There was an awful lot of thought that went into the tests that --

CHAIRMAN WALLIS: But there was very little by way of conclusive results.

MR. ARCHITZEL: I'm not talking about the tests.

CHAIRMAN WALLIS: Oh, I know the joint industry test plan. I've studied it, and I'm concerned about them being locked into just slavishly testing something without thinking about it and without saying, "Gee, whiz, we're getting some strange results. We'd better look at what's happening."

MR. ARCHITZEL: I disagree that it was quite -- there was a lot of work that went into looking at the parameters, how they're representative, what's in the plants, scaled to the plants.

CHAIRMAN WALLIS: But they're arbitrary, not looking at cal-sil, for instance. There's no cal-sil test planned.

MR. KLEIN: Actually there will be cal-sil tested in the --

CHAIRMAN WALLIS: It's not in the test

plan though, is it?

MR. KLEIN: I believe it is.

CHAIRMAN WALLIS: It says "could be extended to cal-sil" or something, unless it has been changed. Has it been changed?

MR. KLEIN: Well, of the approximately half of the tests that are coming up will use calcium --

CHAIRMAN WALLIS: I'm sorry. The test plan must have been changed from what I saw.

MR. ARCHITZEL: It has been changed quite a bit.

CHAIRMAN WALLIS: The test plan says test for lead and chlorine, but there seemed to be no way of putting lead and chlorine in at the beginning. So where does it come from?

There's a whole lot of things like that. I don't want to go into the test plan, but just make sure that you guys think about this test plan and look for things like that, otherwise you may get very confusing results which don't lead to resolving an issue.

It's a difficult problem. Preliminary tests show that it really is difficult. Strange things happen. Some samples gain weight; some lose weight, and in an erratic way. There's no trends with temperature, and so. There's a whole lot of confusing stuff, and chemistry isn't easy, especially when you have a lot of different things that can go on.

So I just warn you that this has to be done very carefully and thoroughly and right because I'm sure it will come back to us some day.

MR. ARCHITZEL: Well, sir, I don't know if I should make the comment. I would think we'd have to come back to you with results of those chemical tests and what we're doing with them, but --

MR. MAYFIELD: Perhaps if I could, this is Mike Mayfield from the staff.

Professor Wallis, I guess I have to feel compelled to take some exception to your characterization of no thought having gone into this. I believe the staff and the industry have, in fact, invested a significant amount of thought into both the test plan, the test setup and the conduct of the test.

CHAIRMAN WALLIS: I don't think you understood my sentence. I said a lot of work had gone into this planning, but if you slavishly follow the plan without having a chance to think about what the results show you as you do the test --

MR. MAYFIELD: And again, I don't quite

what research experiments at least out of my division that you're citing as we slavishly follow a test plan, but that's not the way we conduct research in my division.

CHAIRMAN WALLIS: Good. Thank you. That's good.

MR. MAYFIELD: And we do reevaluate things as we go along and as we learn things. Version 12 of test matrix does, in fact, include cal-sil. I don't know what version you have.

CHAIRMAN WALLIS: We have the version that simply said cal-sil was an option for later or something.

MR. MAYFIELD: It is specifically in the test conditions.

CHAIRMAN WALLIS: I'm very glad to hear it. Thank you. That's good.

MR. KLEIN: I think another point worth making is that there's an intentional step in the test process after the first test to reflect upon results of the first test.

CHAIRMAN WALLIS: You see, we haven't seen any of that. All we saw was what looked like a very limited and very constrained looking test plan. Maybe we have got the wrong information.

MR. MAYFIELD: Again, we would be happy to come back and brief you, and we can certainly provide you the current version of the test plan. If you would like a briefing on what we are doing, we would be happy to do so.

CHAIRMAN WALLIS: I think that would help to reassure us. Right, because we did look at the results of the preliminary test, and we said, look, this is sort of --

MR. MAYFIELD: All we were trying -- excuse me for interrupting. All we were trying to do with those preliminary tests was decide whether this was even conceivable. We couldn't argue it away based on the tests in a beaker, and so we said now we've got to go back and do this in a more scientific, well orchestrated fashion. So the initial rounds were simply can we argue this issue away. We couldn't argue it away based on those very limited tests, and so we had to make the investment to go back and do a more scientific approach to this.

CHAIRMAN WALLIS: Well, Mike, just to give you an example, in the preliminary tests, you did a lot of tests and then found out at the very end that silica had a big influence, and this was a discovery, and I hope that when you do the big test plan, that you have real room for making discoveries and thinking about what they mean. That's all I'm saying.

MR. MAYFIELD: And we agree. And we have brought --

CHAIRMAN WALLIS: That's all I'm saying.

MR. MAYFIELD: -- a lot of additional talent to bear on these tests, on the integrated test series to try and capture that exact, those kinds of issues, as we go along. We're not in a position time-wise or cost-wise to iterate on this a lot.

It's an important issue. We agree, and we're making a significant investment in staff and contractor expertise to look at the results as we go along.

CHAIRMAN WALLIS: In terms of resolving the GSI, this is really still sort of an open question. There's research going on. You're trying to get answers, which is very appropriate,b ut there aren't answers yet. So industry is left sort of not quite knowing where they are.

MR. MAYFIELD: Well, the industry is in the same boat we're in. These are tests being conducted as part of a cooperative program, and they have been involved with us in looking at the test plan and the test conduct as it has been put together.

This is something that caught all of us somewhat late. We do appreciate the fact the committee raised it because it has become a potentially important issue, and we've been scrambling pretty hard to put together a program that addresses the issue in a comprehensive, technically defensible way.

CHAIRMAN WALLIS: When I think about the thin bed effect, this was a surprise. Someone had to figure out what to do when they found it. This could well happen with these chemical effects, too.

MR. MAYFIELD: That's correct.

CHAIRMAN WALLIS: And then you're going to be in a position where -- I don't know what you're going to be in the position of. You may actually have approved some plants and then found out that there were effects that they should have considered that they didn't.

MR. MAYFIELD: We believe that the results will come out of this in a time frame to support the industry's redesign of screens and their reevaluation of the sump capabilities, but there is a potential in all of these things for some new bit of information to come out at the eleventh hour and surprise us, and I think the staff is fully aware of that, and I'm quite sure the industry is as well.

There has been a serious effort to try and put together a program to capture these effects so that we don't get surprised, but you know as well as I thing happen. You learn as you go along.

CHAIRMAN WALLIS: I don't want to put you in the defensive at all. I think you're doing good work here. But I just want to see how it fits in with the resolution of this issue. It seems to be an unanswered area with potential for having a big impact down the road.

MR. MAYFIELD: Yes.

CHAIRMAN WALLIS: And we don't really have much of a clue about where we are in it at the moment.

MR. MAYFIELD: Today --

CHAIRMAN WALLIS: The preliminary test didn't really show us anything conclusive.

MR. MAYFIELD: At -- what is it? -- 11:15 on this day, I have to agree with you. Today I can't put an answer -- give you an answer to the issue. What I can tell you is there's a lot of work going on on a fast pace, which brings with it its own potential pitfalls, just the pace of things, but there's a hard effort going on to try and bring useful information to the table in a time frame to support the industry's reevaluation.

CHAIRMAN WALLIS: So the industry cannot do anything about this until you come up with some results?

MR. MAYFIELD: Well, it's not just me, but I wouldn't say they can't do anything, but I would say that the definitive data are going to be coming out in the next few months. What data we're able to generate will be coming out in the next few months, and I think that people can be making some progress towards reevaluating this issue absent the final word on chemical effects.

But there is the potential that at the end of the day you have to revisit some of what's already been done.

MR. SOLORIO: Dr. Wallis, Dave Solorio.

I'd just like to add to what Mike said by reminding you about the generic letter. We're giving licensees until October of next year to provide their responses. So if this information is able to be finished in a few more months, they'll have a good amount of time to consider it in their solution.

MR. ARCHITZEL: If I could continue on the slides, on Slide 5, there's been some thought to how you look at what species you do have, and we took a reasonable representative in this test program set of chemical conditions for the plants, but we also looked at the thermal hydraulic program that will allow, when it's validated, will allow some extrapolation for conditions.

So we have an ability to look and assess the ability to extrapolate the conditions so that it's not bounded or there's not the dilemma of bounding the conditions.

CHAIRMAN WALLIS: Did these small scale tests relate in any way to the OLI program? Did they help you? Did they confirm or deny or did they have any relationship whatever to the OLI program when it turned out they were finished?

MR. KLEIN: They're still in process. I think we tried to take a measured approach of validating the OLI program for our particular environment, and that started with a look at available literature and then proceeded to beaker test, and we have autoclave testing planned and in progress, and --

CHAIRMAN WALLIS: Well, these are not the small scale tests that have been finished. You're going to do some more ones?

MR. ARCHITZEL: We're not talking about the chemical precipitation tests. There's a series of tests with the program that was used that confirms that it can predict the species and speciation.

CHAIRMAN WALLIS: I'm sorry because there were small scale corrosion tests already performed.

MR. ARCHITZEL: You're talking metal participation tests. These are different here, I think.

MEMBER KRESS: If you feel compelled to prove that a chemical equilibrium program could do its job in gas phase reactions?

MR. KLEIN: Well, I think we recognize that's one of the challenges with trying to apply that type of program to this situation. On the other hand, some of the early validation is encouraging, and I think the staff would like to, if possible, have a toll that enables us to look outside the ultimate conditions that are tested so that when a licensee would come in with a submittal that had conditions outside the test, we might have a means to which to evaluate that.

MEMBER KRESS: My point was just the opposite, that I would have had absolute faith in a thermodynamic equilibrium program to evaluate these kinds of chemical reactions with checking it out.

MR. ARCHITZEL: Dr. Csontos, you had a comment on that?

DR. CSONTOS: Yes. Al Csontos, NRC, NSS.

We're using this code for Yucca Mountain, looking at chemical precipitation, and this code has been validated for those chemical precipitations that are more geologic based.

We're in the process of doing these for metallic corrosion issues with respect to validating the code to, let's say, metallic products, and then those products are then placed into solution in the code, and then it runs through its calculations.

Now, granted this is a thermodynamic program. So it's not a kinetics based program. However, we can't get validation, and we've done validation, that have shown that this is very -- that especially for I think it's boron that the solubility of boron is very well modeled in this OLI code.

We're also doing this for other literature, data sets for similar type of metal species, and then also for actual corrosion tests that we're running now, and also in the past we also did it for just beaker tests, and we're working on an autoclave test to validate this code for these conditions here.

MEMBER KRESS: These are condensed phase reactions you're talking about.

MR. CSONTOS: In liquid?

MEMBER KRESS: In liquid, yeah.

MR. CSONTOS: Yes, and that's why we do an auto --

MEMBER KRESS: You're right. You probably would need some validation for this.

MR. CSONTOS: Yeah, that's what's going on right now. We have already validated for beaker tests and the literature tests, and they've done really well in those.

And with respect to the small scale corrosion tests, those are running right now at Southwest Research Institute. The corrosion tests are used. Right now the corrosion rates are not well developed. We have some that are from the literature, but the corrosion rates are for, for example, concrete. We really just do not have an idea of what species from concrete leach out.

So we're working on this small scale corrosion test. They're opposite to what was going on at LANL before, which was using the metallic salts to form the bed and to get the chill formation there.

CHAIRMAN WALLIS: Does OLI have properties of cal-sil?

MR. CSONTOS: Well, the cal-sil and the

NUKON are an agglomeration of different ceramic type materials.

CHAIRMAN WALLIS: Are they studying cal-sil has all kinds of binders and stuff in it?

MR. CSONTOS: Well, that's true, but from what we can gather, we're not taking into account the binders per se because the binders would have outgassed from the heat generator from the pipes.

CHAIRMAN WALLIS: Well, what concerns me is that all of these things interact, and all of these materials are in there together, and a small amount of chlorine or something coming from some particular ingredient can have an effect on what happens.

MR. CSONTOS: And that's what the beauty of this code is, is that you can go in and manipulate --

CHAIRMAN WALLIS: Well, you have to know that it's there.

MR. CSONTOS: Well, you have to know that it's there, but you can also add it into the program to then see what effects -- to give yourself a brisk baseline to determine that, oh, if you do have hydrochloric acid in there, what effects will it have on the actual final species that come out that could lead to --

CHAIRMAN WALLIS: So you have an advisory panel or something, peer review?

MR. CSONTOS: Yes, ASNW.

CHAIRMAN WALLIS: Because there was some good peer reviewers on the preliminary tests, but I'm not sure that all of their advice was appreciated in the final report. Maybe there wasn't time to use it, but there I thought I caught a few good suggestions. I thought that really knowledgeable experts are involved.

MR. ARCHITZEL: You're talking about the ICETEA (phonetic) test program was peer reviewed. That was peer reviewed.

CHAIRMAN WALLIS: Yeah, but the peer reviewers, I think, had a lot of good points about which group should be taken into account in applying the next test, but just to sort of make sure that, you know, this has all of the checks that it needs so that it's a really good piece of work.

MR. CSONTOS: And I brought up the ACNW because we have gone through the ACNW several times with OLI code calculations pre-Yucca Mountain, and if you want to look at their staff and talk to them, they gave you some more information on the OLI code.

MR. MAYFIELD: Professor Wallis, this

might make it. One of the points I guess I would like to try and make, and it goes back to your issue of making sure we're bringing in the right people, and what we've done is reached across the agency out to NMSS because they had a tool that looked like it could be very useful in helping evaluate this issue, and we're doing some work now to make sure that we can rely on that tool.

CHAIRMAN WALLIS: Well, could you please also bring in -- I think you did in the small scale test -- at least to review the results some really good experts from the outside world, not just within the nuclear community, but people who have a lot of dealings with chemical mixtures of stuff and a lot of experience.

MR. MAYFIELD: Of course, there are people in the nuclear industry that do understand chemistry, but --

CHAIRMAN WALLIS: Yes, but it's useful to have someone who deals with it also outside because you bring in --

MR. MAYFIELD: I understand.

CHAIRMAN WALLIS: Okay.

MR. HSIA: We have for this project, ICETEA project, Professor Griffith from MIT and Bob

Lippman, who both of them will be looking at the test plan, will follow the test, and also --

CHAIRMAN WALLIS: Well, Griffith is not an expert on chemistry. So you need to --

MR. HSIA: I understand that. I know that, but he's an expert in thermal hydraulics and system.

MR. MAYFIELD: You understand that there are large scale integrated tests that are being --

CHAIRMAN WALLIS: Has the loop been built?

MR. HSIA: The first test has --

CHAIRMAN WALLIS: The first test has been run?

MR. HSIA: The first test is within two weeks. It was set yesterday.

CHAIRMAN WALLIS: Good.

MR. CARUSO: MR. ARCHITZEL: With the next two weeks, as we heard yesterday, the first test should be started. There needs to be a shakedown period before that as well.

CHAIRMAN WALLIS: Well, I think -- oh, well, forget it.

MEMBER KRESS: Those slides still test assimilated conditions in the containment?

MR. ARCHITZEL: Scaled.

MEMBER KRESS: Sprays, scaled?

MR. ARCHITZEL: There's a spray aspect to it, and a limited amount of time for the spray, but I think it's a 30-day duration test, but it's not wetted the whole time.

MR. KLEIN: The amount of material and distribution materials is scaled based on industry input.

MEMBER KRESS: The scaling unit, was it scaled according to the surface areas of the materials that you expect to be interacting?

MR. KLEIN: Yes.

MR. ARCHITZEL: In the coolant above the pool, you know.

MEMBER KRESS: Versus the spray flow rate?

MR. ARCHITZEL: Versus the air. No, not the display. It's the volumes. So you've got a sprayed volume that's interacting for a short period of time, and you have a wetted volume with the pool.

So it's how many things are in the pool, how many things in the dry environment that could be wetted. Those are the two separate scalings that were done.

Do you understand what I'm saying?

MEMBER KRESS: Yeah, I understand.

MR. ARCHITZEL: Because the chemical reactions are limited in the sprayed environment. After all, you don't have any sprays to knock it back down.

MR. KLEIN: The amount of material was scaled based upon plant surveys and what was typically done was to take the high end of the amount of each given material and then the low end of sump volume in order to produce a scaling factor to reproduce in the test.

CHAIRMAN WALLIS: And then if I understand, the materials are not allowed to touch each other. They're dangling in this in some way?

MR. KLEIN: Yes. The materials are --

CHAIRMAN WALLIS: In the real plant they're in a sort of sludge or something in the bottom of a sump?

MR. KLEIN: The materials are placed within holder racks, and they are not in contact with each other.

CHAIRMAN WALLIS: You're trying to prevent any galvanic behavior which is possible in a plant. For some reason you're excluding galvanic effects?

MR. KLEIN: Well, our test setup uses a stainless vessel, and material coupons (phonetic) in close proximity to each other, and our judgment was if we try to galvanically couple all of the materials, we may produce results that are not representative of the plant because test coupon arrangement could influence the galvanic effects dramatically in that type of setup, and the mixed potential that could happen from contact with the stainless vessel may also not be representative of a plant containment sump.

MR. MAYFIELD: Professor Wallis, if I could, we're trying real hard to not make a first year graduate student kind of error in mixing too many variables all at one time. So we're starting out -- we started out with these tests, the beaker test. Can we make this go away?

The answer was no, not by inspection. So let's go back, do a, quote, integrated test, carefully controlled conditions, and take the next step.

CHAIRMAN WALLIS: Well, you see my --

MR. MAYFIELD: You're raising a good point about the potential for interactions. There's no dispute about that.

CHAIRMAN WALLIS: You had trouble explaining any of the results from the simple test with one material, which was zinc, but I don't want to get into that.

MR. MAYFIELD: Well, you want us to go back and explain a test that I don't think any of us were all that happy with.

CHAIRMAN WALLIS: Right.

MR. MAYFIELD: So that's a challenge that I'm not sure any of us are up to.

CHAIRMAN WALLIS: There is a possibility that the results of these tests are completely inconclusive.

MR. MAYFIELD: Oh, I don't agree.

CHAIRMAN WALLIS: Well, there's always --

MR. MAYFIELD: Well, let me back up. There's always the possibility of any test result is inconclusive.

MEMBER FORD: I think I haven't seen the latest test matrix, Mike, but back in June I was concerned that we were focused on trying to simulate all of the combinations of material, et cetera, that we would have in a containment.

And I'm simplifying, but by saying essentially that we'll finish up with one test which would be either a go/no go result, my concern was that these items, these zinc and metal items, are connected. Some of the chemicals you have there can be inhibitors. I think I mentioned this to you.

MR. MAYFIELD: Yes.

MEMBER FORD: And so the result that you get at the end of the day, I assume it's a good result and there's no gelatinous stuff there.

If you changed one of the parameters, you may suddenly find that you do get it, but you wouldn't have tested it.

MR. MAYFIELD: Yes.

MEMBER FORD: So my question at this stage is, not having seen the latest test matrix, is there a version of a single effects test that would avoid us falling into that first year student trap?

MR. MAYFIELD: Well, we do vary the test parameters.

MEMBER FORD: Yeah.

MR. MAYFIELD: It's a little different question, and --

MEMBER FORD: You've got an infinite number of system configurations.

MR. MAYFIELD: Maybe Al Csontos can help me a little here.

MR. CSONTOS: Yes, that was the purpose of the OLI thermodynamic calculations, was to go ahead and try to constrain some of these parameters. There are over 15 --

MEMBER FORD: If I could just interrupt.

MR. CSONTOS: Go ahead.

MEMBER FORD: Because you brought up OLI. Again, I brought this one up in the June meeting. The OLI, I think, the thermodynamics program is primarily for processing this if I remember correctly.

MR. CSONTOS: That has been the main use of it.

MEMBER FORD: The main use of it.

MR. CSONTOS: In commercial.

MEMBER FORD: It doesn't tell you anything at all about the kinetics.

MR. CSONTOS: That's right.

MEMBER FORD: And you agree with that. But talking about fairly short-term tests and what we're really concerned about is the kinetics of the reactions, not the thermodynamics.

The thermodynamics will say what might occur in 1,000 years at equilibrium, but it won't tell you what is going to happen in ten minutes.

MR. CSONTOS: But it will provide insight into the separate effects from, let's say, for example, galvanic issues. What we did was we went in there and increased various amounts of area corrosion rates, therefore products inside the sump pool, by orders of magnitude to see what the results were. That gave us some sort of risk understanding of let's say there's a galvanic corrosion issue with respect to aluminum. We increased aluminum content in the pool by two orders of magnitude. What was the effect? Minimal. It just increased slightly.

Therefore, you know, these are the types of things that we are trying to do with this code to constrain some of these parameters that we understand are kinetics based. I mean, some of these formations, especially for gel formations, it will be dependent upon many other things that we can't calculate in this code.

For example, flow rates. Flow rates will have a significant effect on whether gels with agglomerate and form, but this code can't do this. So what will we do is we try to constrain as many parameters as we can through this thermodynamic code. We're using it as an insight, not as a tool to model the entire --

MEMBER FORD: I guess we're taking up a lot of time on this. I guess the concern is, first of all, we haven't seen the latest test matrix, and the second is this underlying gut feeling that this could be a problem, and you will be looking at, I know, expert opinions from outside.

MR. CSONTOS: Yes.

MEMBER FORD: Particularly McDonald.

MR. MAYFIELD: Yes.

MEMBER FORD: I mean, that comes to my mind.

MR. MAYFIELD: You raise a good point, and it's something that I guess I want to think about a bit more, and spend a little more time talking with the staff, Al, and other consultants like Digby (phonetic).

You raise a good point, and I want to make sure that as we go we're not trapping ourselves. That goes back to some of Professor Wallis' issue about make sure we're learning as we go along.

I think we should be coming to the committee, subcommittee to talk about these tests and test results, and I think that's a specific issue that I'd like to see us put on the table and tell you what we've done about it or where we're going with it.

CHAIRMAN WALLIS: Yes, we look forward to that.

MEMBER KRESS: Just one more quick question about the test. The pool, assimilated pool, is it stirred or is it quiescent?

MR. KLEIN: The vessel that we're using, we're trying to simulate the sump flow rates within the water that the coupons are sitting in.

MEMBER KRESS: By stirring it?

MR. KLEIN: No, I believe it's through just circulation from pumping the system. I don't believe we have a stirrer in the tank.

MEMBER KRESS: But you do have a flow system.

MR. MAYFIELD: Yes. It is a flow system.

MEMBER KRESS: Okay. That was my concern.

MR. MAYFIELD: That was one of the other issues that we were concerned about in the earlier test, is potential for gradients and how well mixed or not well mixed things were.

CHAIRMAN WALLIS: How about temperature? Temperature is up to sump conditions?

MR. MAYFIELD: Yes, sir.

CHAIRMAN WALLIS: We don't want what we had yesterday of a range of tests that don't cover the actual sump conditions.

MR. ARCHITZEL: That was one of the specific things we did use OLI for, to try and determine up front do we need to do a pressurized test or not a pressurized test, and the result was, considering expenses, et cetera, that it was sufficient not to do a pressurized test to achieve those temperatures, but we did evaluate that and decide it's okay to go at a temperature range where --

CHAIRMAN WALLIS: And some of these tests take 30 days?

MR. ARCHITZEL: The first one does, and then we can adjust it a long way. The very first one has no provisions to not do 30 days, and after that it's looked at, and it can be shortened.

CHAIRMAN WALLIS: You didn't think of doing tests in parallel to shorten the time or something?

MR. ARCHITZEL: There's only one.

CHAIRMAN WALLIS: There's only one loop. So yeah.

MR. MAYFIELD: There is international work going on not exactly on this, but on a related chemical effects, and we're paying attention to that work. They similarly are paying attention to this work. So that does give us one additional bit of information.

And, secondly, another set of expert eyes to look at this.

CHAIRMAN WALLIS: Do you have workshops

with industry and things like that?

MR. MAYFIELD: This particular piece -- well, yes is the immediate answer, and secondly, this piece has been well coordinated with the industry as part of the cooperative program.

MR. HSIA: Can I add something? Tony Hsia from Research.

The test parameters and test conditions, we work very closely with industry. They were provided by industry as the temperature and pH value and what kind of materials are in there. That's well coordinated.

And also, I would like to point out the 30-day test, so-called 30, or whatever period we do is going to be monitored on a daily basis at least. So it's a continual tracking of the odd chemical.

CHAIRMAN WALLIS: Okay. Can we move on? I really wanted to hear what the industry has to say, but I think Mike has this. Do you want to say some final words to us?

MR. JOHNSON: If I can just add, I know I won't talk very long. I said a lot of what I wanted to say yesterday.

I just wanted to thank the subcommittee for meeting with us, obviously. We value the input that we get.

As I said yesterday, you know, keeping in mind that the likelihood of this situation is low, but we recognize that it is a real situation obviously through the actions that we've taken. The industry has put together an approach to deal with this issue. We have gone through that approach. We have added additional restraints where we think those additional restraints are necessary.

And based on that, we believe that the approach in the baseline with the refinements bound the problem. And, in fact, as you point out, Dr. Wallis, we believe that the industry and the NRC will be in a better place after these fixes are made based on the evaluation from a safety perspective.

We've spent a lot of time discussing the evaluation. One of the points I wanted to make is that the evaluation is really a package. We spent a lot of time talking about the various issues that make up that package, and there are basically two kinds of concerns about the issues. We focused, in fact, on insuring that the assumptions and the approach for those individual issues are correct and sufficiently justified.

And we have got some take-aways from you for that

We talked about clarifying the guidance in some instances where we can say thing in plainer language or be more specific so that the folks who are implementing it can implement, and there are some takeaways associated with that also that we have.

We are committed to making some changes to the SE based on what we've heard. We would look to share with the subcommittee, in fact, the ACRS in preparation for the meeting with the full committee in October.

We believe that based on the changes that we anticipate making in response to what we've heard today, that we will, in fact, have a package, an overall package that despite the differences that we may have on the individual issues, sufficiently bounds the challenge that we have.

You know, there may be areas and specific issues where we can't say whether ten percent difference in the size or the zone of influence is the right number, but I think in general when you look at all that is in the package, we will be at a place to say that we can bound the problem in a way that enables us to have a high degree of assurance that these plants can perform in the case of, again, a worse case accident, if you will, on the demands of the performance of the sump.

And I guess the last point I wanted to make is the industry knows how to do much of what we've talked about here already. The industry is putting on a workshop in December. I don't want there to be a notion that we're going to write this SC and then we ship it off and then licensees in the industry will be struggling to implement what is there without interaction because that's certainly not the case.

So we have spoken just in a few minutes about chemical. You know that there are the things that are ongoing, that an opportunity for a continued dialogue with the industry as we go forward in terms of working the evaluation and the fixes.

And last but not least, as I said, this does begin a new stage or at least when we issue the SE, and the staff is planning on what we will do to review the evaluation and to follow up and ultimately close out the issue in 2007, and there will be, again, we will look for additional opportunities to interface with the ACRS.

And, Mike, if you wanted to add to that?

MR. MAYFIELD: This is something that obviously both office have a keen interest in. I think as Mike and I have talked about, the observations over the last day and a half and some prior engagement on the Reg. Guide 1.82.

I think it is important to keep in perspective that it's an overall analysis. The individual bits and pieces, I think there have been some interesting issues raised, as Mike characterized it, some take-aways. Plainly from the research program we've tot some things to go back and look at, but I do believe when you look at the totality of the overall approach, it provides a set of guidance that's sufficient for the industry to move forward on this issue and improve safety at the plant.

MR. JOHNSON: And, of course, it goes without saying, one last point, it goes without saying that this SE will be one acceptable approach. As our guidance says, it's one acceptable approach. We fully anticipate that there may be situations where licensees come in and propose other approaches that they consider to be acceptable, and we'll have to deal with those.

I just wanted to make sure that --

CHAIRMAN WALLIS: Mike, I just want some quick answers from you, one sentence. Do you intend to go ahead with this before the full committee?

MR. JOHNSON: Yes, we would like to go ahead with this for the full committee.

CHAIRMAN WALLIS: And the SER will change between now and then. So how do we deal with the changes?

MR. JOHNSON: I would propose that we provide those changes in read line strikeout form, and that one of the things we do with the full committee is highlight where we saw -- what we believe the comments were and what we've done to revise the SE in those areas to make them more visible to you.

CHAIRMAN WALLIS: I think you might anticipate that very many of the comments we made yesterday and today will have a probability of coming up again if you present before the full committee.

MR. JOHNSON: Anticipate that. I'm also hoping that when you see what is in the revision, that goes to a number of the areas that you've raised.

MR. JOHNSON: Well, I guess I'd just say I'm looking forward to your revision. Personally I wonder how you're going to do it.

MR. JOHNSON: So is Dave.

MR. JOHNSON: Could we hear from the industry now? Is it an appropriate time to do that?

I'm sorry you've had to wait. I wish we

could have started earlier.

And who is it? Is it Tony or who is -- oh, go ahead. If you go up there, it would be good. I think it's more appropriate to put you on stage than behind us.

I hope you can give us some very quick sharp and important points because we haven't got the time.

MR. BUTLER: I'll keep it very brief.

John Butler, NEI.

Thank you for the opportunity.

First off, let me make the point that we have not had an opportunity to review the safe concept evaluation in any detail. We received it early Tuesday and have heard a lot of the exceptions to the guidance for the first time in the last two days. So we have a lot of questions, and we still have a lot of work to evaluate the importance of some of the exceptions that we're taking.

So my statements are generally going to address my overall impressions of the staff's work and what we've seen of the draft safety evaluation.

One point I would like to make is that there has not been a lot of communication between industry and the NRC since we submitted the evaluation methodology in May. The staff's schedule for providing the safety evaluation report, in effect, did not allow them the luxury of interacting with us to get clarifications.

That being said, I think they did interpret what we intended with the evaluation guidance in most cases properly. I think there are some cases where they did not interpret our intentions.

Our biggest difficulty with the safety evaluation is not how they interpreted our guidance, but how they have then taken exceptions to the guidance to, in effect, make it even more conservative.

One of the things we've been struggling with with this issue throughout is how to make it a practical problem. Clearly, whatever answer you get you have to deal with in the plant and to address it with modification to the design or in other ways so that you can address the issue in a practical way.

The risk aspect of this issue is one way to try to put it into perspective, and we've tried to do that in a lot of our discussion with the staff, and I don't know how successful we were in our evaluation guidance. I think in a lot of ways we put too much emphasis on the low risk significant spectrum of breaks, and our intent at Section 6 with the alternate evaluation was a step in the right direction to try to put some, again, better focus on the more risk significant spectrum of breaks and concerns with this issue.

But, again, it's a very small step, and I can speak to how small a step it is, especially with the modifications that the staff has made in the safety evaluation.

But let me try to make the following point. We understand with this issue that the final resolution is going to be driven by one of two aspects: either the thin bed effect and the head loss you get from that thin bed effect or from the maximum debris accumulation you get on the side.

We maintain -- I think the staff will agree with us -- that the risk significant aspect is a thin bed. The thin bed can occur from a broad range of events. You do not need much debris to be generated. You do not need a lot of particulates to occur before that thin bed becomes a possible player in the significant head loss.

On the other hand, the significant debris that we are being directed to calculate for the full double ended and the conservatism we are placing on the calculation of that maximum debris, it can also lead to maximum head loss, and I'm afraid in the way the guidance is currently laid out, it's going to drive the design.

But what we need to keep in mind is that maximum debris generation is coming from an event that is orders of magnitude less risk significant than the spectrum of breaks that are going to cause the thin bed effect, but we're spending a lot of time, a lot of effort, and a lot of discussion on those factors that play into that national debris accumulation that you will get from that maximum full double ended break.

A lot of the exceptions that staff took to our guidance focus in on how much debris you generate in that maximum proximate break. They don't affect the thin bed.

There are some that do affect the thin bed, and we can look at those in more detail, but generally I'm speaking to a lot of the exceptions that were taken to what we considered to be very conservative guidance to make it even more conservative for an even that is eventually small in probability and treated in a way that is extremely conservative not because we want to do it that way.

It's just that we have difficulty finding a more realistic way to bound all of the uncertainties.

So we've taken our best shot, and the staff's exceptions take us even further along that line. So I feel like I'm repeating myself now, but hopefully I have made my point clear.

CHAIRMAN WALLIS: Can I ask you something?

MR. BUTLER: Yes.

CHAIRMAN WALLIS: Do you understand what this thin bed effect is? Do you understand how to define it, how to predict it, how to say when it occurs? Because it's still something of a mystery to me.

MR. BUTLER: Well, it's clearly going to be dependent on some of the assumptions you've --

CHAIRMAN WALLIS: Well, I know. I just want to see a clear, one-page document which says this is what it is. This is when it occurs. This is how to predict it, and do you have that? Do you guys understand to that level?

MR. BUTLER: We can prepare you a description of our modeling of the thin bed, and that's going to come from, in part, a lot of the experiments that were done with the BWR owners group resolution of this issue, and a lot of it is going to be fixed by our modeling and the assumptions that are made in the analysis.

Some of the assumptions --

CHAIRMAN WALLIS: Okay. Well, it seems to be clear that since this word, these combination of words "thin beds," in fact, comes up so often, we need to be pretty clear what it is.

MEMBER KRESS: Do you think it's possible to design a screen that would not have a thin bed effect?

MR. BUTLER: I am told that that is the case. There are designs that have been tested and have not exhibited the thin bed effect.

MEMBER KRESS: That's like proving a negative. So you need some modeling and some understanding to extrapolate that kind of data. So, you know, I'm perfectly in sympathy with Graham's statement that we need to know more about this mysterious thin bed effect, particularly if the industry comes up with a design that claims not to have a thin bed effect.

I think you gain a lot. You can do, like you say, some risk rationalization of the maximum debris problem, but the thing bed you have to somehow -- it's a killer.

MR. BUTLER: And I appreciate that because it goes to the point I've been making. It focuses in on what is probably the more risk significant aspect of this event.

MEMBER KRESS: Yes. So, you know, we certainly would like to see some physical understanding of thin bed effect and some database that shows that maybe you can avoid it by certain types of filter designs or something.

It seems to me like you could have a lot to gain going in that direction. Enough said, I guess.

MR. BUTLER: To keep this short, I'll just make one point. There are a number of things we've seen in the safety evaluation that we do not quite understand, and you can take a different interpretation of what the staff has meant. If it is interpreted in one way, we have great difficulty with what the impact would be on our evaluation.

And so we would welcome any opportunity we can have to interact with the staff before this becomes final. I'm not sure how that can happen though.

One last thing if you have the time. I did ask Tim Gemistreck of Westinghouse to put together a graphic and some figures to try to put this into perspective of what we're talking about, of the maximum debris generation, of what we're talking about.

CHAIRMAN WALLIS: This will take a couple of minutes or five minutes?

MR. GEMISTRECK: Five or less.

CHAIRMAN WALLIS: Five or less.

MR. BUTLER: As much time as you want it to.

CHAIRMAN WALLIS: Well, maybe since it's visual the information will come across pretty quickly.

MR. GEMISTRECK: It is. If I may, can you queue up?

These graphics were actually generated by Gil Ziegler. The graphic here is for a three-loop PWR, and it shows the effect of the boundary of a 10 psi ZOI, which is basically for a double ended guillotine break.

If you look on the right-hand side in the middle of the red sphere, that's the bioshield that's sort of cutting through it on the outside periphery and the reactor cavity, refueling cavity is slightly to its right.

And those are the robust barriers.

CHAIRMAN WALLIS: Now, to go back to the question the committee had, if you put a jet in there instead of a sphere, that jet would probably read all the way across containment at the same psi.

MEMBER SIEBER: It can't go through the wall.

CHAIRMAN WALLIS: If it didn't hit a wall.

MR. GEMISTRECK: I don't know that it would. It might be defected upwards.

CHAIRMAN WALLIS: But it would reach whatever it could reach without hitting a wall.

MR. GEMISTRECK: Granted, granted. I won't disagree with that. But for that kind of a zone of influence, I did some quick calculations. The total debris that would be generated from a representative steam generator and the associated primary system piping, total volume of fiberglass debris, assuming that the entire steam generator is insulated with fiberglass, is on the order of 14,000 cubic feet, and using the baseline methodology in Chapter 3, of that approximately 5,100 cubic feet of fiberglass would find its way to the sump screen.

Assuming that you had a pickup truck bed that's six foot wide, two foot high, and eight foot long, you'd end up with approximately 53 pickup trucks filled with debris.

CHAIRMAN WALLIS: You know, some of these screens aren't much bigger than a few pickup truck beds.

MR. GEMISTRECK: That's correct.

Now, if you use the refinements as presented in Section 4 and only those refinements, taking no advantage of any plant specific features that you might have, I calculated or estimate approximately 3,500 cubic feet of fiberglass debris making its way to the sump or approximately 36 pickup trucks.

Now, that's --

CHAIRMAN WALLIS: These would be full length pickup trucks, eight foot bed.

MR. GEMISTRECK: Use an eight foot bed.

(Laughter.)

MR. GEMISTRECK: Let me repeat. Now, I even used the little wider bed. It was a six foot, as most pickups are five and a half. I didn't take into account wheel wells either.

(Laughter.)

MR. GEMISTRECK: Six feet wide, eight foot long, six foot wide. So approximately 96 cubic feet per pickup truck.

CHAIRMAN WALLIS: And do you expect to analyze this problem away?

MR. GEMISTRECK: Well, that's why we have some of the options and design options also, Dr. Wallis.

CHAIRMAN WALLIS: This really helps put it in perspective.

MR. GEMISTRECK: Okay, and then if we go on to the next slide, please, we're looking at a six psi ZOI and slightly different steam generator, but again, you see the graphic of where it is, and you're well beyond the bioshield, and you're actually penetrating or touching the outside of the containment wall.

CHAIRMAN WALLIS: This is 100 pickup trucks or something.

MR. GEMISTRECK: Many more, yes.

MR. BUTLER: Dr. Wallis, I point out that going from that first slide to this slide is the impact of the 40 percent increase in the deflection pressure that the staff was asking for.

MEMBER KRESS: Now, when you use this zone of influence, do you include everything in the red area?

MR. GEMISTRECK: That's correct.

MEMBER KRESS: Even though the bio is in between?

MR. GEMISTRECK: No. We just use what's within the robust barriers.

MEMBER KRESS: Okay.

MR. GEMISTRECK: It's only what's put in the robust barriers. Obviously we're not looking at the wall, is a very robust barrier. We're not assuming that what's behind the wall is going to be impacted.

MEMBER KRESS: Okay.

MR. GEMISTRECK: And in the table that was presented yesterday --

CHAIRMAN WALLIS: This is even bigger.

MR. GEMISTRECK: Yes. This goes up to the four psi, and you can see it's well beyond the containment.

CHAIRMAN WALLIS: Well, I'm really impressed by the 36 pickup trucks. I don't really need to have 5,000 pickup trucks.

(Laughter.)

CHAIRMAN WALLIS: I mean, this really helps, and it would help, I think, if when the staff made presentations with all of this regulatory space stuff if they gave us some reality pictures like this so we could understand the scope of things and the range of things.

MR. LATELLIER: May I ask a question to clarify?

Tim, in your graphic illustration of debris loadings, did you limit your estimate to the debris within in the compartment or are you giving us information about the entire volume of that red zone?

MR. GEMISTRECK: The entire volume of that red zone.

MR. LATELLIER: Which you're not actually transporting to the screen. That's the distinction that I wanted to make.

MR. BUTLER: Wait a minute. The entire volume inside the red zone was 14,000 cubic feet, and using the transport methods as described in Section 3 of the NEI guidelines, that reduced down to approximately 5,150 cubic feet.

So what got to the sump screen using the baseline methodology in Section 3 was 5,150 cubic feet, which is a considerable amount of debris.

CHAIRMAN WALLIS: Let's put that in perspective. We were told that some screens are 12 foot, 12 square feet. That's 500 feet thick debris on a screen. Some of them have about 100 square feet. that's 50 feet of debris?

MR. LATELLIER: No, Dr. Wallis. My point is could you please repeat those numbers including only the debris within a compartment because you are taking advantage of truncation of the spheres. So your comparison is a little bit misleading unless you give us the information.

MR. GEMISTRECK: No, it's whatever was within the zone of influence that was on the piping, the steam generator.

MR. LATELLIER: Within the break compartment?

MR. GEMISTRECK: Within the break compartment, yes.

MR. BUTLER: There's very little debris outside that.

CHAIRMAN WALLIS: If it gets onto the screen, who cares about its --

MR. BUTLER: That's correct.

The other thing that was not included in the calculation they did was the coatings debris that would be generated. That is just fiberglass insulation on piping.

CHAIRMAN WALLIS: I think if someone had

given us an engineering perspective starting with this kind of stuff from the very beginning, we might have come up with a quite different solution to the problem.

MEMBER KRESS: You can't build a screen big enough to take care of that problem.

MR. LATELLIER: I'd like to remind the committee that this kind of information had been briefed earlier in the staff's presentation of revisions to Reg. Guide 182 where we did present spatial volumes and also debris estimates in the thousands of cubic feet.

CHAIRMAN WALLIS: This is irrelevant, what was presented before. You're making your final case for a SER and a guidance, and it has got to stand on its own, and it had got to be clear and convincing.

And when we see pictures like this, I think we have to wonder how you can calculate away the problem.

Do you have some more?

MR. GEMISTRECK: No. That's it, sir.

CHAIRMAN WALLIS: Interesting stuff. Do you have a solution?

MR. GEMISTRECK: We do have some solutions, yes. We believe they have their alternatives.

CHAIRMAN WALLIS: It would be very good to see the embodiment of a possible solution which followed the guidance, to see how these 36 pickup trucks of stuff went somewhere else and didn't cause a problem or somehow or other were handled by the sump in some suitable way or whatever.

DR. PETRANGELO: Dr. Wallis, early on we had thought about potential for a pilot for the guidance, and I think the staff saw some value in that also, but there just wasn't enough time to do it given the current schedule.

I see this thing, and it sends shudders up and down my spine because you pile all of these conservatisms on top of each other through every different aspect of the evaluation and you come out with an answer. I think Dr. Kress came to the conclusion that you can't build a screen big enough to handle that plant.

CHAIRMAN WALLIS: Yeah, I think we suggested that long ago in a letter that you ought to consider that there are better ways to keep the core cool or there are alternative ways to keep the core cool, which might well solve the long-term cooling problem, which the real issue is: can you keep the core cool, not how do you handle 36 truckloads of stuff.

DR. PETRANGELO: That's right.

CHAIRMAN WALLIS: If it doesn't affect the core cooling, who cares? If it's just lying on the floor somewhere, it's not going to affect the safety of the reactor.

The problem is you've designed a system to sort of flow it to the place where it does a lot of damage to the --

DR. PETRANGELO: Well, and some of the early discussions talked about trying not to get to that point where you're in recirculation for the more likely events, and maybe that will come later through the change to 50.46.

I hope it does because that's the more risk significant part, and I think that's where we can have the most safety benefits with a change to the --

CHAIRMAN WALLIS: I think it would be very good if we started the presentation to the full committee by saying this is the problem. Now this is the fix that's suggested by the guidance and the staff's SER, and we could see if it seems credible or not.

Then we get something to sort of set the stage for it all. Otherwise it all seems to be going on in a regulatory space, which sometimes gets a little different from what I would call engineering.

MR. JOHNSON: Just recall, DR. Wallis that it's not the staff's fix. It's the staff's evaluation of the industry's guidance to evaluate the problem.

CHAIRMAN WALLIS: But if it can't be applied realistically and get an answer, it's useless. I said that before. You can't go through the motions of constructing some fantasy land. You've got to face the reality of this thing that we see up on the screen.

MR. JOHNSON: Well, I mean, there are fixes that are, I think, envisioned by the industry that aren't just putting in a larger screen.

CHAIRMAN WALLIS: That's what I'd love to hear about, but we don't have time to do it today. I think the beautiful thing would be for some engineer to come in and say, "This is the problem. This is how it's fixed." That I would love to see, but we're not going to get there today. We may not get there for ten years, but I just would love to see it happen.

It's now noon. Does the public have anything more to say?

MR. FEIST: I did have one hopefully very brief comment on one of the exceptions.

CHAIRMAN WALLIS: Who is this?

MR. FEIST: My name is Chuck Feist.

CHAIRMAN WALLIS: I'm sorry.

MR. FEIST: Comanche Peak.

One of the expectations, I believe, is an error and inconsistent with the reg. guide. When the staff took exception to the guidance on secondary breaks, steam line breaks, they were citing the use of GDC-4, MEB-3-1, and licensing basis. Fifty, forty-six wasn't acceptable, and we don't disagree with that.

However, secondary breaks don't involve 50.46. They only involve GDC-4. So, therefore, their argument that you can't use it was invalid.

In addition, they said that you may take credit for dose consequences, which is also not true. For secondary breaks you use the outside containment breaks which are bounding for dose analysis.

So the only purpose of the containment spray recirculation for secondary breaks if for 50.49, for equipment qualification, not 50.46. So the guidance in the NEI guidelines is consistent with the words in the Reg. Guide 182, but the SER is not.

CHAIRMAN WALLIS: I guess the staff will take note of that and do whatever is appropriate.

PARTICIPANT: Yes.

CHAIRMAN WALLIS: Do we have another speaker? Go ahead.

MR. KOSTELNIK: Dr. Wallis, Mark Kostelnik from Constellation Energy.

I just wanted to clarify that and make sure the committee knew that at least one company, Constellation anyway, is interested in an active system. We need NRC support to help bring that to a possibility.

I'm representing Calvert Cliffs and Gunnet, and we'd like to have that option, but in reality we have the schedule, and in my situation, I'm probably as big an advocate in getting this done in 2006 and seven as anybody because I have an extremely short outage in 2008 and nine. We're on a 24-month cycle, and I will add two to $5 million to my project if I can't get this done in 2006 and '07.

That's unacceptable economic impact on our part. So we are doing everything we can to try to get this done in '06 and '07, and I'm here to tell you that we are in extremis right now. We cannot tolerate anymore delay of this SER or any other technical decisions from where we're at. We're going to be making decisions with more risk than we want to take to try to meet my schedule anyway.

I'll be just one company. I think there's others out there that may be in my shoes.

CHAIRMAN WALLIS: I find that also very helpful to put some perspective on this problem.

Anybody else? We may open the doors here with these hearings.

(Laughter.)

MR. OAKLEY: My name is Russ Oakley. I'm with Duke Energy, and I'm a sump engineer at Oconee Nuclear Station.

We have some issues with coating that's going on. I don't know how much the ACRS is aware of it. I know some of the folks here from the NRC are.

I just wanted to make the point of yesterday's. There was some discussion about unqualified coatings, and it conveyed the impression to me that many people in the room have the impression that it's a simple thing to make your unqualified coatings qualified, and that is an untrue statement.

That is an insurmountable obstacle that we couldn't financially accomplish. We would solve that problem almost certainly with more sump screen area as opposed to we are spending a half million dollars this fall on our Unit 3 outage just to reconstitute about

25,000 square feet, which is a relatively small fraction of the entire containment, as you well know.

So the economic impact of that is just much larger than I think was understood.

The other issue that I have a great concern about is the downstream effects that were discussed and the postulated induction of metallic fragments into centrifugal pumps.

I don't know what the answer to that is, and I don't know that anybody in this room does. I can't pick up the phone can call my pump manufacturer and have any expectation that he's going to tell me that that's going to be okay, and I don't know that there is a pump design where you could get a manufacturer to say that that's okay.

MEMBER SIEBER: Yeah, there is.

MR. OAKLEY: That they can digest metallic fragments and that's okay to their pump?

MEMBER SIEBER: For a relatively short period of time.

MR. OAKLEY: Okay. Well, I mean, that's a problem. I mean, don't have relatively short periods of time. If the pump fails, it fails directly when you open it on, right?

MEMBER SIEBER: Yeah, but it typically

completes its mission before it fails.

MR. OAKLEY: But this is the screen in the first hour.

MEMBER SIEBER: That's right. Your pump can run for days.

MR. OAKLEY: With no effect?

CHAIRMAN WALLIS: Anybody else want to say anything now?

MR. OAKLEY: Okay. I'd be interested in hearing where you are getting that information from because I don't hear it from --

MEMBER SIEBER: Go to the Internet and look up Gould's pumps.

MR. OAKLEY: Gould's pumps?

MEMBER SIEBER: Yes, and that's just one manufacturer. There's a bunch of them. You know, pick a pump, vertical de-draft pump.

CHAIRMAN WALLIS: It's not really a question of time, is it? If the right piece of metal gets in the right place it stops the pump, and it's hard to track.

MEMBER SIEBER: Yeah, but if it's harder than the pump face.

MR. OAKLEY: I just don't understand what the NRC's expectation there is, I guess is all I'm saying.

CHAIRMAN WALLIS: I think it would be wonderful to have a kind of workshop where we heard a lot of these things from industry. We don't have time for it, and I think that input is important because this isn't the place for it. We don't have the time. I think it is, however, very important. I wish we had the time.

We've got to go to lunch. Usually we go around the table and get some sort of input from the members.

Oh, someone wants to say something more? I don't think we want to go back to technical issues. We're sort of at the summary stage now.

All right. I think the question that I have, and I certainly want to get input from all the members before the full committee meeting because I have to write a letter if we go forward with this thing.

I just wonder if you all have advice for the staff about whether to go forward with this and if they do go forward with it, what advice you have for them to make things easier for them at the full committee meeting, or anything else you want to say.

Could you each take a minute to give some impressions to the staff of how you're thinking about this?

MEMBER SIEBER: I think over the last day and a half we have pointed out in this meeting a number of defects in the safety evaluation as it is now written, and I think that the staff needs to correct those defects to make them technically correct before they issue the SE.

On the other hand, I don't see, other than the chemical effects work, which isn't done, I don't see major problems with the content of the guidance document or the SE, with the possible exception that I think that the equations that they are using are sort of a reach from the standpoint of describing the physical phenomenon that's taking place.

And of course, you do have the big problem that the data that supports the algorithms used in those equations doesn't match the operating condition of the plant, and strictly interpreted means you can't use the curves.

So that somehow or other has to be rationalized in some way. So those would be my comments.

CHAIRMAN WALLIS: Peter.

MEMBER FORD: I have three questions I ask myself. So I'll just give you the three questions if I answer.

The first question was: have all of the relevant phenomena been addressed? The answer is no. Chemical effects hasn't been addressed, and we don't know an impact of that.

Second question is: are there signed analyses of relevant phenomena confirmed by experiment and plant experience? The answer is a qualified no, the qualification primarily because of a lot of uncertainties into the model inputs. We have been told that on all of those model inputs we've got conservatism hooked onto it. There's a question about how those conservatisms have been achieved, the 40 percent reduction, for instance on the destruction pressure.

So, therefore, the analyses are neither realistic, as was stated. They are presumably bounding. The question I have is: are they over bounding?

The third question is: how do these uncertainties in model and model inputs affect the resultant NPSH and its variation, the margin between the upper limit of the variation and the manufacturer's pump specification?

That has not been done, and until that's done, we have no way of assessing the risk associated with this NEI GR and the NRR SER.

The bottom line is I question the usefulness of going ahead with the full ACRS meeting until we have got some of these uncertainty analyses, until we have gotten some idea of the chemical effects.

I'll write this out in more detail

CHAIRMAN WALLIS: Vic?

MEMBER RANSOM: Well, I focused mainly on the jet behavior and debris generation part, and I still feel there's considerable confusion in terms of how to relate this zone of influence to the ANSI jet model, and the ANSI jet model itself may have problems.

And so I would say at a very minimum, these technical issues need to be cleared up. If you're going to use the ANSI model pressure as a metric for damage, then to unambiguously relate that to the damage pressures for the insulations, and so that's the main issue that I've dealt with, and I see it as a fairly big unknown actually.

CHAIRMAN WALLIS: Tom.

MEMBER KRESS: Well, if you use the

methodology, I think most of the plants will end up with more debris than you can filter out. So they're going to have to rely on some reduction in the conservatisms.

I have no idea how conservative the thing is, and I'll have no idea on which's an acceptable reduction in the conservatism.

The two killers for me, three possibly killers of this whole thing is the thin bed effect, the downstream effects. I have no idea how to implement their guidance on the downstream effects, and I don't know how the industry will implement it.

Plus the chemical possibility. I suspect the tests that are being done to look at the chemical effects will tell us a lot and may be useful in how to deal with the chemical effects.

I don't know how you're going to deal with the thin bed effect. I think the latent debris in practically every plant is enough to give you a thin bed effect, and how you can argue it away I don't know unless you can prove that there is a filter design that won't exhibit a thin bed effect.

I'm very hopeful that the use of the risk informed approach will help on a lot of these things, particularly on the maximum amount of debris that gets there. I think some refinement in the spherical model has to be done in order for that to help though. I just don't like the spherical zone of influence model at all. I think it is way too conservative. I think it can be changed.

So there's enough of these type of things in the methodology in the SER that makes me wonder whether we ought to bring it before the full committee at all.

So I would prefer at this time just to wait and hear some more from the ongoing research and more from the industry. I think the stuff we got from the industry, although brief as it was, was very helpful. So I'm in favor at this time of not even bringing it before the full committee.

CHAIRMAN WALLIS: My advice to the staff would be you're not ready, and if you go ahead with this for a whole host of reasons, nobody is going to be happy. And I would love to make people happy. I don't quite see how anybody on any side of this is going to be happy because I don't see the GSI really being resolved this way at this time. They may just appear as another GSI or something equivalent afterwards.

And I would hate to write a letter which has too many critical evaluations of either the staff or the industry or any other actors, but that may be what ACRS has to do because the ACRS has to tell the truth, and we cannot really mince words.

We'll try to be nice about it, but if we see something which is important which the Commission ought to consider, I think we have to say it, and if we think that this is the state of things, it's our judgment; then we have to give that proper judgment to the staff and the Commission.

So I'm a little unhappy about staff's decision to go ahead with something which may turn out not to do them as much good as they would like.

MEMBER KRESS: Another comment is I'm very sympathetic with the gentleman who's talking about an active screen. I think that might be a solution that could --

CHAIRMAN WALLIS: Yeah, I would love to see that. I don't know why we haven't explored it. We're always in regulatory space trying to sort of regulate the problem away when it may well be that it's an engineering problem and if you look at it that way and figure out is it worth spending so much money to fix this and you can figure out how to show that it works, that's the way to do it.

MEMBER KRESS: And to fix his problem of he can't wait, I don't know if the staff could do something like separate out active screens from all this other stuff and make some sort of judgment on what needs to be done just to approve an active screen.

It would be very helpful to some of the utilities.

CHAIRMAN WALLIS: All of the guidance seems to be the industry has to go away and do a lot of analyses of lots and lots and lots and lots and lots of things. That doesn't offer to me sort of a view of the light at the end of the tunnel or some sort of an answer coming out.

But, anyway, I'd welcome your input between now and when we meet again, and some of us or maybe all of us probably would want to read some of this material we've heard about which might help to clarify some of the things we're uncertain about. It may actually make us much happier by the time or it may not.

MEMBER SIEBER: It may go the other way.

CHAIRMAN WALLIS: Because sometimes reading more makes things worse. I can't predict which way it will go.

If we take a lunch break until one, then we can start the next GSI discussions, and Vic Ransom will assume the chair. I will be very relieved to get out of it.

I very much appreciate all of your efforts to try to explain things to us in the last day and a half. Thank you all very much.

We will now take a break until one o'clock.

(Whereupon, at 12:19 p.m., the meeting was recessed for lunch, to reconvene at 1:00 p.m., the same day.)

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

(1:02 p.m.)

MEMBER RANSOM: This is the continuation of the Thermal Hydraulics Subcommittee meeting and the subject today is GSI 185 control of recriticality following small break LOCA and PWRs. There have been several meetings held on this in the past and I won't go into all the background of this general safety issue. As I understand it, the purpose of the meeting today is to discuss a draft New Reg that was prepared by RES and relative to the resolution of General Safety Issue 185, and generally for the committee to recommend whether the New Reg report should be issued. I might just briefly state what some of the concerns are. I think most of them have been transmitted to RES already but the overriding ones seem to be the report lacked a unified approach. It wasn't really apparent and the possibility of loop seal clearing was not really mentioned in the report.

The mixing model, the technical basis and validation didn't seem that convincing, I guess, and we'd like to hear more about that. Some of the assumptions and justification maybe need to be shown to be conservative and generally, I think the logic for resolution of 185, while it was in the report, I think we'd like to hear a consistent review of that. And there were numerous suggestions for the report improvement. I don't think we're going to go through those in the meeting today but they've been transmitted, I think in separate communications. So with that, why don't we proceed with the agenda which, Dr. Rosenthal, I think is scheduled to give an overview of calculation strategy and followed by Dave Diamond on the calculation of the neutronics part of the accident and then Professor diMarzio to discuss the mixing model, I would imagine and thermal hydraulic calculations. With that why don't we proceed.

DR. ROSENTHAL: My name is Jack Rosenthal. I'm the Branch Chief of the Safety Margins and Systems Analysis Branch in the Office of Research. I'm going to give an overview and then Dave Diamond will present the mathematics calculations, Professor diMarzo mixing and then Dave Bassette really will put it together. We're going to present methods and then results, but I want to spend a few minutes on this.

We're presenting the results to a Thermal Hydraulic Committee but I consider this a fuel damage issue. You know, will we damage the fuel? And we're using for fuel damage limits insights that we've received from experimental work that we've done as well as Japanese work, some French work and especially French work and so that the fuel damage limits that we're thinking of are really not only for fresh fuel but also for burnt fuel and would tend to be far below the current regulatory limits. So I think that there's enough experimental basis now to be reasonably well-founded.

Well, in order to calculate fuel damage limits you have to calculate the enthalpy deposition in the fuel. In order to do that you do reactor kinetics calculations and we can really see the fruition of the investment that we made in the ability to do 3-D space time kinetics calculations succinct from point kinetics calculations to do more realistic analyses which is a commission to be more realistic.

Of course, one can challenge how well have you don't those calculations. Between the comparisons of the 3-D kinetics, the point kinetics typically us versus B&W and then against the Bars Code of Russian Work which is totally independent in terms of microscopic cross sections, I think we could argue that the reactivity -- the kinetics calculations are well-founded.

MEMBER RANSOM: Jack, when you mention

fuel damage, are you talking any fuel damage or is there some amount of fuel damage that would be considered acceptable, I guess?

DR. ROSENTHAL: I'm going to loop around all the way to the end, but what we're seeing is the old number of 280 calories per gram is based on melting the fuel and you get a volumetric expansion of maybe 130 percent and then you would burst the clad. Then you've got to consider hot material going out into the water and then you've got to worry about are you going to have a fuel coolant interaction. Well, the way to take that off the plate is to give yourself some assurance that you're not going to expel hot fuel into the water in the first place.

And for brand new pristine fresh fuel, you can argue that the 180 calories per gram is a good number but for high burn-up fuel, a number more like of the order of 80, 100, 120 calories per gram is a good number and there what I'm talking about is you know, not as a rate guide safety limit which we may come back to you on a year from now, we've already written a research information letter on it, but rather for the purposes of this analysis that we can argue that you don't fail the clad, then that's the end of that -- that terminates the event, so that's the kind of numbers that we should worry about and we'll talk about the extent in a little while.

Okay, so I think that my fuel damage is based on experiment is on reasonably good ground. My reactor kinetics, there's been a fair number of comparisons and some publications came out, so I think the reactor kinetics is on good ground and I think that the investment that we made is worthwhile and we'll use that same model. This is coupled RELAP but that's the same COX code that's couple to tray so that's worthwhile. So then you have to ask, okay, how good are the boundary conditions to the reactor kinetics code do you have and that is how much diluted water can you move from someplace in the primary system into the core and that's the thermal hydraulics part of the assessment.

We're talking about a LOCA -- oh, and on the thermal hydraulics part, I think that we have done sufficiently conservative work that we can say that we're realistically conservative and, of course, that's the hurdle that we'll have to prove this afternoon. In terms of the recriticality analysis, for Westinghouse and combustion, we did pump restart calculations. The size of the piping and the loop seal is just plain smaller than on a B&W plant with, you know, the steam generated. So that we predict that you don't go recritical with a pessimistic slug and if you don't go recritical with a pessimistic slug, I think we've dismissed -- you know, that that concludes the issue for Westhinghouse and CE and what' nice about that is, I believe it's reasonably robust.

I can go up and I can pick the pipe and I say that pipe is five, 10 times smaller than the B&W pipe and see it and so that's robust and as I say, we're not going to go recritical. For Framatome, things are more difficult. The volume is largely -- the amount of slugs that you can put into the core is larger. Professor diMarzo will talk about the slug formation but at least for Westinghouse combustion, I think we have a robust case. Framatome we're more reliant on our understanding of our analysis.

CHAIRMAN WALLIS: And Framatome themselves had some analyses which showed large amounts of energy close to the new fuel.

DR. ROSENTHAL: Right, and they did that with coin kinetic cals and with 3-D space time kinetics you're going to get lower numbers.

CHAIRMAN WALLIS: But their numbers, what they submitted was actually not very good from the point of view of fuel damage, I understand, the numbers they came up with.

DR. ROSENTHAL: Right, not very good in the sense that you would damage fuel.

CHAIRMAN WALLIS: You would damage fuel, right, so why is it up to the staff to show that they didn't damage fuel if they already predicted that they did?

DR. ROSENTHAL: It's whatever the number is, let the truth prevail.

CHAIRMAN WALLIS: Yeah, but it seems to me you're doing to work for them.

MR. DUDLEY: Noel Dudley from the Office of Research. Even B&W identified the fact that they would have core damage, they put a criteria in their emergency response procedure where they would initiate natural cooling initially to mitigate that condition.

DR. ROSENTHAL: So now let me just expound on that a little bit. For Framatome, we predict that yes, you can go recritical and talking to my peers for example, that run a PKL, they're concerned about going recritical and they had stopped. What's different in this analysis is that we said, "Okay, if you do go recritical, what will the excursion be and what will the enthalpy of fuel be and can the fuel take it? So this is considerably different from what the Europeans are doing in the sense of we're taking it to next step, next step, next step.

Natural -- restart of natural circulation which you can't help. It's going to happen whenever the temperature is -- is much slower than if you restart the reactor coolant pump. And what we are predicting is that for restart of natural circulation that you would have an excursion and you're going to hear a lot more about it, but it would be sufficiently benign that you won't damage fuel. For restart of a reactor coolant pump faster, the most recent calculation -- I mean, the calculations we've done show that acceptable results but more severe. It's a faster transient. You'll get numbers later.

CHAIRMAN WALLIS: One would suspect it would be the other way. You can have a slug which never mixed and you slowly move it into the core, it takes more time in there. It would be worse transient because you don't pump it through quickly.

DR. ROSENTHAL: Well, David and I were thinking, you know, Doppler, you're thinking milliseconds, right? Fuel rods, you're thinking -- the newer rods are thinner, so but a more traditional number might be seven, eight seconds is a typical fuel time constant with newer rods, maybe six seconds for a time constant and then the mass transport that you're describing is much, much slower. There's plenty of time for the Doppler feedback to work.

CHAIRMAN WALLIS: With the pump or the circulation, it's still more time.

DR. ROSENTHAL: Well, surely more time with natural circulation.

CHAIRMAN WALLIS: Yes, much more time.

DR. ROSENTHAL: So you would expect a more benign result because there's more time for the feedbacks to take place. Okay, so and I'll speed up because I've gone over this somewhat. We did couple thermal hydraulic neutronics calculations and I'm proud that we invested in the tools because now we have the tools to actually deal with the issues. You'll hear from Dr. diMarzo in his mixing models and slug formation which I thought at a prior subcommittee meeting you were comfortable with.

And then Dave will discuss systems analysis and end results and okay. Conclusions, no recriticality procedure in Westinghouse. I would argue that that's a -- we should -- that ends it. B&W a problem but I think it's a low consequence event and so that regulatory action isn't needed. I'm sorry, let me go back. Okay, so now we've --

CHAIRMAN WALLIS: Has this problem been resolved in other countries?

DR. ROSENTHAL: No.

CHAIRMAN WALLIS: So it's still under study in Germany or places like that or France?

DR. ROSENTHAL: In Germany there's PKL Experiments, that's an OECD project contributed by several other countries. The focus of that work is on the fluid transport in PKL where, as I said, they haven't taken these additional steps.

CHAIRMAN WALLIS: Well, we went to see them several years ago. They had CFT calculations of the --

DR. ROSENTHAL: Of the fluid.

CHAIRMAN WALLIS: Yeah.

DR. ROSENTHAL: But not of the neutrons.

CHAIRMAN WALLIS: And they haven't resolved the problem yet?

DR. ROSENTHAL: It's still being studied and we are participants in the OECD project which focused on the -- as I say on the fluid dynamics and impacts. Okay, it focuses on the fluid dynamics.

In the -- what we're talking about is a reasonably low probability event. You have to have a small break LOCA. You have to terminate that LOCA.

You have to have a -- I'm sorry. You have a small break LOCA. You interrupt ECCS injection and then you regain that system and there's a window. In terms of criticality it's got to be reasonably early in the fuel cycle or the problem at end of cycle is no boron left. So it has to be early in the fuel cycle.

CHAIRMAN WALLIS: So was we go to power up rates for PWR --

DR. ROSENTHAL: The window would become bigger, worse.

CHAIRMAN WALLIS: -- you get more boration in the beginning to counteract the higher reactivity, there would be more time when --

DR. ROSENTHAL: The rate of fraction with cycle, yes.

MEMBER SIEBER: It depends on whether they use soluble boron --

DR. ROSENTHAL: Gadolinium or dysprosium or God knows what else.

MEMBER SIEBER: Right, and so you can't say for sure exactly what --

DR. ROSENTHAL: The window would be.

MEMBER SIEBER: Right.

MR. CARUSO: You only looked at the lower loop plants, not -- the raised loop B&W plants don't have that problem so -- this problem, right?

DR. ROSENTHAL: We only looked at the lower.

CHAIRMAN WALLIS: Because otherwise they're up there and the water drains out and --

MR. CARUSO: Well, they don't have as much volume.

CHAIRMAN WALLIS: So it's a small number of plants.

MR. CARUSO: Davis Bessie is a raised loop plant, right?

DR. ROSENTHAL: Yes.

MR. CARUSO: So this is an accident that cannot occur at Davis Bessie, maybe the only one that they can never experience in their lifetime.

DR. ROSENTHAL: Think in terms of cubic meters.

PARTICIPANT: I would say that's correct because Davis Bessie has a much smaller loop seal.

MR. CARUSO: You're sure there's no way they can figure out a way to -- they're very creative there.

MR. BASSETTE: They'll figure out a way, sure.

DR. ROSENTHAL: Okay, if you have the

excursion then you have control rods in the reactor. You're still at the reactivity excursion will -- if the maximum power occurs in a new fuel rod, then you have -- then you're probably more like the 280 calorie per gram limit. If the fuel pattern is such that the maximum power enthalpy deposition is in the -- is in a high burn-up rod with -- conceivably could be, I don't know what the rod patterns would be, some time in the future, then you ought to use the lower 80, 100, 120 calories per gram as a measure.

CHAIRMAN WALLIS: It doesn't matter what the maximum is. It matters about whether or not the new fuel has 280 and the old fuel has 80.

DR. ROSENTHAL: Right.

CHAIRMAN WALLIS: The new fuel would be hotter than the old fuel and the old fuel is still at risk. So it's not the hottest place that you worry about.

DR. ROSENTHAL: It's combined with -- right. That's the extent of it and also we have an extent of an axial limit, the extent axially of damage, so that it's not the entire core that we're talking about. We're talking about some -- and you'll hear more from Dave Diamond on a limited extent of the core radially and axially. The last thing is I'm talking about now an event in which I have ECCS by definition, okay. So if I damage fuel, which we don't think will happen, then it would occur in a system -- primary system integrity and ECCS as the consequences, we perceive, would be reasonably small.

Okay, the last thing is we think that the plant with the problem is the B&W lower loop plant where the operators turn on the reactor coolant pumps and that's the very situation in which operating plant procedures already exist. So when I put that together I can say that I believe that the consequences of the event would be reasonably small. Okay, so that's on the technical stuff.

Now, just a couple of the admin --

CHAIRMAN WALLIS: Have you look at from the beginning of life to end of life? Is the beginning the worse situation?

DR. ROSENTHAL: Right, Dave will --

CHAIRMAN WALLIS: It's better as you go on, I guess.

DR. ROSENTHAL: Right, I mean, in the limit, at end of cycle there's zero boron concentration.

CHAIRMAN WALLIS: Right, there's no boron.

DR. ROSENTHAL: So there's no issue. And

then somewhere through the cycle it's an issue.

CHAIRMAN WALLIS: Well, somewhere in the cycle the control rods control. You don't need the boron.

DR. ROSENTHAL: In PWR --

CHAIRMAN WALLIS: You really need the boron for about half the cycle or something, whatever it is.

DR. ROSENTHAL: In a PWR the rule of thumb is that half the reactivity is held down by the rods and about half by the solid boron --

CHAIRMAN WALLIS: At the beginning.

DR. ROSENTHAL: -- at the beginning of cycle. And you can -- even at the beginning of the cycle, you can to down to like three, 400 F on rods alone. Okay, now, I'm virtually done. So we make these presentations to the subcommittee and I think that we were reasonably persuasive technically but we had not written up a comprehensive story. Do Dave Bassette wrote the new one which we provided in draft form. Your comments are well-taken. Dave's attempted to address or fix comments, issues. Marino is prepared to speak. I mean, everybody is prepared to speak to the issue but it was -- thank you. I'm sorry.

CHAIRMAN WALLIS: Is a new draft available?

DR. ROSENTHAL: I did not produce a new draft. And the -- and the reason is that I didn't want to reset the clock on the process. I mean, we can provide you what we've written.

MEMBER SIEBER: Is it on the website?

DR. ROSENTHAL: Excuse me. It's not on the website yet. It's not published. Now, how you want to handle -- I mean, we can discuss how you want to handle Dave's rewrites of sections is fine. I didn't want to introduce new material because as soon as I introduce new technical material, then I think I owe the committee, you know, a full time span to review technical material for editorial clarifications or moving some of the material from the main body to appendix, et cetera.

CHAIRMAN WALLIS: Well, I think you could certainly -- whether it's new material or not, you could certainly clarify some of the assumptions about mixing and the verification of those assumptions by testing and perhaps a sensitivity to some of those assumptions in a clearer way than we saw before.

DR. ROSENTHAL: Right, and diMarzo is nodding his head up and down behind you that we think that we've done that, so I'm going to get out of the way so you can hear technical talk. But in terms of resolving the issue, I think we have a -- you know, as distinct from how well is this report written, that we've now amassed all the information in one place.

CHAIRMAN WALLIS: Is this going to the full committee in a week, two weeks?

PARTICIPANT: Yes.

CHAIRMAN WALLIS: Well, how can we do it if you don't have a final document?

DR. ROSENTHAL: Well, we've given you the report.

CHAIRMAN WALLIS: You've given us the new report?

DR. ROSENTHAL: No, we've given you a report --

CHAIRMAN WALLIS: Well, that's not good enough.

DR. ROSENTHAL: -- that contains all of the information. I'm sorry, I'm beginning to sound argumentative, so I apologize.

CHAIRMAN WALLIS: So you want us to make a decision based on --

DR. ROSENTHAL: On what we've given you.

CHAIRMAN WALLIS: -- an unmodified report.

DR. ROSENTHAL: Right.

CHAIRMAN WALLIS: Have you carte blanched it and modified it any way you like? We usually like to review the final thing, then we know what we've done.

MEMBER KRESS:: Except if he's right and all he's doing is making editorial changes.

CHAIRMAN WALLIS: If it's editorial changes, but if it's something significant --

DR. ROSENTHAL: No, then you're right and that's what I'm saying that we'd have to reset the clock.

CHAIRMAN WALLIS: It's just editorial changes.

MEMBER KRESS:: Reset what clock?

DR. ROSENTHAL: If I introduce -- in my own mind, if I introduced new technical materials which altered this -- the technical substance of the report, then I ought to resubmit that to you let a couple of months go by and meet again with you. If the technical material is as we said and we're just clarifying or moving text, et cetera, around, as an editorial exercise, but not for the purpose of changing the conclusions or the technical substance, then we can do that.

MEMBER RANSOM: Well, the one thing that seemed inadequate in the report was the discussion of the mixing model and what it was, how it was used, you know, in the calculations and without that being clarified, I don't see how you could pass that report because it is not convincing the way it is.

DR. ROSENTHAL: Let's -- why don't we proceed if that's okay, get some substance up here and then figure out what to do?

CHAIRMAN WALLIS: You mean, you gave us no substance, Jack.

DR. ROSENTHAL: Now, comes the heavy -- no, I consider that an overview, but I hope --

MEMBER KRESS:: Jack, that was one of the better overviews we've had.

CHAIRMAN WALLIS: Yes, I think it was.

MEMBER KRESS:: Now, are you a refined diamond or a diamond in the rough?

MR. DIAMOND: In the rough. Good afternoon, gentlemen. I'm David Diamond from Brookhaven National Laboratory and I'm going to explain to you how the analysis of the boron dilution transient was carried out using reactor analysis capability that has been developed by RES. Let me -- before I tell you exactly what I'm going to be doing, let me just acknowledge my co-workers at Brookhaven, the people at Purdue who work on -- the people at Purdue who developed the code and who provide support to us on an almost daily basis as we apply the code. People at Penn State, I want to acknowledge, they've provided the cross section data that we used in this study and of course, my colleagues at the NRC.

MEMBER SIEBER: The cross sections that were provided, are they different that the ENDFB?

MR. DIAMOND: They're based on the ENDFB.

MEMBER SIEBER: Okay, so what's special about them?

MR. DIAMOND: They're processed down to -- as a matter of fact I'll explain that a little bit this afternoon and hopefully, you'll be able to see how they different. ENDFB are a very fundamental set of data which has to be processed in the context of the reactor that you're using it for.

MEMBER SIEBER: That's right.

MR. DIAMOND: So you have to take into account the energy spectrum in the reactor and the spacial distribution within the reactor in order to reduce --

MEMBER SIEBER: That's right.

MR. DIAMOND: Okay, and I'll explain this in a little bit more detail momentarily.

MEMBER SIEBER: All right.

MR. DIAMOND: As a matter of fact, I'm going to spend very little time talking about the objectives of the study. Most of the time I will spend talking about the reactor analysis methodology which does involve generating cross sections. So please ask some more questions at that time. I'll spend a little bit of time talking about the results of the transient analysis that we did. I also want to say a little bit about the fuel cycle and the potential for boron dilution. You just started to bring that subject up as Jack was finishing his talk. So I'll say something about that. That's kind of independent of PARCS relap but I think it might be of interest to you and then I have some conclusions that I'd like to state and I guess I'm going to have news instead of bullets throughout. I hope I don't have any other surprises.

MEMBER SIEBER: You'll find out.

MR. DIAMOND: Yeah. So our objective, that is at Brookhaven, our objective was rather straightforward, to understand the consequences of a boron dilution event as defined in GSI 185 and as Jack just described. And what that means is to provide deterministic calculations of the peak fuel enthalpy or the --

CHAIRMAN WALLIS: Or -- go ahead.

MR. DIAMOND: -- or equivalently, the energy deposited into the fuel as a result of this power exertion that can potentially happen --

CHAIRMAN WALLIS: I don't understand enthalpy being the variable. You're putting energy in and it's not doing work. There's no flow. Why does PV appear in --

MR. DIAMOND: Okay, because generally, when people talk about fuel behavior, they characterize whether or not you're going to have damage according to what the increase in fuel enthalpy is in the pellet. This is for the type of transient in which you have a paracooling mismatch as a result of having a rapid power excursion. Obviously, if you had a paracooling mismatch caused by a decrease in coolant, and you had an increase in clad temperature as a result of that type of mismatch, then you characterize the fuel damage according to DNB or critical heat --

MEMBER KRESS:: I think you'll find out that the enthalpy is CPT in this case.

CHAIRMAN WALLIS: So it --

MEMBER KRESS:: So it doesn't --

CHAIRMAN WALLIS: It doesn't vary, it's just the same as energy, internal energy.

MEMBER KRESS:: Yeah.

MR. DIAMOND: Oh, it is. It is. That's what we're talking about energy deposition, right.

CHAIRMAN WALLIS: So we won't get into whether or not enthalpy is the right word. We know what you mean.

MEMBER KRESS:: Well, not maybe later if you're going to calculate that gets done by the expansion.

CHAIRMAN WALLIS: This is the kind of thing that you ask at a doctoral exam to find out if the fundamental thermodynamics are correct. I understand what you mean. I'm not going to quibble but I'm not sure why you use enthalpy as the variable. That's okay.

MEMBER SIEBER: It's traditional.

MR. DIAMOND: It's traditional and that's as good a reason as any.

MEMBER KRESS:: And there's no reason not to because it is just CPT.

CHAIRMAN WALLIS: See, you put the energy in.

MR. DIAMOND: Correct.

CHAIRMAN WALLIS: And it goes into energy of the fuel also pushing back the surroundings. Pushing back the surroundings is the PV part but that doesn't stay in the fuel. It's gone out, it's been gone, so it's really energy presumably but I'm not going to quibble about it. It's gone.

MR. DIAMOND: It could be Jules, it could be -- you know, it's energy. And as I say, because the fuel damage limits are given in terms of the increase in fuel enthalpy rather than Jules or something else, that's the parameter that we calculate and we define it as the average over the pellet.

CHAIRMAN WALLIS: Truly the temperature you care about.

MR. DIAMOND: It's the temperature. It's exactly the same thing that the parlance that we use is fuel enthalpy but you're absolutely right.

CHAIRMAN WALLIS: Enthalpy never hurt anybody but temperature did.

MEMBER RANSOM: How is it deposited in the fuel model itself, you know, which has an energy source term basically throughout the radius of the pellet and then conduction is -- there is a conduction model and out through the clad and so on.

MR. DIAMOND: Yes.

MEMBER RANSOM: You say this is the average.

MR. DIAMOND: It's the average enthalpy. The energy is deposited making an assumption about its radial distribution across the pellet.

MEMBER RANSOM: Is it uniform then?

MR. DIAMOND: Yes, the assumption that we make is that it is uniform. However, we have done parametric studies to look at the effect of having the energy deposition peaked towards the periphery of the pellet where -- as it is truly.

MEMBER SIEBER: That's just an artifact of the way you modeled it because you can actually model the pellet as --

MR. DIAMOND: No, we could model it and we have modeled it.

MEMBER SIEBER: Yes, right.

MR. DIAMOND: And as I say, this is --

MEMBER KRESS:: It probably doesn't matter much because when you develop the acceptance criteria that 80 through 100 some calories per gram, it's calculated this way and so you know, it washes out in the acceptance criteria.

MEMBER SIEBER: If that's the benchmark,

then you need to calculate based on the benchmark.

MEMBER KRESS:: You need to calculate -- the way you do it, do the calculation to get the actual damage state.

MR. DIAMOND: Right, and so you have to calculate the pellet radial average. Whether you assume that the energy deposition in the pellet is uniform or is peaked towards the edge is is something that we look at in terms of parametric studies that we do and it's not an important effect here.

CHAIRMAN WALLIS: Would this apply to MOX field?

MR. DIAMOND: It applies to any field.

CHAIRMAN WALLIS: MOX field which had plutonium not mixed in very well?

MEMBER KRESS:: There's some question about that but there's some research going on.

MEMBER SIEBER: Well, there's some self-shielding that goes on. You can model that but most people don't. They just look at the overall --

MR. DIAMOND: The limits on fuel enthalpy were of course, derived for --

MEMBER SIEBER: Uranium fuel.

MR. DIAMOND: -- yeah, for uranium fuel and for burned -- you know, it's supposed to be applicable to burned fuel but obviously, not to MOX.

CHAIRMAN WALLIS: The problem with MOX is if the plutonium isn't mixed thoroughly in, you get regions where it's different.

MR. DIAMOND: Sure, yeah. Okay, so this is what our objective is, to calculate the fuel enthalpy throughout the core and, of course, we do different parametric studies to determine the effect of assumptions, one of which I just mentioned, namely the way the energy is deposited within the pellet. Okay, flow rate is another example of a parameter that we've looked at in the past, and of course, we've looked at different parameters which describe the slug and also describe the reactor.

MR. TRAIFOROS: Let me ask you, David, did you bottom up for resolution of this issue or top to bottom? I mean the way I'm trying to say, have you looked at all the slugs for a duration and the size and concentration to see what kind of -- in the space of duration of the -- I mean the velocity of the flow rate and concentration and the size basically?

MR. DIAMOND: We've looked at a variety of cases sufficient in our minds to give us an idea that we understand what's going on given the boundary conditions.

MR. TRAIFOROS: So this statement that flow rate, lower flourate would be less severity to clad is true?

MR. DIAMOND: Yes, and --

MR. TRAIFOROS: Under all -- independent of concentration?

MR. DIAMOND: It's not totally independent of concentration but --

MR. TRAIFOROS: Do they have a kind of relationship --

MR. DIAMOND: Yes, but for the cases that I'm going to show here, the answer is yes, that the flow rate is key because as I will show you -- why don't I wait until I show them to you? Okay?

So let me continue by digressing a little bit because I'm going talk now about the methodology which has been developed by RES and just to give you an appreciation of the tools that we're using. And of course, that methodology is the coupling of RELAP 5 with PARCS and you're all intimately familiar with RELAP 5. I'm not going to say much about it but I will talk about PARCS which, of course, calculates the neutron kinetics and hence, the pellet distribution throughout the core as a function of time.

CHAIRMAN WALLIS: Well, RELAP lumps the

core into certain regions, does it?

MR. DIAMOND: Yes.

CHAIRMAN WALLIS: But PARCS looks at everything, individual channels?

MR. DIAMOND: Yes, and PARCS looks at everything as I will show you and RELAP models thermal hydraulic channels but it models multiple thermal hydraulic channels so that you can have a thermal hydraulic channel for each fuel assembly and that's how the --

CHAIRMAN WALLIS: So you're actually going down to that detail, your modeling each fuel assembly in RELAP?

MR. DIAMOND: Yes, and I'll show you that.

CHAIRMAN WALLIS: Okay, thank you.

MR. DIAMOND: I have a list here of capabilities in the code which I am not going to talk about. I'm not even going to read this list but I put it in your package there so you know that PARCS is even more sophisticated than what I'm going to describe to you. So these are not relevant to the study today but if at some other time, you wanted more information about those capabilities, we could talk further.

So what I have to do is I have to tell you a little about the theoretical models in PARCS, a little bit about the cross section data and other nuclear data which goes into PARCS because this is just as important as the theoretical models. And then, of course, the description of the reactor within PARCS is also equally important and I'm going to touch on all three of those things. So let me start with fundamentals. The equations that are solved in PARCS or the neutron balance equations for two neutron energy groups and those neutron balance equations are based on diffusion theory which has been found to be valid for oh so many light water reactor core analysis problems.

Diffusion approximation, which you're all familiar with, allows you to simplify the Boatsman equation down to the diffusion equation but for kinetics in addition to that neutron balance for the two neutron energy groups, you have to have the neutron precursor groups and there are six of those and so there are additional equations which couple to the neutron balance equations. And that allows you to solve for the flux in each of two energy groups, so G equal 1 and 2 here. And that's a flux that's based on Cartesian geometry so it's a function of X, Y and Z, and of course, it's a time dependent solution.

PARCS has the ability to look at full core, half core, different symmetries which makes it useful. And it has the ability to look at different boundary conditions at the outside of your solution space. But, of course, what's most important is that you pick the boundary condition which gives you the correct solution in the fueled part of the reactor because as I will show you, you actually model not only the fuel part of the reactor, but the reflector region adjacent to the reactor.

And then what it requires are homogenized assembly properties and I'm going to say a little bit more about --

MEMBER RANSOM: Are there any conservatisms in the analysis or should this be considered a realistic analysis?

MR. DIAMOND: It should be considered a realistic analysis based on the limitations of the model, yeah.

MEMBER SIEBER: So what you've prescribed so far is like a PDQ7.

MR. DIAMOND: Exactly.

MEMBER SIEBER: Kind of calculations.

MR. DIAMOND: Sure. So here is a plainer view of a core and you see that it's mapped into boxes. Each box is a fuel assembly except that the yellow boxes represent regions of the reflector. And there is a whole bunch of stuff in those reflector regions; steel, water, and the same is true at the top and the bottom. You're going to have reflectors. So each of these boxes is a fuel assembly and the fuel assembly consists of many fuel rods. What we have in the PARCS representation is a homogenization of these fuel rods, so you no longer have this hetrogenious structure but rather the structure has somehow been homogenized and I'll explain that.

MEMBER RANSOM: So you have like a single rod but it's multiplied by the number of rods that you have in the system in terms of energy?

MR. DIAMOND: No, it's more complex than that because the bundle is not necessarily a repeating array of signal rods. It depends on what the bundle looks like and you can imagine VWR bundles are, of course, more complex than PWR bundles but even a PWR bundle has different things in there. It may have uniform enrichment across here but over here there may be a control rod and in the center there may be a guide to, just for an instrumentation tube, so it's not a repeating array. It's a complex hetrogenious assembly. Nevertheless, it goes through a process which allows you to describe it using uniform properties.

MEMBER SIEBER: Do you have the burn-up gradient built into that homogenized cell?

MR. DIAMOND: Yes, that burn-up gradient will be represented at this level but when you do the homogenization, it disappears because the properties are uniform everywhere in this homogenized fuel assembly and that's what goes into the PARCS calculation.

MEMBER SIEBER: So you actually can't find the hot rod. You find that average assembly power which is an under-estimate of whether you're going to get damage or not.

MR. DIAMOND: Actually, there is a dehomogenization process that you can get to go through and that's represented over here. This dehomogenization process allows you to reconstruct the detail power pin by pin --

MEMBER SIEBER: So I'm your straight man, right?

MR. DIAMOND: Yes. But --

MEMBER SIEBER: Why do they go to that trouble? Why don't they just carry it through like the old fashioned color --

MR. DIAMOND: Oh, the old fashioned because there's a big savings here.

MEMBER SIEBER: You can do this on a PC I take it.

MR. DIAMOND: Yeah, instead of 17 by 17 is what 300 additional --

MEMBER SIEBER: Right.

MR. DIAMOND: -- pieces that you have to keep track of. It makes the computation difficult. One day it will be done that way, but we're not there yet.

MEMBER SIEBER: Okay.

MR. DIAMOND: And I'm talking about even for research tools, not just for production tools.

MEMBER SIEBER: Right. So there is -- how much accuracy do you lose through the homogenization/dehomogenization process because the reason why you do that is to run it on a PC.

MR. DIAMOND: Let's put it this way; you are not -- for the case of interest today, the boron dilution event, you're not sacrificing anything in here because we're not interested --

MEMBER SIEBER: It's all relative.

MR. DIAMOND: Yeah, we're not interested in such precision for this analysis that we need --

MEMBER SIEBER: You're actually looking for the delta in enthalpy as opposed to the absolute value.

MR. DIAMOND: Yeah.

MEMBER SIEBER: So you require less precision to find the delta than you do that absolute value, in my view because all these anomalies sort of cancel out.

MR. DIAMOND: Yeah. No, all I'm saying is though is that if the peaking factor across here is you know, plus or -- you know, it's 1.2 plus or minus .2, that's within the accuracy of this calculation, so I don't care.

MEMBER SIEBER: Okay, all right.

MEMBER RANSOM: David, what I was asking you earlier, I guess, how many rods are explicitly modeled in terms of conduction, energy generation, you know, in this homogenized model?

MR. DIAMOND: Okay, so there would be one in the RELAP model.

MEMBER RANSOM: One.

MR. DIAMOND: There would be one average rod modeled for this fuel assembly.

MEMBER RANSOM: Okay.

MR. DIAMOND: Okay, so now let me get into the cross section modeling, which, as I said, is quite important. And first, I just want to give you an idea of the complexity. When we say we need cross section sigma, well, what do we mean by that? Actually, for the two-group equations there are nine cross section types that need to be supplied to solve those neutron balance equations. They need to be supplied at each mesh at which the diffusion equation is being solved, each mesh or each what we call fuel composition. So you need cross sections which represent the -- are related to the absorption rate. They're related to neutron transport and that's equivalent to thinking in terms of a diffusion coefficient.

You need things that tell you something about the rate at which neutrons are produced, that's Nu fission, the rate at which energy is generated, that's Kappa fission. Kappa is the energy for fission and you need information about the scattering from group 1, the fast group, down to group 2, the thermal group. So sigma must be known for each mesh and hence, it depends on the fuel type in that mesh and it depends on the effect of burn-up. Now this is important because what you have initially is you have a fuel rod that's got U-235, it's got U-238, it's got some oxygen, but bam, you put it in there and after a day, you've got a dozen actinides in there, you've got 100 fission products and how do you keep track of that.

And the way in which we keep track of that is to characterize burn-up with very simple parameters. The most simple parameter is just the exposure in terms of gigawatt days per ton. That tells you how much energy has been generated, that tells you how many fissions there are, but that's not sufficient because if those -- if that energy has been generated in a neutron energy spectrum that is more towards thermal energies or more towards fast energies, that makes a difference. And so we introduce a second parameter to characterize the burn-up which has to do with the spectral history and for PWR that's convenient to choose that as moderator density history.

And that says, ah, the fuel is burned at the top of the core where the density is less than the fuel that was burned at the bottom of the core.

MEMBER RANSOM: David, I don't want to minimize this part of the analysis but I think this part was the part that was most understood, trusted and consequently there were not as many, I think doubts or questions about this, so you can probably go a little bit quicker and --

MR. DIAMOND: Okay, sure.

MEMBER RANSOM: -- unless somebody really has a concern with this.

MR. DIAMOND: Okay, I didn't want to get up and lecture, but, you know, it was felt that this might be of interest, so let me skip the --

MEMBER RANSOM: I think the particular aspects that we'd like to know about is how is this patched into the rest of the analysis and are there -- what are the concerns there.

MR. DIAMOND: Okay, and so even this might be a little bit peripheral so let me just continue.

MEMBER SIEBER: Well, the neutronics part is just one element of the whole problem.

MR. DIAMOND: Right, right. So okay, so let's pretend we've gotten a thorough understanding of this and I'll say a little bit more about the reactor model. I just wanted to show one graphic which would show you an application of this because it shows what PARCS is able to do and what I'm going to show you is a calculation of a steam line break and I'm going to show you the power as a function of radial position. So this is average radial power during a steam line break where first the primary circuit starts to cool

Then you get control rods entering.

You get ECCS entering. The reactor starts to shut down but it's still being cooled by the broken steam line, so the power starts to come up again. It's assumed that the cold water is coming in, in one quadrant and in this case it's assumed that there's a stuck rod in that quadrant. But this just can give you an idea of the result of a calculation.

And, of course, this is a visual that's nice for --

MEMBER RANSOM: Well, this is over time as the action progresses.

MR. DIAMOND: Yes, it's over time. It gives you general information. Obviously, the information that you really want out of the code is something more specific like you know, how many calories per gram in that -- well, maybe not in this case, but you know, what's the fuel temperature here or the moderator temperature or whatever.

CHAIRMAN WALLIS: What's the scale vertically?

MR. DIAMOND: This is relative power where one is nominal power.

MEMBER SIEBER: That's 100 percent power.

MR. DIAMOND: Yes.

MEMBER RANSOM: Can you show us one of those for the boron dilution accident?

MR. DIAMOND: No, I'm sorry, I don't have that. Jack eluded to the fact that PARCS has had a lot of validation. There's a list here of different benchmarks and these benchmarks have been done in the past. I will say that the validation process continues to this day with other benchmarks, both numerical and experimental.

Okay, let's get to the scenario of interest. What we want to talk about is what happens when you have either restart of natural circulation or an operator mistake when he restarts the pump. Slug flows into the core. That's a reactivity insertion and the question is, do we get fuel damage. So here's our lowered loop B&W design and for the calculations that we're going to do, we're not modeling this entire system. We're going to model what goes on here in the core. And so the RELAP model has a series of parallel, one-dimensional models. These are -- excuse me, thermal hydraulic channels, these parallel thermal hydraulic channels, each one of them represents an assembly.

And we also model the inlet plenum and the outlet plenum.

MEMBER RANSOM: How many parallel channels?

MR. DIAMOND: There are 29 in the core and one for a by-pass region and the reason that we can -- we do take advantage of the one-eighth symmetry in the reactor. That's the reason there are 29 rather than 177 thermal hydraulic channels.

MEMBER RANSOM: So you won't be -- this assumes that all the steam generators are behaving the same, I guess, right, in terms of boron?

MR. DIAMOND: No. One steam generator is initiating this event but we're assuming sufficient mixing so that the distribution across the --

MEMBER RANSOM: The core is uniform.

MR. DIAMOND: -- core inlet in uniform.

CHAIRMAN WALLIS: Isn't that a big assumption? If you have -- boron comes in one side of this, goes down a --

MR. DIAMOND: It is an assumption but --

CHAIRMAN WALLIS: It's supposed to mix uniformly right across the lower plenum?

MR. DIAMOND: Yeah, we're going to discuss this at length and I don't know whether Marino wants to say something now or later.

CHAIRMAN WALLIS: Well, the report seems

to just assume it happens.

MEMBER SIEBER: That it mixes.

MR. DUDLEY: This is Noah Dudley from the Office of Research. That is something that Professor diMarzo will go into in his presentation of the slug formation.

CHAIRMAN WALLIS: Did this happen first because you wanted to have an eighth of a core and diMarzo was asked to justify it?

MEMBER SIEBER: Well, I think a lot of the mixing has to do with whether it's force pumping or natural circulation.

CHAIRMAN WALLIS: No, but did you assume mixing because that was the easiest way to analyze the core or because it was really the realistic assumption because --

MR. DIAMOND: The realistic assumption.

CHAIRMAN WALLIS: -- if you didn't mix the lower plenum you'd have to analyze the whole core presumably.

MR. DIAMOND: No, actually in the parts model, we actually model 177 fuel assemblies.

CHAIRMAN WALLIS: So there would be no problem for you to do that.

MR. DIAMOND: So we actually do an

averaging of eight assemblies to get the input to the RELAP thermal hydraulic channel. So we could have -- albeit it would certainly make it more computational intensive but that was not our intent.

CHAIRMAN WALLIS: So diMarzo has evidence that if you put cool water on one of the hot legs, uniform blue water would get into the core everywhere?

PROF. diMARZO: No, what we basically did is that the input boundary condition that we supply for code is bounding all the evidence that we have from CFD calculations.

CHAIRMAN WALLIS: The worst case is to put the slug right across the whole core.

PROF. diMARZO: No, you'll see what we put. I'll show you in detail when I come up.

CHAIRMAN WALLIS: You did the bounding. And you show what happened if the slug only came in one-half of the core?

PROF. diMARZO: No, I'll show you where the slug goes and how much you got and then what we did --

CHAIRMAN WALLIS: You said bounding. I just wondered if you --

PROF. diMARZO: You'll see what I mean, because we have a distribution of concentrations and you'll see where the curve that we feed on this.

MEMBER RANSOM: Well, Dave showed that steam line break which clearly showed an isometric behavior in terms of the cold water coming into the core.

MEMBER SIEBER: No, no, no. That was because of the stuck rod.

MEMBER RANSOM: Was it assumed that way or --

MEMBER SIEBER: Well, there was an assumed stuck rod which gave you the peak in one quadrant.

MR. DIAMOND: Yeah, the idea of that calculation was to show as severe an event as possible in order to demonstrate the capability --

MEMBER RANSOM: Okay, the isometric energy deposition was due to the stuck rod, not the --

MR. DIAMOND: No, it was due to both in that case, but as I say, the idea there was to show the capability of PARCS, not to discuss what the mixing was in the lower plenum.

MEMBER SIEBER: But that was a natural circulation context, where you don't get mixing.

MR. DIAMOND: Okay, so if we can come back to the mixing, let me go forward.

CHAIRMAN WALLIS: So again, what did you

just say.

MEMBER SIEBER: Well, in natural circulation you don't get the degree of mixing that you do with pump circulation.

CHAIRMAN WALLIS: He's going to still assume mixing in his natural circulation scenario, isn't he?

MR. DIAMOND: Yes.

CHAIRMAN WALLIS: So you've just said what you think happens was not in the model?

MEMBER SIEBER: I think there is less efficient mixing under natural circulation conditions than there is under pumped conditions, just because of the turbulence.

CHAIRMAN WALLIS: I would think so, too.

MEMBER SIEBER: Yeah.

MR. DIAMOND: For whatever it's worth, we've looked at steam line break from Apex and you see uniform temperature distributions around the downcomer despite the fact that, you know, the generator is broken so that indicates quite a bit of mixing.

CHAIRMAN WALLIS: If you actually draw a picture of the lower plenum, it's a little bit hard for me to imagine what comes in one side --

MEMBER SIEBER: Gets to the other side.

CHAIRMAN WALLIS: -- gets to the other side and mixes uniformly. I would think when you first get that boron slug in one side, some of it is going to go into the core before any of it gets to the other side of the lower plenum.

PROF. diMARZO: I have something on that. I have a couple of slides on that.

CHAIRMAN WALLIS: Okay.

PROF. diMARZO: We'll address that in some detail in a few minutes. So let me go through what our model is. We modeled a B&W designed core, it happened to be TMI-1. And this is at the beginning of cycle, because as was discussed, that's the most important time in terms of severity of the event. We model -- actually each assembly is modeled as a two by two mesh, 28 axial meshes and the starting point for the boron dilution transient is that all banks are inserted, control is shut down. The fuel in the moderator at 25 K, 2500 PPM of boron is assumed to come in as a result of the ECCS having turned on during this event. So you're about 15 hours shut down.

Three percent flow is -- well, that's where we start our calculation and then we assume certain boundary conditions. We assume that the boron concentration in the lower plenum has a certain time dependence and that's based on the model which Professor diMARZO will talk about. And then we assume the flow rate based on either natural circulation or one-pump restart. So here is the core layout. Again, the 177 assemblies. In this case I show the presence of control banks and these banks are all inserted at the time of the dilution event because you've had the reactor trip. And because of the symmetry, we only have to model one-eighth of a core in the RELAP analysis and --

CHAIRMAN WALLIS: It looks different. Does it relate to the core?

MR. DIAMOND: Yes.

MEMBER RANSOM: I'm kind of wondering why you didn't go ahead and model the steam generated and put it on this and then you would have had the entire loop model at once.

MR. DIAMOND: Well, then we'd have to worry about the mixing ability in the analysis. You mean to analyze starting -- starting from the boron transient or starting from the whole small break LOCA?

MEMBER RANSOM: Well, both actually, but right, you would have to incorporate the mixing model, whatever that is and into this calculation.

MR. DIAMOND: Yeah, that would be a much more ambitious calculation.

CHAIRMAN WALLIS: So it's more than an eighth of a core because the middle one is shared by all of these segments. The middle one is unique.

MR. DIAMOND: The center of the core is right here.

CHAIRMAN WALLIS: Yeah, and you're actually modeling more than an eighth because those ones that -- if you take your laser and move it horizontally --

MR. DIAMOND: Well, yes.

CHAIRMAN WALLIS: -- the plain of symmetry goes halfway through that.

MR. DIAMOND: Okay, you have one-eighth core symmetry. You have one-eighth core symmetry.

CHAIRMAN WALLIS: That's true. Right, that's true.

MR. DIAMOND: In order to model one-eighth core symmetry --

MEMBER SIEBER: You've got to model more.

MR. DIAMOND: -- you have to model more than one-eighth times 177.

CHAIRMAN WALLIS: Thank you, that explains it.

MR. DIAMOND: Okay, because you model the center assembly even though it is -- you don't model one-eighth of the center core assembly. Okay, but again, this is just in the thermal hydraulics. The PARCS neutronics actually is calculating the solution in 177 fuel assembles. So this is -- it's convenient to think of this as our solution --

CHAIRMAN WALLIS: And PARCS gives you a symmetrical solution anyway so --

MEMBER SIEBER: Yeah.

MR. DIAMOND: Yes, it does and as a matter of fact, that's one of the things that you always look at to make sure that your PARCS neutronics is doing what it's supposed to because it damned well better be the same here as over there.

CHAIRMAN WALLIS: You hope there's no oscillation of this type.

MR. DIAMOND: Well, that's the case when you don't have a good code. That's the same way when you start up a reactor and you do a symmetric measurement because you want to make sure --

CHAIRMAN WALLIS: In this time scale you don't get any of those oscillations.

MR. DIAMOND: No, no. No, it's a stable type of calculation.

CHAIRMAN WALLIS: And, of course, the thermal hydraulics is stable.

MR. DIAMOND: Of course. The yellow assemblies here are where there are control rods present. I've just written down here the burn-up for these particular assemblies and I just marked in -- that's orangyish, brownish I don't know what color that is but there are no control rods here and as you can see, this is the -- these are the fresh fuel assemblies and this is where the peak power occurs when we do the transient and the peak power also occurs at the bottom of the reactor. And here is the result where the flow is three percent of nominal and if we look at the blue curve first, the blue curve is the boron concentration input into the calculation and you see it starts at 2500 ppm and goes all the way down to zero and then comes back up. We're talking about on the order of 100 seconds.

MEMBER RANSOM: Now, what is that input into the lower plenum?

MR. DIAMOND: Yes, uh-huh.

MEMBER RANSOM: And so level 5 is an idea mixing model so the lower plenum is going to be homogenous and the progress up the channel.

PROF. diMARZO: I'll address that in

detail, too.

MR. DIAMOND: Yeah, we'll talk about that a little bit more.

CHAIRMAN WALLIS: This is three percent flow.

MR. DIAMOND: This is three percent flow, so the time scale is quite long.

CHAIRMAN WALLIS: It is compared with the fuel but if you have 100 percent flow, this would presumably look quite different because things happen very quickly in a few seconds.

MR. DIAMOND: Yeah.

MEMBER SIEBER: Three seconds, two, three seconds.

MR. DIAMOND: Yeah, let me show that in a moment. The corresponding curve of reactivity versus time in dollars is shown here and it goes from being quite shut down to -- actually to being prompt critical at this point, about 35 seconds. And then it goes through a bunch of oscillations as you have the struggle between the dilution that's occurring and the feedback from fuel temperature, feedback, Doppler feedback and moderator density.

CHAIRMAN WALLIS: Does the --

MEMBER SIEBER: Is that what stops the

pump,

Doppler?

MR. DIAMOND: Yeah, what stops -- and --

MEMBER SIEBER: Well, there is a time constant associated with Doppler. It's short.

MR. DIAMOND: 10_{9}. It's pretty small, yeah.

CHAIRMAN WALLIS: Does the border boil?

MR. DIAMOND: Yes, uh-huh.

CHAIRMAN WALLIS: So you have to consider voids and all that sort of stuff.

MR. DIAMOND: Yes. Power here is --

MEMBER SIEBER: That's helpful.

CHAIRMAN WALLIS: Does that also shut down the reaction, the nuclear --

MR. DIAMOND: Yes.

MEMBER SIEBER: Yeah.

MR. DIAMOND: Yes. Power is in red and that initial reactivity spike where it goes prompt critical causes the power really to jump up. This is a logarithmic scale here. So you're going from quite shut down to above nominal power, 100 percent, and in a very short time, so this is -- you know, this is like a rod ejection accent or something that's really like --

CHAIRMAN WALLIS: This actually doesn't dump much fuel power, much enthalpy in this.

MR. DIAMOND: No, and then it goes through -- this is total power recognized and it goes through a series of oscillations to reflect that fact that, you know, you have this complex behavior in the core because of the boron dilution and the feedback effects. What I have in blue is the maximum fuel powered enthalpy in calories per gram. So this is a local quantity and you can see that the initial jump here is not very much. It's only about 20 calories per gram as a result of that real --

MEMBER SIEBER: Prompt.

MR. DIAMOND: -- that real hit.

CHAIRMAN WALLIS: It doesn't matter how rapidly it's put in.

MR. DIAMOND: Okay, if you're worried about how rapidly it's put in, this is your fuel enthalpy increase, but if you're interested in what the maximum is over time, you see that the maximum is about 90 calories per gram.

CHAIRMAN WALLIS: I worry about the rate it's put in if it's put in much faster than the sort of relaxation time for conduction in the fuel and so on. You're going to have to worry about peaks in the fuel.

MR. DIAMOND: Well, but this --

CHAIRMAN WALLIS: That's very slow.

MR. DIAMOND: This represents the fuel enthalpy in the fuel.

CHAIRMAN WALLIS: Average across the --

MR. DIAMOND: Across the pellet.

CHAIRMAN WALLIS: Because if it were a very, very rapid transient, you'd bits hotter than others.

MR. DIAMOND: Yeah, uh-huh.

MEMBER SIEBER: and that's the interval of the power.

MR. DIAMOND: That's right, that's right, but it's only about 90 calories per gram in this particular case.

CHAIRMAN WALLIS: No, it's not just interval of the power because you had some cooling. Otherwise you would continue to go on.

MR. DIAMOND: Yes, uh-huh, I'm sorry, of course. The conduction is important here.

CHAIRMAN WALLIS: Otherwise it would go up through this whole transient.

MR. DIAMOND: Absolutely.

MR. CARUSO: Is that the enthalpy for the peak pellet over time or is that the composite maximum enthalpy of any pellet in the core? Did you follow one pellet through time?

MR. DIAMOND: No, no, no, no, we followed the maximum.

CHAIRMAN WALLIS: This is the worst pellet.

MR. CARUSO: So it changes -- the location changes at different times then.

MEMBER SIEBER: Yes.

MR. DIAMOND: Correct.

MR. CARUSO: That's what I was wondering.

MR. DIAMOND: The reality is that it's at the bottom of the core and generally in those high burn-up -- excuse me, low burn-up fuel assemblies.

CHAIRMAN WALLIS: So this is calories per gram in one of the fresh fuels.

MEMBER SIEBER: Right.

MR. DIAMOND: Yes.

CHAIRMAN WALLIS: What is the worst in terms of the oldest fuel. Presumably, that's -- you also worry about that. I mean, you couldn't -- 80 calories per gram in an old fuel is much worse than 90 calories per gram in a new fuel.

MEMBER SIEBER: It's not as reactive.

MR. DIAMOND: Right, and --

CHAIRMAN WALLIS: Yeah, but I'd like to see what it is, though.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: Did you check every fuel assembly for --

MR. DIAMOND: No, if we were looking at a criterion which was a function of burn-up then what you're saying would certainly be applicable. In this particular case, the burned fuel -- I have to look back at that diagram.

CHAIRMAN WALLIS: The criterion is not a function of --

MR. CARUSO: So you'd have really high burn-ups there.

MR. DIAMOND: Yeah, I think that all of the -- in this reactor all the high burn-ups have control rods in them, and therefore, they will be at the lower power level.

MR. BASSETTE: In this particular case these enthalpy increases are below the thresholds of -- or high thresholds.

CHAIRMAN WALLIS: But you've got some on the edge at 48. You don't have the control rods in.

MR. DIAMOND: Yeah, that's correct, but of course that's on the edge, so the power is a little bit lower because it's on the edge but you're absolutely right, in order to -- if your criterion was a function of burn-up, you would have to --

CHAIRMAN WALLIS: So these guy's criterion is not a function of burn-up?

MR. DIAMOND: The present criterion is not a function of burn-up.

CHAIRMAN WALLIS: Although Jack Rosenthal told us that the damage is a function of burn-up.

MR. DIAMOND: Yeah.

MR. BASSETTE: The proposed limits or threshold required in failure has some functions of burn-up in it and it goes down with burn-up but these -- the enthalpy increases you see here are below the threshold for high burn-up fuel.

MR. DIAMOND: So in this particular case it's safe to say that we don't expect --

MR. BASSETTE: Even though these are for low burn-up assemblies, even if it was for a high burn-up assembly.

CHAIRMAN WALLIS: So your criteria is how many calories per gram to make it acceptable?

MR. BASSETTE: It's for -- well, I'll go into that in my presentation.

CHAIRMAN WALLIS: Well, is it about --

MR. DIAMOND: Let's put it this way, it's more than 77.

MR. BASSETTE: For hybrid or pure it increases about 80 calories per gram.

CHAIRMAN WALLIS: But what we saw in your figure was more like 90.

MR. DIAMOND: Yeah, I apologize, this is the increase in fuel enthalpy and the new criteria that people talk about now relate to the increase in fuel enthalpy rather than the absolute fuel enthalpy so you have to subtract off 17 calories per gram from your old thinking to get to the new think.

MEMBER SIEBER: Now, this number is probably subject to change as people continue to consider experimental data, right? That's not some firm -- that number there is sort of new, within the last two years.

MR. DIAMOND: Which number?

MEMBER SIEBER: Seventy-seven.

MR. DIAMOND: This is what we calculated.

MEMBER SIEBER: Oh, okay, I mean, the limit, the limit.

MR. DIAMOND: The new limit that's been proposed is, of course -- is a function of oxide thickness and so it's -- there isn't a one to one correlation to burn-up but it's in this range for very high.

MR. DUDLEY: This is Noah Dudley again. At this point, the NRC limits that you are familiar with are the same limits that are required of operating plants. The new limits that we're discussing come out -- are they in New Regs, the Reg guides?

MR. DIAMOND: Right now, they're in the form of a research information letter.

MR. DUDLEY: So the new limits we're talking about are simply in a discussion stage here and do not represent the requirements that the NRC is placing on licensees.

MEMBER SIEBER: Yeah, but if you were to place those requirements on licensees now, that would have an impact on the Appendix K calculations?

MR. BASSETTE: No. Not on Appendix K. These are reactivity insertion accident type limits.

MEMBER SIEBER: Okay.

MR. DIAMOND: Okay, so let me move onto a case --

CHAIRMAN WALLIS: So how much of the slug has got into the core by the time you've reached this peak? It's only a piece of the slug, isn't it?

MR. DIAMOND: Yes, because -- let's see.

CHAIRMAN WALLIS: So you don't need as big a slug as they have in order to cause this to happen because only a piece of it's gone in by the time you got to 45 seconds or something.

MR. DIAMOND: Yeah.

CHAIRMAN WALLIS: So the fact that it's a longer slug doesn't apparently make any difference as long as it's a certain size because the peak is early in this transient.

MR. DUDLEY: Can you go back a slide that has the boron concentration reactivity?

MR. DIAMOND: Sure.

CHAIRMAN WALLIS: Yeah, there, you see, where the slug is continuing to come on, decreases itself down to 80, but the peak is at 45 in terms of cals per gram.

MR. DIAMOND: That's right, yeah. And again, you have -- this now is a long enough time frame where you do have conduction out of the fuel element.

CHAIRMAN WALLIS: But this indicates to me that if we had a slug that was half the length, it would be just about as effective because you don't have to wait for 80 seconds. You just get that 40-second slug. It does the job.

MR. DIAMOND: Yeah. Let me show --

MEMBER RANSOM: What you're saying is you can get it with a smaller slug, but it doesn't make it any worse.

CHAIRMAN WALLIS: It doesn't make it any worse, but I'm just saying you could get it with a smaller slug. So you could back off a bit on the amount of water that's stored in the steam generator and so on.

MR. DIAMOND: To a certain degree, but when you get down to an order of magnitude of a smaller slug --

CHAIRMAN WALLIS: Then it doesn't work, right.

MR. DIAMOND: -- it makes a difference. And here's a case now where the flow rate is 25 percent of nominal and here is the -- the blue again is the boron over here, boron concentration and in this case the minimum value is only about 400 ppm of boron because there's more mixing as Professor diMARZO will explain.

CHAIRMAN WALLIS: Because you've turned the pump on, you've stirred things up at least in the pump.

MR. DIAMOND: Yeah. Well, it's more complicated than that, but -- and again, you have a similar reactivity signature in that it starts out and starts to climb up to prompt critical and then you get this balancing of feedback versus the forcing function and the result in terms of power, the red curve, is quite different because now you get a spike that goes up to about 2700 where --

CHAIRMAN WALLIS: Twenty-seven times nominal power?

MR. DIAMOND: Twenty-seven times nominal power, yes. Okay.

MEMBER SIEBER: For a short period of time.

MR. DIAMOND: Yes, for a very short period of time.

MR. CARUSO: Is that interval core power or is that the maximum in the --

MR. DIAMOND: Interval, this is interval, this is total.

CHAIRMAN WALLIS: Now, I've really noticed that when I read this, maybe this is naive but 27 times nominal power even for a short time, sounds pretty exciting.

MR. DIAMOND: Well, in term of calories per gram, that first rise is about 25.

CHAIRMAN WALLIS: Yeah, but it means you've got to shut it off pretty darn quick. You've got to shut that off pretty darn quick when you have the 27 times the --

MR. DIAMOND: Well, yeah.

MEMBER RANSOM: Doppler feedback is what shuts it down?

MR. DIAMOND: Doppler feedback, yeah.

MR. CARUSO: Did you have anyone look at the power behavior with those sort of peaks.

MR. DIAMOND: With those parameters.

MR. CARUSO: You're just assuming an average value.

MR. DIAMOND: That's right.

MEMBER SIEBER: The increase in stored energy is not very much, so that when --

MR. CARUSO: -- really high peaking factors inside the pellet and --

MEMBER SIEBER: Well, it won't have the same profile that it would under steady state conditions I would think.

MR. DIAMOND: But this is the bottom line here in terms of the peak pellet.

MR. CARUSO: I mean, in order to get the Doppler to turn it around that fast, it must be getting awfully hot very quickly, right?

PROF. diMARZO: Yeah, but don't -- locally, this is the maximum that you're seeing. This is core-wide so there's -- you know --

MR. CARUSO: Is that a real number, is it real?

MR. DIAMOND: Yes, that is a real number, yeah.

MEMBER RANSOM: Well, you presumably can show the clad temperature for these situations. Has it changed appreciably?

MR. DIAMOND: I don't think the clad temperature would change appreciably here. It would certainly change out here.

MEMBER SIEBER: At around 12 seconds.

MR. DIAMOND: Yes.

CHAIRMAN WALLIS: How far has the slug gone in, in that time when you get the peak? Not very far presumably, because it's such a short -- well, I guess the slug has gone in but the concentration which is enough to do anything hasn't been achieved. It seems to me, you're already at the flat part here, are you?

MR. DIAMOND: Yeah.

CHAIRMAN WALLIS: And yet it's enough to make this -- because you've got it in far enough.

MR. DIAMOND: That's right, you have to take into account the dynamics here. This is coming in much faster, so at this point here, you've pushed a lot more than you have when it's only three percent.

CHAIRMAN WALLIS: When you've got it how far up the core does it suddenly go critical?

MR. DIAMOND: Well, I mean, the entire core goes critical. It peaks at the bottom.

CHAIRMAN WALLIS: But the slug has come in maybe a core away or something up the core?

MR. DIAMOND: You mean where is the front?

CHAIRMAN WALLIS: Yeah, where is the front.

MR. DIAMOND: At this point in time --

MEMBER SIEBER: It's seven seconds.

MR. DIAMOND: Seven seconds in.

CHAIRMAN WALLIS: It's gone quite a long way, hasn't it?

MR. DIAMOND: I forget now how many --

MR. BASSETTE: The water is going about three feet a second.

CHAIRMAN WALLIS: So it's gone 20 feet.

It's gone all the way through the core? It's gone through the core?

MR. DIAMOND: Yeah.

CHAIRMAN WALLIS: That's why --

MR. CARUSO: That's nominal.

MR. BASSETTE: No, three feet a second is at 25 percent flow.

MR. CARUSO: Oh, okay.

CHAIRMAN WALLIS: So it's gone through the core.

MR. CARUSO: I know what my concern is. Go back to the other one. Doppler is a function of temperature. Enthalpy is also a function of temperature, right, or it's --

MR. DIAMOND: Enthalpy is temperature.

MR. CARUSO: Enthalpy is temperature. So why isn't the -- I guess what's disconcerting me is the enthalpy doesn't follow the temperature to my mind here because it seems like the --

CHAIRMAN WALLIS: It's the interval.

MR. CARUSO: -- turnover in Doppler because it's reached a high temperature which means high enthalpy. Is there something I've -- maybe I don't understand.

MR. DIAMOND: Well, how high is high

enough, I guess.

MEMBER RANSOM: David, explain, in neutronics, you have vibrations is what effects the -- neutronic vibrations effects the Doppler and that's not necessarily sensible heat. I believe that's --

MR. CARUSO: Okay, that's what I'm asking.

MEMBER RANSOM: So the temperature is going up but the Doppler is not sensible heat. It doesn't have to get up to --

MR. DIAMOND: No, it is sensible heat.

MEMBER RANSOM: Well, that's true because it has the --

MR. DIAMOND: It is sensible heat.

CHAIRMAN WALLIS: It's the kinetic energy.

MR. DIAMOND: It's the kinetic energy.

MEMBER RANSOM: But you don't have to get up to 1,000 degrees -- a delta of 1,000 degrees aft to get it to turn over.

MR. DIAMOND: Yeah, I think that's the bottom line, is how much temperature rise do you need throughout the core in order to get it to come back. Doppler has a very strong effect.

CHAIRMAN WALLIS: Doppler is the temperature of the neutrons essentially.

MR. BASSETTE: Doppler is the temperature of the U-02.

MR. DIAMOND: It's the temperature of the fuel, primarily U-238 actually.

CHAIRMAN WALLIS: Well, Ralph's point is it's hardly gone up.

MR. DIAMOND: But it has gone up sufficient to cause the feedback.

MR. BASSETTE: It's probably gone up from 300 c to 1,000 c or something.

CHAIRMAN WALLIS: It's vibration of the fuel molecules that does this.

MR. DIAMOND: Yes, uh-huh, and that's instantaneous just about.

MEMBER RANSOM: The fact that the enthalpy went up to a lower level, at the same time the reactivity went up even higher, you know, 27 times as high, it still shut down.

MR. DIAMOND: Well, because what happens is, I mean, the higher this goes, the narrow the pulse. So you have to -- you're not looking at the same pulse here. It looks like the same pulse but this time scale, you know, we're talking about milliseconds here and we're talking about you know, a difference of, I'm going to guess, you know, a difference of 50 milliseconds between one pulse or another could be the difference between half the energy. So the time scale that we're talking about, you know, if you -- if the pulse width drops by so many milliseconds, that's a major difference in --

CHAIRMAN WALLIS: Isn't it so short, the one end of the core neutronically doesn't know what the other end is doing. I mean, the neutrons don't actually effect the top because it takes awhile for the neutrons on the bottom to have some effect on the top of the core.

MR. CARUSO: Well, I'm looking at the other -- I also look at the other spikes there and the other power spikes and the enthalpy rises that are associated with them, I guess they're so broad so that's the important thing. So why don't they turn around the power -- there's more to it than just Doppler turning it around.

MR. DIAMOND: No, there's the density feedback as well.

MR. CARUSO: Okay.

MR. DIAMOND: But what happens is that the fuel enthalpy now goes up to quite large values. Now we're talking about getting into a range where you're certainly going to have central line melting. To what extent that melting progresses, you know, we really don't know but the bottom line is we're up in a range here where we can assume or we should assume fuel damage as a result of the energy deposition into the pellet. This is what's bothersome here.

CHAIRMAN WALLIS: Can you show us something about the temperature and flux profiles axially in this thing? Is that --

MR. DIAMOND: I don't have those with me, but --

CHAIRMAN WALLIS: I mean, is there a big variation axially?

MR. DIAMOND: There is a variation axially.

MEMBER SIEBER: It's probably skewed.

MR. DIAMOND: Because -- I mean, the power starts to grow first in the bottom of the core. There also is another quirk with this reactor in that the control rods in this reactor don't go down to the end of the fuel region. There's another little quirk in this reactor but so yeah, certainly you're going to have a bottom -- you know, I'm just drawing a curve here, but --

CHAIRMAN WALLIS: So this max enthalpy occurs somewhere near the bottom of the core?

MR. DIAMOND: Yeah, uh-huh.

MR. SCOTT: Wasn't that extremely short, David. I mean, it's only a few --

MR. DIAMOND: Yeah, it's at the bottom of the core.

MR. BASSETTE: I think the peaking actor is about 10 in this calculation, wasn't it, if I remember?

MR. DIAMOND: I honestly don't remember. You're probably right. I'd have to go look at the numbers. Yeah, I'm sorry, I just don't remember that number.

MR. BASSETTE: I guess the whole reason for presenting this is to say with 25 percent flow, we would expect fuel damage.

MR. DIAMOND: Exactly. This increase in fuel enthalpy at the peak value is bothersome. The -- you know, appropo of our conversation about that peaking, you'll notice that the peak reactivity here is 1.44. Before it was like 1.14 calories and the increase here is only up to about 33 calories per gram. This is for the initial increase in enthalpy. And again, I want to point out that you just can't look at the top of that power spike. You've got to look at the width of the spike as well.

MEMBER SIEBER: Right.

MR. TRAIFOROS: You aren't very sensitive to velocity. Has any calculation been done on borated water going in to see what is the threshold on velocity that less than that you don't have any problem?

MR. DIAMOND: No, we never did that.

MR. TRAIFOROS: I would think that calculation would be useful, you know, what kind of velocity goes to the core that we don't have to worry about the slug.

PROF. diMARZO: Can I interject a thought here? If you have a maximum circulation, you have a maximum velocity which is what he's done. And then --

MR. TRAIFOROS: No, I want minimum velocity, not maximum velocity. If you have unborated water because another parameter here is concentration of boron, so if I want -- if I wanted to -- I mean, we'd better understand it if we know the lower velocity. I'm asking if your natural circulation fluid is over-estimated whether -- how much of low velocity -- if you have very low velocity no matter what concentration we have, we don't have a problem.

PROF. diMARZO: Right, I mean, if you have seen three percent velocity, you could go like to one percent velocity or even less, it would be even more mild.

MR. TRAIFOROS: No, if you bring the concentration down. The reason if you -- you see this effect is that you put a time constant to mixing which is a Row V of lower plenum divided by flow rate. Now by decreasing the flow at the same time you are increasing your time constant of the dilution, so you have two effects. By lowering the flow you have less concentration of boron going to the core.

PROF. diMARZO: We went to zero down in the concentration. How can it be lower than zero? We're going from 2500 all the way down to zero, so we cannot have less than zero.

MR. TRAIFOROS: No, I'm talking about concentration. You are bringing -- you are not -- you are mixing whatever slug you have with the lower plenum, so you are not going to have to go to zero then.

CHAIRMAN WALLIS: Well, there must be some velocity because it doesn't mix very well, because it's going into --

MR. TRAIFOROS: That's exactly my point, at a certain velocity you may not have --

PROF. diMARZO: Right, yes, but if you want to --

MEMBER SIEBER: You're saying a sharp interface.

PROF. diMARZO: If we're not going to zero, you want a comment on that because you gave me a comment on that situation. If we never reach those low concentration, what happens then?

MR. DIAMOND: Well, I mean, I was going to say something about this momentarily but the consequences of the event do depend on the minimum boron concentration.

MR. TRAIFOROS: Exactly, my concern is suppose if we don't have that much mixing on low velocity, what kind of minimum concentration you have that you don't -- minimum velocity?

MR. DIAMOND: But we saw that with -- okay, we don't have a minimum but we spanned --

MR. TRAIFOROS: You spanned between 72 kilogram per second to -- I mean, three percent to 25 percent but initiation of natural collision may be lower and you may not have enough mixing. Not that I am saying there's not enough mixing but your assumption that there is no mixing in the downcomer, no mixing in the ECCS. Now, if you don't mix all of it and it comes from downcomer, the flow is low, it doesn't penetrate all the way to lower plenum, it comes to the core. That may be lower velocity but my question was kind of interesting to know what kind of -- at the same time, the argument is that lower velocity you don't have that much severity. But what is the relationship between the concentration velocity here, kind of -- suppose you have diluted completed unborated water. Is there any velocity that less than that you don't see any problem?

MR. DIAMOND: Well, we didn't do those analysis.

CHAIRMAN WALLIS: Remind me, the non-boron or the pure water --

MR. TRAIFOROS: Yes, pure water.

CHAIRMAN WALLIS: -- it's cold, is denser.

MR. TRAIFOROS: Not necessarily because --

CHAIRMAN WALLIS: It doesn't have boron, because it's colder. I think that's a bigger effect than the boron.

MR. TRAIFOROS: Yeah, but not necessarily because when you have condensate, the condensate are on saturation temperature. If you don't assume any mixing, a lot -- we are doing repeat phase of the small --

CHAIRMAN WALLIS: But since it --

MR. TRAIFOROS: -- we are doing a lot of

injection has come to lower plenum. Now lower plenum and downcomer is colder than the slug.

CHAIRMAN WALLIS: Because of the injection flow.

MR. TRAIFOROS: Because of injection. That has been actually observed in PKL experiments.

MEMBER RANSOM: You're saying the condensate is hotter than the downcomer.

MR. TRAIFOROS: Exactly.

MEMBER RANSOM: So it would tend to stay --

CHAIRMAN WALLIS: Without mixing at all.

MR. TRAIFOROS: If you are consistent in your assumptions. If you don't assume any mixing in the core and downcomer, then the exact temperature -- we follow the temperature, that would be hotter.

MEMBER RANSOM: I'm wondering, do they have any calculations of the accident to show what these temperatures are like or conditions where you could get that?

MR. DIAMOND: B&W did some calculations, RELAP calculations during the event to show what kind of temperatures and pressures would be expected and of course, the entire primary is at lower temperature and pressure due to the assumptions of the small break.

But whether there are significant differences between the temperature in the vessel versus the temperature in the cold leg at that point in time, I don't know.

MEMBER RANSOM: I'm curious why you're saying PKL experiment actually saw this?

MR. TRAIFOROS: PKL has done -- what they're doing --

MEMBER RANSOM: What they're doing --

MR. TRAIFOROS: -- a lot of combination of equipment. I mean, they had one loop break and they -- on some tests they see this hot coming in and --

MEMBER RANSOM: Well, you're fairly boiling in the core.

MR. TRAIFOROS: Not -- I'm looking at condensing in the steam generator is usually is during refuel doesn't see that much of the injection. The injection goes to downcomer and lower plenum so that's cooler. When you restart -- re-establish natural circulation, so this would be hotter coming to the core.

MEMBER RANSOM: So it's mainly the downcomer temperature versus the condensate temperature that you have to worry about.

MR. TRAIFOROS: Yes.

CHAIRMAN WALLIS: The injection is still

going on while this is taking place?

MR. TRAIFOROS: If you don't have injection, you don't have refuel to restart natural circulation. The level has come down.

CHAIRMAN WALLIS: The injection mixes with this boron.

MR. DIAMOND: That's right.

MR. TRAIFOROS: For Westinghouse, yes. It was the back-flow and --

CHAIRMAN WALLIS: -- really mitigate the whole thing.

MR. DIAMOND: Well, that's right. We didn't take credit for that --

MR. TRAIFOROS: But again --

CHAIRMAN WALLIS: Well, how does B&W inject?

PROF. diMARZO: B&W injects into the cold leg at a high velocity.

CHAIRMAN WALLIS: So this will help to stir everything up and mix up with the --

MR. TRAIFOROS: But B&W injection is on the slope side of the cold leg, so all the injection goes toward downcomer until you really bring the level up.

MR. DIAMOND: Yeah, but for natural

circulation --

MR. TRAIFOROS: So you don't see that much.

MR. DIAMOND: -- the HPI would mix with any flow from the loop seal.

CHAIRMAN WALLIS: I would think that would prevent any --

MEMBER RANSOM: Let me point out that PKL also has a lot -- I mean, doing the experiments, they have a lot of -- they have a hard time forming -- and then moving it. From a physics standpoint, the worse thing I could do is put in a lot of cold water, so you're talking about anything that's warmer it's more benign.

CHAIRMAN WALLIS: But don't you have to shut off the injection and start the pump to get the worst case?

MR. DIAMOND: That's right.

CHAIRMAN WALLIS: So they have to do two things. They have to shut off the inject and stop the pump to get the worst case?

MR. DIAMOND: Yeah, in fact, PKL, they couldn't maintain a diluted loop seal when they turned HPI on. The loop seal disappeared, the dilution disappeared.

CHAIRMAN WALLIS: It helped pump the slug in to the core, but mix it on the way.

MR. TRAIFOROS: Yeah, we look at the PKL experience and to items came out of that.

CHAIRMAN WALLIS: I guess we're getting onto your part.

MR. DIAMOND: I have one other subject that I was going to discuss but I don't know in terms of time whether it's appropriate that I start or not and that's how the reactivity balance impacts this event and why this means that the event is only of interest in the first portion of the fuel cycle. Do I have time to, Jack?

DR. ROSENTHAL: If the staff it -- I'm sorry, if the ACRS is happy with that, we don't have to discuss it but it's their choice. Dr. Ransom, it's your choice.

MEMBER RANSOM: Go ahead, it looks like 10 minutes.

MR. DIAMOND: I'm going to go to conclusions then. I'd be happy to come back and give a lecture on that some other time when you have more time.

MEMBER RANSOM: All right.

MR. DIAMOND: The conclusions are firstly that RELAP via PARCS is a viable method of analysis for this particular problem. Secondly, fuel enthalpy increase only significant if the volume of the diluted water is large enough and that's based not just on what I showed today but on the fact that we did calculations of these events assuming the volume you would expect in a Westinghouse and combustion engineering plant. And also the fuel enthalpy increase is only significant if the rate of injection is large enough.

CHAIRMAN WALLIS: Now, wait a minute. Suppose that you took a huge volume of diluted water and injected it slowly forever, would there never be a problem?

MR. DIAMOND: Well, no.

CHAIRMAN WALLIS: There must eventually be a problem if you're injecting pure water. It just cures itself?

MR. DIAMOND: Wait a minute, if you totally -- yes, I mean, if you completed eliminated all the boron in your plant, you would have a problem. That's why you need soluble boron.

CHAIRMAN WALLIS: That's what --

MEMBER SIEBER: But you wouldn't get the prompt jump.

CHAIRMAN WALLIS: Taking yours to the limit here, if you injected slowly for a long time, you say the rate of injection is not large enough, so there's no problem.

MR. DIAMOND: Okay, let me put a proviso on --

CHAIRMAN WALLIS: But you can't go on forever --

MR. DIAMOND: Let me put a proviso here.

CHAIRMAN WALLIS: -- water in the core.

MEMBER SIEBER: You'd go to power.

MR. DIAMOND: For -- for the range of volumes that we're interested in.

DR. ROSENTHAL: You slowly dilute, you slowly warm up, you slowly turn on the negative moderator temperature coefficient, you slowly turn on Doppler, and you end up at some power, I think it's hot full power maybe in which it all balances out.

CHAIRMAN WALLIS: I think these two variables are not independent. If you have enough water and then you can inject slowly and you still get the effect, because if you had a huge reservoir of diluted water and just inject it slowly, eventually, you'd run into trouble.

PROF. diMARZO: No, but you go to power.

You don't get into trouble.

MR. DIAMOND: I mean, eventually, if you wipe out all of the boron in the vessel, you will have a problem because --

MEMBER SIEBER: Well, your reactor will heat up to around 400, 425 degrees and level off there.

CHAIRMAN WALLIS: So you don't need this excess reactivity control from the boron at all?

MEMBER SIEBER: Well, it will climb to an equilibrium temperature where the reactor is just critical and that's something below normal TF.

MR. DIAMOND: The main reason they operate with the boron in the water is they can keep the rods out that way.

MEMBER SIEBER: Yeah.

MR. DIAMOND: And that takes the place of the rods for reactivity control during normal operation and you'd rather use soluble boron than burn out your control rods fast.

MR. BASSETTE: But you want to also have this with or without pumps on, you know, so that you can have cooling for, you know, what power level.

CHAIRMAN WALLIS: This is so that you can control at shut-down. That's what it's for. As long as it's at power it's all right.

MR. DIAMOND: That's right and for making sure that you're in cold shut-down, you need soluble boron.

CHAIRMAN WALLIS: So you could have a huge slug and inject it forever and it wouldn't do any harm. It would just heat up the reactor to some sort of level.

MEMBER SIEBER: That would put the turbine on.

MR. DIAMOND: I would say the plant would be in trouble but not because it had experienced --

CHAIRMAN WALLIS: You couldn't shut it down.

MR. DIAMOND: Not because it experienced fuel damage as a result of energy deposition.

CHAIRMAN WALLIS: But you couldn't go to any kind of cold shut-down.

MR. DIAMOND: Right, exactly. Exactly.

MEMBER RANSOM: David, in your calculations, have you accounted for the increase in boron concentration due to the boiling off of the water in the core?

MR. DIAMOND: No, we haven't.

CHAIRMAN WALLIS: Isn't that the whole

thing?

MEMBER RANSOM: Do you know how much margin that provides?

MEMBER SIEBER: That takes a long time.

MEMBER RANSOM: Well, if you can boil off enough water to get the slug build-up, all the boron that was in that water is going to be left in the core.

CHAIRMAN WALLIS: Well, I think the water displaces the borated water when it comes in, that's the whole idea.

MR. DIAMOND: Yeah, so you're absolutely right and had I shown the slides that I didn't, I would have made the point that it really is independent of where your ECCS is at. What's really most important is what PPM your reactor is critical at and in other words, most reactor cores now are designed with the initial boron concentration, you know, in the neighborhood of 1500, 1700 ppm. You can only go through this event down to about maybe 1200 ppm. In other words, there's only this window of opportunity at the beginning of the cycle and I got off the track here as to what the point I was making.

CHAIRMAN WALLIS: Well, the point is that the deborated water pushes out the borated water so --

MR. DIAMOND: Oh, that's right. So what you have there initially is less important than -- in other words, what you have there is a result of the ECCS system, is less important than two factors. One is your reactivity of the system or your initial boron concentration when you're at operating conditions and the other is what that slug comes in at. Does it come in at zero ppm or 400 ppm, that's also important?

MEMBER RANSOM: So this energy deposition is pretty much a local effect. It depends on the local concentration of the boron in the flow?

MEMBER SIEBER: No, huh-uh.

MR. DIAMOND: Well, we -- it's a local effect in that, yeah, I mean, the reactor is large and things happen with different rates and different positions.

MEMBER RANSOM: And I'm not sure I understand then, why the initial boron concentration in the core itself is not an important factor.

MR. DIAMOND: The initial reactivity of the core and hence, the hot operating powered boron concentration is important. The fact that you increase to some level during this even and then come back means that it doesn't matter what you increase to. So if you increase to 2500 but maybe your RWST is really -- maybe the tech spec is at 2400, that's not important.

MEMBER RANSOM: The thing I was asking is you've been in this boiler condenser mode for some period of time. During that time, you're increasing the concentration of boron on the core itself.

MR. DIAMOND: Right.

MEMBER RANSOM: And then finally, the deborated slug is going to displace some of this borated water, but I believe you said that your calculations do not include the -- you know, the increase in global boron concentration in the core as a result of this boil-off.

MR. DIAMOND: Yeah, well, what I'm saying is that it doesn't matter, because you are that much more shut down as a result of -- say you're not at 2500. Say the concentration went up to 2700. You're that much further shut down but then you come back up through that anyway because your slug is assumed to be at a much lower concentration. So it doesn't matter.

MEMBER RANSOM: Well, that's what I was asking, the reactivity is more a local effect, I guess. It doesn't matter what the boron concentration in most of the core is like.

MEMBER SIEBER: The whole reactor goes

prompt critical as opposed to for example, a naval reactor where you have really high enriched fuel and they can go critical --

CHAIRMAN WALLIS: I think Vic is right, but you have to push out that borated water, don't you? The slug actually fills the core. The clean slug pretty well fills the core. So it flushes out your borated water.

MR. DIAMOND: Yeah.

CHAIRMAN WALLIS: But I think the reason you get the spike is because the Doppler thing is so core-wide and yet the local heating is intense in certain places.

MR. DIAMOND: Well, you get the spike because you reach a point where you're prompt critical.

CHAIRMAN WALLIS: Right.

MR. DIAMOND: And the neutron kinetics are such that once you reach that point, power just tends to take off and it's --

MEMBER SIEBER: It shuts you down.

MR. DIAMOND: Right, but you have --

CHAIRMAN WALLIS: You have to heat up the whole core to get the Doppler.

MEMBER SIEBER: Well, the heating of the

core is the integral of the power.

CHAIRMAN WALLIS: But the heating of the particular place you're worried about is the spike.

MEMBER SIEBER: Yeah, but the spike occurs throughout the core. The whole core pulses.

CHAIRMAN WALLIS: Yeah, but the hottest point goes off much more rapidly.

MEMBER SIEBER: You end up with a profile that's bottom skewed.

MR. DIAMOND: The highest power heats up the ferris, but most of the heating is the uranium daughter products, U-02 daughter products. Those are deposited over very localized area.

CHAIRMAN WALLIS: We hope.

MEMBER RANSOM: We'd better move on to the mixing. Why don't we get back at five after 3:00? We'll take a break.

(A brief recess was taken.)

MEMBER RANSOM: We're back in session.

PROF. diMARZO: I am Marino diMARZO from the Department of Research and I'm going to try to illustrate two things in this presentation. I'm going to try to figure out how RELAP, as presented by Dave Diamond actually represents the mixing in the vessel the way it has been described and then I'm going to talk about how do we generate the boundary condition to that model, to the RELAP and then been coupled with PARCS. So these are the two things that I'm going to try to touch upon.

So I'm going to spend most of the time here in the first item because we need to get the sense from some experimental evidence that we have, some CFD calculation that have been performed and from some other evidence as to what's happening and how well are we representing that.

MEMBER RANSOM: By the RELAP model, I assume you're including the mixing considerations.

PROF. diMARZO: The RELAP model --

MEMBER RANSOM: I know RELAP and what it does.

PROF. diMARZO: No, no, but the RELAP file that David Diamond has just showed you, it's essentially a time dependent volume feeding --

MEMBER RANSOM: Right, right.

PROF. diMARZO: -- that represents lower head that feeds into a junction and that feeds in all these channels. That's basically what is there.

MEMBER RANSOM: Right. I hope you're going to tell us how you get those conditions to feed in.

PROF. diMARZO: That's right. That's one part but the other part is we have to realize what that model does as coded because that's part, integral part of the deal. So I'm going to -- first of all, I'm going to follow a lot the comments that Vic, you made, and you'll see down here at the bottom of the slide all the references to the question that you raise.

But the first thing I'm going to do is review the mixing model which it's kind of a historical tool that enables us to simply understand what's going on in the different components of the system or in a way to interpret that. And then I'm going to talk about what happens inside the vessel, specifically what happens in a core channel, one of those vertical core channels, in the lower head and the most important in the combination of downcomer lower head in terms of mixing. And then I'm going to talk about what do we feed to the vessel through the cold leg from the outside. So this is the breakdown of what I'm trying to talk about. And you have seen this -- some of this in bits and pieces but I'm trying to give you something a little bit more organized here.

So this is the plant. Again, we are

looking at lower loop. This is the area where the steam generator tube, steam generator outer plenum, the cold leg suction to the pump is the region where the slug will be stored or formed, I should say, and placed. Then it could be moved somewhere and eventually it could be moved towards the core. But that's the volume where we're going.

So the first item is to review the mixing models. This is just historical because it's 1962 which was some time ago. I'm going by Levenspiel, which is the guy I know -- I mean, was taught to me in school, so that's -- but I'm sure that there are other versions of this and other formulation. The man identified two extreme case, the plug flow situation where basically if you have a step function, you just translate that and then the back mix flow which is also known as the mixing cup, the perfect mixing reactor, anything you want. It's just an overflowing cup in which you feed. You know, so these are just two conceptual idea of the extreme possibilities.

CHAIRMAN WALLIS: Back mix flow applies if you have a volume in which you can mix things, if you have a plate, then you have some other --

PROF. diMARZO: If you have a pipe that is extremely well agitated by some mechanical means --

CHAIRMAN WALLIS: You take that was a mixing --

PROF. diMARZO: That could be a well-mixed volume.

MEMBER RANSOM: One of the other terms is infinitely stirred reactor.

PROF. diMARZO: And infinitely -- yeah, there are --

MEMBER RANSOM: Homogeneous.

PROF. diMARZO: Yeah, it's just a little box with a little propeller inside. And so the formulation for the back mix flow is basically this. What you have is an initial concentration in the volume and then as time progresses you have a forcing function and you have a curve and you do a convolution of this thing and that gives you the concentration that trickles out of the cup. So that's basically what that is.

CHAIRMAN WALLIS: Thank you. I haven't seen an equation in some time.

PROF. diMARZO: Now, this goes to say because we were asked what do we do with this. The first thing we did was to define the transit time. That transit time is the volume of the slug divided by the flow metric flow rate. So essentially, it's the time it takes for the slug completely unmixed to go through a cross section. Okay? So that is basically the non-dimensional time.

CHAIRMAN WALLIS: It's related to the mixing transient time and the mixing volume.

PROF. diMARZO: Right, so in a sense if you let two and a half -- if you go back to this equation here, if you let two and a half transit time go by, you have swamped the volume. Once you have put two and a half times wine in a glass of water basically, it's all wine.

CHAIRMAN WALLIS: So in the slug it takes one time to sweep everything out and in the mixing thing it takes two or three times --

PROF. diMARZO: Two or three times, that's the area. So the transit time enables us to now non-dimensionalize, I mean, the time and the equation becomes this. The nice feature of this is the time has gone away in a sense and you have now the volume of the slug compared to the volume of the component as the term that you can call a time constant, a non-dimensional time constant and that essentially says that if the slug is far larger than the component into which mixing has occurred, you'll swamp it. On the other hand, if the slug is smaller than the component in which you are going in, you will never --

CHAIRMAN WALLIS: You'll just mix the ends of it.

PROF. diMARZO: That's correct. So that's very physical for me to understand what goes on. I mean, I can understand in this formulation the --

MEMBER RANSOM: Are you proposing like in the thermal hydraulic code framework, volume by volume?

PROF. diMARZO: You'll see. I cannot do that because that is -- that's in fact what B&W's owner's group did, Framatome did but there is -- in my opinion there is an intrinsic flow there because you decide how much you segment or whatever it is, and once you decide how much you segment or whatever it is, you are imposing the mixing, and so that is not an acceptable way of doing business.

MEMBER RANSOM: So your volume of slug is the entire --

PROF. diMARZO: Yeah, it's a component. It has to be a component.

MEMBER RANSOM: A component?

PROF. diMARZO: Yes, it could be the pump, it could be --

MEMBER RANSOM: V sub S, what is V sub S?

PROF. diMARZO: V sub S is the volume of the slug the original slug.

MEMBER RANSOM: The entire slug.

PROF. diMARZO: Yes, the entire slug. And this is the volume of the component in which we arbitrarily decide there is full mixing. Again, this is an interpretation tool. It doesn't have to be a predictive tool at all. It's just a way to look at things and to define whether they're well mixed or not well mixed or in between. Now there was a question as to how do we implement this and obviously, it all depends on how simple this forcing function here is. If it's very simple, it's an analytical form you can just integrate. If it's not that simple, well, basically what you do is you take your slug and you divide it in say 100 little chunks and then it becomes a summation type of process where now each segment of the slug is sent through and the whole process.

CHAIRMAN WALLIS: Each one has a piece of influence on the other?

PROF. diMARZO: Right, a parcel, a little parcel and you parcel it all out, you know, and at this point you get your concentration this way. So when the slug is not simple, in other words, not as step, it's nothing simple, we use this formulation which is a little code. So you're asking, are you using a code? Yes, I'm using a what 20-line quick basic code, yes, to do this.

MEMBER RANSOM: Okay.

CHAIRMAN WALLIS: So everything is going to be either types or volume of something. You're not going to have any sort of tailor mixing where you leave behind boundary layers and all that, nothing like that.

PROF. diMARZO: No, nothing like that. The pipe -- you'll see what the assumptions are. Now, go back to the formulation that David Diamond put forward. This is what is connected to PARCS. This is the RELAP 5 coding that is interfacing with PARCS. That's what happens there. There is not downcomer. There is just the lower plenum, that's it and this box, we'll have to figure out what it does, okay? And these things we'll have to figure it out what they do.

CHAIRMAN WALLIS: This lower plenum is --

MEMBER RANSOM: Well, it has a lower plenum and then the branch, too, that has some volume in there.

PROF. diMARZO: It has the branch and it's got this thing, and I'm trying to figure out what they do by just -- because I asked David to sending a step function which is known as an F function in Levenspiel language and then I look at RELAP itself when this thing goes through and then based on that, I make an interpretation and say, this is what it looks like.

CHAIRMAN WALLIS: RELAP actually has a well mixed plenum so it puts the same concentration of boron into each channel.

PROF. diMARZO: Into each -- well, it depends then on flow, yes. At this point, yes.

CHAIRMAN WALLIS: Because there's no mechanism of doing anything else.

PROF. diMARZO: Yeah.

MEMBER SIEBER: Or any lateral.

PROF. diMARZO: There is no lateral here. This is a junction.

MEMBER RANSOM: But it is a mixing volume though.

PROF. diMARZO: This is the mixing volume, yes.

MEMBER RANSOM: Well, the other one is, too.

PROF. diMARZO: This?

MEMBER RANSOM: Yeah.

PROF. diMARZO: This is just junction.

MEMBER RANSOM: It's what?

PROF. diMARZO: A junction.

CHAIRMAN WALLIS: I junction with no volume.

MEMBER RANSOM: Oh, it's a junction.

PROF. diMARZO: It's a junction.

MEMBER RANSOM: Well, then actually, all of those pipes are connected to the mix volume down.

PROF. diMARZO: Yes, yeah. So, I mean, I'm trying to give you that because I want to now analyze what that does the same way I would analyze a chemical reactor by sending in a tracer. That's basically what we're doing here. So let's first of all look at these channels, okay. So Taylor is saying that if you put in a channel fluid B following a fluid A with a sharp interface, as this moves along and spreads and diffuses away, the distance between the plane in which you have Point 1 concentration and the plane where you have 99 percent concentration is S.

CHAIRMAN WALLIS: This is Taylor mixing.

PROF. diMARZO: This is Taylor mixing.

CHAIRMAN WALLIS: This is okay.

PROF. diMARZO: This is in the channel.

CHAIRMAN WALLIS: Yeah, this is in the pipe.

PROF. diMARZO: In the pipe, in the

turbulent pipe, so S is basically how much you have smeared this initial flow. Initial S is zero and as you move along.

MEMBER RANSOM: And that will depend on the run --

CHAIRMAN WALLIS: How much is it spread o out?

PROF. diMARZO: Exactly.

CHAIRMAN WALLIS: And you've got some numbers for this?

PROF. diMARZO: Yes, so, our number is -- our -- once you put all the numbers that you know essentially the channel diameter, you put in your V start over -- your V start being some shear at the wall divided the density of the fluid, square root of that. You put all this in. This is what you are left with. S equals 0.57, square root of X for our case, X being how far you have gone into the channel.

CHAIRMAN WALLIS: It's almost universal because V star over U doesn't vary very much.

PROF. diMARZO: Right, so that's the idea.

MEMBER RANSOM: Now, what is the velocity assumed?

PROF. diMARZO: The velocity is the velocity that was given to me in the calculation that

I use this again, so I think it was one meter per second or 1.3 meter per --

MEMBER RANSOM: I mean is that representative of the reflux phase?

PROF. diMARZO: It's representative of the kind of velocity that we will see, the three feet per second that Dave was talking about. The start of the pump.

MEMBER RANSOM: Start of the pump?

PROF. diMARZO: Yeah, one pump.

CHAIRMAN WALLIS: So if you have one pump it's about --

PROF. diMARZO: So it's 1.3 meters per second, something like that, but the point is, I asked David Diamond to send me a step in concentration on top of this flow. And then I looked at the RELAP results to see what we were --

CHAIRMAN WALLIS: You're an SI unit.

PROF. diMARZO: I'm in SI unit.

CHAIRMAN WALLIS: Because you've got S equals square root of X. You've got to be some unit. It's SI unit.

PROF. diMARZO: SI unit. Now, another way to do this is along with Levenspiel which talks in terms of dispersion, dispersion being a parameter that looks like D over Ul, D being basically this molecular -- some sort of a dispersion term which he doesn't define very will but L and U the length --

CHAIRMAN WALLIS: A diffusing number.

PROF. diMARZO: Yeah, it would be some type of diffusing type of number. So I can use also that terms and I have a chart which I'll just show you that gives me this value and then nothing else is but diameter divided by length. The importance of this is we are over here. That's where we are in this particular situation in the chart and you can see that Taylor's theory is this line over here, right? So once you go a little bit beyond five ten to the fourth here, Taylor and the experimental data are the same. So what I'm trying to say here is that there is experimental evidence that what we are getting here from the filler side of things --

CHAIRMAN WALLIS: This is for the core?

PROF. diMARZO: This is for the channels in the core.

CHAIRMAN WALLIS: The channels have spacers.

PROF. diMARZO: No, this is just --

CHAIRMAN WALLIS: That screws everything up, it changes everything. Taylors is for the straight pipe.

PROF. diMARZO: This is for the straight pipe.

CHAIRMAN WALLIS: I'm sorry to introduce such complications but --

PROF. diMARZO: Yeah, but I just took --

CHAIRMAN WALLIS: The spacers actually mix everything up.

PROF. diMARZO: I know but I just took what RELAP does. That would be nice. You will see that's very nice what you're saying.

CHAIRMAN WALLIS: The spacers actually make it --

MEMBER SIEBER: The grids would even --

PROF. diMARZO: Which is good because it will bring RELAP closer to reality in a strange way but --

CHAIRMAN WALLIS: So you're taking a limiting analysis here --

PROF. diMARZO: Exactly.

CHAIRMAN WALLIS: -- as if there were not spacers.

PROF. diMARZO: Correct, but now the problem is, as you will see, RELAP has got this thing called numerical diffusion that we go the other way, so let me show you what we get. Now, you can do another thing. You can take the answer from RELAP too, right, and you can essentially calculate that there are two standards deviation between the sixteen and eighty-fourth percentile. This is, again, along with Levenspiel. So you can calculate what this standard deviation is and you can get the dispersion from the actual data coming out from RELAP.

So the theoretical number from Taylor is 0.0011. The number that you get from RELAP, 0.0020.

CHAIRMAN WALLIS: Does this depend upon the node size and that sort of thing?

PROF. diMARZO: That depends on the node size, yeah, but that's what RELAP has done. And what RELAP has done is about twice what Taylor is saying for a pipe straight.

CHAIRMAN WALLIS: I think actually it's bigger than that because of the spacers.

PROF. diMARZO: Correct. So Taylor is not 0011, it's probably higher, closer to this, but that's not really very important. The other thing you can do is go back and calculate the S according to Taylor, the distance of the smeared slug and again, here are two sets of numbers for aft core and end of core.

CHAIRMAN WALLIS: It smears out quite a

bit.

PROF. diMARZO: It smears out quite a bit. Now, the point of all this is that Levenspiel calls small dispersion anything that is less than 0.002. What he means is whatever is less than 0.002 is close to a plug flow.

CHAIRMAN WALLIS: With the spacers it might be 005 or something like that.

PROF. diMARZO: It would be more mixed, that's fine, but what I'm saying is that we are in the neighborhood of what is defined by Levenspiel as being a small amount of dispersion in this --

CHAIRMAN WALLIS: Actually it will be actually more but again the spacers have some influence.

PROF. diMARZO: The spacers have some influence, we didn't go there but what I'm saying is if these numbers are -- all I'm trying to say is tha the numerical diffusion in RELAP is not doing us a tremendous disservice.

CHAIRMAN WALLIS: They don't have any effect, since we're on this topic, that total dispersion is because the velocity of the middle of the --

PROF. diMARZO: Yeah, it will just diffuse one more and -- yeah, the representation of that will be a plug flow and then a little mix reactant.

CHAIRMAN WALLIS: A boundary layer which stays behind.

PROF. diMARZO: Right.

CHAIRMAN WALLIS: And the spacers may actually mix that up and prevent that.

PROF. diMARZO: Stir it up a little bit. Yeah, but the point is that we are in the real of what is defined as small dispersion. So we are in the real closer as you will see closer as you will see in a figure that I'll show you.

CHAIRMAN WALLIS: As long as the spacers don't make it --

PROF. diMARZO: Yeah, fairly close, as long as the spacers don't stir up everything, we are closer to a plug flow. So that's the first part. And that's what I mean. This is a small amount of dispersions, 0.002 is this curve here.

CHAIRMAN WALLIS: Is there experimental evidence of mixing, axial mixing in the core?

PROF. diMARZO: No.

CHAIRMAN WALLIS: There's no experimental evidence whatsoever?

PROF. diMARZO: Not that I know.

CHAIRMAN WALLIS: That's sort of strange, because people have tested rod bundles ad nauseam for all kinds of purposes.

MEMBER RANSOM: Well, there's a little bit, I think on the heat transfer effects. You know, the spacers again, they've seen significant effects on the heat transfer of the rods but --

PROF. diMARZO: That would have been --

MR. BASSETTE: I think when people look at DMB in the core, you get some information as to spacer effect.

PROF. diMARZO: The net sense of all this is that the core channels are around here. So the representation of the flow in the channel by RELAP 5 connector 2 parts is reasonable, that's my conclusion.

Now, let's talk about that node, that node that represents the lower head.

CHAIRMAN WALLIS: It's the other extreme.

PROF. diMARZO: Right, the node that represents the lower head I just take what comes out of RELAP again, going through that node, and this line here is on this plot what that node does to a step function. And this line here is the infinitely mixed volume or the back mixed volume, the one represented by those equations that I showed you.

CHAIRMAN WALLIS: But the RELAP is very, very, very close to the back mix flow.

PROF. diMARZO: Right, in the sense, look at --

CHAIRMAN WALLIS: The equations look just like the back mix fluid equation I would think that RELAP solves.

PROF. diMARZO: I'm not so sure because there are some options in there. I'm not so sure what the options have been exercised in that node.

CHAIRMAN WALLIS: Maybe it's this forward and backward differencing in the --

PROF. diMARZO: That's right but I didn't go into that. I took the result and I said, look, if it was an unknown reactor to me, this is what it does and I can say, well, if I make this assumption here of this volume, I am close in representing what it means. So all I'm saying is --

CHAIRMAN WALLIS: So it's reality.

PROF. diMARZO: Exactly.

CHAIRMAN WALLIS: Not just RELAP but reality.

PROF. diMARZO: Exactly, we'll get there.

MEMBER RANSOM: You're saying the RELAP 5 is close to just a propagated --

PROF. diMARZO: The one single node --

MEMBER RANSOM: No, no, well-mixed.

PROF. diMARZO: That node that represents the lower head is very close to a weld steel reactor and the channel are very close to a plug flow.

MEMBER RANSOM: Right.

PROF. diMARZO: That's what that model --

CHAIRMAN WALLIS: It pretty well has to because that's the whole mathematical equation.

PROF. diMARZO: That's the deal, right. So, I'm not sure that it applies, so I mean, that's my interpretation of that. Now, what is reality we have to figure out still.

MEMBER RANSOM: Mr. diMarzo, one thing you might say, you know, on the RELAP 5 it's a transient calculation. It goes time step to time step, so things tend to be propagated every time step somewhat which results in diffusion.

PROF. diMARZO: That's right.

MEMBER RANSOM: And I'm wondering if it runs at the material limit, then it's propagating only as the velocity and but theoretically, if you ran this thing a very small time step, you would see very rapid diffusion. How do you account for those differences?

PROF. diMARZO: That I don't know because we didn't do and we didn't went into that level of detail from the calculation. David, do you have some idea of what --

CHAIRMAN WALLIS: It depends on node size and time step and all kinds of things.

PROF. diMARZO: Yes.

MEMBER RANSOM: Well, it would be interesting to know, I guess what time step these calculations were run at then, so that you would know whether this is --

PROF. diMARZO: I don't know that calculation. That's a very good point but what we took was the global outcome of that calculation. So that was to get the sense for what we are actually coupling with PARCS because that is something that we have to get conscious of. So now in the following, what are we trying to do now. We are trying to get some experimental evidence and some means to figure out what's reality which is exactly the question that Graham posed. And we don't have any mean to separate downcomer from lower head, per se, unless we could.

So we will look at the whole in vessel thing in the end, so let me progress step-wise and show you what experimental evidence we have.

CHAIRMAN WALLIS: So why is it a plug flow model of the downcomer?

PROF. diMARZO: The downcomer is not existing.

CHAIRMAN WALLIS: Does RELAP not exist? I mean, RELAP has a model for the downcomer, doesn't it?

PROF. diMARZO: No.

CHAIRMAN WALLIS: No, RELAP doesn't say the downcomer exists?

PROF. diMARZO: No, there is no downcomer. We're feeding in the lower head.

CHAIRMAN WALLIS: How does RELAP get the flow into the lower plenum?

PROF. diMARZO: From a time dependent node.

CHAIRMAN WALLIS: It just appears magically from somewhere?

MEMBER RANSOM: I think one thing needs to be clarified. He's talking about RELAP 5 in the sense that the one that's used with the PARCS model.

CHAIRMAN WALLIS: Right.

MEMBER RANSOM: Now there could be a RELAP model of the entire system which --

PROF. diMARZO: There is no downcomer in that --

CHAIRMAN WALLIS: I doesn't go that far back.

PROF. diMARZO: That's right, so the question now is, is that good enough or should I do something?

CHAIRMAN WALLIS: But it's not clear that it has a plug flow model of the downcomer. It accepts whatever you tell it comes in.

PROF. diMARZO: Correct.

CHAIRMAN WALLIS: So if I assumed that all that is modeled in the vessel is what was presented in the condition is essentially that I'm saying that the downcomer has to operate like a plug flow model. Do you see what I'm saying because it's just simply not there.

CHAIRMAN WALLIS: Are the inputs the same as the input from the --

PROF. diMARZO: Time shifted that's all. That's what we look like at this point, okay? And again I don't know whether that is true or not or whether that is real or not. I have to figure it out.

CHAIRMAN WALLIS: I can run with concentrations of stuff as well.

PROF. diMARZO: Sure, but what I'm saying is --

CHAIRMAN WALLIS: What does RELAP do with the downcomer if you model a downcomer in RELAP?

PROF. diMARZO: A downcomer would be complicated because you'd have to have a three dimensional presentation of the downcomer and that is deemed problematic, highly problemmatic.

CHAIRMAN WALLIS: So RELAP thinks the downcomer is a pipe.

PROF. diMARZO: If you put down a pipe, then basically you don't have much to do with what's going on here, so I don't know exactly how you would handle that with RELAP but we can handle it with fluent in the CFD model.

CHAIRMAN WALLIS: It depends on how many nodes you have in a downcomer. If you have just one big node, then it's like a mixed vessel. If you --

PROF. diMARZO: You can have one stack on node and that's a representation and you could have a three dimensional representation but then there are other issues.

MEMBER KRESS:: The PTF program found it to be well mixed.

CHAIRMAN WALLIS: Yeah, but that's not saying it is.

PROF. diMARZO: Look, RELAP is not a

mixing tool for a three dimensional space.

CHAIRMAN WALLIS: We're talking about reality.

PROF. diMARZO: Right, exactly. So we don't want to go there. Finally it's now what do we have?

CHAIRMAN WALLIS: I think you'll probably say it's conservative to have no mixing in the downcomer.

PROF. diMARZO: Sure, sure. I mean, that's -- what we've seen better, what do we have. E have a CSNI experiment at Maryland, which I didn't run, so it's independent.

CHAIRMAN WALLIS: Where is this place, Maryland?

PROF. diMARZO: Somewhere in -- somewhere on the Beltway. And then we have a research CFD calculation of the same thing, okay, and that was performed within that CSNI operation, so we're going to tap into that. So the experiment wasn't done in concentration. It was done in temperature but since there was not much heat losses, basically there was an equivalence between the two and the idea was to send in a cold front of 12 degrees C in a 72 decree centigrade downcomer at seven liter per second, the downcomer is around 100 liters, just to give you an idea.

CHAIRMAN WALLIS: You'd duplicating food number or something?

PROF. diMARZO: Yes, there is a whole report and details and whatnot. Then we have affluent calculation done by research of that space with about half a million node which where is that will give you a resolution in the latter part of the downcomer where the cold legs are inserted essentially around eight nodes across and then below the expansion, 14 nodes across three dimensional.

CHAIRMAN WALLIS: I hope that the transcript records affluent calculation, not effluent calculations the way these words sometimes get screwed up.

PROF. diMARZO: Yeah, CFD calculation, affluent is a commercialism, so CFD. Okay. And basically the pressure drop -- this is very important because we're going to go to that again later. In order to model the various sets that are between the lower head and the core a pulse meter was inserted there and you will see that in the future slides just to give you a better sense of that, to match the pressure drop that was observed in the --

MEMBER RANSOM: Are the same temperatures assumed for --

PROF. diMARZO: Same everything.

MEMBER RANSOM: -- the affluent calculations, though?

PROF. diMARZO: Right, that was basically an initial condition fairly well monitored in the cold leg which was given to affluent and then it's --

MEMBER RANSOM: So it's cold water going down into --

PROF. diMARZO: Cold water going down.

MEMBER RANSOM: -- hot water, right?

PROF. diMARZO: Experiments have been done to correct for the cold water by salting the hot water. I mean, all kinds of variation and gyration have been done there. I mean, we've got data.

MEMBER RANSOM: One thing we need to keep in mind, Dr. Norbush has brought up that maybe it's hot water.

PROF. diMARZO: It could be hot water and -- we have all the data on that kind of a thing but you'll see what kind of happens. So let's talk about what is in the lower head because that's a relevant problem here. In the lower head there is a free space and angular -- I don't say angular, but there's a gap here in the hemisphere, a free-flowing gap with the exception of the measurement instrumentation here. Then there is a first distributor, a first perforated plate.

CHAIRMAN WALLIS: It's a colander.

PROF. diMARZO: A colander, yes. Above that there is a distributor plate, okay. Then above that there is basically a big spacer plate with perforation again. Then there is another distributor plate and then another perforated plate. That is the reason why this whole contraption here in the CFD calculation has been like a porous media looking like a hemisphere.

MR. TRAIFOROS: Basically a permeability tensile has been developed there?

PROF. diMARZO: Yes, and so that was the way it was represented because there was no way to do this thing.

MR. TRAIFOROS: So it's not isotropic type of porous media. There's tensile in there.

PROF. diMARZO: Yeah. So that's basically what has been done. And that explains also the kind of results that we're getting from Maryland where we do have a duplication of all of these things. We don't have all these porous sets but I mean, we have some of them. So these are all the grids through which the flow should go through. Now, there were several tests run at Maryland, I'm told because again, I didn't do it, but basically they were done one after the other in similar conditions, okay. And then the results of these tests were averaged out, 16 identical, quote unquote "identical" repeat tests averaged.

That explains why there is an error bar. An error bar, we shouldn't call it an error bar. There is a bar here because there are 16 experiments. This is the average value and those are the variation.

CHAIRMAN WALLIS: This is from the CF --

PROF. diMARZO: No, this is experiments.

CHAIRMAN WALLIS: One is --

MEMBER RANSOM: This is hot water, cold water?

PROF. diMARZO: These are the experiments, okay? Now, what can change? Well, the temperature might change slightly, the way it's injected may change slightly and so forth. The important part, which goes to the point, is that the maximum variation is not down here nor in the beginning but in this intermediate portion.

CHAIRMAN WALLIS: What do you mean by

normalized temperature? Where is this temperature?

PROF. diMARZO: This is -- remember there is a temperature pre-existing in the vessel, which is one. And there is slug that has a temperature zero.

CHAIRMAN WALLIS: And this is the temperature as the --

PROF. diMARZO: As time goes by --

CHAIRMAN WALLIS: -- as the liquid goes into the vessel.

PROF. diMARZO: This is one downcomer worth of liquid.

CHAIRMAN WALLIS: As the liquid comes into the vessel.

PROF. diMARZO: Into the vessel and two downcomers --

CHAIRMAN WALLIS: But whereabouts across the cross section of the bottom of the vessel?

PROF. diMARZO: Okay, the liquid comes in in one cold leg. And this the averaged --

CHAIRMAN WALLIS: It's the average temperature going into the vessel.

PROF. diMARZO: No, not going into the vessel, at the bottom of the downcomer.

MEMBER RANSOM: It's at the --

PROF. diMARZO: There are a bunch of

thermal couples at the bottom of the downcomer and the average value of those.

CHAIRMAN WALLIS: So it's the average value. How much does it vary around the --

PROF. diMARZO: I'll get there. I have that. Now, I have that at the entrance of the core from the --

CHAIRMAN WALLIS: So this is expediential behavior, right?

PROF. diMARZO: Yeah, this is basically what you have. The variations here which are larger, are due to the fact that --

CHAIRMAN WALLIS: Your expediential mixing model would give something very similar.

PROF. diMARZO: No, it's more complicated. We did some more --

MR. TRAIFOROS: You're neglecting this mixing in your model.

PROF. diMARZO: Yeah, yeah, but let me rephrase. There are several possibilities depending on the density. When the flow enters the upper downcomer, it can go around and sink on the other side or it can just go down. That depends a little bit on the densities. Then there is another twisting factor. When -- remember that the lower head is empty. When the stream hits the lower head on the other side you basically get an upward. Since you're averaging in that plane, you're getting also the inflow --

CHAIRMAN WALLIS: Well, this looks as if the downcomer is behaving like a pretty good mixer.

PROF. diMARZO: Not really. In order to match this curve, it's very complicated.

CHAIRMAN WALLIS: But if there are no mixing, it would just be a step function.

PROF. diMARZO: Yes, but the point is this; there are two factors. You have to introduce two complications in the mixing formulation in order to get the curve that looks like this. You've got to introduce a dispersion now, which is not zero, not infinity, an intermediate dispersion and then you have to introduce the concept of participating volume because not 100 percent of the downcomer is participating in the mixing but the portion is so-called stagnant feature. So once you introduce these two variables into the process, then you can get the curve that matches this. So it becomes very complicated very quickly. We did that extensively but there is a whole different deal.

CHAIRMAN WALLIS: If it mixes in half the volume, then things will happen twice as fast.

PROF. diMARZO: That's right but there are two extremes.

CHAIRMAN WALLIS: So you --

PROF. diMARZO: If the flow comes in and goes down like crazy like that, it mixes a lot but the participating volume is very small.

CHAIRMAN WALLIS: Did you show these three theories superimposed on the data?

PROF. diMARZO: Yes, we did that but the problem is this --

CHAIRMAN WALLIS: Are you going to show that?

PROF. diMARZO: No, no, because the point issue is this; this is a reduced scale experiment. So if I make a reduced scale experiment like this, and I want to demonstrate the scaleability of a reduced scale experiment like this to plant, at this stage of game, I have basically no hope. So I cannot go with an intermediate dispersion and a participating volume.

CHAIRMAN WALLIS: Well, I'd rather have another figure where you show the MM as well as the --

PROF. diMARZO: Exactly, hang in there.

CHAIRMAN WALLIS: That's what we're going to get to.

PROF. diMARZO: Exactly.

MEMBER RANSOM: No, the CFD calculation is again, an average?

PROF. diMARZO: The CFD calculation is the average in the plane.

MEMBER RANSOM: And the error bars that you have there are --

PROF. diMARZO: Well, let me show you on the next slide, because I didn't want to put --

MEMBER RANSOM: All right.

PROF. DiMARZO: But what I'm trying to say here is the CFD calculation is a fairly good representation of that data at reduced scale. That's the point I'm driving at. So remember the CFD calculation is not just the downcomer. It includes also a lower head. It includes this porous portion that represents that. I will make use of that calculation exactly without touching anything to infer what's happening at the entrance of the core because it does very well the downcomer. I'll continue it and extract data at that plane.

So this is a question that I don't know how to answer completely but in a sense we have use the Maryland facility in a number of situations in a number of scenarios, I mean, there is ample literature on this since `82. So in terms of the representation of plant from the Maryland facility and the scaling and so forth, there's an ample amount of literature that in different situations in different transients can give you an idea of how representative that might be.

But let's go to the slide that Graham is talking about because this is important. So if I take -- make the assumption that the downcomer is totally unmixed, and that lower head is, as we said, a fully mixed node, this black line here is what that MM -- that mixing model will give me. It's just a mathematical expression. It doesn't mean any more than that.

CHAIRMAN WALLIS: This is for the concentration going into the core or --

PROF. diMARZO: This is the concentration or temperature whichever --

CHAIRMAN WALLIS: In the lower plenum or where?

PROF. diMARZO: One is the pre-existing concentration. Okay, this is at the entrance of the core.

CHAIRMAN WALLIS: Entrance of the core, whereas the previous --

PROF. diMARZO: Was at the end of the

downcomer. Entrance of the core. This time I have plotted here -- what I've plotted -- in that plane, okay, there are a number of nodes, so I essentially took the 10 percent of those nodes that show the lowest temperature and I've seen at what temperature that is and I took 10 percent of the node at the highest temperature and I measured the temperature and these are these two points.

CHAIRMAN WALLIS: So actually, if you fit the curves with an expediential, your model is about twice the K rate of the CFD.

PROF. diMARZO: Yeah, but let's look at this first and then I'll get there, but this point is the average on the plane. These two points represent a 10 percentile discarded on the lowest temperature and the highest temperature. The first thing that jumps out is that it's skewed. What does it mean? It basically means that there are fingers of low concentration coming into the core, if you wish in this region here, at this concentration level. Then it narrows down again pretty uniform.

Now, go back to the presentation that David Diamond had before. What are the two fundamental problems? First is how sharp is the injection because that determines the initial pulse.

So at high concentration, concentration is not an issue. What's an issue is how fast you decrease the boron concentration. Okay, so in this region here, what matters is how sharp this thing comes down. In this region here, what matters is how low you go. Now, this mathematical representation here, very simplistic mathematical representation, essentially skirts the minimum values of the calculation.

CHAIRMAN WALLIS: I think in David Diamond's nothing much happened until the concentration got quite low.

PROF. diMARZO: Exactly. But remember that initially what's important is how fast you drop and that's giving you the spike. Remember that there was two sets of slides, one at three --

CHAIRMAN WALLIS: Three percent.

PROF. diMARZO: -- three percent and 25 percent, so that addresses this part of the curve. And then the other part of the curve is how low you go. So essentially, this representation, which again, I don't claim any physical -- how can I say -- any physical direct truth to it, okay, but as the power of giving you a sharp variation at this point where sharpness is an issue and it gives you a low concentration on this portion where low concentration is an issue. So that is what we say conservative in a sense.

MR. TRAIFOROS: But the reason for your sharp because your boundary condition here is sharp. You did not put the values that out of the downcomer mixing coming out as an input. If you would have put it, it would not be --

MS. MUIR: No, no, if you put --

MR. TRAIFOROS: The boundary conditions --

PROF. diMARZO: -- if you get to this drop here, right?

MEMBER RANSOM: Well, if I understand it, you're mixing model is two volumes, right, the downcomer and the lower head.

PROF. diMARZO: That's absolutely correct.

MEMBER RANSOM: The downcomer is plug flow, meaning no mixing and then good mixing in the lower head.

PROF. diMARZO: That's right and by no means, this is not the physical representation of --

MEMBER RANSOM: I understand that.

MR. TRAIFOROS: But the data is mixing in the downcomer plus mixing in lower plenum.

PROF. diMARZO: Absolutely.

MR. TRAIFOROS: So in actuality, if you

wanted to see the validity of well-mixed lower plenum, you have to put input to this model the output from the downcomer experiment.

MEMBER RANSOM: Well, the CFD is doing that.

MR. TRAIFOROS: No.

MEMBER RANSOM: Yes.

PROF. diMARZO: The whole thing. CFD is not -- CF is artificial where you break. It's a --

MR. TRAIFOROS: In a way you are trying to scale up some of the mixing in the downcomer giving credit to the lower plenum mixing.

PROF. diMARZO: That's correct.

MR. TRAIFOROS: But you mentioned that there is a question of a scalability of this.

PROF. diMARZO: No, I didn't scale anything. This is -- this is U scale is done by CFD under U scale and the models are the models, a mathematical expression so it doesn't have really scale.

CHAIRMAN WALLIS: But I think he has a point that if there isn't complete mixing in the lower plenum but you get some credit for mixing in the downcomer and then you attribute it all to --

MR. TRAIFOROS: To lower plenum.

CHAIRMAN WALLIS: -- you may be over-estimating the lower plenum.

PROF. diMARZO: And underestimating the other one, yes. But the matter of fact of that is that the impact on PARCS is quite conservative because down here you are under-predicting essentially the average value but bounding the minimum and other hand, you are over-predicting the sharpness of the front which is crucial to get that initial reactivity insertion.

MR. TRAIFOROS: Isn't CFD -- not CFD, universal --

PROF. diMARZO: No.

MR. TRAIFOROS: -- because you assimilate it. You don't have measurement of those coring --

PROF. diMARZO: No, no, it's not accessible.

MR. TRAIFOROS: No temperature measurements in lower plenum.

PROF. diMARZO: There's too much stuff in there to get. If we had that it would have been very nice.

MEMBER RANSOM: Well, could it be argued this is a conservative model then because if you included mixing in the downcomer, it would spread out this effect even more than --

MR. TRAIFOROS: Yeah, I think extending the mixing in the downcomer of the model of University of Maryland to the full power, full power -- full plant basically.

PROF. diMARZO: No, wait a minute.

MR. TRAIFOROS: You are using CFD as a scaling way of --

PROF. diMARZO: No, no, this is all at reduced scale.

MR. TRAIFOROS: But you are validating the mixing in the lower plenum.

PROF. diMARZO: Reduced scale --

MR. TRAIFOROS: Your assumption is lower plenum is well mixed, okay?

PROF. diMARZO: Yes.

MR. TRAIFOROS: So if that's indeed the case, input to the lower plenum, the concentration that you have measurements from University of Maryland in the downcomer.

PROF. diMARZO: Yeah, the but the problem is this, I'm not claiming --

MR. TRAIFOROS: You would not have been conservative, so you don't have fully mixing in downcomer, in lower plenum.

PROF. diMARZO: Yes, but I do have mixing in downcomer from which I don't take credit. So all I'm saying here is the combined effect of having a no-mix downcomer and a full mix which is a mathematical expression is not -- it doesn't have any per se modeling quality.

MR. TRAIFOROS: And this is independent of scale of the University of Maryland?

PROF. diMARZO: No, no, this is --

MEMBER RANSOM: These are high Reynolds number.

PROF. diMARZO: Yeah.

MR. TRAIFOROS: That's not my point, high Reynolds number.

MEMBER RANSOM: No, high Reynolds is similarity is easier, I guess.

PROF. diMARZO: All we are saying at this stage, all we are saying --

MEMBER RANSOM: So the only other one is geometric similarity.

PROF. diMARZO: Exactly, all we are saying is --

MEMBER RANSOM: So we have both of those.

PROF. diMARZO: All we have said so far is that these models -- models is not the right word, these mathematical expressions is probably the better word, they're fake of a reality which is much more complex and represented reduced scale by experiment and CFD. That's all we are saying at this stage.

MEMBER RANSOM: The one thing that would be worried though is your comment about stratified conditions would not be representative.

MR. TRAIFOROS: No, the high Reynolds number buoyancy is not of importance. I mean, these are very high fluid.

MEMBER RANSOM: Right, not in this case but if you look at the lower flow case.

MR. TRAIFOROS: Again, what we are arguing if CFD meet critique something at the downcomer, I can make the conclusion that it will predict other aspect of the problem the same accuracy.

PROF. diMARZO: Okay, yes. That's a well-taken point but the structure of the flow between the downcomer and the lower head, you're arguing is completely different and I don't know if CFD does a good job, is that what you're saying?

MR. TRAIFOROS: Exactly, that's one of my questions. I mean, you are -- a code has -- for example, the way you model the downcomer is quite different than you model the lower plenum.

PROF. diMARZO: We don't model -- you mean the CFD.

MR. TRAIFOROS: The CFD, yeah. So since you know how these --

PROF. diMARZO: I don't think there is a difference in modeling. It's just the --

MR. TRAIFOROS: You have a porous medium model for --

PROF. diMARZO: For that portion, yes.

MR. TRAIFOROS: Okay, so you are -- you may have a code, have two aspects of an experiment predict well and the --

PROF. diMARZO: Yeah, but the reality is this. As it stands today, we collectively do not have a mixing code assessed within this regulatory space, if you wish that we can use to say if this is according to scale, I now am going to plant with it. There is no such a thing around here.

MR. TRAIFOROS: But at the same time we are using the knowledge of the downcomer mixing to extrapolate it is a well-mixed lower plenum.

PROF. diMARZO: No, the lower plenum is not --

MR. TRAIFOROS: That is not the case.

PROF. diMARZO: The lower plenum is not

well-mixed, it's doing this.

MR. TRAIFOROS: Because your input -- so this is basically what you are saying that if I don't use mixing in the downcomer and well mix in lower plenum, effectively --

PROF. diMARZO: This is what you get.

MR. TRAIFOROS: So you are not --

MEMBER RANSOM: It's a conservative result.

PROF. diMARZO: Yes.

MR. TRAIFOROS: But at the same time, you are stating in your report you don't give any credit to downcomer mixing.

PROF. diMARZO: In this black line here, no. In the CFD calculation, sure, there is mixing in the downcomer and there is mixing in that lower head. All I'm saying is that this mathematical expression provides me with a representation of reality if you want to call it that, at reduced scale. Now the benefit of this is very simple. Once I have a representation and this is where the --

MEMBER RANSOM: Let me ask you one more question about this.

PROF. diMARZO: Sure.

MEMBER RANSOM: This is high fall, you

know, with a pump on. What about restart of natural circulation?

PROF. diMARZO: We have no data yet on that. We have data but we don't have a CFD of that.

MEMBER RANSOM: So everything you're saying is addressing only the restart of a pump.

MR. TRAIFOROS: Your assumption is that -- you have used the same assumption for the natural situation.

PROF. diMARZO: Yes, yes.

MEMBER SIEBER: It's the same thing.

PROF. diMARZO: Remember the --

MR. TRAIFOROS: Wouldn't natural --

PROF. diMARZO: Yes, but the problem is this; remember that the natural circulation which has less mixing has also a benign front because it comes in much slower. So the problem is that the natural circulation situation you have to keep in mind, although the slope could be coming in very slowly and everything, whatever, as far as fuel damage is concerned, not as far as restarting the reactor, I mean, you're going to go to full power. There's no question about it. If you don't do anything, you're going to go to full power but the point is that you're going to go there without damaging the fuel.

CHAIRMAN WALLIS: In reality, there is mixing in both the downcomer and the low plenum.

PROF. diMARZO: Right.

CHAIRMAN WALLIS: In order to have an input to PARCS, you have to have complete mixing in the lower plenum, otherwise you would be putting different --

MS. MORRIS: Absolutely.

CHAIRMAN WALLIS: -- in different parts of the reactor.

PROF. diMARZO: Absolutely, I have no choice.

CHAIRMAN WALLIS: And they can't handle that so you have no choice and you're trying to show that if you do it with the have no choice part and put all the mixing in the lower plenum, you get something which is conservative.

PROF. diMARZO: Right, that's exactly what I'm saying and the part that is --

CHAIRMAN WALLIS: You're not trying to represent the reality, you're trying to show that your representation is conservative.

PROF. diMARZO: Now, in terms of scale, I don't know what to add, because I cannot do anything to this thing, to the mixing models, to introduce scale except through the volumes, through the actual volumes because --

CHAIRMAN WALLIS: The reality may be effected by scale.

PROF. diMARZO: That part there remains --

CHAIRMAN WALLIS: That's why you use things like food numbers and all that.

PROF. diMARZO: Yes, but those things are scaled and are reasonably well, so that's the only thing you have. So to summarize here, I got downcomer data in Maryland, and I got the CFD calculation of the same, so this has been there. I am extrapolating since this calculation though, includes the lower head and then comparing that with what comes from the top and what comes from the top is a representation to these mixing models that I showed you, the no-mix downcomer and the fully mixed lower head which is basically what this PARCS has in it.

CHAIRMAN WALLIS: That's as conservative as you could get because if you put some mixing in the downcomer, it would make it --

PROF. diMARZO: Right, remember that I cannot --

CHAIRMAN WALLIS: -- which you're constrained to do.

PROF. diMARZO: Remember that this line here is a line of great uncertainty no matter what I do. Because if you come with a partially mixed volume across this line, I frankly don't know how to defend it, whatever numbers do I get or a partially participating volume across this line. It would be very hard to say that 30 percent of the downcomer is unparticipating in Maryland and then therefore 30 percent of the real downcomer would be in the same situation. I have no way to make that argument, nor do I have an argument to make about the dispersion that they would be equivalent of something. You see my predicament here.

So that is basically the first part.

MEMBER RANSOM: Incidentally, your mixing model is just a simple code I guess.

PROF. diMARZO: Yes, it is a simple code. it is conservative and the intent is to eliminate the scaling morass which I don't want to go into because we have no tools to address that. so in the end what I'm saying is that the RELAP PARCS representation that we have and as far as the vessel is concerned, is a conservative representation but reasonable. That's all I'm concluding.

MEMBER RANSOM: There RELAP 5 meaning the

RELAP 5 PARCS model.

PROF. diMARZO: Yes.

MEMBER RANSOM: Because the other model would have mixing in the downcomer.

PROF. diMARZO: Yes, but then again, how good is RELAP doing it, how do you go scaling, you know, whatever, how much do you want to believe that, all those --

MEMBER RANSOM: I assume eventually you want to extend this to include the cold leg and all the rest of --

PROF. diMARZO: Okay, so now we are at the vessel. Now we have to go and feed something from that cold leg into the vessel and that's the second part of my talk.

CHAIRMAN WALLIS: You have to go actually talk to us about mixing in the pump and all that.

PROF. diMARZO: Right, that's the second part.

CHAIRMAN WALLIS: That's going to take some time, isn't it?

PROF. diMARZO: No, no, I hope not.

CHAIRMAN WALLIS: Well, never get to Mr. Bassette.

PROF. diMARZO: It's a six-slide thing.

I hope you read the paper, what can I say? Now, I'm making again assumptions, assumptions as good as anything, okay? And the assumptions are that pipe don't mix and that the only two things that mix is the pump and the steam generator for outer plenum. Again, remember what I'm talking about here, a mathematical expression, so the only thing I'm saying that there is a volume which is well-mixed and that's okay because you haven't defined what that volume is, okay.

You can make it as big and as small until you fit the data. I'm saying that that volume is the whole steam generator for outer plenum and it's the pump volume. Now, obviously, one can say, well, wait a minute, the mixing generated by a pump happens downstream of pump because you've started the flow and the mixing happens down there. Yes, granted, there is a volume in which this mixing occurs that you can think of as a way mixed volume of a certain size. All I'm assuming is that volume of certain size is of the size of the pump. That's all I'm saying. Again, you an argue this is not right or this is right but --

MEMBER RANSOM: Are you including the cold leg in that as --

PROF. diMARZO: No, no, just the volume of the pump.

MEMBER RANSOM: Okay, fine.

PROF. diMARZO: Again, it's an assumption. It's good. Okay, but go back to what was done in the scaling of the steam generator. The idea was I'm going to try something on you and then I'm going to compare it with the data and as long as it works, what I'm doing is okay. The number I picked is okay.

CHAIRMAN WALLIS: So you have no mixing in the cold leg.

PROF. diMARZO: No mixing in the cold leg.

CHAIRMAN WALLIS: You have no injection in the cold leg?

PROF. diMARZO: No injection in the cold leg. No, there is no injection in this experiment.

CHAIRMAN WALLIS: And there's this uniform slug in the steam generator which it doesn't matter if it mixes or not because it's uniform.

PROF. diMARZO: Yes. So first of all, let me give you a sense, okay? The slug that I'm going to look to validate this assumption is of the order of 470 liters. Okay, I'm sorry, the scale of the slug is 470. The slug is 56 liters or so. In other words, if you take the volume where the slug sits in the plant and you take the volume where it sits in Maryland facility. The ratio is 470. The typical ratio of the facility is around 500.

These are the component. The steam generator tubes are 48 liters in Maryland and 41 cubic meter in plant with a ratio of again, around 500. The steam generator outer plenum is smaller in plant. It's bigger in measurement because those are the pieces you can buy. The cold leg suction is about right. The pump is a little bigger in Maryland but then -- bigger in plant, I'm sorry. And then cold leg discharge is a little bit smaller. Okay, so these are the numbers just to give you a sense.

So let's look, this is something you were referring to at the beginning. The slug is a cold water slug which is represented by this white portion here, injected artificially from the bottom of a cold leg with a hose and it fills up the steam generator up to here and this up to here. And at that point it's closed and this pump is started.

CHAIRMAN WALLIS: So what's the black stuff?

PROF. diMARZO: The black stuff is what was there which is original water.

MEMBER SIEBER: Original water.

PROF. diMARZO: Original a warm water, so we inject a colder water under it and we push it up and then times zero we start the pump.

CHAIRMAN WALLIS: No, in reality there's nothing above this slug. This is --

PROF. diMARZO: No, no, this has nothing to do with bottom mixing. This is simply to say --

CHAIRMAN WALLIS: Nothing to do with --

PROF. diMARZO: Nothing to do with bottom mixing. It's just to try to think where there mixing only in those two volumes analytical.

CHAIRMAN WALLIS: It's to test out your theory --

PROF. diMARZO: It's to test out the theory.

CHAIRMAN WALLIS: -- with some kind of experiment which isn't --

PROF. diMARZO: Doesn't have anything to do with bottom mixing, okay. But the nice important thing about this experiment is that this part will be pumped only through the pump. The front of the slug will only go through the pump.

CHAIRMAN WALLIS: This is a kind of system effects demonstration of the model fitting some data.

PROF. diMARZO: Correct. But the point is that the front of the slug goes only through the pump. See? But as the tail has to go through the steam generator outer plenum and through the pump, so it has to do that two in series. So that is the test that I want to run.

CHAIRMAN WALLIS: This is just a test. There is no vessel, there is no --

PROF. diMARZO: There is no considering anything. Now, initially the slug is here. Okay, what you've got there are all the liters as where the slug is and as you start pumping, you're going to start moving the slug and at the very end, it's all in the vessel and we can go into the details, but I don't think we have the time nor interest. This describes what it is. When it's like this, it's a mixed thing, so I don't know how much of each is in that particular piece of equipment.

So the model is close form in this particular case and the expressions are very simple, so I don't want to bother you with that. but this is what you get. The front goes only through the front which is a very small volume, so it's almost a shell front. The tail goes through both, goes through a big mix volume and then the pump, so it's much more gradual because of the subsequent mixing. And that's basically the data on top of it that you measure at that location.

Now, the problem was that we wanted to do a blind of this using OSU and that unfortunately never happened.

MEMBER RANSOM: One part of this scenario, this assumes that you've refilled the system before you start the pump, I guess, right?

PROF. diMARZO: No, no, the pump pumps and then that's it.

MEMBER RANSOM: But I thought you said there was borated water sitting on top of the de-borated water?

PROF. diMARZO: Yes, you pump that through and then it will end.

MEMBER RANSOM: Right, so the system is full.

PROF. diMARZO: No, no, no, the system --

MEMBER RANSOM: Full of water.

PROF. diMARZO: No, no, not really.

MEMBER RANSOM: You mean it's partially voided?

PROF. diMARZO: Yeah, the system is full up to here. When you flush all this stuff out, there is nothing left.

MEMBER SIEBER: Right.

MEMBER RANSOM: Why is the borated water

sitting on top of the de-borated --

PROF. diMARZO: It's not borated, it's warmer water.

MEMBER RANSOM: Huh?

PROF. diMARZO: It's just warmer water. Why do we have water there?

MEMBER RANSOM: Well, yes.

PROF. diMARZO: We have water there because we want to know how the tail mixes, how the back of the slug mixes and we need something above it to mix it with.

CHAIRMAN WALLIS: He's not modeling the reality of it.

PROF. diMARZO: It doesn't have anything to do with what I'm mixing. It's attempt to validate the assumptions made, that's all.

MEMBER RANSOM: Well does the tail have any significance as far as the accident is concerned?

PROF. diMARZO: None whatsoever. It's just as --

MEMBER RANSOM: So it doesn't matter then.

PROF. diMARZO: But it's a tool to show that if you pass it through these two volumes, you get the right results.

MEMBER RANSOM: Okay.

PROF. diMARZO: That is the only purpose of this.

CHAIRMAN WALLIS: Did you do any tests where you went on from this to put it through the vessel --

PROF. diMARZO: No.

CHAIRMAN WALLIS: You didn't couple this piece of it with the vessel part?

PROF. diMARZO: No.

CHAIRMAN WALLIS: That would be a nice test. You didn't do that. The guys didn't give you enough money or something?

PROF. diMARZO: The reason I'm behind is at the time --

CHAIRMAN WALLIS: It wasn't big enough, maybe this part wasn't big enough.

PROF. diMARZO: Right, at the time it was not big enough.

MEMBER SIEBER: Maybe they bid it and didn't come out.

PROF. diMARZO: So the problem is this, the simple idea of considering those mixing volume and the other on-mix provides you with results that at that scale, at least, work. And so that is the rationale for then using this in general for that. we do realize that this partial.

CHAIRMAN WALLIS: You're showing that having some mixing volumes and some pipes connected together sort of duplicating your model, can work out in terms of the experimental theory.

PROF. diMARZO: And the point again --

CHAIRMAN WALLIS: Really, as long as your model is true enough to reality, I think we'll believe it. We probably would have believed it before as long as it's true enough to reality.

MEMBER SIEBER: I think one of the key assumptions there is that the mixing volume is the component volume.

PROF. diMARZO: That's right, that's right.

MEMBER SIEBER: And if that wasn't a valid assumption, these curves would not match the --

PROF. diMARZO: Right, but now the component volume has the distinct advantage that there is a component here and there is a component at the proper typical scale. But as you first start saying, oh, gee 72 percent of the pump volume, we're going to start laughing here first, so that isn't going to go anywhere in a sense. Do you see what I'm saying? It's clearly a -- it's clearly a simple representation of the process but at this level, now, what are the alternative to that? Well, first of all, we could have done some more data to assume blind and see whether we were getting that. We were trying but we didn't succeed in that at that time.

Other options, there are not because RELAP is not in a position of giving us information on this because again, it depends on generalization so we can't go anywhere. CFD there is no question that you cannot do this thing for the level of detail, so that's that in a sense. And so that's what's been used to feed into the RELAP PARCS.

CHAIRMAN WALLIS: This is an interesting and probably useful model. Is there some way you can do some sensitivities or something? Well, you can't really because you've got to have perfect mixing here and no mixing there. You can't do partial things to do sensitivity studies.

PROF. diMARZO: We have done a lot of partial mixing, okay?

CHAIRMAN WALLIS: You have?

PROF. diMARZO: We have but my problem to you is the scaling issue. So I can play with it and we did and we did experiment in pipes and things like that but again, I cannot come here and say, okay, guys, this is going to scale, because it's not going to happen.

MEMBER RANSOM: In the report there was a lot of information on this international standard problem which was related to the same thing. Are you going to say anything about how that data was -- information is useful or --

PROF. diMARZO: The information concerning that was used here to validate the CFD calculation. The only purpose of that piece of information there was to show that the CFD calculation was representing well the reality that it was trying to model. That's the purpose of that data. There is no real other purpose of that.

DR. ROSENTHAL: Marino, let me just review this to make sure that I understand it. I take the maximum volume of slug that I can form by the geometry of the real player. I assume --

PROF. diMARZO: With any other volume, whatever you want, yes.

DR. ROSENTHAL: Okay, but I'm taking a max slug.

PROF. diMARZO: Yes.

DR. ROSENTHAL: I then mix that slug in a component --

PROF. diMARZO: In two components.

DR. ROSENTHAL: I don't mix it in the cold leg. I don't mix it in the downcomer, both of which would be conservative. I solely mix it in the lower plenum.

PROF. diMARZO: No, at that time you're done. You feed it to pipes and it's fully mixed, yes.

DR. ROSENTHAL: And then I mean, logically, and I fully mix it in the downcomer equivalent and so I'm conservative, conservative until the -- until the lower plenum.

PROF. diMARZO: Yes, no, the combination of --

DR. ROSENTHAL: But everybody here is saying, but hey for a high Reynolds number, we would expect --

PROF. diMARZO: No, downcomer to lower head I showed you what it does.

DR. ROSENTHAL: Everybody, I mean intuitively or whatever is saying we would expect it to be rather well mixed. I just can't quantify --

PROF. diMARZO: Right, I mean, if you want to go more --

DR. ROSENTHAL: That's my pump flow case. So that the argument is that overall it seems like it's a reasonable or reasonably conservative, total --

PROF. diMARZO: That's the bottom line. And so now, with this tool combined with PARCS RELAP, Dave is going to come in and essentially run you through the results, the so-called consequence. Now, there is one part which Jack alluded to which hasn't been told about which is how big is the slug. And that is what you are referring to are you doing some sort of a best estimates calculation and that's the part which I think is very relevant in all this and --

CHAIRMAN WALLIS: In this mixed lower plenum, you may have a collander and you may have perforated plates, but so if you see how the stream lines come from a downcomer, they're coming from all around the downcomer and --

PROF. diMARZO: No.

CHAIRMAN WALLIS: Just one side of the downcomer?

PROF. diMARZO: Yes.

CHAIRMAN WALLIS: One side of the downcomer?

PROF. diMARZO: Yes.

CHAIRMAN WALLIS: It's still difficult for me to see how stuff coming in one side mixes with stuff way on the far side.

PROF. diMARZO: There might be distribution.

CHAIRMAN WALLIS: Okay, but you know, if it just follows stream lines it's going to be very difficult for stuff flowing in here to go around the other side.

PROF. diMARZO: Sure, absolutely, but remember from the data it shows that there are fingers at different concentration. It's not that there are major streams at different concentrations. So when you go through all this colanders with something that is not really very large you get LOCA mixing.

CHAIRMAN WALLIS: Is this spread here -- I'm not sure, is it the fingers as a result cause this spread here?

PROF. diMARZO: No, this spread here, the spread you're talking about this one.

CHAIRMAN WALLIS: Is that because some of these are fingers on one side of the downcomer and some on the other?

PROF. diMARZO: Yeah, the -- I got data on that. When the slug comes in, the downcomer is low flow resistance in that angular part, in that open part. So when you're coming from this side, it would show up on the other side if we go all the way through. Remember that there is a very strong resistance to going to the core.

MS. NEWMAN: It doesn't all go through but there's a lot of it peels off on the way.

PROF. diMARZO: Fine, but the problem is there is very little resistance in that region compared to how much resistance is here.

CHAIRMAN WALLIS: It still peels off on the way, so this --

PROF. diMARZO: It does peel off on the way but it's like having a manifold with extremely large resistance on each --

CHAIRMAN WALLIS: You're saying it's a really good distributor.

PROF. diMARZO: Correct, it's a big manifold with very strong resistance at each outlet, that's what it is.

MEMBER RANSOM: Maybe one way to answer that question would be just say what happens if it does not well mix?

CHAIRMAN WALLIS: Well, then you're going to have trouble --

MEMBER RANSOM: Most of the cold water going up one portion of the core --

PROF. diMARZO: Right, if the cold water

goes down, it immediately gets sucked into the core in one location, yes, but the problem is that --

MEMBER RANSOM: What would the neutronics say about that situation? Is that worse or --

MEMBER SIEBER: Well, non-mixing case it the conservative case.

PROF. diMARZO: But remember that you're feeding into him the minimum of the minimum, not the average. If we were fitting into --

MEMBER SIEBER: -- cold water or the deborated water --

PROF. diMARZO: Yeah, but Vic, we are -- we can --

DR. ROSENTHAL: For the transcript, why don't we --

MEMBER SIEBER: One at a time.

DR. ROSENTHAL: -- one at a time. Your turn first.

PROF. diMARZO: Okay, we understand that there is a variation at the entrance to the core, but we are feeding the lowest concentration. We're not feeding the average. This curve is not here, it's on the minimum. So now it's as if you are feeding the minimum concentration everywhere.

MEMBER RANSOM: Right, right.

CHAIRMAN WALLIS: Which is conservative.

PROF. diMARZO: Which is conservative.

CHAIRMAN WALLIS: I like your argument about the distributor plate but if the resistance through the colander is governing everything compared with the resistance on the outside --

PROF. diMARZO: There are five colanders.

CHAIRMAN WALLIS: -- then you would expect it to be a pretty uniform -- once you've got all these little jets squirting into the space, things are going to mix up pretty well, a bit like the steam generator.

PROF. diMARZO: I would imagine it's very close to -- my vision of it is a manifold with very strong resistances on each --

CHAIRMAN WALLIS: I think that's a very good argument and I think if you -- are we going to go to the full committee with this? I think you ought to make that argument there and explain very clearly, this stuff flows in and squirts in there pretty uniformly through all those holes.

MEMBER SIEBER: You can't say that in the natural circulation case. That's only the pump case.

PROF. diMARZO: Yes, because the pressure drop isn't there.

CHAIRMAN WALLIS: Yeah, and that's the

circulation, maybe the buoyancy effects will be important.

MEMBER SIEBER: Yeah, and the Reynolds number is low and --

CHAIRMAN WALLIS: But then there's also the Richardson number.

PROF. diMARZO: But the natural circulation is a power up. Remember the issue is fuel damage. Natural circulation is a power up, it's not the --

CHAIRMAN WALLIS: It's not the problem.

PROF. diMARZO: It's not the problem.

CHAIRMAN WALLIS: Well, I think we're going to get to that.

MR. TRAIFOROS: The mixing in lower plenum is the problem?

PROF. diMARZO: No, no, because if you go slowly as you would go in natural circulation, you essentially don't have that initial spiking in reactivity.

MR. TRAIFOROS: But the minimum concentration was important, too.

PROF. diMARZO: Absolutely, but that's a power up --

MR. TRAIFOROS: It's not only the

velocity.

PROF. diMARZO: We are not saying that you are not going to power up. We're just simply saying that you're not going to master the fuel.

MR. TRAIFOROS: But if you have natural circulation, you expect lower concentration than what you are calculating now because this is validation. It's for the pump case or a lot of --

PROF. diMARZO: Not necessarily, I don't know. I really don't have any idea.

MR. TRAIFOROS: No, you don't know but I'm saying that --

PROF. diMARZO: I would imagine it would be not too different from this.

MR. TRAIFOROS: Can we extrapolate your result of mixing to lower flow?

PROF. diMARZO: My result of mixing are not based on velocity because, again, its model --

MR. TRAIFOROS: It's run -- you validated -- University of Maryland has high velocity tests.

PROF. diMARZO: Yes, but the problem is the mathematical expression, the velocity is gone.

MR. TRAIFOROS: Yes, but the reality natural circulation, I understand, you're not looking at LOCA effects, but in reality when you have lower flow, your validation of your simplified model may be questionable.

PROF. diMARZO: That's correct.

MR. BASSETTE: Under natural circulation the Reynolds number is still up around 100,000 in the downcomer.

PROF. diMARZO: Yes, it's still very high in Reynolds.

MR. BASSETTE: The Maryland experiment with the pump had Reynolds numbers in the downcomer similar to what the plant has in the natural circulation.

CHAIRMAN WALLIS: I don't think it's a Reynolds number problem.

PROF. diMARZO: No.

CHAIRMAN WALLIS: It has to do with buoyancy, buoyancy versus --

PROF. diMARZO: Be sure buoyancy has been explored and that's a relevant issue but there are -- we have experiment on very buoyant slug and very mixed slug and so forth but --

CHAIRMAN WALLIS: You do?

PROF. diMARZO: -- yes, but in terms of overall results, it's not that different.

CHAIRMAN WALLIS: Because if you did have a very buoyant slug it would certainly squirt through on one side of the core.

PROF. diMARZO: But you see a difference.

CHAIRMAN WALLIS: If you blew a bubble down the downcomer, very big density, it's not going to distribute itself uniformly across the core.

PROF. diMARZO: Absolutely.

CHAIRMAN WALLIS: It will squirt in on one side and --

PROF. diMARZO: No, it would come out -- a very buoyant slug would tend to come in from all sides. A very buoyant slug floats down all together.

CHAIRMAN WALLIS: Oh, because it's stratification. So that limit is okay, too.

PROF. diMARZO: It would be okay. I mean, I don't see -- we don't see much of a different in the two cases.

CHAIRMAN WALLIS: Okay.

DR. ROSENTHAL: Dr. Ransom, Dave had about an hour's worth of presentation. So -- and I assume he can talk fast.

CHAIRMAN WALLIS: I think he might take longer with questions.

DR. ROSENTHAL: I mean, it's your -- I mean including some questions and then I think it would be really good if we could just take five minutes at that end to just decide what to do with all this, so -- I mean, we're your servants.

CHAIRMAN WALLIS: I think we're so far along that we're pretty well committed to going to the full committee; isn't that the case?

MEMBER KRESS:: I think so. Don't you think so?

CHAIRMAN WALLIS: We can't sent it back again.

MEMBER RANSOM: I have very little experience in this so I'm going to depend on you two.

MEMBER KRESS:: We'll have to see things that are far enough along that they're -- we can say yes or no on them and I think this one is.

MEMBER RANSOM: You're pretty happy with this then?

MEMBER KRESS:: Oh, I like what I've seen so far.

CHAIRMAN WALLIS: It's far enough along we can say yes.

MEMBER KRESS:: Yes.

MEMBER RANSOM: It's unfortunate the report didn't convey a lot of this.

MEMBER KRESS:: I know, but this clarified a lot of it.

MEMBER RANSOM: But there still seems to be some open issues, I guess, natural circulation.

MEMBER KRESS:: Well, I think what diMarzo said about the natural circulation is fairly well --

MEMBER RANSOM: In other words, the Reynolds number being high?

MEMBER KRESS:: Yeah, and the buoyancy effects that are accounted for and the way it comes down to the downcomer. I'm pretty happy with the thing.

CHAIRMAN WALLIS: All right, if you're happy with it. Now, let's see if you're happy with what Dave has to say.

MEMBER KRESS:: Yeah, let's hear what Dave has to say.

MEMBER RANSOM: I don't have any problem with staying late tonight because I'm staying over.

MEMBER KRESS:: Me too.

CHAIRMAN WALLIS: We have to move on.

DR. ROSENTHAL: We can discuss the documentation issue at the end.

MR. BASSETTE: So I'm going to talk about consequences and probability to foreign dilution events. I'll start off with what's on the books right now in terms of regulatory basis for this kind of a reactivity insertion accident. The general design criteria 20 through 29 concern themselves with various aspects of radioactivity control and the one most relevant of these is GDC 28 which says, "Reactivity control systems shall be designed with appropriate limits, potential amount of rate, and so on, to assure that such events don't result in damage to the reactor coolant system pressure boundary greater than limited local yielding", that's pretty severe.

And "Nor should the sufficiently disturb the core, its support structures or other reactor vessel internals to impair core coolability". Aside from there is no direct regulatory guidance for boron dilution accidents but we do have regulatory guide 1.77 which was written with rod ejection accidents in mind. And this reg guide 1.77 identifies a limit for a peak fuel enthalpy, that's average enthalpy of 280 calories per gram. And this limit was determined from experimental data that existed in 1974 and basically it corresponded to the point at which we start to get significant LOCA U-02 melting and cladding failure basically by melting and plastic floating cladding. This 280 calories per gram was based on the way they measured enthalpy in these experiments where they actually measured total energy deposition and rather than a peak.

They didn't measure fuel pellet enthalpy. They measured total enthalpy deposition through coolant temperature measurements. And when you use like some sort of a fuel code to back out from that 280 calorie per gram what the peak fuel enthalpy was, you get a number like 230 calories per gram. So basically at 280 calories per gram when identified in reg guide 1.77 should really be interpreted as a peak fuel pellet enthalpy of 230 calories per gram. That's what's on the books.

So what are the basic probability considerations for boron dilution event you have to start off with a small break LOCA and we're talking about a break size of about 1.4 to 2 inches and the frequency for that type of event is about $2E_{-4}$ per year.

MEMBER KRESS:: Where does that number come from?

MR. BASSETTE: This is the -- you see this kind of number in different places over the years. this $2E_{-4}$ I'm quoting from the SECY 04-60 I think it is, the SECY that was issued earlier this year --

MEMBER SIEBER: For LOCAs.

MR. BASSETTE: -- for LOCAs, yeah, it talks about the frequency of LOCAs for different categories of break size from very small breaks up through very large breaks.

MEMBER KRESS:: That was in the expert opinion.

MR. BASSETTE: That was in the expert opinion which is still going on and still being finalized so this is the numbers in the SECY that was issued this year.

CHAIRMAN WALLIS: It seems like a pretty low number.

MEMBER SIEBER: It's a pretty small part.

CHAIRMAN WALLIS: Just for the event itself with no mitigation or anything.

MR. BASSETTE: Yeah, that's exactly so. You're starting off with a low number. You also have to include this category of events which is --

CHAIRMAN WALLIS: That's also a small LOCA really.

MR. BASSETTE: -- which is also a small LOCA, exactly the same right size the you're looking for.

CHAIRMAN WALLIS: That's the frequency? I mean at the time of TMI it seemed to be happening every few months.

MR. BASSETTE: Well, you might have had valve --

CHAIRMAN WALLIS: We had lots of problems with torbs in those days, that's all been fixed now so it doesn't happen any more?

MEMBER KRESS:: That's per plant.

CHAIRMAN WALLIS: Oh, that's per plant, okay.

MR. BASSETTE: Per ECY year, so it's like one --

CHAIRMAN WALLIS: There's 100 plants.

MR. BASSETTE: -- one every five years.

CHAIRMAN WALLIS: Oh, so things have improved since --

MR. BASSETTE: Yeah, things have improved since the TMI. So the other thing is the initiating event itself is not alone to cause -- by itself does not cause substantial dilution of loop seals. The reason is there is insufficient time spend in a dilution mode and there must be additional failures or operator errors. So from these numbers you have a subset of what we call boron dilution small break LOCAs of a lower probability and for example, one of the things you'd probably need to do is completely fail HPI and to do that, you're talking about a factor of 10^{-2} to 10^{-3}.

CHAIRMAN WALLIS: By blocking the sun, for instance?

MEMBER SIEBER: No for high pressure.

CHAIRMAN WALLIS: So dilution comes from somewhere else.

MEMBER SIEBER: Not for high pressure.

MR. BASSETTE: It's basically like a station blackout or something.

CHAIRMAN WALLIS: HPI does not come from the sump.

MR. BASSETTE: It comes -- your first --

MEMBER SIEBER: RWST.

MR. BASSETTE: Your first hour of HPI is not coming from the sump, it's coming from the storage tank.

MEMBER KRESS:: So it's 10^{-6} or 10^{-7}.

CHAIRMAN WALLIS: Before you've even started to analyze it.

MR. BASSETTE: So, yeah, before you've gone too far, you're down to 10^{-6} or thereabouts.

CHAIRMAN WALLIS: Also it's only at certain times in the fuel cycle.

MR. BASSETTE: Yes, yes.

CHAIRMAN WALLIS: We're going to make this thing go away before you've even done anything.

MR. BASSETTE: Yes.

MEMBER SIEBER: That was the plan.

MR. BASSETTE: So this was where we started off with a couple years ago when we did a prioritization study when it's first -- this GSI was first proposed and in that study these are the numbers that were used, 10^{-3} for the initiating event, this is early in the fuel cycle.

CHAIRMAN WALLIS: That should be higher than that, shouldn't it? It's not just a few days. It's actually quite a long time.

MR. BASSETTE: In their study, it was about -- looking at an 18-month cycle, it was about two weeks or so?

CHAIRMAN WALLIS: That's all?

MR. BASSETTE: Yes.

CHAIRMAN WALLIS: That's amazing. I thought there was a lot more reactivity in the boron.

DR. ROSENTHAL: It's okay, it's okay, because that's the number that was used in the prioritization, five percent that we heard from Dave Diamond that a better number might be 20 percent.

CHAIRMAN WALLIS: That's more like it.

DR. ROSENTHAL: So let's bump it up on order of magnitude, you know, in our mind, and continue on.

MR. BASSETTE: Well, we can put a multiplier of four on there if we want to.

CHAIRMAN WALLIS: That's more reasonable.

DR. ROSENTHAL: Okay, make it $5E_{-1}$ instead of $5E_{-2}$. Continue on.

MR. BASSETTE: The probability of slug formation the initial study assumed it was one and we are giving it a probability of about 10 --

CHAIRMAN WALLIS: That's because the operators wouldn't do the right thing to make it happen?

MR. BASSETTE: Yeah, because you have to fail HPI or something like that.

CHAIRMAN WALLIS: All sorts of stuff, yeah. And the operators have to do things, too, right?

MR. BASSETTE: Yeah, and then when you're looking at their pump restart, this was given a probability of .1 and we did some actual human factors analysis at Brookhaven and at Sandia and they both came up with a number like .01 probability that the operator would turn on the reactor coolant pump when he was totally not supposed to.

MEMBER KRESS:: So this is a drop priority then.

MR. BASSETTE: The initial prioritization it was kind of a marginal event to begin with. It's $5E_{-6}$ and we see it more like E_{-8}.

MEMBER SIEBER: Which after our adjustments, E_{-7}.

CHAIRMAN WALLIS: And that doesn't take account of the -- that accounts for all the probabilities of a pump start and all those things which might damage the fuel? The pumps were all self-analyzing and everything on top of this.

MR. BASSETTE: Well, pump restart is here.

CHAIRMAN WALLIS: Okay, so it accounts for everything he's done, too.

DR. ROSENTHAL: And let me remind you that the -- this is the estimate of an event that may damage some fuel and we're normally in terms of -5, -6 for damage.

CHAIRMAN WALLIS: That's based on these 19 calories per gram or something like that?

MR. BASSETTE: Yeah, I'll get into that.

CHAIRMAN WALLIS: You will get into that.

DR. ROSENTHAL: Okay, moving on.

MR. BASSETTE: So in the consequences, we did some sensitivity studies on the -- we're focused now already on the -- from here we can see we start to focus on B&W because we did some sensitivity studies on different slug sizes and below about 11 or so cubic meters, we don't go recritical. And with CE and Westinghouse plants we're dealing with maximum loop seal volumes three and a half cubic meters, so we're well below the possibility of going recritical.

At 14 cubic meter slug, this is all with restart of the reactor coolant pump, we start to go recritical. The peak centerline temperatures are at about 2,000 degrees C which is normal operating conditions. Increased it some more to 18 cubic meters is when we start to see consequences. So you can see where the dividing line is on the slug size. We start to see peak enthalpies of about 175 calories per gram. We start to expect centerline melting. We're still below the reg guide 1.77 limit.

But -- and we're still below our cladding failure threshold that we've put in the research information letter earlier this year. And these are for increases of 170 calories per gram for basically low burn-up or nearly fresh fuel and it is a decrease in two steps down to 100 calories per gram for intermediate burn-up and 75 calories per gram for high burn-up. And this burn-up basically is a surrogate for prior oxidation of the cladding, how much cladding is oxidized before you start this event.

MEMBER KRESS:: These are acceptance criterias.

MR. BASSETTE: Excuse me?

MEMBER KRESS:: These are acceptance criteria?

MR. BASSETTE: These are threshold -- you might say these are threshold criteria for when you might start to see clad cracks.

MEMBER KRESS:: So there's not bee acceptance criteria for --

DR. ROSENTHAL: Yeah, we have to be regulatorily careful here. The reg guide 1.7 are the regulatory limits. Research wrote a research information letter from Research to NRR which they are acting on, which says that we believe that lower numbers for incremental enthalpy rise are more appropriate to avoid what we called cladding failure threshold limits. Some time in the future the regulatory numbers ought to get straightened out. There's also an EPRI document before the staff for review. But for the point of this presentation, we just would like you to compare those numbers to the 50.

MR. BASSETTE: Yeah, these lower limits are associated with the cracking of the cladding.

CHAIRMAN WALLIS: Why are we comparing it with 50 and not 175? The step is 50.

MR. BASSETTE: The step. Well, see --

DR. ROSENTHAL: We talk about ductility and --

MR. BASSETTE: Yeah, that's another thing. These failure thresholds are associated with step increases in enthalpy because that's the way these reactivity insertion accidents are run. They have a power pulse. They have a single step --

CHAIRMAN WALLIS: In fact, the melting has nothing to do with the step. It has to do with the absolute enthalpy.

MR. BASSETTE: Yeah, these limits are basically a high stress, low ductility failure of the cladding, cracking of the cladding. If you allow time for the cladding to heat up and become ductile, you won't get -- you won't see these kind of failures. You'll see failures from melting of entire enthalpies.

CHAIRMAN WALLIS: Now, when you go to faulting why does the peak enthalpy step go down?

MR. DIAMOND: Excuse me, I think there might be a typographical error on that slide and the 30 applied to the 18 cubic meter slug and the 50 to the 40 cubic meter slug.

CHAIRMAN WALLIS: That would make more sense but is that true?

MR. BASSETTE: I'd have to look again to make sure --

CHAIRMAN WALLIS: Staff never makes typographical critical errors like that.

MR. BASSETTE: This is what I recall from when I last saw this.

CHAIRMAN WALLIS: Does it make sense though? You've got more slug and you've got less effect.

MR. DIAMOND: If we look at the --

MR. BASSETTE: No, it's a coupled thing.

MR. DIAMOND: As a matter of fact, it's in your slides here. It looks like it goes up to 50.

CHAIRMAN WALLIS: Wasn't there a 90 that

came from what we heard earlier? Where did that come from?

MR. BASSETTE: That's the restart of natural circulation. These are all restart reactor coolant pump cases. We ran restart reactor coolant pump feeding these different slugs.

DR. ROSENTHAL: So this is not the natural circulation case.

CHAIRMAN WALLIS: Diamond had both of them, didn't he, earlier on today?

MR. BASSETTE: Yeah, he had showed -- but he didn't show this. He showed this bottom one.

CHAIRMAN WALLIS: He showed 90 for a natural circulation.

MR. BASSETTE: Yeah, and he showed this one for --

CHAIRMAN WALLIS: Well, we had 180 or something.

PROF. diMARZO: 180 is the other, total.

MR. BASSETTE: Yeah, this is the one he showed.

CHAIRMAN WALLIS: It's changed from 10 to 180 so the change is 170?

PROF. diMARZO: No, it's the 190.

CHAIRMAN WALLIS: Well, what's this peak

enthalpy step got to do with things? I mean, it goes up in steps but what you care about is how far it gets to, don't we?

MR. BASSETTE: Well, you care about both. you care about both because a step -- your first step especially, determines whether you get these -- you have to worry about these failure thresholds for the cladding. So basically you start to look at the peak enthalpy and --

CHAIRMAN WALLIS: Well, there must be a limit, some exceptions for the peak enthalpy. You're still going with 290 for the peak?

MEMBER SIEBER: 230.

MR. BASSETTE: This says 230.

DR. ROSENTHAL: If you melt the fuel, I believe you get a volumetric expansion of about 130 percent or some number like that.

MR. BASSETTE: It's not that much.

DR. ROSENTHAL: It's smaller, and that fails the clad, so that is just done. Now, these activity insertion events, you're talking about a very fast time scale in which the fuel is heating up and the clad can't heat up fast enough to grow to get out of the way of that clad and that's why the increments are --

CHAIRMAN WALLIS: That's the step.

DR. ROSENTHAL: That's the step.

CHAIRMAN WALLIS: We talked about that earlier, yeah, the step must have some role in all of this.

MR. BASSETTE: Yeah, because these thresholds are associated with step increase and it's because you're dealing with a different failure mechanism here than you are here. This is -- this kind of failure mechanism is basically --

CHAIRMAN WALLIS: Well, is this problem going away because the probability is 10^{-8} or because together with these somewhat uncertain consequences of 190 not being too far away from 230 is acceptable?

MEMBER KRESS:: No, it's a regulation on the book and it --

CHAIRMAN WALLIS: It's a combination of the two.

MEMBER KRESS:: No, you go -- it goes away on a generic issue because they drop it because of the low probability but it's still on the books as a regulation.

MR. BASSETTE: Yeah.

MR. DUDLEY: This is Noah Dudley.

MR. BASSETTE: The thing you're still

worried about is core coolability that was GBC 28. So and how do you measure core coolability? Well, you start to say, let's to a failure --

MR. DUDLEY: Yeah, this is Noah Dudley and I suggest that the staff is approaching the justification for closing the GSI based on consequences being low and based on --

MEMBER SIEBER: Probability being low.

MR. DUDLEY: -- the probability being low and then taking a look at the consequences and what we're looking at here is the consequences and we find those low or negligible for Westinghouse and CE and we do find there are consequences of fuel damage failure, probable damage for B&W with a pump start.

MEMBER KRESS:: But they meet the criteria that are in the regulations right now.

MR. DUDLEY: That's correct.

MR. BASSETTE: And this slide says we have all this criteria and for the natural circulation restart for B&W their below these limits so the consequences are negligible.

CHAIRMAN WALLIS: So even if the probability were 8_{-4} you'd still say it was okay because the consequences are okay.

MR. BASSETTE: Yeah, so you might worry if it was low probability, high consequence but if it's low probability, low consequence then it's a --

CHAIRMAN WALLIS: It's just the low consequence part seems wishy washy that we talk about there isn't really a regulatory position and this comes from somewhere else from reactivity insertion accidents and in fact, there's certain kinds of old fuel which might be damaged at lower peak enthalpies and all that. So it's a little bit uncertain.

MR. BASSETTE: Well, I don't know if it's wishy washy but it's complicated.

CHAIRMAN WALLIS: Well, it's not clear what the limit should be. If you had old fuel with this being zapped with 190 calories per gram, it might well leak.

MR. BASSETTE: But then old fuel doesn't get to 190 either.

MEMBER KRESS:: It can't get there.

CHAIRMAN WALLIS: Because it's in a certain place in the --

MEMBER KRESS:: Because it's old.

MR. BASSETTE: It's low reactivity.

CHAIRMAN WALLIS: Low reactivity so it doesn't produce so much heat.

MR. BASSETTE: That's right. This color

doesn't show up so well.

CHAIRMAN WALLIS: It would be good, perhaps, if you could show some results for the old and new fuel in the same table there instead of just having one number. Wouldn't that help?

MR. BASSETTE: I'll have Dave Diamond send me that. This is basically the loop seal volume. It's kind of highlighted here. It's the lower third of the generator along with the cold leg piping up to the level of the pump.

CHAIRMAN WALLIS: That's the 40 cubic meters.

MR. BASSETTE: That's the 40 cubic meters.

CHAIRMAN WALLIS: And the volume of the reactor is -- the volume of the core is --

MR. BASSETTE: The volume of the core is about 40 cubic meters.

CHAIRMAN WALLIS: About the same.

MR. BASSETTE: And the downcomer and lower plenum are together about 40 as well.

MEMBER RANSOM: What is the inventory fraction? I guess I could figure it out but do you know? About 65 percent or less?

MEMBER RANSOM: For this inventory?

MEMBER RANSOM: The inventory that you

were -- when it's filled to the cold leg?

MR. BASSETTE: Oh, it's about 60 percent.

MEMBER RANSOM: Okay, and the natural circulation cessation is around 65 percent I guess.

MR. BASSETTE: It's complicated in the B&W plant. For a Westinghouse CE design, you lose two-phase natural circulation at about 60 percent. In B&W because of the elevations and all that. It's a little more complicated. It depends. The secondary site level only goes to about 20 feet but then at times they'll have the auxiliary spray on, auxiliary feedwater on and if feedwater is on, this comes in through spray nozzles out here. So it really depends if feedwater is on or off as to where your tendencies for natural circulation but basically you lose liquid continuous natural circulation once the level drops below the top of the candy cane, so that's about 90 percent or so. And then you regain circulation when your level drops here which is about 85 percent but you're in a boiler condenser mode. Your natural circulation is by producing vapor and condensing vapor.

CHAIRMAN WALLIS: And it restarts when you fill the system up enough so that you can get up over the top again?

MR. BASSETTE: Yeah, so eventually when you turn HPI on, you're going to get the level up to where it's up here again.

MEMBER SIEBER: There is no vent up there, though, so it's hard to do.

CHAIRMAN WALLIS: Well, when that starts, doesn't it begin to dilute the back end of the slug?

MR. BASSETTE: Yes.

CHAIRMAN WALLIS: Well, is that in diMarzo's model?

MR. BASSETTE: We don't take credit for that.

CHAIRMAN WALLIS: Well, it takes quite awhile to get that slug out.

MR. BASSETTE: Chances are it's going to destroy the slug before you start up a natural circulation.

MEMBER RANSOM: It looks to me that you go through some cycles of HPI injection, you know, loss of natural circulation and HPI injection which puts in a lot of borated water well ahead of ever getting down to this 40 cubic meters of deborated slug.

PROF. diMARZO: Just if I can comment on natural circulation, you have two possibilities in natural circulation. If you refuel very fast, that means you have a lot of subcooling going on in the system. In that situation, our experience has been the natural circulation starts intermittently because you got the warm up and then moves a little bit and stops and goes. If you start moving and stopping that slug, you're not going to find much of it left by the time it gets down there. The other option is to restart natural circulation refuel slowly in which case you can start natural circulation at once and go.

But in that case, as soon as you start putting water on top of the slug, because you're doing it slowly, you're going to mix it and so that's like, Graham said, you're going to chew the tail of the slug. So it's kind of a compromise but the end results say for example in PKL 2 is extremely difficult to get this thing together and go into natural circulation.

MEMBER RANSOM: Yeah, what I'm wondering is if there are other degrees of conservatism that haven't been considered.

MR. BASSETTE: Yes, there has been serious doubt about whether you'd even have these slugs to begin with. These are the boron concentration boundary conditions we fed to the RELAP PARCS analysis for the four basic sets of scenarios we looked at, Westinghouse, CE and --

CHAIRMAN WALLIS: This non-dimensional slug transit time overemphasizes the Westinghouse effect. The Westinghouse slug is very much smaller but it looks sort of comparable here because of the way it's non-dimensionalized, isn't it?

MR. BASSETTE: That's right, yeah. Yeah, it makes these plants look similar when, in fact, they're much different. I think Dave Diamond showed this one or something quite similar.

CHAIRMAN WALLIS: This one, he showed this one.

MR. BASSETTE: This is the boron concentration. This is for a restart of natural circulation in B&W. The boron concentration drops to zero and this is in terms of the boundary condition we feed to RELAP PARCS and then we feed the power history here, basically introducing power for about 40 seconds or so while the slug is being transported to the core. This is the maximum fuel pellet enthalpy from that trace. Now, this is the 90 calory per gram you're remembering. This is formed the natural circulation case. You see this step increase of about 25 calories per gram and another increase up to about 90 and then so on.

This is the -- that was the natural circulation restart. This is for the pump restart.

CHAIRMAN WALLIS: And the log scale enables you to capture the peak power.

MR. BASSETTE: Yes, where you'll notice it's up at 2500. So this is the boron concentration in the power. You can see this very high first peak and then it's -- it stays roughly around 100 percent, 50 percent and 200 percent. And this is the peak pellet enthalpy for the pump restart case. You see it goes up. The first increase is about 20 calories per gram and then of course, in about four seconds it goes up to about 185 and then goes down again. And so basically the slug transit time here is about six seconds through the core from beginning to end.

To give you some reference points, for these enthalpy numbers, at standby conditions let's say for everything is at 550 F, about 15 calories per gram. At full power, core average enthalpy is 50. Again at full power to peak pellet radial average is about 100. And the peak pellet radial average for onset of central line melting is about 150.

CHAIRMAN WALLIS: You get that in some operational transients, don't you? You get actually up to that?

MR. BASSETTE: There are centerline melting, I think that's one of the design limit criteria --

MEMBER SIEBER: You aren't supposed to.

MR. BASSETTE: -- is to avoid centerline melting. It's one of the basic design criteria. And here's the reg guide 1.77. The limit is 230 and this is --

CHAIRMAN WALLIS: Is that a very old reg guide? That's one of these that hasn't been revised.

MR. BASSETTE: Since 1974.

CHAIRMAN WALLIS: Yeah.

MEMBER KRESS:: What is that number?

MR. BASSETTE: This is the actual U-02 melting point, it's 267.

MEMBER KRESS:: 267 what?

MR. BASSETTE: Calories per gram.

MEMBER SIEBER: That's an enthalpy, right?

MR. BASSETTE: Enthalpy, yes. So that's like 5000 F at that point.

CHAIRMAN WALLIS: That's distributed uniformly across the whole thing?

MR. BASSETTE: Well, wherever it is.

MEMBER SIEBER: Wherever it is.

CHAIRMAN WALLIS: What happens to the

cladding by then?

MR. BASSETTE: So basically this old reg guide 1.77 limit you can see that for these experiments quit a bit of the fuel has probably gone -- had reached melting.

MEMBER SIEBER: In these pulse type transients, however, the clad is probably not much different after the transient than before because it's so short.

MEMBER KRESS:: Yeah, it doesn't have time to transfer the heat.

MEMBER SIEBER: In fact, these pulses, if you look at the plutonium particle size data where you put a pulse into a plutonium fuel rod and it can burst through the clad, these pulses are 100 times shorter than the required pulse width, pulse length, to cause a clad perforation by that mechanism. So these are really, really short pulses.

CHAIRMAN WALLIS: Now, you might get some fuel damage but there's really no --

MEMBER SIEBER: It's going to be in the pellet though, as opposed to the clad.

CHAIRMAN WALLIS: -- there's no --

MEMBER SIEBER: Well --

CHAIRMAN WALLIS: -- so there's no real

risk to the public. There's a risk to the owner of the reactor, you'd want to clean things up a bit and fix things.

MEMBER KRESS:: The regulatory guide 1.77 limit at 230. Above that, you start getting fuel coolant interactions. You might blow something.

MR. BASSETTE: Yeah, that's how the limit was established because above those enthalpies, you started to see pressure pulses in the coolant in these experiments. Below there was no pressure pulses.

CHAIRMAN WALLIS: This is all based on what happens to the uranium, not what happens to the cladding?

MR. BASSETTE: Well, it's both, it's both.

CHAIRMAN WALLIS: When you talk about centerline temperature and so on, what's happening to the cladding? I'm presumed it's still being cooled by the water without going through DMB?

MR. BASSETTE: Well, I'll show you. That's on this --

MEMBER SIEBER: It depends on where you started that.

MR. BASSETTE: It's right here. So we're looking basically now at B&W with the maximum slug of 40 cubic meters. Here's the consequences for restart of natural circulation and restart of the pump. The maximum enthalpy at 90 calories per gram here. That's definitely -- that should be 185 calories per gram.

CHAIRMAN WALLIS: You've got to clean things up here. You've got to fill in that blank space, too.

MR. BASSETTE: Yeah, I asked Dave Diamond to get me that. I haven't got it yet. I think that's 2500.

MEMBER SIEBER: Yes.

MR. DIAMOND: Yes, it's 2700.

MEMBER SIEBER: That's the big one.

MR. DIAMOND: And the 85 calories should be about 185.

MR. BASSETTE: Yeah, it should be 185, that's right. Fuel centerline temperature of 2000 degrees C here which is basically normal operating conditions and here you've got -- you're definitely getting melting and the cladding, the minimum DMBR here's 1.3 so you haven't entered dry-out and here, obviously, you're in dry-out.

MEMBER SIEBER: It's already happened.

MEMBER RANSOM: I have one concern. How confident, I guess are you in the 40 cubic meters per slug and based on your previous discussion of where natural circulation ceases and, you know, boils down to this cold leg level, why can't you get a situation where you fill up the steam generator and there would be some flow back into the core too, but as high as the candy cane, which presumably would give you a much larger slug of water and --

PROF. diMARZO: I can address that.

MEMBER RANSOM: Is that impossible?

PROF. diMARZO: It's impossible because in order have BCM, you need to have virtually -- you need to be at mid-level of the steam generator roughly in terms --

MEMBER RANSOM: You need what?

PROF. diMARZO: The collapse liquid level in the primary should be around the mid-level of the steam generator or so. If you exceed that, you're going to start getting carry-over on top of the candy cane and you're going to mix what you're generating. So in order to have a clean slug --

MEMBER RANSOM: Why do you get carry-over?

PROF. diMARZO: Because it is a two-phase flow off that cold leg -- off that hot leg.

MEMBER RANSOM: Hot leg?

PROF. diMARZO: Yes, so basically, you start spilling over. And so then you deborate essentially. So in order to keep --

MEMBER RANSOM: Okay, you've got to keep the hot leg clear.

PROF. diMARZO: You've got to be very low.

MEMBER RANSOM: Yeah, I remember that now.

MR. BASSETTE: There's also, you get a lot of in-vessel circulation in B&W because you have about eight square feet of bed valve area and the RELAP for these small braces are about 200 kilograms a second flow going through these vent valves.

MEMBER RANSOM: Those are in the downcomer areas, right?

MR. BASSETTE: Yes, to that you get a lot of in-vessel circulation.

MEMBER RANSOM: Okay.

CHAIRMAN WALLIS: Does this help dilute the slug, too?

MR. BASSETTE: It would.

CHAIRMAN WALLIS: Yeah.

MR. BASSETTE: We didn't take into account HPI in-vessel circulation and so on.

CHAIRMAN WALLIS: So your 40 cubic feet is based on the worst possible case where the level is somewhere up the steam generator? It's half way up. I thought it was just --

MR. BASSETTE: It's one-third of the way up the steam generator.

CHAIRMAN WALLIS: Actually one-third of the way up.

MR. BASSETTE: Yeah, to the level of the cold leg.

MEMBER SIEBER: The cold leg.

CHAIRMAN WALLIS: So it's a dropped steam generator. It's down.

MR. BASSETTE: That's right. So we took that as our maximum volume and said we can't be any worse than this.

MEMBER RANSOM: I think, isn't there some discussion, too, about the length of time it would take to actually achieve this and --

MR. BASSETTE: Yes, I'll get to that.

MEMBER SIEBER: It's in the --

CHAIRMAN WALLIS: Maybe you start the pumps before this has ever happened.

MR. BASSETTE: So what do we need to form a -- dilute a loop seal? We have to have small break LOCA, welling in the core. The steam generator has to be the heat sink and the liquid level on the primary side has to have declined sufficiently to prepare the vapor phase condensation path to get the dilution effect.

CHAIRMAN WALLIS: And this has never happened before?

MR. BASSETTE: Not too much. Oddly enough TMI had these idea conditions and in fact, there was some dilution seen at TMI.

MEMBER SIEBER: Well, they tried to start the coolant pump a number of times and eventually was successful and that got the core average temperature down but not the peak.

MR. BASSETTE: But it is interesting that TMI did achieve these conditions for awhile. But even if you achieve them, it's difficult to maintain them because they're a function of decay heat, break flow, HPI flow, all of which are varying with time. So what do you need --

MEMBER RANSOM: Well, did TMI achieve this partly as a result of shutting off the HPI?

MR. BASSETTE: They shut of the HPI. They had a break that they had open and then closed and they had intermittent feeding of the generators while they -- with a low level.

MEMBER SIEBER: And they had a level in the cooling system at right about the right place.

MR. BASSETTE: So they had the necessary

conditions. Now if they had fed the generator more than they did, they wouldn't have had the accident. they would have had enough decay heat removal.

CHAIRMAN WALLIS: If they hadn't left the trellis closed --

MR. BASSETTE: Yeah, could have, should have, would have.

MEMBER SIEBER: But they still would have depressurized the cooling system which was --

CHAIRMAN WALLIS: Eventually.

MR. BASSETTE: So decay heat must exceed the energy removal from the break and for break sizes, what we are dealing with. If the break is open, it's sufficient to remove decay heat. And in addition, HPI, if it's on -- it's actually a little surprising, it's the best sensible heat capacity, HPI flow to remove decay heat. And as I said, primary level must be below secondary level to have a condensing surface and contrary to TMI, the level must be above the top of the core or else you have other things to worry about. So the best way to get these conditions is open a small break, keep HPI or at least degrade it until the inventory drops into the hot leg which is about 60 percent as I mentioned in initial. Then close the break where gain, you force the generators to act as a heat sink.

CHAIRMAN WALLIS: How do you close the break if it's a broken pipe?

MEMBER RANSOM: Close the valve.

MR. BASSETTE: Well, the best process is a stuck open valve that you close. But that's why simply having a smaller break LOCA is not going to do it for you. You have to have some LOCA that you isolate.

MEMBER SIEBER: Well, you don't get the separation phenomena.

MR. BASSETTE: Yeah, you have the break in order to lose the inventory. You have to keep the HPI off and then you've got to close the break still keeping HPI off. And then you operate in boiler condenser mode for approximately one hour and the experiments at PKL, it took them one hour to dilute the loop seals from the initial value of 1,000 ppm down to 50 ppm. The University of Maryland they did the same -- they used boron in these experiments, soluble boron. The Maryland experiments used salt. It took them 70 to 90 minutes to dilute the loop seals.

CHAIRMAN WALLIS: Well, the best prospect is a stuck open pressurizer or would they actually close the block valve and then they get nervous about something else and turn off the HPR and it just gets -- at TMI they succeeded -- they didn't close the block valve, see doing a lot of things which you wouldn't have expected.

MR. BASSETTE: Yes. Yeah, at TMI they closed the block valve at 140 minutes.

CHAIRMAN WALLIS: Too late.

MR. BASSETTE: At HPI lost all the time. They had a line partially where a block valve was open but anyway they did get some dilution at TMI.

CHAIRMAN WALLIS: The level was too low by then to --

MR. BASSETTE: Yeah, the level was already below the top of the core by then.

MEMBER SIEBER: Well, and they weren't too fast in closing the block valve because it was in the next shift.

MR. BASSETTE: That's right.

CHAIRMAN WALLIS: Until the next shift arrived.

MEMBER SIEBER: They had to go through a shift change.

MR. BASSETTE: So break size, of course is a factor. Very small breaks HPI is sufficient to compensate for break flow to maintain control of things, keep -- the pumps aren't tripped. This is the breaks range that you're interested in. It's big enough to lose sub-cooling which the operators trip the pumps. With an ordinary short break you may have a short period of time where you have natural circulation. The duration depends on the size of the break. And this will continue as long as the primary system temperature is above the secondary system temperature.

MEMBER SIEBER: And the RCS is full.

MR. BASSETTE: And the RCS is pretty full, yeah. Anyway there's a limited time in this ordinary sequence to maintain quality condenser mode or reflux condensation. You get to breaks much larger than two inches, and the inventory and pressure decrease so rapidly that you just won't have any substantial rate of natural circulation. We looked at -- we had relap calculations available for all three vendors for break spectrum. So for a 1.4 inch break, we didn't lose inventory control. We don't lose force circulation. If you double the break size to two inch, now this is break diameter. So you double the break area to a two-inch break. If you're just -- all your decay heat was going into boiling, this is the kind of vapor generation rate you get.

In the RELAP calculations, it's difficult to say exactly but you're in BCM for about 1,000 seconds and the reason you're in BCM at all basically is you get a lot of -- you're dealing -- most of your energy source is not the decay heat as much as it is the initial system energy that you're still trying to get rid of. And we see --

CHAIRMAN WALLIS: Well, you're still on your small proportion there at the 35 kilogram per second ends up as condensate in 1,000 seconds, it's only one kilogram per second that condensate?

MR. BASSETTE: Yeah, well, some of it is going out the break.

CHAIRMAN WALLIS: Yeah, but that still seems a small amount.

MR. BASSETTE: Well, let's see, assuming you're getting let's say -- I mean, potentially you could have 3,500 kilograms, no 35,000.

CHAIRMAN WALLIS: Thirty-five thousand, that's a lot more than one.

MR. BASSETTE: So most of it's going out the break or -- but -- all right, so this is what we get from RELAP. But this time in BCM, you've got to realize most of the energy producing is going out the break.

CHAIRMAN WALLIS: It's funny because with the bigger break, 22.8 inches, you apparently build up more condensation, so you have less going out the break. It doesn't seem to make sense somehow.

MR. BASSETTE: It's doesn't which --

CHAIRMAN WALLIS: I wonder if that 1400 is a different typo.

MR. BASSETTE: It's difficult to get this out of RELAP. I don't trust the -- I got this from looking at the vapor generation but I don't particularly trust these numbers. But I think --

DR. ROSENTHAL: Let's rip that slide out and move on.

MR. BASSETTE: So at any rate, for a B&W steam generator with decay heat levels and so on, you're looking at roughly 60 minutes or so boiler condenser mode to dilute the loop seal and that's apparently on the same order of what we've seen in the experiments.

CHAIRMAN WALLIS: What do you have on the previous slide, the one you're going to rip out? You had --

MR. BASSETTE: Oh, the previous slide it said -- the previous slide as best as I can tell from

RELAP, which I'm not quite -- I don't quite trust, we saw dilutions of about three to five percent.

MEMBER RANSOM: Are those dilutions, that column?

MR. BASSETTE: Well, what I should say, the condensate as a percent of the volume.

MEMBER RANSOM: It's three percent of the volume of the loop seal?

MR. BASSETTE: Yeah.

CHAIRMAN WALLIS: So the volume when you get the 40 cubic foot slug, how much mass is that?

MR. BASSETTE: That's 35,000 kilograms.

CHAIRMAN WALLIS: So that's the worst case and you need to get to the worse case to have any consequences.

MR. BASSETTE: Yes.

CHAIRMAN WALLIS: So your message here would be that you never get to the worst case?

MR. BASSETTE: That's right. If you remember earlier in the slide I --

CHAIRMAN WALLIS: If the number is right for two-inch -- the number for two-inch may be 4100 and not 2200 -- 14, it's not a typo.

MR. BASSETTE: In one of the earlier slides I said that you don't start seeing significant recriticality until you get to about a 14,000 kilogram slug and then the maximum is 40,000 or 35,000, whatever.

CHAIRMAN WALLIS: And you never get a vapor generation with a very small break, repeat why that is.

MR. BASSETTE: Because you don't lose sub-cooling. HPI is sufficient --

CHAIRMAN WALLIS: Well, but your HPI is lost in order to get this to happen.

MR. BASSETTE: Yeah, I was just looking at it. These are strictly vanilla LOCA calculations.

CHAIRMAN WALLIS: But if you turned off the HPI, couldn't you get this happening with a smaller break?

MR. BASSETTE: You could.

CHAIRMAN WALLIS: So this is a bit misleading because that's the condition for it to happen anyway, isn't it?

MR. BASSETTE: Well, you know, then you have to start postulating how long you keep HPI off and so on. This was simply a break structure that we did. Now, this is the procedure that's in place. We talked about the possibility of pump restart. This is the procedure that's in place at the B&W plants. If you have a small break LOCA and you're thinking about pump restart, this is what the plants will do.

They must have stable sub-cooled natural circulation.

CHAIRMAN WALLIS: And how do they know they've got that?

MR. BASSETTE: They have a -- well, of course, they know they're sub-cooling because they've got the meter in the control room now instead of, you know, the time at TMI.

MEMBER SIEBER: And you can look at TH and TC to determine whether you've got natural circulation or not.

MR. BASSETTE: Yeah and --

CHAIRMAN WALLIS: How do you know the flow rate?

MEMBER SIEBER: You don't.

MR. BASSETTE: You don't.

MEMBER SIEBER: You don't care either. You can calculate the flow rate from the temperature difference.

MR. BASSETTE: You can see how much water you have in the feed --

CHAIRMAN WALLIS: As long as the core isn't heating up, yeah. So they can pretty well be sure that they understand when they have achieved this state?

MR. BASSETTE: Yes.

CHAIRMAN WALLIS: Because the one problem at TMI was they didn't know it was going off.

MR. BASSETTE: Yeah, they had no notion of sub-cooling and so on.

DR. ROSENTHAL: I think that the point about it being, you know, typically an hour or more to get into this condition. By that time the TSC is there, so now you have procedures for the operators, you have instrumentation for the operators and the operators have all the help in the world and that's why we gave it a minus twoish probability that they still mess up.

CHAIRMAN WALLIS: You only get this natural circulation if you have the HPI off.

MR. BASSETTE: You've got to refill the system.

CHAIRMAN WALLIS: You've got to refill the system.

MR. BASSETTE: You've got to refill the system.

CHAIRMAN WALLIS: Otherwise you've got another problem. You've got to have the HPI off for awhile and then you've got to put it on or something. You've got to do an awful lot of things.

MR. BASSETTE: That's right. So the objective is, of course, to prevent pump restart until well after any possible --

CHAIRMAN WALLIS: You said there's a 99 percent chance that will happen.

MR. BASSETTE: Yes.

CHAIRMAN WALLIS: They have to know what their break size is in order to make this decision?

MR. BASSETTE: No. No, there's nothing -- that's the other thing the operator has no way of knowing is how big the break is, so this is -- I mean, that's the good thing about using sub-cooling and --

CHAIRMAN WALLIS: I think you've got to have very good procedures, well-thought out, probably not misinterpreted and so on.

MR. BASSETTE: Yeah.

MEMBER RANSOM: Could you go back one more slide? I didn't understand what you had on that one. What are those -- once your steam generator slug volume and then you've got the times. Do you mean the time to form a slug?

MR. BASSETTE: Yeah, basically the time to form a complete slug volume, you know, 40 cubic feet of slug volume.

MEMBER RANSOM: And what's the other one that's about three or six cubic meters for the U-2?

MR. BASSETTE: U-2, yeah. You're dealing with, of course, much smaller times.

MEMBER RANSOM: What were the volumes, though?

MR. BASSETTE: It's three and a half meters. It's about one-tenth the volume of a B&W.

MEMBER RANSOM: Okay, I understand it.

CHAIRMAN WALLIS: This is a relevant slide, really? I think you've got an awful lot -- to much information here for the full committee.

DR. ROSENTHAL: Oh, way, way too much.

MR. BASSETTE: So this is just repeating human error probably for pump restart. You have estimates done by BNL and Sandia. So conclusions --

CHAIRMAN WALLIS: So you have very little experience that verifies any of these predictions for what operators would do in the event of an accident.

MR. BASSETTE: Yeah, of course, I think like Jack says at this point there would be emergency response center activated and so on and so forth.

MEMBER SIEBER: Yeah, let's hope that's helpful.

MR. BASSETTE: So basically we're dealing with a probability of something around 10^{-8} for this pump restart.

MEMBER RANSOM: On your first probability, does that include the fact that it has to be only within a certain range?

MR. BASSETTE: The first one is simply the occurrence of the event, the stuck open valve or the broken pipe. The second is the fact that it happened in the first two weeks or so --

MEMBER RANSOM: I was asking though, did you consider the fact that not all small break LOCAs --

MR. BASSETTE: Yeah, the third is not all small break LOCAs end up with a large loop seal dilution and then the fourth is that the operator doesn't follow procedures and starts his pump --

CHAIRMAN WALLIS: And then you could put in a fifth which is relative fuel damage even when this happens.

MR. BASSETTE: Yeah. Well, that's the --

CHAIRMAN WALLIS: That might be something you'd want to do instead of being so wishy washy about all these calories per gram and stuff.

MR. BASSETTE: Well, here's consequences.

MEMBER KRESS:: The probability in the first two weeks, you've got that by taking the yearly probability and multiplying it by the ratio and the time.

CHAIRMAN WALLIS: The easiest calculation.

MR. BASSETTE: Yeah, it's a simple this amount of time over a total fuel cycle. These are consequences, no recriticality, B&W divided up into restart, natural circulation in the pump. So this maximum slug now, you've got recriticality but fuel enthalpy within normal full power operation, DNB not reached, no fuel damage. Restart of the pump, core coolability, there's no impact based on being below the 230 calories per gram.

Cladding damage, you can't rule out that some fuel might get cladding cracks. This is, again, for maximum slug size. Some fuel centerline melting, as well in the high power rods.

DR. ROSENTHAL: Done.

MR. BASSETTE: Done and then these were the assumptions. This is the last slide. The following value for slug size, zero concentration in the loop seal of boron, no boron left in the loop seal. Marino talked about --

CHAIRMAN WALLIS: The Marino magic, yeah.

MR. BASSETTE: -- talked about RELAP/PARCS. We used the up to date estimates of small break LOCAs and stuck open valves. Initial probability that you'll get a large scale seal dilution, took into account early cycle life and we included current estimates for fuel damage thresholds from reactor --

CHAIRMAN WALLIS: So this could probably be summarized in about five slides? I think we could summarize your stuff in five slides. Marino, I'm not sure how much of that we need to have presented to the full committee. It's a long story, really, to get through and if you get through a little bit of it, you end up with all sorts of questions.

DR. ROSENTHAL: Well, I mean, it truly is a multi-disciplinary thing. I wanted to introduce the fuel damage limits at least here. I think we belabored them. Just to make the point that you know, for those who know about Cabri, yes, there's emerging evidence and we thought about it and even so, and stopped there and then --

MEMBER SIEBER: We're going to bring it up.

DR. ROSENTHAL: Well, that's why I'm going to nudge, yes, and even so and then stop there and then when Dana has his fuel meeting, we're going to be discussing that in detail. So --

MEMBER SIEBER: Dana won't let you stop there. Dana will not let you stop there.

DR. ROSENTHAL: He won't let me.

MEMBER SIEBER: He will dig in.

DR. ROSENTHAL: So even using more pessimistic limits, we think that the story is reasonable, period.

MEMBER SIEBER: Yeah, well, that's what he's been pushing for because there is data out there that justifies a lower limit and you've got it.

DR. ROSENTHAL: Yeah.

MEMBER SIEBER: It's taken a long time.

MEMBER KRESS:: I think you have a good story, Jack.

MEMBER SIEBER: Yeah, it's good enough.

DR. ROSENTHAL: Okay, now how much -- thank you. How much time do you think we have with the committee? I want to get some of the documentation. Half hour?

MEMBER KRESS:: What's on the agenda?

MEMBER RANSOM: I have to go look at the agenda.

MEMBER KRESS:: Yeah, we probably already have it set up.

MEMBER SIEBER: Yeah, the agenda is in the Federal Register notice.

MEMBER KRESS:: You've got an hour and a half.

DR. ROSENTHAL: And you're saying Dave gets condensed down to five slides, Marino to --

CHAIRMAN WALLIS: Five.

MEMBER SIEBER: One equation per slide.

DR. ROSENTHAL: That's good.

CHAIRMAN WALLIS: Two minutes, and then the ACRS ask questions for half an hour.

DR. ROSENTHAL: And Diamond, do you want to hear from him?

CHAIRMAN WALLIS: That needs to be cut back.

MEMBER SIEBER: We ought to be not in the editorial mode.

MR. CARUSO: Fifteen slides, all together.

MEMBER SIEBER: For the full committee we should not be in the tutorial mode. This should be information transfer.

DR. ROSENTHAL: Right.

MEMBER KRESS:: And I think you ought to make Dudley give the whole talk.

CHAIRMAN WALLIS: What's he doing here anyway?

DR. ROSENTHAL: Now, in fairness, on the report, you know, and these slides refer to specific pages in the report, the fact that it took us this much time to explain is proof positive that Dr. Ransom was right, that we had not been as clear as we should have been. One way to do it is to just -- is to staple the slides onto the draft report as further explanation.

MEMBER SIEBER: No, I wouldn't do that.

CHAIRMAN WALLIS: Maybe have some backup slides.

DR. ROSENTHAL: At some point, without -- I would like at some point to go into the public docket that a year from now that there should be a better report than I have now. Okay? And I would like to take the report that you have --

MR. CARUSO: We have a report. You don't want to revise that report at all?

DR. ROSENTHAL: Yeah, I want to revise the report but I want to go to the full committee. I don't think it's -- I want to go to the full committee as quickly as we can clear some PTS out of the way to free up Dave, we want to take Dr. Ransom's comments.

We want to editorially revise that report.

CHAIRMAN WALLIS: Bring it back to the subcommittee and we can --

DR. ROSENTHAL: No.

CHAIRMAN WALLIS: -- make a report to the full committee that you've fulfilled your obligations?

DR. ROSENTHAL: No, no, no.

CHAIRMAN WALLIS: I don't think we want to go to the full committee with a revised report where there's not much substantive difference than there was before.

DR. ROSENTHAL: No.

CHAIRMAN WALLIS: It's not worth it.

DR. ROSENTHAL: Right.

CHAIRMAN WALLIS: But you might want to go to this subcommittee for maybe just send it to us.

DR. ROSENTHAL: Send it to you for courtesy.

CHAIRMAN WALLIS: We can look at it and say --

MEMBER KRESS:: That would be the thing.

CHAIRMAN WALLIS: -- whatever we want to say.

DR. ROSENTHAL: But I think we can get on in regulatory space with the draft report and these slides are all public information.

MEMBER KRESS:: Yeah, I think you can.

MEMBER SIEBER: We can't write a report if the full committee doesn't hear the presentation.

MEMBER RANSOM: I think it would behoove you to clean up that report. It had a lot of problems in it and certainly your message could be made much more crisp and I think, then, this issue would go away. so I guess we're all in agreement. The session is closed then.

(Whereupon, at 5:21 p.m. the above entitled matter concluded.)