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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

552nd MEETING

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THURSDAY,

MAY 8, 2008

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ROCKVILLE, MARYLAND

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., William J. Shack, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

WILLIAM J. SHACK	Chairman
MARIO V. BONACA	Vice Chairman
SAID ABDEL-KHALIK	Member
GEORGE E. APOSTOLAKIS	Member
SANJOY BANERJEE	Member
DENNIS BLEY	Member
CHARLES BROWN, JR.	Member
MICHAEL CORRADINI	Member

COMMITTEE MEMBERS PRESENT: (cont'd)

OTTO L. MAYNARD Member

DANA A. POWERS Member

JOHN D. SIEBER Member

JOHN STETKAR Member

INVITED EXPERTS PRESENT:

HAROLD RAY

NRC STAFF PRESENT:

SAM DURAISWAMY, Designated Federal Official

AMY CUBBAGE

BRUCE BAVOL

TOM TAI

MIKE SNODDERLY

DENNIS GALVIN

RICHARD LEE

MIKE SCOTT

ALAN KURITZKY

ALSO PRESENT:

JIM KINSEY

GEORGE WATKINS

M.D. ALAMGIR

WAYNE MARQUINO

JESUS DIAZ-QUIROZ

ALSO PRESENT: (cont'd)

RICHARD STATTEL

JAMES BONGARRA

BERNARD CLEMENT

LOUIS CHU

I-N-D-E-X

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

(8:30 a.m.)

CHAIRMAN SHACK: The meeting will now come to order.

This is the first day of the 552nd meeting of the Advisory Committee on Reactor Safeguards. During today's meeting the Committee will consider the following: selected chapters of the SER associated with the ESBWR design certification application, insights from PHEBUS FT Tests, the draft NUREG/CR report on PRA methods for digital systems, and preparation of ACRS reports.

A portion of the session on ESBWR design certification application may be closed to protect information that is proprietary to General Electric-Hitachi and its contractors.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Mr. Sam Duraiswamy is the Designated Federal Official for the initial portion of the meeting.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's session.

A transcript of portions of the meeting is being kept. It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so they can be readily heard.

I will begin with some items of current interest. Mr. Charles Brown is now an official member of the ACRS, and we'd like to welcome him aboard. He'll bring much-needed expertise in digital systems, and we are looking forward to his participation in our meetings.

Mr. Harold Ray is attending the meeting as an invited expert. Subsequent to completion of all necessary paperwork, he will become an official member of the ACRS, and we're happy to have Harold here and look forward to completing that final paperwork to make him an official member.

(Applause.)

Our first item of business today is some selected chapters of the SER associated with the ESBWR design certification application, and Mike Corradini will be leading us through that.

MEMBER CORRADINI: Okay. Thank you, Mr. Chairman.

As you all remember, we have now had four

-- excuse me, five Subcommittee meetings relative to the ESBWR, and most recently two Subcommittee meetings looking at Chapters 4, which is the core design; 6, ESFs; 15, in transient analysis. So we're bringing back GEH and the staff here to essentially present a summary of their items relative to those four chapters. Oh, I'm sorry, and also Chapter 18, human factors engineering. Excuse me, I forgot one.

We'll bring back -- or the staff and GEH will be coming in to talk to us about that in a summary fashion. Most or many of you were at the January and the April Subcommittee meetings.

And so with that, I'll just turn it over to Amy Cubbage --

MS. CUBBAGE: Sure. Thank you.

MEMBER CORRADINI: -- to kind of give people a little bit more information.

MS. CUBBAGE: Great. Thank you.

This is Amy Cubbage, Lead Project Manager for ESBWR design certification. We really appreciate the interactions we have had up to date on these chapters. We think they have been very useful to the staff. We have asked several RAIs resulting from the issues that have been raised by the Committee -- we are going to discuss some of those briefly today.

They are still open items at this time.

And we also appreciate the Committee's advance guidance on this meeting to direct what you would like to hear. And on that note, most of the presentation will be by GE-Hitachi, to provide additional details on some topics that were addressed at the previous Subcommittee meetings.

I'll let Jim Kinsey introduce those topics, and then briefly the staff will give an overview of the status of those chapters, and then we'll move on to Chapter 18.

I understand that this morning some of the GE folks have not quite arrived, and we may switch the order and have the staff go first. But I'll let Jim do an introductory remark.

MR. KINSEY: Thank you. This is Jim Kinsey from GE-Hitachi. As Amy mentioned, our purpose this morning was in a couple of specific areas. We wanted to follow up to address some Subcommittee questions from previous sessions related to the containment and some of the components associated with containment. So we focused our presentation around three primary areas.

I think in the last Subcommittee session the Subcommittee was interested in the gravity-driven cooling system and the potential for gas binding from non-condensables. I think we had a presentation prepared but didn't quite get to that at the end of the agenda. So our intention this morning was to start off with that discussion and present those slides.

We intended, then, to follow up with a discussion of the overall response of the containment to a LOCA event. I know the Subcommittee had a lot of questions around the formulation and management of non-condensable gases, so we've established an updated presentation in that area to make that picture a little more clear.

And then, we'll follow that up with some follow-on information related to the vacuum breakers and how their seating arrangement is established and how their position indication is managed. So those are the three primary areas in the Chapters 6/15/21 arena.

As we get through those three items, then we'll -- again, we'll interact with the staff on that topic. And then, the other item that we have for today is just a brief follow-on discussion around Chapter 18 and the human factors engineering area that you heard a presentation on last month. Basically, the three topics related to containment and then the coverage of Chapter 18.

MS. CUBBAGE: Great. Thanks. And we do have the staff ready to go first, and the GE folks are signing in, are in the building and will go right after the staff.

MR. BAVOL: Good morning. My name is Bruce Bavol. I'm the Project Manager for ESBWR design certification, Chapters 4 and 15. What I'd like to do is just go over briefly some of the items -- RAIs and topical reports -- that we have been covering since the January Subcommittee meeting.

Since January 2008, RAI status for Chapter 4, we have resolved 14 RAIs and subsequently issued 23 new RAIs associated with topical reports that we have received. And currently the number of open items is 39.

For Chapter 15, additional RAIs resolved since January has been seven. We have initiated 27 new RAIs, again associated with topical report reviews, and currently we have 45 open RAIs.

I wanted to also provide you with a listing here of new topical reports and revisions that are currently under review since January 2008. As you can see, there's six, eight currently under review since January. And with this listing, we propose for a future Subcommittee meeting NEDE-33338, which is the ESBWR feedwater temperature operating domain accident analysis, and, of course, any other topical reports as needed.

MEMBER CORRADINI: Can I just ask --

MR. BAVOL: Yes.

MEMBER CORRADINI: -- we have seen a summary of that proposed change -- or not change, but modification to operation I think in December, if I remember correctly, or maybe it was February.

MS. CUBBAGE: January, I believe.

MEMBER CORRADINI: January. Was it January? Do we now have -- we do have that NEDE report, do we not?

MR. BAVOL: Yes, we do.

MEMBER CORRADINI: Okay. No, I meant the Committee.

MR. BAVOL: Oh.

MS. CUBBAGE: You should.

MEMBER CORRADINI: Okay. I thought we did. So are you going to talk any more about that, or do you have an idea when you want to have that, or is it open to us to --

MR. BAVOL: Well, it's currently open. I

brought Dr. Weidong Wong here, if there was any specific questions.

MEMBER CORRADINI: No, that's fine. I just wondered --

MR. BAVOL: Okay. But that is open. that date is open.

MS. CUBBAGE: Right. And, Bruce, I will just add that we do have an RAI milestone for issuing RAIs to GE-Hitachi, and I believe that's in June, correct, Bruce?

MR. BAVOL: June 13th.

MS. CUBBAGE: And we've already issued some RAIs. I don't anticipate there will be a significant number of additional RAIs, but we want to wait until we get all our RAIs out and get a little further down the road on that topical. So perhaps in the fall would be the appropriate time.

MEMBER CORRADINI: Okay. Thank you.

MR. BAVOL: Okay. With that, I'd like to turn it over to Tom Tai, who is going to go over the status of Chapters 6 and 21.

MR. TAI: Okay. My name is Tom Tai. I'm the Chapter PM for Chapters 6 and 21. Since January, we have resolved 54 RAIs. As a matter of fact, this slide is a little out of date since a couple of days ago when we prepared it. So right now what you see on the slide is the current open items -- 37. I think it's already down to 33.

On Chapter 21, since January we resolved -- this says seven, but actually it is 11. So the current open items would be 26. So we are making some progress, slowly but surely.

And what we have is the -- on Chapter 21, since January, we issued four new RAIs based on comments from the Committee, and these are all on Topical Report 33083, which is TRACG model, and we probably will bring back this for the Subcommittee to look at.

And which brings us to the two items that we know that we will bring back -- we will bring back -- it will be the Chapter 6 containment analysis and the TRACG open items.

MEMBER POWERS: That last -- I mean, you have an RAI here, requested GEH to address non-condensable gases and steam moisture flow in the GDCS lines.

MR. TAI: Under Chapter 6.2, yes, we do.

MEMBER POWERS: It doesn't tell me very much. I'm trying to understand what it is that they are to address about non-condensable lines and gases.

Is this a -- kind of the same thing with -- having a non-condensable gas issue here?

MS. CUBBAGE: If I may, we asked -- two issues. One would be non-condensable gases that may be in the line during operation that may be a blockage for flow, and then also an issue was raised by the Committee about the potential for steam entering the GDCS line that might impede the injection flow. And GE-Hitachi is planning to do a presentation on that topic today.

But as far as the staff is concerned, we have not seen the RAI response, and it's an open item.

MEMBER POWERS: In general, just giving me a list and say, "I have 37 RAIs," really doesn't give me a good understanding of where your troubles are here.

MS. CUBBAGE: I can appreciate that.

MEMBER POWERS: It would be more useful to say, "Look, in general, we're finding incompleteness, or phenomenologically we're finding this major gap in the analysis." Can you characterize your RAIs in some general term other than the number?

MR. TAI: Well, I think GE -- Wayne, you can tell me that -- in the next hour GE is going to go through a quick overview.

MEMBER POWERS: This is why I'm more of a victim of just the ordering of the presentation.

MS. CUBBAGE: Perhaps. And also, we tried to really limit the staff's time, because there were some significant topics that the Committee wanted to hear from GE again coming out of the Subcommittee.

MEMBER POWERS: But I'm not sure you serve the Committee well by just giving us a number of RAIs.

MS. CUBBAGE: Okay.

MEMBER POWERS: It would be better to say, "Our RAIs are simply issues of completeness of the record or they're phenomenological" --

MS. CUBBAGE: I would say there are still --

MEMBER POWERS: -- "vulnerabilities in the application."

MS. CUBBAGE: There are still some significant technical issues remaining. I don't think there are any fundamental issues that would call into question the viability of the design. There are issues that need to be resolved, though. These are not minor documentation issues at this point.

MEMBER POWERS: That's useful. Numbers is not.

MS. CUBBAGE: Thank you. And we can --

you know, if you want, after GE's presentation, the staff can come up and briefly reiterate some of those issues. I know Mike Snodderly, the Branch Chief of the Containment Branch, could probably summarize briefly his main remaining open issues.

MEMBER POWERS: We'd love to have Mike in front of us.

(Laughter.)

Time to get even.

(Laughter.)

MEMBER BANERJEE: The previous slide -- and this goes into the record -- there is nothing called slot chum flow or angular flow.

MEMBER CORRADINI: It was the carriage recognition software.

MEMBER BANERJEE: All right.

MEMBER POWERS: Sure you have chum flow, right after a boat when you're looking for sharks and things like that.

(Laughter.)

MEMBER BANERJEE: I hope not in the BWR, though. No sharks.

(Laughter.)

MS. CUBBAGE: So at this point, would you like us to proceed with GE-Hitachi? Okay.

MEMBER POWERS: Yes, that would be good.

MS. CUBBAGE: Thank you.

MEMBER CORRADINI: Do you have them here?

MS. CUBBAGE: They are here.

MEMBER CORRADINI: As they're setting up, let me remind everybody that on the -- at the April 9th meeting, we went through a detailed presentation of their limiting accident, which was a main steamline break. We then went through vacuum breaker discussion and discussions about the vent fans, or I should say the post-72-hour fan.

And we did not have a chance to go through the discussion about non-condensable gas blockage or potentialities of steam backflow, and so we are going to hear what we weren't able to hear that day as part of this set of presentations.

MR. WATKINS: Good morning. My name is George Watkins. I'm a Lead Regulatory Affairs Engineer for General Electric-Hitachi. I have primary responsibility for Chapter 6, and a lead over other engineers working on Chapters 15, 16, and 21.

Today we have three presentations dealing with Chapter 6, issues. The first one will be on gravity-driven cooling system interaction with steam and non-condensables, and that will be presented by

M.D. Alamgir. Then, Wayne Marquino will discuss containment pressure response after a LOCA, focusing on non-condensable gases and where they go during the sequence of events.

And then, we have Jesus Diaz-Quiroz, who will talk about our vacuum breakers. He will discuss our vacuum breaker test program to provide some assurance on how robust they are and what type of materials they can withstand on their seats and still be leak-tight. And we will discuss the isolation logic for the vacuum breaker isolation valve and how that will function and answer any questions in that area.

So we'll begin now with M.D., who will present his presentation.

MR. ALAMGIR: Good morning. Thank you for allowing me to present this issue on GDCS interaction of steam with -- steam and non-condensables with the GDCS pool.

I am told I have only 10 or so minutes. I have to rush through some of the slides. Please stop me if there is a fundamental question on phenomena. I am also available during the break to answer questions.

Two issues -- as GDCS flows, will steam

impede or prevent its flow into the vessel? And, second, if there is non-condensable from any source, will that degrade the GDCS flow to the vessel?

The summary is, if I've got only 30 seconds to present, is we have looked at it and it is insignificant.

I'll go through the slides --

MEMBER BANERJEE: Before you go on, what's the size of the pipe again? Remind me.

MR. ALAMGIR: We have four divisions, thanks to Jesus, who just confirmed -- an eight-inch pipe coming out of the GDCS pool. Each division has two lines injection line, six inches each, pipe size. And then, of course, near the vessel there is a venturi with a diameter of three inches.

MEMBER BANERJEE: Are there any elbows?

MR. ALAMGIR: There are. As we will show in the diagram, there are bends, 90-degree bends, etcetera. And we are addressing those through slopes.

MEMBER BANERJEE: If you will give us some basis for your answers.

MR. ALAMGIR: In 10 minutes, what I can.

All right. So on the first item, the CCFL -- CCFL, as you all know, stands for counter-current flow-limiting. In operating plants it is very important. We found that for ESBWR it is not, because the water level in the bundle is always above the chimney.

However, for the GDCS line, the issue is, if the water starts flowing and steam is rushing to meet it, will that cause any binding?

One of the important things to realize is that the water level is above the GDCS line when the CCFL -- when the GDCS flow starts. So initially there is no competition. There is, however, a period of about a few hundred seconds, and the plot will show it, when the GDCS line is uncovered and that's where the question arises, will it impede?

From our analysis, we find that there is so much condensing capacity in the GDCS flow it is almost thrice the amount of condensing capacity that it can condense the steam in the facility. And that is why it kills the steam before it can even start producing any difficulty.

MEMBER BANERJEE: How much subcooling is there?

MR. ALAMGIR: In terms of -- it's a factor of three condensing capacity. So I would say 317, 319 K --

MEMBER BANERJEE: Was this water at room

temperature?

MR. ALAMGIR: It's basically about -- yes, close to room temperature, and now the reactor is at about a few bars. So very large condensing capacity.

MEMBER CORRADINI: So the subcooling is about 80 C approximately, right? You said a few bars. That's about 407 Kelvin and 319?

MR. ALAMGIR: The temperature is around 400 or so.

MEMBER CORRADINI: Yes.

MR. ALAMGIR: Yes. So about 80 -- you are right.

MEMBER CORRADINI: Right.

MR. ALAMGIR: And, of course, the pressure is still decreasing at that time.

MEMBER CORRADINI: Yes.

MR. ALAMGIR: All right. So having said that --

MEMBER BANERJEE: The idea is that the steam will not enter the line because it will simply condense in the outflow on the line?

MR. ALAMGIR: Initially, when it uncovers, of course it sees cold water rushing out.

MEMBER BANERJEE: Right.

MR. ALAMGIR: So there is a complex

phenomena there, but from all we have seen, including some data that I will show -- and, George, we may need to get the backup slide -- shows -- this is a set of experiments related to water hammer in Slovenia, but they had a pipe and we'll show that when steam meets the water, very cold water, the cold water floods the -- I mean, flows through the pipe in about 10 to 12 seconds, under generally similar conditions also, although slightly higher pressure.

MEMBER ABDEL-KHALIK: Does the line always run full?

MR. ALAMGIR: In the --

MEMBER ABDEL-KHALIK: Even in later stages?

MR. ALAMGIR: Yes. Full in the sense that if you don't assume any GDCS, any non-condensable event, and we have analyzed the case where let's say you put some non-condensable coming from the GDCS pool, may be a burst of something, who knows. We assume the worst, and I will show you that the effect on the GDCS flow magnitude is small.

MEMBER ABDEL-KHALIK: So you never form a free surface --

MR. ALAMGIR: That's correct.

MEMBER ABDEL-KHALIK: -- inside the line?

MR. ALAMGIR: That's correct. As far as from our analysis, no.

MEMBER BANERJEE: Not from the steam.

MR. ALAMGIR: Not from the steam, not from -- well, from non-condensables, if you have a bubble, you have some surface, but not a free surface, not stratified flow. That's all I see.

We have some sensitivities where we put in non-condensables on the other side of the squib valve, let it reside for a few seconds, and then let the GDCS flow come in, and see if it gets into trouble. It doesn't. It pushes it out.

MR. MARQUINO: We have provided detailed information to the staff on the water levels in the pool and the reactor vessel. The time required for the pool to drain into the vessel is on the order of half an hour. In the long term, we end up with a low level in the pool, and it equilibrates in the -- with the water level in the reactor vessel.

I think your question is directed at the drain-down period, right? And, yes, during the drain-down period the line remains full, because the level in the pool is above the suction of the pipe.

MR. ALAMGIR: About seven-plus to 10 meters.

MEMBER BANERJEE: What is the velocity in the pipe?

MR. ALAMGIR: In the GDCS pipe? Typically, less than 10 meters. At the throat of the venturi it's about 10 to 12 meters per second.

MEMBER BANERJEE: And in the line itself?

MR. ALAMGIR: It's, I would say -- I looked at it at different times -- two to three meters.

MEMBER BANERJEE: So it's quite a high velocity.

MR. ALAMGIR: Yes. Yes.

MEMBER BANERJEE: Given by quite a large head.

MR. ALAMGIR: Large head, large condensing capacity, large velocities.

MEMBER BANERJEE: But you've still got quite a lot of pressure in the reactor, three or four bars, right?

MR. ALAMGIR: Yes.

MEMBER BANERJEE: What is the differential pressure?

MR. ALAMGIR: Between?

MEMBER BANERJEE: The outlet of the pipe and the -- at the --

MR. ALAMGIR: It's the head.

MEMBER CORRADINI: It's just the gravity head. By this time, it should be in communication. So whatever the pressure is in the vessel is the pressure in the drywell.

MR. ALAMGIR: About eight to 10 meters of solid water.

MEMBER ABDEL-KHALIK: So what is the volume of pipe between the squib valve and the check valve?

MR. ALAMGIR: In terms fraction?

MEMBER ABDEL-KHALIK: No, just total volume. Cubic feet.

MR. ALAMGIR: I don't have that number. I can --

MEMBER ABDEL-KHALIK: What's the distance?

MR. ALAMGIR: We have a diagram. It's on Slide 3.

MEMBER ABDEL-KHALIK: When you did these parametrics of allowing non-condensable gas in the line, did you go all the way to the point where that entire space between the check valve and the squib valve is filled with non-condensable gas?

MR. ALAMGIR: At time zero, yes. We filled it with up to 30 percent non-condensable.

MR. KINSEY: Excuse me. This is Jim Kinsey from GEH. I think the slides that we have may answer many of your questions, and I think you may be able to --

MEMBER CORRADINI: I think you may need to move on.

MR. KINSEY: -- move through that, and then --

MR. ALAMGIR: Yes, okay.

MR. KINSEY: -- come back to them --

MR. ALAMGIR: All right.

MR. KINSEY: -- if we don't cover something.

MEMBER CORRADINI: You can tell us to wait. It's okay.

MR. ALAMGIR: All right. Thank you. I was not sure about the etiquette in the morning.

(Laughter.)

Thanks.

Yes, we did put in some non-condensable --

MEMBER ABDEL-KHALIK: We will talk about it when you get to it.

MR. ALAMGIR: All right. Yes.

Next slide, so the summary is that -- on Slide 3 -- or Slide 2 is that none of these effects are important, and TRAC has models for CCFL. It has models to handle non-condensables. It has models for handling stratified flow.

MEMBER BANERJEE: The problem is CCFL at elbows. There is quite a drop in CCF --

MR. ALAMGIR: If steam can get there.

MEMBER BANERJEE: Yes. If there is, so your defense is saying that the steam never gets there.

MR. ALAMGIR: Correct.

MEMBER BANERJEE: But the fact that TRAC has a model for CCFL may not be there -- the right model, because TRAC has a model for interfacial friction. It doesn't have a model explicitly for CCFL, unless you put one in.

MR. ALAMGIR: We have CCFL model.

MEMBER BANERJEE: If you put it in --

MR. ALAMGIR: Yes. Based on Professor Wallis' correlation, we have backed out interfacial sheer.

MEMBER BANERJEE: That's what I mean.

MR. ALAMGIR: Yes.

MEMBER BANERJEE: You have a model for interfacial friction, not for CCF --

MR. ALAMGIR: We have CCFL as a limiting

condition flow. It checks.

MEMBER BANERJEE: Yes, it checks it, but the model is for interfacial friction. You have backed out --

MR. ALAMGIR: Right. It calculates the velocity of the interface. Then, it checks against the critical outset correlation.

MEMBER BANERJEE: Yes. The problem at an elbow is you get a hydraulic jump, so it tends to give you a much more rigorous than with Graham Wallis' correlation.

MR. ALAMGIR: I agree, if we get CCFL available. In this case, we do not.

MEMBER BANERJEE: In this case, we are saying steam never gets there, but --

MR. ALAMGIR: Right.

MEMBER BANERJEE: -- the non-condensables could.

MR. ALAMGIR: We analyzed that, and non-condensables vented.

MEMBER BANERJEE: I'm not -- if you are running this with TRAC, I'm not 100 percent sure that it captures the right phenomena. We can discuss this in more detail as I -- as we go along --

MR. ALAMGIR: Right.

MEMBER BANERJEE: -- but I think --

MEMBER CORRADINI: Keep on going.

MR. ALAMGIR: I have also thought about it from a phenomenological point of view, relativity velocity, how it separates.

MEMBER BANERJEE: There were a lot of experiments done on this, because Ontario Hydro has elbows in its feeders. The feeders are smaller than your pipes, but of course the limiting points are at the elbows. And it's also found in oil gas pipelines when you have counter-current flow -- the same phenomena actually. It's much more limiting, because of the hydraulic jump, as you get a draining film of draining liquid, because a jump which tends to block the pipe.

MR. ALAMGIR: This particular one I might clarify -- it's got a 10-meter driving head. Anything on its way is pushed out.

MEMBER BANERJEE: Well --

MR. ALAMGIR: We can discuss --

MEMBER BANERJEE: -- this is what your calculation will show, right?

MR. ALAMGIR: That's the reality of gravity acting on fluid. It will make it flow through the hole at -- with that velocity, square root of h.

MEMBER BANERJEE: Okay.

MR. ALAMGIR: All right. Moving on, if I may -- thank you. In the next slide we show the layout schematic of the GDCS line. In the top left-hand corner is the GDCS pool, and we show one division here, which is one eight-inch pipe coming out. And then, from there we have two lines -- A and B. We show A going into the vessel, and as we can see there is a -- where is the pointer again? I haven't done this before.

All right. So this is an eight-inch line, and it comes down, and here is the squib valve. And then, it -- this is water-sealed, prevents gas from going through. And our current design is focused on the fact that we will slope away from the high points. This is a high point, that's a high point. We'll slope away, so that the non-condensables can vent.

And there is this GDCS venturi nozzle here that limits the critical flow. Also, GDCS break, also there is a check valve here that allows -- doesn't allow backflow. That is the configuration.

And, Professor Khalik, I think you asked a question about what fraction of this line is on either side of the squib valve. Is that correct?

MEMBER ABDEL-KHALIK: Just the one side,

between the squib valve and the check valve, the distance. You said that you assume that you have 30 percent non-condensable gas --

MR. ALAMGIR: Yes.

MEMBER ABDEL-KHALIK: -- by volume. Why 30 percent?

MR. ALAMGIR: No, I -- what we did is a sensitivity study, and we will show in the next slide where we have put in up to 30 percent of non-condensable gas on either side of the squib valve, just to see if non-condensable degrades the magnitude of the GDCS flow or if it binds in it.

MEMBER ABDEL-KHALIK: There is a mechanism for non-condensable gas to accumulate between the check valve and the squib valve. There is always the potential that that entire volume would be filled with gas.

MR. ALAMGIR: We can put 100 percent. I am sure that it will drive it out because of the head.

MEMBER ABDEL-KHALIK: Do you have calculations to support that?

MR. ALAMGIR: We are running it currently, yes, but we are showing up to 30 percent. If it's --

MEMBER CORRADINI: So let me -- just to clarify, so you are doing a range of calculations.

But what you are going to show us is up to 30 percent gas fraction.

MR. ALAMGIR: And, by the way, 30 percent is a very large number for --

MEMBER CORRADINI: I want to make sure I've got what you said correctly. So we're going to see results for up to 30 percent, right?

MR. ALAMGIR: That's correct, yes.

All right. In the next slide -- so this one shows the routing. Again, it shows more numbers, elevations, and orientation/arrangement. This is something HRS wanted to see. And the red arrow shows one line, one division coming out, and then we follow it through one injection line that --

MR. MARQUINO: And I apologize, we don't have the length of pipe between the check valve and the squib valve indicated on this drawing. But we will get that information to you, to answer your question.

MEMBER ABDEL-KHALIK: But that line is not a horizontal line. It's part vertical, part horizontal. Is that correct? Am I reading this graph correctly?

MR. ALAMGIR: Let's understand it. This is a squib valve. So -- and this is like the loop seal --

MEMBER ABDEL-KHALIK: The loop seal.

MR. ALAMGIR: -- water seal.

MEMBER ABDEL-KHALIK: Right.

MR. ALAMGIR: So it goes up, and then this is horizontal or sloped slightly. Does that answer --

MEMBER BLEY: All of the horizontal sections are sloped.

MR. ALAMGIR: That's our intention right now in the design, correct.

Jesus?

MR. DIAZ-QUIROZ: Yes, I would like to add that the check valve itself is -- the design calls out for having it be open at all times. So during standby mode it will be opened, and then it will close during initial opening of the squib valve due to back pressure initially. So that negates some of the possibilities of accumulating non-condensables between the squib valve and the check valve for -- during standby mode.

MEMBER BLEY: So it's a swing check and it's hung --

MR. DIAZ-QUIROZ: It's not a swing check at this point, no, it's not. But here it's shown in the vertical position, but that's the orientation that more than likely gravity will assist any -- in this case, a piston-type check valve that -- that it would be selected, and that will keep it open. But it will be gravity-assisted as far as in the standby mode to stay open.

MEMBER ABDEL-KHALIK: Now, what is the purpose of this check valve?

MR. DIAZ-QUIROZ: The purpose of this check valve is initially during blowdown it's a depressurization of the vessel is -- it has not come down far enough. There is a timer on the squib valve when initial blowdown occurs to where the pressure in the vessel is much higher than the available gravity head available from the pool to the injection point.

So this allows any backflow to be stopped from going up the line initially, and then it --

MEMBER ABDEL-KHALIK: So it prevents flow from the vessel to the tank.

MR. DIAZ-QUIROZ: To the tank, yes, initially.

MEMBER ABDEL-KHALIK: So you are saying you are running online with this valve open?

MR. DIAZ-QUIROZ: Yes. But the squib valve provides the seal -- the seal during normal operation. And then, having it open alleviates any issues of it accumulating gas and such.

MEMBER ABDEL-KHALIK: So a squib valve failure during operation would have water from the reactor vessel go up that line, into the tank, and probably spewing all --

MR. DIAZ-QUIROZ: That is the -- the check valve will close during reverse flow.

MEMBER ABDEL-KHALIK: Thank you.

MR. ALAMGIR: And I want to add that we are -- as far as gas venting goes, very quickly we are aware of the NEI guidelines for addressing the venting of accumulation of gas and inclusion of gas. And, therefore, we have considered that actively in our design.

MEMBER BANERJEE: Do you have vent points along this line?

MR. ALAMGIR: He is our chief GDCS line engineer.

MR. DIAZ-QUIROZ: Right now, there aren't any vent points because of the sloping of the lines themselves. In this case where you start upstream of the squib valve, that's sloped up, and then you have vertical runs along with sloping upwards towards the pool that allow anything to vent up into the pool, which is connected to the drywell airspace itself.

So --

MEMBER BANERJEE: Have you ever done any experiments with this?

MR. MARQUINO: We have done gravity drain system experiments. GE has done them in San Jose, and Toshiba has done them in Japan.

MEMBER BANERJEE: So you have full-scale draining experiments.

MR. MARQUINO: Full height. Yes, full height with some volumetric scale.

MEMBER BANERJEE: Did you have the system mocked up fairly precisely compared to this with the little slopes and things?

MR. MARQUINO: Yes. The slope -- the pipes were sloped consistent with our design.

MEMBER BANERJEE: Did you put any non-condensables in to see what happened?

MR. MARQUINO: I don't think so. I'm not sure.

MR. ALAMGIR: There was a first attempt -- 1992, I did the TRAC modeling of --

MEMBER BANERJEE: This was for the ESBWR?

MR. ALAMGIR: Yes, the facility that was in the backyard.

MEMBER BANERJEE: You have dismantled all your useful facilities by now, right? In other words, you can't do this again.

MR. MARQUINO: We moved to North Carolina.

MR. ALAMGIR: There is some --

MS. CUBBAGE: Excuse me. I think we're going to run out of time. I mean --

MR. ALAMGIR: Okay. Next slide shows back to the test. This is a LOCA GDCS line break. It shows uncovery of the GDCS line, the circles. What you are seeing is the curve for two-phase level, which goes down. This is the downcomer two-phase level, and it -- the two circles show where it first time covers, the GDCS line, and then when it recovers.

Uncovers at about 500 seconds, recovers about 940 seconds. So there's about a good eight minutes, seven to eight minutes of uncovery.

MEMBER BANERJEE: What's the pressure at the start of the uncovery? Is there already a high flow established at the time of uncovery?

MR. ALAMGIR: As I mentioned, when it is covered, the GDCS flow starts. So it's already underway.

MEMBER BANERJEE: When does it start up here?

MR. ALAMGIR: As you see in the next

slide, if we can go -- we can -- the GDCS flow starts at 460, 450, 460 seconds, and it uncovers at about 500. So slightly less than a minute, about 40 seconds of full flow.

And it establishes the flow rather quickly, reaches the plateau. The plateau indicates that that's the driving head.

MEMBER ABDEL-KHALIK: I'm sorry. Could you go back to the previous graph?

MR. ALAMGIR: Yes.

MEMBER ABDEL-KHALIK: What's being plotted here?

MR. ALAMGIR: What is plotted is the two-phase level in the ESBWR downcomer versus time during a GDCS line LOCA -- GDCS line break LOCA.

MEMBER CORRADINI: The various DCs -- just for our clarification, DC-1109, 1114, these are --

MR. ALAMGIR: These the nodes of TRAC.

MEMBER CORRADINI: Ah, thank you. All right.

MEMBER BANERJEE: And these are actually two-phase levels.

MEMBER CORRADINI: Two-phase levels, yes.

MR. ALAMGIR: There is also a collapsed level, the black line that's running behind. After a single phase they're synonymous.

Moving on, so we can see that the GDCS flow establishes rather quickly, plateaus, and then as the -- it is driven by the difference in pressure between the GDCS pool and the vessel, as well as affected by condensation that is occurring due to the cold water in the vessel. And, therefore, at some point the water level starts rising, both in downcomer and chimney. When it is above the chimney, it is totally full, and, therefore, there is slight oscillation going on with --

MEMBER BANERJEE: I'm still trying to go back to this slide. Your two-phase level seems very high compared to the collapsed liquid level. Is that because you have very high voidage?

MR. ALAMGIR: As you can see, there is a blip in t hat -- in the collapsed level as well as in two-phase level there is flashing, so everything is charging up.

MEMBER BANERJEE: But even so, I mean, what is that black line comparable to the red line in terms of if I wanted to get an average void fraction in the -- to get the two-phase level?

MR. ALAMGIR: I do not have the number. I can find it in --

MEMBER BANERJEE: I'm just wondering, because this is like six feet or six meters, and the other thing is 14 meters.

MR. ALAMGIR: It's a --

MEMBER BANERJEE: Is it not comparable to each other?

MR. ALAMGIR: It's swell due to flashing.

MEMBER BANERJEE: But swell due to flashing, that seems a pretty high swell.

MR. ALAMGIR: I have lived through it through test facilities. I have seen it. It occurs.

MEMBER BANERJEE: But that means that the void fraction is over 50 percent, 70 percent. You don't get bubbly flow here then, right? It's churn-turbulent or some --

MR. ALAMGIR: At that low pressure with the very high specific volume, very large specific volume, you can get -- and we are getting into --

MEMBER BANERJEE: Well, in that case, you can also get steam going into the line during that period.

MR. ALAMGIR: No, that's a separate issue, and main steamline break is the more limiting case to show whether it goes and entrains into the steam --

MEMBER BANERJEE: Well, I'm wondering

whether you can get significant steam into the line during the period between 460 and 500 seconds.

MR. ALAMGIR: Not in this case.

MR. MARQUINO: I want to answer one of your previous questions. You asked what pressure it's at. When flow begins, the reactors depressurize to about 250 kiloPascal. So that would be around 40 psig. So the -- because it's a gravity drain system, the flow -- the system has to depressurize before flow begins.

MEMBER BANERJEE: This is what you see on the red curve. It's slowly starting to go up.

Now, what I'm wondering is, because you've got so much voidage that -- now, whether the voidage is in the vicinity of the GDCS line outlet or not, I don't know. I'd have to look in detail at what you have done. But it seems to me that there is a potential for steam entering certainly during that period, right?

MR. ALAMGIR: Of course steam is flowing. You are saying liquid entering.

MEMBER BANERJEE: Well, whatever entering, because you've got 80 percent void fraction mess out there.

MR. ALAMGIR: I believe Dr. Chester Cheung has looked at it in the main steamline break.

MEMBER BANERJEE: How has he looked at it?

MR. ALAMGIR: He --

MEMBER BANERJEE: With his eyes, or an experiment, or --

MR. ALAMGIR: No, no, through TRAC calculation.

MEMBER BANERJEE: TRAC calculations.

MR. ALAMGIR: Yes.

MEMBER BANERJEE: And you believe that TRAC calculations are able to track this?

MR. ALAMGIR: Unless you give me some better tool.

MR. MARQUINO: What would be the mechanism to force gas flow through this line and downward?

MEMBER BANERJEE: It just may not be filled with these very low velocities.

MEMBER CORRADINI: I think what Sanjoy is saying, unless I misunderstand, is -- and we could be misunderstanding the graph, but that's --

MEMBER BANERJEE: Yes, right.

MEMBER CORRADINI: In Graph 6, you're getting a void fraction just by a height ratio that is large enough that one would expect at the outlet of your isometric here that water would just start leaking out. So you could fill that first part of the line. I'm not sure the loop seal is going to -- I don't think loop seal is below it. I was looking at elevations. Your loop seal still could be filled with water, but I think his question, unless I misunderstand it, is this initial portion here would just purge itself of water.

MEMBER BANERJEE: It could be -- it could be not completely filled.

MR. ALAMGIR: That is not a problem. If the two-phase mixture -- if two-phase mixture gets into the portion of the GDCS line, it's flushed out when the squib valve opens. And we have that sensitivity. My answer is based on TRAC.

MEMBER CORRADINI: And that section of the line is full of cold water, so that -- that section of the line is not going to be flashing like the downcomer during the depressurization.

MEMBER BANERJEE: I guess we are so focused on this because this is one of the few -- this is a unique aspect of this design, and it really has to work if you are going to have this reactor cooled.

MR. ALAMGIR: Yes, and we believe it works. We will keep working at it, so that it works.

MEMBER BANERJEE: Experiments would be

good.

MR. MARQUINO: We have experiments, but to put this in perspective, I think you probably would agree that this phenomena that you're discussing would -- even if it exists, it would only be for a small period during the depressurization. And at the end of the depressurization, you have a situation where the vessels, a tank -- yes, the water level in the tank is below the nozzle, and you have cold water in a pool that has to drain into the vessel.

At that point, there is no mechanism to drive gas flow back into the pipe and down, so people --

MEMBER BANERJEE: I think with the steam ingress I agree that it would be a short period of time. I'm not all that concerned about that. It could be that you'll get some steam in whatever, but it will condense probably. The problem more is whether you can get a bubble of non-condensables sitting somewhere in that line hanging up the flow over a long period of time. Clearly, that's the concern.

MR. ALAMGIR: And I did a sensitivity up to 30 percent void, putting a bubble there, both sides of the squib valve, and see what happens when that squib valve opens.

MEMBER BANERJEE: Okay. Let's continue.

MR. ALAMGIR: Thanks for good questions.

All right. Here we show -- this is a digression going back to the condition of steam meeting water, cold water meeting hot steam. This is an experiment in Slovenia, NUREG-12, Pittsburgh, a couple of years ago, one year ago.

There are four -- this is a straight pipe, about three centimeters, or seven -- diameter about three meters long. Conditions are about 30 bars of higher than -- somewhat higher than GDCS condition.

But what is important to see is when cold water starts going out, or is being injected in the lower left-hand corner, and steam is being injected in the upper right-hand corner, how the temperatures -- T1, T2, T3, and T4 -- show the migration of the cold water interface and whether or not -- this is not a water hammer test. There are a series of tests -- water hammer for -- in this experiment, but this is not one.

It shows how cold water goes from station 1 through 4, and CFD analysis accompanying it. Okay? So --

MEMBER BANERJEE: What is the CFD

analysis?

MR. ALAMGIR: This is something done at -- over there by CFX and Neptune modeling this test, how cold water meets steam, and whether or not --

MEMBER BANERJEE: How do they sustain an interface?

MR. ALAMGIR: I have -- I don't want to get into that detail. I have a slide that shows how the cold water interfaced --

MEMBER BANERJEE: Do you need the CFD or not? If you don't need the CFD, forget it.

MR. ALAMGIR: I do not need -- the point I want to make is that there is no binding here under such harsh conditions.

The next slide shows the test as cold water goes from Section 1 through 4, so you go T1, T2, T3, T4, and the red lines show how the steam -- the temperature drops from steam temperature to cold water temperature, meaning that waterfront has moved very rapidly.

MEMBER BANERJEE: So CFD calculation?

MR. ALAMGIR: Data. Red is data, and the rest is CFD. So under about 10 seconds the pipe runs full, three-meter long pipe.

MEMBER BANERJEE: Okay. What does the

temperature show me there? Can you explain that?

MR. ALAMGIR: Yes. So when the temperature is high, that means it is steam. When it is low, that means cold water has moved in from left to the right. So in about two seconds the cold water reaches the first temperature measurement station. In about 10 seconds, it -- eight seconds it reaches the fourth station. So it's moving at fairly uniform speed of about two seconds per station, and that would be less than a meter.

There is no rollback of the steam forming bubbles, and so on. That was some of the concern.

MEMBER ABDEL-KHALIK: So what is that second peak in T3?

MR. ALAMGIR: I am just quoting their plot.

MEMBER ABDEL-KHALIK: T3.

MR. ALAMGIR: T3, the second peak.

MEMBER ABDEL-KHALIK: Right.

MR. ALAMGIR: I have not read thoroughly, but I would imagine that there is a steam bubble that --

MEMBER ABDEL-KHALIK: If you don't understand it --

MR. ALAMGIR: -- hanging there, but it

collapses.

MEMBER ABDEL-KHALIK: You said you haven't read this thoroughly, and yet you're using this as a justification.

MR. ALAMGIR: I have not read the test report. I don't have access to it yet. I have read their paper. The paper is available, and we can --

MEMBER ABDEL-KHALIK: The question remains: what does this peak represent?

MR. ALAMGIR: In my interpretation, the peak represents that there is a steam bubble temporarily for about a second, which collapses as the waterfront comes in.

And, George, could you please go to the slide that --

MEMBER CORRADINI: I think we're going to run out of time. So unless you desperately need to use this as part of your justification, I recommend we move on.

MR. ALAMGIR: Yes. We have a CFD slide that can answer. We can show you during the break.

All right. So next slide shows the summary of the non-condensable sensitivity. So here, as I mentioned to you, we put non-condensables in various locations.

First, to put in a GDCS pool, we made it 30 percent void fraction with air, not steam. That means essentially it will have 30 percent less static head to drive in the end.

Second, we put up to 30 percent on either side of the squib valve.

MEMBER BANERJEE: Up to 30 percent? What do you mean by --

MR. ALAMGIR: Zero, 15, and 30, those were the three --

MEMBER BANERJEE: Do you mean the length of pipe is --

MR. ALAMGIR: Void fraction.

MEMBER CORRADINI: No. But he wants to know over what length.

MEMBER BANERJEE: Yes, over what length.

MR. ALAMGIR: The entire length. Entire length. We put initial -- assuming that -- suppose -- now it would be the gas valve to the right of the squib valve in this diagram, in the previous diagram. So we put up to 30 percent --

MEMBER BANERJEE: Out of the GDCS pool, up to the elbow?

MR. ALAMGIR: Up to the squib valve, all the way down. Assuming that the entire pipe -- this is the worst scenario in this --

MEMBER BANERJEE: So the squib valve is in a sort of a little seal, right?

MR. ALAMGIR: That's correct.

MEMBER BANERJEE: So you put 30 percent void on each side up to what point?

MR. ALAMGIR: All the way up to GDCS pool on one side and all the way to the RPV on the other side.

MEMBER BANERJEE: Ah.

MEMBER CORRADINI: The whole pipe. The entire --

MR. ALAMGIR: Whole pipe.

MEMBER CORRADINI: Everything.

MR. ALAMGIR: Yes.

MEMBER CORRADINI: That's what we didn't understand.

MR. ALAMGIR: Okay.

MEMBER BANERJEE: Now, did you put this as a continuous thing, or did you just put a --

MR. ALAMGIR: Initial condition.

MEMBER BANERJEE: So if you put, say, initially -- instead of putting 30 percent all the way to the GDCS, if you just took a piece of the pipe and put 100 percent in that region, like a bubble sitting there, did you do something like that?

MR. ALAMGIR: We went 100 percent void.

MEMBER BANERJEE: Correct. Yes, between the squib valve and the --

MR. ALAMGIR: Locally it can be 100 percent, but I did not. I put up to 30 percent. That means there is a bubble.

MEMBER BANERJEE: So you made a stratified initial condition with 30 percent.

MR. ALAMGIR: Thirty percent is bubbly.

MEMBER BANERJEE: The horizontal pipe at rest? How can it be bubbly?

MR. ALAMGIR: I put it as a uniform void fraction and let the core sort out what it is.

MEMBER BANERJEE: A uniform void fraction has a high surface area, therefore, it will get driven out.

MR. ALAMGIR: We can do more -- the point here was to just see, first, the --

MEMBER BANERJEE: Well, I understand what you've done now. So you've put that 30 percent uniformly distributed using some bubble size --

MR. ALAMGIR: Right.

MEMBER BANERJEE: -- as an initial condition. What was the bubble size?

MR. ALAMGIR: We put in void fraction, not the bubble size.

MEMBER BANERJEE: You put in the void fraction. So TRAC found the void -- the bubble size.

MR. ALAMGIR: Yes.

MEMBER BANERJEE: What was that bubble size?

MR. ALAMGIR: We can back it out. I don't have the number.

MEMBER BANERJEE: If it was small, obviously it will be driven out.

MEMBER ABDEL-KHALIK: I mean, you know, if you're assuming homogeneous flow throughout this line, of course it will go through.

MR. ALAMGIR: However, TRAC has the stratified flow model. It will look at the gravity head in the JSM portion and create stratified flow.

MEMBER BANERJEE: TRAC does not automatically take a bubbly flow and make it stratified, except through a flow regime map of some sort.

MR. ALAMGIR: Yes, it has a flow number --

MEMBER BANERJEE: It doesn't have a vapor disengagement model in it. So --

MR. ALAMGIR: It has a flow number

condition, and we can get into that. Yen Sanderson can answer that.

MEMBER BANERJEE: Whether your initial condition was representative of, say, a bubble being trapped between the horizontal leg and, say, the vertical leg, now it's sloped so it will tend to clear itself -- I agree with that. But if you started with that condition, you know, the question is whether the flow would be slowed down enough that it wouldn't get swept out. If it was homogeneous initial conditions, clearly it would --

MR. ALAMGIR: It is not a homogeneous flow condition. We have certain size node in TRAC. Putting a 30 percent void does not mean that we are saying it's not a bubble there. We can --

MR. MARQUINO: You have said that the flow would stop or you're implying that there would be zero flow. In my experience, if I have an eight-inch pipe and it's full of gas, air --

MEMBER BANERJEE: You'd still get some flow, yes.

MR. MARQUINO: -- and I have a pool with a couple of meters of water above it, I'm going to get flow that's going to drain down. And I'm not an expert.

MEMBER BANERJEE: The flow will be the bubble-rise velocity.

MR. MARQUINO: I'm not going to tell you what regime it's going to be in, but there will be flow from the pool through this pipe.

MEMBER BANERJEE: If you look at the flow coming around a vapor bubble, if you have a flow which is equal to the bubble-rise velocity, it will just stay still.

MEMBER CORRADINI: It will be an equal volume flow in opposite directions. I think we're going to have to move on, but I'm just going to say that I think the takeaway from the Committee at this point is we are interested and we are still wanting to understand a bit more. And we can get to more later.

MR. ALAMGIR: Yes.

MEMBER CORRADINI: I have one thing that has nothing to do with calculation. You said something that I heard, but I want to make sure I heard it correctly. You have no intent to put anywhere in any of these elbows a vent line to test to make sure?

MR. DIAZ-QUIROZ: Test lines, yes.

MEMBER CORRADINI: Well, I'm trying to understand in this isometric where you are going to put little valves and little pipes to check to see if something is there, or you're not. That's what I want to make --

MR. DIAZ-QUIROZ: I'm sorry. When the question was asked, were vents going to be put in place, I assumed vents to continuously vent the line.

MEMBER CORRADINI: Okay.

MR. DIAZ-QUIROZ: No. There will be test lines, and --

MEMBER CORRADINI: And those are yet to be determined where they will sit?

MR. DIAZ-QUIROZ: They will be -- right. But it will have them between -- on either side of those squib valves.

MEMBER CORRADINI: Okay.

MR. DIAZ-QUIROZ: And as well at the high points. In this case --

MR. ALAMGIR: Mr. Corradini, we are engaged with them through NEI guidelines to understand this and --

MEMBER CORRADINI: Okay. But I thought I wanted to check again, because I misunderstood.

MS. CUBBAGE: And at this point -- at this phase in the review, we are looking for the Committee to concur that the staff has identified the appropriate open items. This was an open item that was added based on the Committee's concern.

Staff has not received a response from GE on this. I think they are hearing a lot of good feedback here that they should try to address in their RAI response, and you will be briefed on this when the issue is resolved by the staff.

MEMBER BANERJEE: Venting is good also. Experiments and vents both.

MR. ALAMGIR: Thank you.

MR. DIAZ-QUIROZ: Good.

MR. ALAMGIR: So, finally, moving to this almost last slide here -- this is the last slide -- it shows the effect of the sensitivity study. Three things -- as expected, because of the voiding in the void -- put in the GDCS line and the pool, we get less static head. So with larger voids, we get delayed GDCS onset, slight delay, a few seconds.

But the magnitude, as you can see, is not impacted that much. The 30 percent void case is within 80 percent of -- or 85 percent of the real case. Fifteen percent shows pretty close to original.

So my takeaway from this is I believe non-condensables trapped even on the other side of the squib valve will not impact GDCS flow.

MEMBER ABDEL-KHALIK: I don't think this shows that. I don't think --

MR. ALAMGIR: We can have a discussion on that.

MEMBER ABDEL-KHALIK: -- this shows that.

MR. ALAMGIR: I am very interested in a discussion that can show the other ways.

MEMBER BANERJEE: I think I agree with Said that until -- I mean, first, putting so much credence on TRAC is -- I know your faith in it is -- but it's somewhat touchy.

MR. ALAMGIR: It's somewhat beyond TRAC as well. As you know me --

MEMBER BANERJEE: I think you probably need to appeal to some of your old experiments. These have been done. But the other thing is that you certainly shouldn't distribute the void evenly. So it may be that you get the same answer if you don't, but --

MR. ALAMGIR: We'll put void fraction. The core sorts out what flow regime it is in.

MEMBER BANERJEE: I would say --

MEMBER CORRADINI: Just to end his point, I think what he is saying is spatially you may have 30 percent, but he wants -- what I hear both of these gentlemen saying is you want to think through where it might pocket and look at essentially putting 100 percent, block his air, and watching how it develops.

MR. ALAMGIR: Correct.

MEMBER CORRADINI: That's what they're saying.

MR. ALAMGIR: After reading your suggestion is empty the line.

MEMBER BANERJEE: No, not the whole line. Just empty a part where you might trap a bubble.

MEMBER CORRADINI: Yes, right.

MR. ALAMGIR: I think I have done it. I have --

MEMBER CORRADINI: We can do it later. We can talk about it later.

MR. ALAMGIR: All right. So that was the conclusion. No CCFL effect. It doesn't impede the GDCS flow. And as far as the suggestions, very good suggestions. We'll look into these other sensitivities. Don't see any major impact of non-condensables for the sensitivity studies I have run. So I'm convinced that it's a good machine.

MEMBER BANERJEE: The other thing you might want to check is that your CCFL correlation does account for elbows. I can give you a couple of references in the International Journal of Multi-Phase Flow --

MR. ALAMGIR: Sure.

MEMBER BANERJEE: -- on both experiments and analysis of this.

MR. ALAMGIR: That would be applicable to the elbows you see in the routing diagram.

MEMBER BANERJEE: In CCFL conditions.

MR. ALAMGIR: Yes. We have a CCFL slide. We can put it there.

MEMBER BANERJEE: Right.

MR. ALAMGIR: We cannot overnight put very different CCFL models. We use --

MEMBER BANERJEE: It would be sort of a JGJ type. I mean --

MR. ALAMGIR: Right. Professor Wallis' type of --

MEMBER BANERJEE: Yes, it would be the same thing.

MR. ALAMGIR: Any other questions? Otherwise, I will exit. Thank you very much.

MEMBER CORRADINI: Thank you.

MR. ALAMGIR: Spotlight goes to Mr. Marquino now.

MR. MARQUINO: My name is Wayne Marquino.

I work for GE-Hitachi. We have had many -- I think two interactions with ACRS on our LOCA analysis, and now we're back to the full Committee.

Unfortunately, Dr. Chester Cheun is performing one of his civic duties today. He is going to jury selection in San Jose, so he couldn't be here. I worked on this presentation with him, and it's a summary of our containment analysis and a good focus on the non-condensable gas treatment in our analysis.

I hope to answer a lot of your questions today. But you may have a question or two that I can't answer and will have to take back.

Next slide, please.

And following this presentation Jesus Diaz-Quiroz will talk about the vacuum breakers in ESBWR.

Next slide.

Just as our ESBWR reactor has evolved from natural circulation, free separation reactors, through BWRs, through BWRs with steam generators and forced circulation, that's the evolution of the reactor. Well, our containment evolved also.

It started with dry containments, and then we went to pressure suppression containments built out of free-standing steel vessels. And now we have, in

ABWR and ESBWR, concrete-reinforced containment vessels. In terms of the volumes, they are similar to the Mark II containment.

The takeaway from that is that in terms of hydrodynamic loads we are based on test data that has been developed from full-scale Mark III and ABWR tests, so we don't consider that there is any issues in that area. And we haven't heard any from the staff or the ACRS.

At the beginning of the SBWR program, we were in a test and analysis program description phase. And the purpose of this was to define what tests would be necessary to get us through the licensing and certification of the plant with a lot of focus on qualification of our computer code.

So we looked at what the scenarios are for accident safety analysis, what phenomena would occur, and how we would qualify the models of our codes. Of course, in a lot of areas they are completely based on qualification that we had in place already for the operating plants, but there were some unique tests that were identified as necessary for SBWR.

And this slide highlights some of them. We have some full component prototype tests like the depressurization valve, the DPV, and the vacuum breaker, which was specifically designed for SBWR to meet a high leak-tight reliability, as you'll see later.

We also have full-scale prototype testing of the heat exchangers, the PCCS heat exchanger used for decay heat removal, and the isolation condenser heat exchanger used for decay heat removal at high pressure.

We benefitted a lot from the international participation here. The heat exchangers were built and tested in Italy. We had gravity drain cooling system tests at the GIST facility in San Jose, but we also had tests at a Giraffe facility in Japan that also picked up parts of the containment system and integral containment tests.

The largest scale test facility is the Panda test facility that was built in Switzerland. This is a full height -- again, all of these are full height -- containment test facility, and it was 1/20th of the volume of SBWR, and that's about 1/50th of the volume of ESBWR.

And I want to point out on the bottom left-hand corner is a picture of the Panda facility, and you can see that there are two tanks, and the intent of that test was to force maldistributions of non-condensable gas, so that we could see how areas would purge or not purge.

And we've qualified our computer code against these tests. We went through a review by the staff, the ACRS looked at it, and we finally received a safety evaluation report for our application methodology or our procedure for analyzing containment pressure with TRAC.

Next slide, please.

Now I'm going to go through the sequence of LOCA and the LOCA analysis for containment pressure calculations. We start with normal operation, and we set up a set of parameters identified on the bottom left at bounding valves. Reactor power, which is pretty -- which is required by regulation to consider uncertainties -- we set the ECC pool and the -- at the maximum tech spec temperature, the drywell temperature and pressure -- the drywell pressure at a maximum of tech spec temperature to maximize the non-condensable gas loading in the containment.

The drywell temperature is actually set at a lower-than-tech-spec maximum, because that maximizes, again, the non-condensable initially. The wetwell temperature is set at a maximum, and the suppression pool temperature humidity is set relatively low to maximize non-condensables, suppression pool levels set low to minimize the heat capacity in the pool.

Reactor pressure is set high to maximize the break flow rate, and the reactor water level is set high to maximize the energy, the blowdown energy in the reactor.

On the bottom right, you see some of the modeling parameters that we set at the end of their uncertainty range for the LOCA analysis. So we consider uncertainty in the break flow rate, including the DPV critical flow rate, decay heat, heat transfer, loss coefficient to the passive heat exchanger, the PCC, heat transfer on the PCC, and flow loss in the vacuum breaker.

MEMBER ABDEL-KHALIK: I thought you said you biased the drywell temperature low. Is --

MR. MARQUINO: Yes.

MEMBER ABDEL-KHALIK: Is 115 degrees F biased low?

MR. MARQUINO: Yes.

MEMBER ABDEL-KHALIK: Vis-a-vis 110 in the wetwell temperature?

MR. MARQUINO: Yes, 110 is high for the wetwell temperature.

MEMBER ABDEL-KHALIK: And 115 is low for the drywell?

MR. MARQUINO: Yes.

MEMBER ABDEL-KHALIK: What is the tech spec limit for the drywell temperature?

MR. MARQUINO: Present -- well, it's a bracketed value, meaning we haven't nailed it down yet, but we're going to unbracket it in the next revision, and it will be 150 degrees F.

So the drywell operates a lot hotter than the suppression pool. There is no heat sources in the suppression pool.

Next slide, please.

Okay. As I go through this series of slides, you will see often on the top left there is a diagram of the reactor building with the primary containment, and it's color-coded to show the distribution of non-condensable gas -- a mix of non-condensable gas and steam, a mixture that is primarily steam that is colored yellow, hot water colored green, and cold water colored blue.

So we started with cold water in the pools, hot water in the vessel, no water on the floor of the containment, and now we have a guillotine break of the largest pipe on the reactor, the main steamline.

This will fill the drywell with steam. It's a very energetic blowdown. Will be purging steam and non-condensables through the main vent system into the wetwell pool.

During the blowdown, about half of the non-condensables will move from the drywell into the wetwell airspace. Near the end of the blowdown we start GDCS injection, and we fill the vessel with cold water. It takes some time for that water to heat up and begin boiling, so there will be a period of reduced steam flow out of the vessel.

MEMBER CORRADINI: And that's in that period of about a quarter of an hour through about three-quarters of an hour.

MR. MARQUINO: Yes.

MEMBER CORRADINI: And that's the only time, given your assumptions and how you do the analysis, that the vacuum breakers are lifting, as I understand it from the Subcommittee meeting.

MR. MARQUINO: Yes.

MEMBER CORRADINI: Okay.

MEMBER BANERJEE: And the drywell is essentially pretty well mixed. Is that the reality of the situation?

MR. MARQUINO: Yes. During the blowdown period, the drywell would be well mixed.

MEMBER BANERJEE: The steam jet.

MR. MARQUINO: So certainly the area around the break will be mixed. We've got open areas in the top. Any liquid from the break will be draining down into the lower drywell at -- that should be pretty well mixed.

There are some confined spaces, like the drywell head area, the space between the vessel head and the drywell head. We expect some hideout of non-condensable gas there. GDCS pool airspace would probably have some non-condensable gas in it.

MEMBER BANERJEE: So when you show this yellow, fairly uniform, it's at a stage where everything is sort of mixed, including above the GDCS pools and above the vessel.

MR. MARQUINO: I didn't try and show you the exact concentration in every TRACG node. I'm just trying to --

MEMBER BANERJEE: I'm just wondering why this is not -- some parts are not light yellow.

(Laughter.)

He's got two yellows there.

MEMBER CORRADINI: He is hoping for a

light yellow somewhere.

MR. MARQUINO: Our standard answer to this is it's a cartoon. When you --

MEMBER BANERJEE: How do you do these calculations? Do you compartmentalize these and you have some sort of a mixing coefficient between the compartments?

MR. MARQUINO: We show the nodes used in the calculation in the DCD. I think there is something like 100 nodes in the drywell GDCS, wetwell airspace, suppression pool, and --

MEMBER BANERJEE: The real thing is you have to have some sort of a mixing coefficient between the bulk of the drywell and what happens above the GDCS pools in the head regions, right?

MR. MARQUINO: Luckily, we have one of the experts on the TRAC mixing that will be -- M.D., would you like to comment on that?

MR. ALAMGIR: Yes. Initially, there will be some trouble in mixing.

MEMBER BANERJEE: I thought he did his work on homogeneous nucleation.

MR. ALAMGIR: Professor Banerjee, that was 25 years ago. Since then, I have done some other work, which may not be as --

(Laughter.)

But there is no assumption of any specific mixing coefficient between compartments. It is best estimate in that sense. You start out with some initial conditions, such as uniform distribution of steam and non-condensable, especially in the --

MEMBER BANERJEE: So how do you handle the, say, mixing? Because obviously there is -- there is a barrier to mixing between the top of the GDCS pool and the rest of the containment. Is that --

MR. ALAMGIR: Let me clarify at the outlet -- outset. Air, which we model, and steam -- they move the same velocity, except that they are different -- they are tracked separately in terms of the mass, but they move at same velocity.

MEMBER BANERJEE: Right.

MR. ALAMGIR: So it's a single fluid in that sense.

MEMBER BANERJEE: Right. But initially you've got air on top, right?

MR. ALAMGIR: Yes.

MEMBER BANERJEE: So now you're going to have the steam going in and --

MR. ALAMGIR: And then it mixes up.

MEMBER BANERJEE: Yes. But when you look

at the regions where there are relatively -- like barriers to flow or small openings, it is just a convective component that is going through from one mixing cell to the other? Just based on the velocity?

MR. ALAMGIR: That's correct. If it has a restriction, it will follow the typical --

MEMBER BANERJEE: There is no eddy diffusivity in this model.

MR. ALAMGIR: I had a model. He didn't turn it on.

MEMBER BANERJEE: All right.

MR. ALAMGIR: To be on the conservative side. I have a total mix-in model based on --

MEMBER BANERJEE: It's all pure convection.

MR. ALAMGIR: Pure convection.

MEMBER ABDEL-KHALIK: So these results are independent of the break location?

MR. MARQUINO: There are some sensitivities to the break location that you can see in our DCD results. For example, in the main steamline break, you will see in some later charts I think two more. There is a point where we get spillover of -- we fill the whole vessel up to the break, and at the point we get spillover that forces flows --

MEMBER ABDEL-KHALIK: But as far as containment response, are these results independent of break location?

MR. MARQUINO: What do you mean --

MEMBER ABDEL-KHALIK: Pressure history in the containment, non-condensable gas concentration in containment.

MR. MARQUINO: Well, the pressures are a little different. You can see the pressures are different depending on the break location.

MEMBER ABDEL-KHALIK: Okay.

MR. ALAMGIR: DCD has specific break cases, main steamline and so on, for different types of breaks.

MEMBER CORRADINI: But, I mean, using that as an example -- I mean, we are going to have to move on, but using that as an example, though, confuses it because you have -- it's a high energy line break. You have different enthalpies as well as location. So you're right, they are different, but is it the enthalpy that you are spewing out, or is it the location?

I think what Said is asking is, if I just took the same main steamline break, and I put it in three different locations, do you get about the same answer, or do you get wildly different answers? I think it's about the same answer.

MR. ALAMGIR: Do you mean the location along the line?

MEMBER CORRADINI: Yes, or --

MR. MARQUINO: We can even get a different answer. I think we did a sensitivity that we provided the staff on that where we took the -- we put a pipe from the main steamline down into the bottom of the drywell, and we ran that case and provided it to the staff.

MEMBER CORRADINI: Oh.

MR. MARQUINO: And it is a little different, because it will purge the non-condensable gas from the lower drywell.

Now, we've got some -- in a couple more slides I am going to talk about some of the treatments of non-condensable gas that we provided in our nodalization to come up with a maximum containment pressure answer.

MEMBER BANERJEE: Let me ask you more of a sort of first-order question. Suppose for some reason you were wrong and you had more non-condensables hung up in the region above the GDCS -- in the open space in the GDCS pool and at the top region above the vessel. Would it make any difference?

MR. MARQUINO: Not much, because the -- in terms of compressing the airspace in the wetwell, if we have non-condensables in the drywell that's directly going to decrease the containment pressure. So our focus has been making sure that we don't underpredict the amount of non-condensables that got into the wetwell airspace.

MEMBER BANERJEE: So having some hop in the drywell here and there doesn't make too much of a difference.

MR. MARQUINO: Doesn't make too much of a difference. The reason --

MEMBER CORRADINI: It does lower the total pressure.

MR. ALAMGIR: The figure of merit is the wetwell condition. We don't want the pressure to be too high.

MEMBER BANERJEE: Right. You are putting as much non-condensables as possible into the wetwell, right?

MR. ALAMGIR: That's correct.

MEMBER BANERJEE: Under your current set of calculations.

MR. MARQUINO: Where it has an effect is when we have non-condensables that are getting into the PCC, they will cause the PCC to have to vent. And then, that causes drywell/wetwell differential pressure and drives leakage into the wetwell airspace.

MEMBER BANERJEE: If your drywell pressure was a bit higher, what would happen due to non-condensables?

MR. MARQUINO: Well, let me get back to that. So if -- if you have a scenario where over three days you are -- basically, this is what we have now is over three days we have a continuous venting, because we have continuous radiological gas production. So I think we've pretty much maximized that effect, too.

What was your question?

MEMBER BANERJEE: I was just saying if you didn't clear the drywell of non-condensables, there was some hanging around in various pockets here and there, what were the implications of that? So from a pressure point of view, potentially because you have less non-condensables in the wetwell, the pressure might be a bit lower.

But what does it do to other things -- PCCS --

MR. ALAMGIR: I can answer that question in a positive way. If it hypes out and it comes out later, PCCS is self-regulating. So we have shown that it doesn't really matter. It's self-regulating. The system is in balance, and we don't -- our pressure in the wetwell is not affected by this --

MR. MARQUINO: The best case would be it never comes out. So if we had --

MEMBER BANERJEE: No. But if it comes out slowly over a period of time. So then what happens? Does anything happen to your long-term cooling scenario?

MR. MARQUINO: Then it causes differential pressure, because the PCC is venting. But as I said, we have some radiological gas production that is being formed anyway and causing this differential pressure to be basically maximized over the whole three days.

MEMBER CORRADINI: We are going to have to move on. They still have to get to Chapter 18 eventually.

MEMBER BANERJEE: What would be reassuring is to hear that if you made mistakes in your calculation it doesn't make too much of a difference.

MEMBER CORRADINI: I think, though, from the Subcommittee meeting what we heard, both by the

GEH and the staff and their consultant, was that as -- to get back to Wayne's answer, which is if you have continual production, then you have this leakage effect, which will build total pressure.

But the more you hold it up, the lower your wetwell pressure is. And then, the PCCS, because you will get this bursting effect, you will get this oscillatory -- clearing, condensing, clearing, condensing -- it will regulate through. So they would like this to be able to show us that there is less non-condensables in the wetwell. But they can't, so they assume the most they can possibly transport. That's the way I understood the explanation.

MR. ALAMGIR: Correct.

MR. MARQUINO: Next slide, please.

MEMBER BANERJEE: He is not going to challenge that, Mike.

MEMBER CORRADINI: I am just repeating what we heard. I just want to make sure I'm hearing it correctly.

MR. MARQUINO: The reason I'm showing this slide is because --

MEMBER CORRADINI: I was going to say, do you really want to show this slide?

MR. MARQUINO: I'm not going to say

anything about it, except we showed it to the Subcommittee and we didn't have the curve numbers in chronological sequence. So we have revised it, and they are in chronological sequence now and --

MEMBER CORRADINI: So it's not what?

MR. MARQUINO: -- there's good information on here. I'll only say that we -- in the prototypical tests and the Panda tests, we investigated all five different possible flow conditions in the PCCS system.

Next slide, please.

There is two -- we talked about some of this already. When we refilled the vessel, it's full of -- it's continuing to get flow from the GDCS pool, and some of the decay heat is going into -- taking out the latent heat from that water.

So in the main steamline break scenario, the steaming from the core during a period will be less than the decay heat. Because the steaming isn't happening, that decay heat is not being removed by the PCC, so you can see on the main steam break plot a deficit between the PCC heat removal and decay heat in the early portion of the event.

And then, around 15 to 18 hours, there is a point where we spill cold water out of the break at the top of the vessel. And this causes a little blip and redistribution of non-condensables in our analysis.

So we had some hideout non-condensables in the drywell head, for example. They become available to the PCC. The PCC has to vent them to the wetwell, and we see a reduction in the PCC heat removal during that venting phase.

Next slide, please.

MEMBER POWERS: Before you go on --

MR. MARQUINO: Yes.

MEMBER POWERS: -- could you explain to me how you calculate the radiolytic gas production in the core?

MR. MARQUINO: We use the same NRC G value that is used for combustible gas calculations. I don't have the specific document on the tip of my tongue, but I can get it for you before the end of this morning. But we are using a high value for the radiolytic gas production in the analysis.

MEMBER POWERS: It's a gas-based G value?

MR. MARQUINO: Yes, it assumes boiling in the core.

MEMBER POWERS: It seems to me that there has been quite a lot of work on that issue in recent years. Is it consistent with what you --

MR. MARQUINO: Yes, it is, in that we don't have a lower -- we haven't found anything that would indicate a higher value than what we are using. And I'd like to get some references from you, because we are very interested in justifying a lower value for this.

MEMBER POWERS: What if you are not happy with what it gives you? I bet you'd want to hear about it.

MEMBER CORRADINI: They are open to the proper information.

MEMBER BANERJEE: Is it much higher?

MEMBER POWERS: Yes, I would say it's double, depending on your circumstances.

MR. MARQUINO: So double the regulatory guidance, or double other tests?

MEMBER POWERS: I'm not familiar with the regulatory guidance.

MEMBER CORRADINI: I think right now, though, to get to your point, which is we asked this I think in the January meeting, is that they are essentially following the reg guide, which -- and I don't remember if -- at the Subcommittee Tom was asking in detail about feeling it was too high. But that's what they're using now is they're using a regulatory guide value.

MEMBER POWERS: There has been a lot of water run over the dam since the reg guide --

MEMBER CORRADINI: Right.

MEMBER POWERS: So that's I have. I guarantee you the reg guide was written -- they did not have the specifics of the ESBWR in mind.

MEMBER CORRADINI: Yes.

MEMBER POWERS: So it will be interesting to look at that.

MR. MARQUINO: We're --

MEMBER POWERS: What kind of dose rate do you have in your atmosphere?

MR. MARQUINO: I don't know offhand. I'll have to get back to you on that.

MEMBER POWERS: In the core region, it must be a pretty ferocious dose rate.

MR. MARQUINO: Yes, yes.

MEMBER POWERS: It must be, what, 30 megarad kind of dose rate?

MR. MARQUINO: It's --

MEMBER POWERS: Thirty megarads per hour?

MR. MARQUINO: It's a power -- the power density is similar to BWR-6 and ABWR.

MEMBER POWERS: So you've got, what, about

38 megawatts?

MR. MARQUINO: Fifty-four kilowatts per liter power density.

MEMBER POWERS: Okay.

MR. MARQUINO: All right.

MEMBER BANERJEE: If you doubled your radiolytic gas generation rate, would it matter?

MR. MARQUINO: It would matter because it would provide more non-condensables that pressurize the wetwell airspace. And if it lowered it, it -- we'd pressurize the wetwell airspace less.

I think you would have to lower it very significantly before it would affect the -- whether the PCC was purging or not, and at that point you would see a big reduction in the containment pressure.

MEMBER BANERJEE: But raising the gas production rate, would it just mean that you have to -- I mean, you have your -- these fans that get turned on and things like that, right? Would you have to turn them on a bit earlier than --

MR. MARQUINO: It wouldn't affect -- no, no, it wouldn't affect that. But the -- and we've done sensitivity studies where we -- we've turned off the radiolytic gas production. We see like a three psi reduction in the containment pressure between the value we are using and no radiolytic gas.

MEMBER BANERJEE: For what period of time?

MR. MARQUINO: Over 72 hours' effect on the maximum containment pressure.

MEMBER BANERJEE: So if you doubled it, you'd get three psi more, in rough terms.

MR. MARQUINO: Yes, more or less.

So our focus to this point has been calculating a pressure at 72 hours, or three days, during which we are coping passively in the plant. After three days, we have some written assistance that we can credit, and I'll get to those in a moment.

There are some features in our analysis that are intended to maximize the pressure at 72 hours. One is radiolytic gas production, as we discussed. Another is drywell nodalization to mix non-condensable gases. This was established in the initial TRACG review, and then we had some interactions with the staff where, because of design changes, we changed the nodalization and they pointed out that we were retaining some of the non-condensables in the drywell. So we made a change at the staff's suggestion to better mix the GDCS airspace.

MEMBER ABDEL-KHALIK: So of the wetwell

pressure of roughly 375 kiloPascal at 72 hours, what is the contribution of the non-condensable gases? What's the partial pressure of non-condensable gases?

MR. MARQUINO: I think it's two-thirds to three-quarters of the pressure, and the remainder is the saturation pressure of steam in the pool.

MEMBER ABDEL-KHALIK: And of that two-thirds, how much of that is from the radiolytic gases?

MR. MARQUINO: It's a pretty small fraction. Less than 10 percent, maybe less than five percent.

MEMBER ABDEL-KHALIK: So if that amount were to double, your total pressure would not exceed your design pressure. Is that correct?

MR. MARQUINO: We are -- in this present analysis, no. In the future, I'll show a couple of slides that we're going to be setting the analytical pressure at the design pressure. So in that case it would be above the design pressure. But as I said, I think we have a conservative value for the radiolytic gas source.

MR. ALAMGIR: Professor Khalik, 60 psi is the design limit. Three-quarter means it's about 45 psi of non-condensable. We just heard three psi is the effect of radiolytic gases. So three versus 40, 45, so that's the effect.

MEMBER ABDEL-KHALIK: No. I'm trying to see how much of an error bar do I put on that pressure history line.

MR. ALAMGIR: Based on core radiolytic gases?

MEMBER ABDEL-KHALIK: Right.

MR. ALAMGIR: Based on the numbers, it looks like three out of 45.

MEMBER BANERJEE: The other issue that arose, of course, related to stratification and heat conduction in the wetwell airspaces. How sensitive are the results to that?

MR. MARQUINO: That's exactly the focus of this slide.

MEMBER BANERJEE: Okay. So maybe you can tell us, then.

MR. MARQUINO: In general, our philosophy is, if mixing is bad, assume mixing. And if mixing is good, assume no mixing. So --

MEMBER BANERJEE: Is that the same as stratification?

MR. MARQUINO: Yes. So it talked about how we produce mixing in the drywell because it's bad. In the wetwell airspace, mixing is good, and we -- we block the topic of the airspace from the lower part of the wetwell to force stratification there and keep any steam that leaked in from the drywell up at the top, keep it from condensing on the surface of the pool.

MEMBER BANERJEE: All right.

MR. MARQUINO: And, similarly, in the wetwell pool, we force stratification. We know that when there is steam discharge at a certain elevation in the pool, that does produce good mixing. So early in the event when we are discharging through the bottom vents or through the safety relief valve quenchers, we'll have mixing throughout the whole pool, and we credit that.

But then, later when the vents start to turn off and the bottom two of three vents are turned off, we block flow from the bottom of the pool to the top, and we force stratification, and that maximizes the temperature of the upper layer of the pool.

MR. ALAMGIR: Which maximizes the pressure.

MR. MARQUINO: Yes.

MEMBER BANERJEE: And what about the heat conduction and things like that in this region to the walls? There was some discussion about this, which I didn't completely follow, so it --

MR. ALAMGIR: Can I just -- as Wayne said, we don't take credit for the drywell -- the wetwell head condensation.

MEMBER BANERJEE: Right.

MR. ALAMGIR: We did a sensitivity study on the effect of condensation on the walls of the drywell. Very little impact on some of the results we see here.

However, if we did take that, the convection currents due to condensation, that would promote mixing. That's my personal opinion, having worked in mixing area.

MEMBER CORRADINI: We are going to have to move on.

MR. ALAMGIR: Yes. So that's --

MEMBER CORRADINI: We've got to get to the Chapter 18. So I'm going to ask you to conclude. And if you want to move to the vacuum breakers, that's the last thing I think we have to discuss.

MR. MARQUINO: Okay. Can I just talk about the last two bullets? If you can back up. The question was about heat transfer. We've considered heat transfer for the areas where it has an adverse effect, and that's heat transfer from the drywell into the wetwell airspace. So we certainly have those heat transfer paths modeled.

We also have some paths modeled which would reduce drywell pressure, and that is from the wetwell airspace to the reactor building. We don't have credit for the drywell to reactor building heat transfer. That would be beneficial, but we don't think it's a significant effect anyway.

Next slide?

MEMBER ABDEL-KHALIK: Mr. Chairman, I do understand the pressures of time, and I am sympathetic to that. However, early on in the presentation we spent 10 minutes listening to people telling us how many RAIs are open and how many have been closed, which was totally vacuous in terms of information transfer.

So perhaps we ought to be sort of more insistent on, you know, value added in these presentations in the future.

MEMBER CORRADINI: Point taken. But I don't want to get -- and put Chapter 18 at a loss of time.

MEMBER ABDEL-KHALIK: I totally understand, totally sympathetic to the concern. But I would like to register sort of a complaint about the nature of earlier presentations, inasmuch as it takes away from the time available to the Committee to discuss relevant issues.

MS. CUBBAGE: I understand. And if additional detail is needed on these topics, I think it would be more appropriate for a Subcommittee. We were trying to respond to the Committee's request to address it here today.

MEMBER BANERJEE: But we need to write a letter, right, Mike?

MEMBER CORRADINI: If you don't want to, we don't have to.

MS. CUBBAGE: We are not requesting at this time that the Committee prepares a letter that says that these issues are closed, because they are not closed.

MEMBER CORRADINI: I think the key thing is that these are all open items that we are hearing information on and as they progress towards answering the staff's questions.

MEMBER ABDEL-KHALIK: Okay. Thank you.

MR. MARQUINO: You have the handouts. You can read these slides. The point of this slide is in the Rev 5 DCD we are going to be maximizing the drywell-wetwell leakage, which will put the containment at its design pressure, within like one or two percent.

The point of doing that -- next slide -- is to provide additional margin to the utility for their surveillance testing, to make sure that within the measurement accuracies of this leakage flow we will have some operating margin.

Next slide?

MEMBER BANERJEE: Now, this is very interesting, actually. I guess at the Subcommittee meeting this wasn't presented.

MEMBER CORRADINI: We had this.

MEMBER BANERJEE: We had this?

CHAIRMAN SHACK: It doubled it basically over the four.

MEMBER BANERJEE: Ah, okay.

MR. MARQUINO: One square centimeter, two squared centimeters.

MEMBER BANERJEE: Yes. We had it for one. Okay. So now you have gone to four. Because I hadn't --

MR. MARQUINO: We are going to two.

MEMBER BANERJEE: Two, okay.

MR. MARQUINO: Next slide?

After three days, we take credit for active systems, and they are classified as RTNSS --

RTNSS regulatory treatment of non-safety system.

For the containment, we refilled the pools, recovered the heat exchangers, and we also -- that provides initially a good reduction in pressure. But the pressure would rebuild basically back up to the same level once boiling begins in the pool compartment.

To achieve a sustained reduction in pressure, we turn on small fans that are in the vent line of each PCC, and that circulates the non-condensable gas around, so that instead of the heat exchanger being blanketed along a length of the two with non-condensable it has the same non-condensable fraction over the whole length of the two.

We had some questions from the Subcommittee about whether the fan was going to have anything to pump. The fan always has something to pump. It will either be pumping steam and circulating steam around or a non-condensable and steam mixture.

MEMBER BANERJEE: Well, there was a concern as to whether it would pump around liquid. That was the issue. Because obviously you get some condensation during this process, if there was steam going through the heat exchanger, and there would be some liquid. Now that has to be separated out before the pump.

MR. MARQUINO: Yes. And the vent location in the lower drum is designed to prevent liquid, as much as possible. But there might be some liquid droplets that --

MEMBER BANERJEE: Carryover, yes.

MR. MARQUINO: -- that would be going through the fan, yes.

MEMBER BANERJEE: Because that was not meant to separate things under forced convection conditions, right? I mean, it is basically an impacter from what I see.

MR. ALAMGIR: The concern was whether the fan will function properly with such droplets.

MEMBER BANERJEE: Yes.

MR. ALAMGIR: Is that --

MEMBER BANERJEE: That's correct. I think one might have --

MEMBER CORRADINI: I don't think you're going to want to answer this on the fly.

MEMBER BANERJEE: But that was sort of the concern at the Subcommittee meeting, if I remember, if there is some liquid drawn in, what would happen. Maybe the liquid can't be drawn in. You could convince us of that. But if it could be drawn in, then how would it affect the operation?

MR. MARQUINO: I am trying to give the people that aren't on the Subcommittee a brief overview. The other RTNSS system is passive autocatalytic recombiners, a catalyst that combines hydrogen and oxygen, and they would take out this radiolytic gas source that is compressing the wetwell.

We only credit them at 72 hours, but realistically they'd be working over the whole -- over the whole duration of the event, or at least from four hours to 72 hours also.

So get a good reduction. We could stay in this mode indefinitely, but we also are documenting the post-LOCA recovery.

Next slide?

And in the post-LOCA recovery, we'll use an active system that takes water out of the suppression pool, puts it through a heat exchanger, a pump, and then initially returns it to the suppression pool. After cooling the suppression pool down and reducing the energy in the containment, we put it into a vessel injection mode, and there -- that's the eight-day curve.

The second hash from the right you see a reduction to almost atmospheric pressure when we go to vessel injection.

CHAIRMAN SHACK: Since your PARS is a passive system, why don't you credit it earlier?

MR. MARQUINO: Because it would make it high regulatory oversight in a tech spec system.

MEMBER CORRADINI: That's what I thought.

MR. MARQUINO: So we're continuing to work with the staff to answer questions. There is some -- there is some people involved that are doing a very thorough review of our calculations, and that's good. We want to get them the data they need to do alternate calculations, and we want to provide you the data from our calculations. The staff has done audits in this area.

Thank you very much for your attention.

MEMBER CORRADINI: Thank you.

We are going to move on to vacuum breakers briefly.

MR. DIAZ-QUIROZ: Thank you, and my name is Jesus Diaz-Quiroz. I'm from GE-Hitachi, and I will be giving a quick summary of the vacuum breaker, some qualification, going through some of the results, and also the logic involved in isolation valvage which is placed upstream of that valve.

Here on the slide it's quite wordy, but

the vacuum breaker isolation valve demand logic is -- I want to make it clear that this valve is independent of QDCIS, which is our safety-related control system, and any other control systems.

It's -- demand logic is processed on similar ATWS SLIC NUMAC-type components. That's a lot to do with common cause mode failure. We want to make sure that's not -- doesn't come into play here.

Each vacuum breaker isolation valve logic is independent of the others, and there's a total of three -- of course, three vacuum breakers. There are four divisions of instruments per vacuum breaker isolation valve, and vacuum breaker, that is, too, as well.

This provides or prevents inadvertent closure of that value, the isolation valve. It also provides N minus two requirements, so you can have one division off service and still be able to survive this.

MEMBER STETKAR: Because of the time here --

MR. DIAZ-QUIROZ: Would you like me to just go ahead --

MEMBER STETKAR: Let me just ask you a couple of questions.

MR. DIAZ-QUIROZ: Sure, go ahead.

MEMBER STETKAR: Two quick ones. Number one, I seem to recall reading that those vacuum breaker isolation valves are solenoid-operated valves and they fail closed. Is that correct?

MR. DIAZ-QUIROZ: No, no, no. That was an earlier preliminary design, and -- which was subsequently being changed out.

MEMBER STETKAR: So do they --

MR. DIAZ-QUIROZ: They do not --

MEMBER STETKAR: What type of valve is it?

MR. DIAZ-QUIROZ: At this point, we've chosen preliminary valve. It's a triple-offset butterfly valve. It does not close. It is not a fail close. It's fail as is.

MEMBER STETKAR: Fail as is, okay.

MR. DIAZ-QUIROZ: Fail as is. And it's pneumatic, of course, nitrogen operated.

MEMBER STETKAR: Nitrogen operated.

MR. DIAZ-QUIROZ: Yes.

MEMBER STETKAR: Okay, great. Thank you. And it's DC power supplies for the solenoids?

MR. DIAZ-QUIROZ: Yes.

MEMBER STETKAR: From where?

MR. DIAZ-QUIROZ: This would be safety-related power as well.

MEMBER CORRADINI: And one last -- just clarification. It's upstream of the actual --

MR. DIAZ-QUIROZ: Right. And we'll show that a little bit --

MEMBER CORRADINI: That's fine.

MR. DIAZ-QUIROZ: And, again, this logic does not attempt to quantify the leakage of the vacuum breaker, but to determine that it is leaking, and that's based on sensing a differential temperature, which I will quickly discuss.

I will quickly go in here -- the primary demand logic, single, will be the result of a temperature differential between the vacuum breaker cavity, which I will show in the later figure here. And when the wetwell exceeds -- when that differential exceeds a predetermined setpoint, how long would it not be bypassed, that division. Two out of four divisions, of course, for the bump conditions. And also, two out of four divisions in which -- provided by LOCA permissive.

Next slide, please.

MEMBER BROWN: That's that last one about?

MR. DIAZ-QUIROZ: If there's a LOCA permissive, in this case you want to make sure that you are in the LOCA. So we'll go into what that is.

MEMBER BROWN: So these valves only operate if you're in the LOCA condition.

MR. DIAZ-QUIROZ: Right. You don't want to isolate during normal conditions.

Next slide, please.

Here again, there is also secondary demand logic, which is similar to what -- which is the same logic features as the primary logic, but in this case there are four proximity probes on the seat of the vacuum breaker, each 90 degrees apart, which indicate not full closed. And there is also one proximity probe on the stem, which I'll point out on the figure, which indicates a full open.

And the vacuum breaker isolation valve will close automatically if it sends us a differential temperature between the -- again, that cavity between the vacuum breaker and the isolation valve and the drywell. And, of course, that has to be -- exceed a predetermined setpoint. And along with that as well there is that same LOCA permissive, and the proximity probes would indicate the vacuum breaker not full closed.

There is -- there are provisions for manual control, so that the operator in the control room can open and close these individual vacuum breaker isolation valves as needed. And these manual controls are independent of the primary and secondary logic.

MEMBER BROWN: Even if there is a signal to close it, the operators can open it.

MR. DIAZ-QUIROZ: The operators -- yes, are allowed to do that, yes.

Next slide, please.

Here --

MEMBER BROWN: Can I just mention one -- if you didn't notice it, in Wayne's slides on page 4 there is an actual photograph of one of these.

MR. DIAZ-QUIROZ: Yes. Sorry I failed to mention that. It's fairly vague, as you've noticed. It's got a footprint of about two and a half, two and a half --

CHAIRMAN SHACK: We call it a manhole.

(Laughter.)

MR. DIAZ-QUIROZ: I'd like to quickly point out where -- this is the isolation valve, this is the vacuum breaker itself. The temperature sensors that would give that input into the logic would be located in the drywell next to the outlet screens inside the cavity which is created by the vacuum breaker and isolation valve and in the wetwell itself.

I also would like to point out near this area that's highlighted where the elastomeric EPDM seat is shown here, and the proximity probe -- again, there is four of those. They are 90 degrees located apart, and another proximity probe that indicates full open is located on the upper bearing stem here.

MEMBER SIEBER: The isolation valve is not leak tight, right?

MR. DIAZ-QUIROZ: Not leak tight, you said?

MEMBER SIEBER: Yes.

MR. DIAZ-QUIROZ: Yes, it would have to be to the same specifications as the vacuum breaker itself.

MEMBER SIEBER: No seat?

MR. DIAZ-QUIROZ: Right, right. It has no -- it has no soft seat, if that's what you're asking.

MEMBER SIEBER: Yes.

MR. DIAZ-QUIROZ: Right. It's this type -- we can give more specifics about the actual preliminary design of that, if you'd like.

MEMBER SIEBER: That's all right. I understand the drawing. Thank you.

MR. DIAZ-QUIROZ: Yes. The drawing of

course is --

MEMBER CORRADINI: A drawing.

MR. DIAZ-QUIROZ: -- a drawing.

Next slide, please.

MEMBER STETKAR: Jesus? Could you go back to that other --

MR. DIAZ-QUIROZ: Sure.

MEMBER STETKAR: -- slide, just for a second? Because we're really tight on time.

MR. DIAZ-QUIROZ: No, that's fine.

MEMBER STETKAR: The damper piston --

MR. DIAZ-QUIROZ: This?

MEMBER STETKAR: Yes. There's a couple of bearing surfaces there. Have you looked at the -- I mean, you're paying a lot of attention to the seats and things like that. We're concerned about the valve not reclosing. If those things -- if there's any kind of interference there, the valve is not going to reclose.

MR. DIAZ-QUIROZ: Right. And later on there's a slide that summarize some of the --

MEMBER STETKAR: Okay.

MR. DIAZ-QUIROZ: -- some of the testing, which was actually placed on the stem and such --

MEMBER STETKAR: On the stem?

MR. DIAZ-QUIROZ: On the stem all throughout the inside of the terminals of the valve, and it was cycled quite a few times.

MEMBER SIEBER: You can't lose concentricity by grit and other foreign matter.

MR. DIAZ-QUIROZ: Right. To bind it in that --

MEMBER SIEBER: It could foul up the bearings.

MR. DIAZ-QUIROZ: Right. So grit was used for that.

LOCA temperature evaluation was conducted to be able to look at what should -- that temperature differential should be, and that was looked at for large, medium, small break LOCAs.

LOCA permissive -- the LOCA permissive signal has been established drywell temperature -- difference, that is. Here it is shown as just actual, but it is really a difference, of greater than or equal to 90 degrees C. So that would be the permissive itself. That would be the temperature difference.

This will enough margin to cover normal plant operation. Of course, again, we don't want to isolate these valves during normal operation.

This has been shown to give a reasonable response -- for the large to small break LOCAs, half-second to 50 seconds. And for the very small, in this case for instance a standby liquid control line break, 600 seconds.

And here we show that delta. How is that calculated? Again, it's just drywell temperature minus the wetwell. And this delta T throughout the vent from initial blowdown to 72 hours varies from 90 degrees, the high 225 degrees.

And the next slide will give a summary of -- for each type of break that was looked at and what that delta, as -- from initial -- around initial blowdown to 72 hours, and you can see, again, bottom drain line break, which is the BDL row there, the second-to-the-last row there, shows the 90 degrees. And you can see at 72 hours you get this 25 degree delta for most of the events.

When the vacuum -- how do you isolate again? What's the signal that's given to the vacuum breaker isolation valve? And, again, it's -- we're looking at the temperature in the cavity as it approaches the temperature in the drywell. That means it's leaking, it's heating up inside.

So that, again, is -- it's the difference between the cavity and the wetwell has to greater than or equal to some percentage of that difference between the drywell and the wetwell, which will give that signal. Right now that percentage is around 80 percent, and it is yet to be finalized.

Next slide, please.

This is a very busy slide here, but this discusses the sealing surfaces of the vacuum breaker. And, again, it's the elastomeric seal, so on and so on.

In the test program, it was -- went through aging degradation. Leak tightness was simulated with the LOCA debris. It was fully tested and qualified, and it was confirmed.

Some of the sensitivities that went along with that were various pieces of chips were put between the soft seal and the hard seal, from 12 to 50 mils, and this showed that -- and I'll show a later curve that shows the summarized test results, that the leak caused by that was well under what we set out to be the acceptable leakage.

Again, this last bullet just discusses proximity probes.

Next slide?

MEMBER SIEBER: Again, these -- I asked

about grit on the pistons. These are all grit on the sealing surfaces.

MR. DIAZ-QUIROZ: On the sealing surfaces as well. Right, it was --

MEMBER SIEBER: I didn't see anything here that says you actually looked at grit on the piston.

MR. DIAZ-QUIROZ: Right. And I apologize for the slide. It's not -- does not encompass all of the test reports. I believe those were made available to the staff, and that goes into more detail as to how it was introduced into the vacuum breaker.

MEMBER ABDEL-KHALIK: How big is the clearance on that stem?

MR. DIAZ-QUIROZ: I'm sorry. The clearance?

MEMBER ABDEL-KHALIK: How big is the clearance on the stem?

MR. DIAZ-QUIROZ: Well, and that's -- I don't have that detail.

MEMBER ABDEL-KHALIK: So if the thing is tilted --

MR. DIAZ-QUIROZ: It's a bearing. It has to give it a -- it's tight. I don't have that number.

Next slide, please.

Here, this is a -- this quickly summarizes the test results where you can see A through C shows the leakage for range of differential pressures across the vacuum breaker. And you can see they are very low. Only do you start seeing any leakage at E here, curve E, where we actually introduced that grit. So that actually did cause some leakage.

But as you can see, the acceptable -- maximum acceptable leak rate here --

CHAIRMAN SHACK: Is that in the days when you had a 1 cm area as the maximum?

MR. DIAZ-QUIROZ: No. This maximum here?

CHAIRMAN SHACK: Yes.

MR. DIAZ-QUIROZ: No, this is actually based on two-tenths of that -- of one centimeter squared, so .2 centimeters squared. That's that leakage. That's what that's based on. So it's much lower than the one previously used and the two now that's going to set the level -- maximum bypass leakage.

And then, thereafter, it went through reliability testing. It was cycled 10,000 cycles, and through that cycling leakage did increase, but it did stay well below that acceptable leakage that was established.

And the other curves here -- F prime, F

double prime, are just -- the surfaces were cleaned and such, so --

CHAIRMAN SHACK: How many cycles did you say you had after the grit?

MR. DIAZ-QUIROZ: Ten thousand.

CHAIRMAN SHACK: Ten thousand.

MR. DIAZ-QUIROZ: So, and then, thereafter -- so that's the top curve right there. And then, F prime, F double prime shows after they were cleaned out, and the soft seal itself was pasted back on. It was removed and cleaned and put back on. So that did improve its performance, but, again, that was --

MEMBER SIEBER: When you qualified the soft seal, did you do Arrhenius temperature testing and radiation testing?

MR. DIAZ-QUIROZ: Yes, it was aged thermal radiation, yes. And that's included in the test reports as well.

MEMBER SIEBER: Nuclear radiation or -- you said thermal radiation.

MR. DIAZ-QUIROZ: It was both. Sorry.

MEMBER SIEBER: Thermal aging and radiation.

MEMBER BANERJEE: In some of these curves, the leak rate goes down as the differential pressure increases. Why does that happen?

MR. DIAZ-QUIROZ: Right. And that would --

MEMBER CORRADINI: That's how it's designed.

MR. DIAZ-QUIROZ: Right. From a soft sealed point of view, yes, they -- the greater the differential pressure, the more you clamp down on that, so you create a tighter seal. That's the reason why you see it. But as you can see, it does go down, but it doesn't increase dramatically.

MEMBER BANERJEE: How it goes up, E.

MR. DIAZ-QUIROZ: I'm sorry?

MEMBER BANERJEE: E goes up.

MR. DIAZ-QUIROZ: E goes up, right. And E is, again, where grit was adjusted and went through several cycles. And in that case, you had -- right, it goes up. So it seems kind of at odds. I would have to --

MEMBER BANERJEE: Yes. You'd have to look at this and try to understand --

MEMBER CORRADINI: This is a curve from the test program report you sent us, Jesus.

MR. DIAZ-QUIROZ: Right.

MEMBER CORRADINI: So you won't have to go back and --

MR. DIAZ-QUIROZ: It's a curve that consolidates all of those results there.

CHAIRMAN SHACK: I was going to say, if we look at the data, we might see some interpretation of how you drew the curves.

MEMBER CORRADINI: I was going to say, there might be some scatter.

MR. DIAZ-QUIROZ: Right, right. There will be scatter, right. So this is an interpretation of that data. Right.

In conclusion, I just -- I would like to add that this vacuum breaker was well tested. It was -- it did go through some rigorous testing, and now that, of course, it has been clearly identified and it always has been so that this is a critical component, leave that as this isolation valve to assure that any leakage will be stopped if it does occur.

MEMBER MAYNARD: I have a comment on your isolation valve.

MR. DIAZ-QUIROZ: Sure.

MEMBER MAYNARD: And it's a very large butterfly valve. I have experience with butterfly valves. It's typically difficult to get them to set up for real good leak-tight isolation. I think that might be a real maintenance issue and a testing issue.

MR. DIAZ-QUIROZ: Right.

MEMBER SIEBER: That's not even built like a regular butterfly valve.

MR. DIAZ-QUIROZ: Right. It's --

MEMBER SIEBER: It doesn't have a seat. It's a clearance fit.

MR. DIAZ-QUIROZ: Right.

MEMBER SIEBER: It's just flowing down and the air was going through it.

MEMBER MAYNARD: I understand. For what we're talking about, for the size of this thing and for the leak rate that we are trying to maintain and stuff, I think it could be a maintenance issue and trying to keep that within the specifications or --

MEMBER SIEBER: I don't know that we have a spec for that.

MR. DIAZ-QUIROZ: Yes. We have taken it under consideration.

MEMBER ABDEL-KHALIK: The diameter of the pipe in which this butterfly valve is located is --

MR. DIAZ-QUIROZ: Is approximately 24 --

MEMBER ABDEL-KHALIK: Inches.

MR. DIAZ-QUIROZ: -- inches.

MEMBER ABDEL-KHALIK: Right. Which means

that for two centimeters squared leakage area it's a tenth of a millimeter.

MR. DIAZ-QUIROZ: Right. It's not -- right. So you have a hard seat and a soft seat to assure that, and this disk itself -- the vacuum breaker disk itself is about 200 pounds itself. So it's a very large, heavy disk.

CHAIRMAN SHACK: Right. We were worried about the butterfly valve.

MR. DIAZ-QUIROZ: And the butterfly -- right, right. And this is -- right, because it's the backstop. Right. It doesn't have a seat. It's sort of a -- I can go into more details as to the selection that we went through, but --

MEMBER BLEY: Did you do testing on the leakage of the butterfly valve?

MR. DIAZ-QUIROZ: No, that's vendor provided.

MEMBER BLEY: That's not going to come anywhere near the --

MR. DIAZ-QUIROZ: No.

MEMBER BLEY: If the check valve is not doing what it's supposed to do, this isn't going to help much unless it's wide open.

MEMBER SIEBER: Well, it prevents the

check valve from lifting.

MEMBER STETKAR: That's right.

MEMBER SIEBER: It will sit there and maybe flutter a little bit, but if --

MEMBER STETKAR: But if it's stuck open, it's not going to --

MEMBER SIEBER: That's really what the purpose is.

MEMBER STETKAR: -- it's not going to isolate it.

MEMBER MAYNARD: The 2N series might keep you --

MR. DIAZ-QUIROZ: And with that --

MEMBER CORRADINI: Thank you.

Will GEH proceed on with Chapter 18, or will we go back to the staff?

MS. CUBBAGE: Actually, while they're setting up, I think Mike Snodderly would like to make a few comments.

MEMBER CORRADINI: Sure.

MS. CUBBAGE: And he can go ahead and start while --

MR. SNODDERLY: Yes. While people are getting set up, I think I just wanted to mention two things. Well, first of all, I wanted to say I thought that this was the best presentation GE has given on these issues, on the containment issues, and we're following all of those.

There's only two issues that I've heard come up at the Subcommittee and this morning that the staff is not pursuing that the Committee has shown some interest in, and I'll just mention two of those. One is, as Dr. Powers mentioned, radiolysis. They are using the G values that are in Reg Guide 1.7, Revision 2. It's the one that Richard Guito at Sandia National Labs helped develop.

That G value is a function of pH, so if the pH is below four, the radiolysis rate could be doubled. But we haven't seen evidence that would suggest that we believe it would be that low, so we are comfortable with that -- with the G value that they have chosen. So the Committee might want to bring that up.

The only other thing was whether the wetwell ceiling was modeled adequately or not. We feel that the modeling of the ceiling was conservative and is acceptable. So those are -- and we -- I did plan to be here this evening and tomorrow during the letter-writing session, if you want to discuss anything else.

And I will just quickly, just to address Dr. Khalik's -- you know, we knew that we wanted GE to go first, and we apologize for having some trouble getting everybody up here. And the staff was going to go last, because we -- it was more important for them to get you that information and not to have us update on the stats. So we apologize for that, but that was not our intent.

Okay? Thank you.

MEMBER POWERS: Mike, I understood from General Electric that they were using a gas-phased G value.

MR. SNODDERLY: Yes.

MEMBER POWERS: And I don't understand how a gas-phased G value would be affected by pH.

MR. SNODDERLY: Well, if the pH was lower, then that gas value could be as high -- it could be double, based on literature searches that we have done. So, in other words, if the pH in the RCS was lower, then that would cause greater gas production.

And that's why I was saying that we were comfortable with the value that they used based on Reg Guide 1.7, Rev 2. But it could be higher if the pH was lower.

MEMBER POWERS: I'll look at the reg

guide.

MR. SNODDERLY: Okay.

MS. CUBBAGE: And if there is a concern on that, please let us know, and we can pursue that.

MR. STATTEL: Good morning. My name is Richard Stattel, and I want to mention I'm a software engineer as my job title. My background is mainly with I&C engineering, and I am currently assigned to the HFE team of the ESBWR project.

And I mention this because this team is a very diverse team, and it has been a real exciting opportunity to work with them. And I'm very pleased to represent this team and their ongoing activities, because this is really the up-front activities of the overall plant design, as you will see.

So the first slide here we have is basically the conceptual design that we have for the control room design, and there is just a couple of points that I would like to mention -- point out on this diagram.

The safety-related systems are -- we have identified those. The safety-related systems, we have identified some dedicated displays -- if I can figure out how to work this. Right here we have some four -- the four divisions of safety-related channels are right here, and we also have a backup set of safety-related displays right here.

And we provide redundancy on these touch screens. And for the non-safety-related electronic we have the wide panel displays, which are a mode-dependent display, which is a departure from our predecessor design, the Lungman design.

The main reason for that is, for those of you who have been at the old powerplants, typically if you walk into a control room and they're operating in mode 5 and refueling operations, it's very common for an operator -- what's presented in front of him, about 90 percent of what's there is really meant for the 100 percent power operating reactor. And it makes -- it's a challenge for the operators to really make the right decisions and to do the right things in those non-standard modes that the plant really wasn't originally designed for.

So with the new technology, we are able to create some very custom displays and present the operators with the information he needs for the operating mode of the plant.

We have sit-down consoles set up, and you can see that the -- on the non-safety displays here we have a large variety, and these are doubled up for the purpose of supporting use of electronic procedures, and for some plant automation features. Okay? So --

MEMBER SIEBER: If an operator wants to open a valve, what does he do?

MR. STATTEL: What does he do?

MEMBER SIEBER: Take a mouse and put the arrow on a -- on a schematic on the screen and hope he clicks it without going to the wrong one?

MR. STATTEL: Well, what you say is true, and a lot of the different methods for operating equipment are being considered during the HFE process.

MEMBER SIEBER: Have you decided what method you are going to use on this yet? Whether it's going to be mouse, typing in? I notice there is no --

MR. STATTEL: There are actual -- right, there is no real hard switches in the design.

MEMBER SIEBER: Screens and keyboards.

MR. STATTEL: Pretty much what we're coming up with. And these design features haven't really been locked in, but pretty much what we're coming up with is there will be alternate methods of operating equipment. Many of the system mimics will have dedicated portions of the display that would ensure that interlocks are met prior -- you know, to help the operator to determine the correct course of action when operating a valve or a pump.

And then, also, we have some integration -- we are working on some integration efforts for plant automation, such that the procedures that the operator is following on perhaps the upper display, he would be able to actually observe the equipment actually operating on the lower display, and alternately on the large-panel displays that are in the background there, to make that visible to everyone in the control room, because it's really a team effort and that's -- the idea is to maximize the visibility to the shift supervisor and to all of the operators that need to be aware of plant status.

MEMBER SIEBER: So the answer to my question is yes.

MR. STATTEL: Yes. Yes, it is.

MEMBER SIEBER: Thank you.

(Laughter.)

MR. STATTEL: Okay? Okay. So just a few of the program highlights. This is truly a human-centered design, and that's the basis of the program.

MEMBER BROWN: Can I interrupt? Do you have any experience with the touch screen controls and all of the rest that --

MR. STATTEL: Actually, me personally, I

do.

MEMBER BROWN: Do we ever get plants in service where you have this complete layout of nothing but digital control --

MR. STATTEL: Well, actually --

MEMBER BROWN: -- microprocessors --

MR. STATTEL: -- the ABWR design --

MEMBER BROWN: -- is able to start these things?

MR. STATTEL: The actual ABWR design does use plant automation. And they operate equipment using touch screens with interlocks and confirmation. So there is experience there.

There is a limited amount of experience in the domestic plants, the operating plants -- some of the digital upgrades that have been performed and post-accident monitoring systems, and actual turbine control systems using the Mark 6E use this technology.

So -- but the whole scale, where everything is operated electronically, with -- using the displays, this is something that is rather new to the U.S. industry. But it is widely used in the foreign plants.

MEMBER BROWN: What kind of analyses do you do to make sure that you have the failure -- I mean, failures of hard switches and things like that are well characterized for the most part, but there is -- you are now going through multiple -- you are using software effectively to go and control every component in the plant. Is that correct? That's what I got out of your statement.

MR. STATTEL: That is correct.

MEMBER BROWN: Out of your question.

MR. STATTEL: That is correct.

MEMBER BROWN: He answered yes.

MR. STATTEL: Well, the failure modes differ from the old designs with the hard switches. However, with the -- with the new designs, we are able to design in redundancy. As you can see, basically every piece of equipment can potentially be operated from every one of the displays that is there in the control room.

So certainly a failure of a display or a driver for that display, a node box for that display, we consider that in the failure modes and effect analysis.

As far as control system redundance, the Mark 6E, which is the primary backbone for the controls on the ESBWR, has a -- basically, a three-channel redundancy scheme. So that we believe that the redundancy that is established there really covers those failure modes, to a much greater extent than is --

MEMBER BROWN: So all you're doing is saying you have got redundancy, such that if something fails you have got something else with which to operate it, but not necessarily the effect of a failure on inadvertently activating or starting a pump, opening a valve, turning something on or off, what have you.

I mean, I -- that's what I was talking about, not necessarily the -- although both are important, not necessarily the multiple ways to go and operate something.

MR. STATTEL: Right. Well, the design is not --

MEMBER BROWN: Some of my experience with some of the -- in some of the shipboard areas that we had, they used the touch screens and they ended up with inadvertent operation of a number of kind of interesting devices --

MR. STATTEL: Right.

MEMBER BROWN: -- in the aircraft carriers, and they decided that they wouldn't do that any more. They went back to -- that was one of the initial propulsion control schemes. And they ended up getting some engine orders that were undesired at the wrong time, and they went back to the old --

MR. STATTEL: Right. I will mention that we are really only using all digital and all touch screen controls on the non-safety systems. On the safety systems, for the highly important components, there are a set of hard-switch controls that are used as backup for those type of features, for the safety functions that have been identified.

MEMBER SIEBER: So it's going to be a trip switch.

MEMBER BROWN: They are a hard switch. They are backups.

MR. STATTEL: Yes, they are.

MEMBER BROWN: But it still doesn't address the issue of having an inadvertent operation due to the --

MR. STATTEL: No, the inadvertent operation of safety systems is really addressed through the use of a hazards analysis. We do have four independent divisions of the safety systems, and we also have the diverse protection system. So it really -- the single failure of a division doesn't prevent the safety function from occurring, nor does it -- nor would it accurate the safety function.

MEMBER BLEY: I think you are missing what he's asking, if I understand you right, Charlie. The question really deals with the human using that interface --

MEMBER SIEBER: That's right.

MEMBER BLEY: -- and problems that can occur because of him trying to use that touch screen and getting the wrong thing going at the wrong time.

MEMBER SIEBER: That's one item. Yes, that is.

MR. STATTEL: And you had asked if we had performed analysis, and, really, that is part of our process, because it is really -- the HFE process that is outlined in NUREG-0711 really has us get the operators together, and we're designing this control room around the operator rather than the traditional backfitted and do the HFE activities after the fact.

So we are currently having meetings. We have a group of about 30 operators, and people from other disciplines participate in the functional requirements analysis, and also the task analysis activities. And when we have those meetings, we -- part of the process is we do discuss the potential failure modes of the systems and how the operator reacts to that.

Now, down the line, when we have our simulator built, we will validate all of the assumptions that we're making during these task analysis activities today in the actual control room, so we'll actually monitor the performance of operators that are running through the postulated failure modes and events that we are discussing today.

MEMBER BLEY: May I ask you a related question? During the Subcommittee meeting on this chapter, we talked some about the fact that most of the hardware or software design, and procedures coming out of the HFE process --

MR. STATTEL: That is correct. That is correct.

MEMBER BLEY: -- won't actually be in place and reviewed until the COL stage. We were given assurances much like the talk you have given us so far that these designs are moving right along in parallel with the rest of the design development, and that actually some of the boards are designed and some of the procedures are actually drafted.

And I was referred to the NEDO documents where I could see some of that real product that would be approved in the COL stage. I've got a dozen of the

NEDO documents, and essentially all of them, the ones I have found, are only telling me about the process, which is a good process, but aren't showing me any product. Did I get wrong information last time about --

MR. STATTEL: No, that's correct. And we are actually -- that has been brought up as a couple of open items, and we actually do have some lower-level procedures, some implementing procedures that we are actually working to that are -- that are going forward and developing the procedures and the products.

And we are currently working with the NRC to set up an audit, so that they can come in and review those procedures, because those are really beneath the licensing basis here. They are the subset of those procedures.

MEMBER SIEBER: They do have operators on the teams putting this together, which --

MR. STATTEL: Right.

MEMBER SIEBER: -- without them you would really be --

MEMBER BLEY: I would be really upset.

MEMBER SIEBER: -- in really bad shape.

MEMBER BLEY: Yes. I think the process is a good one, but we -- we were told we could see some of these interim products, because some of us are not completely --

MS. CUBBAGE: I think that might have been a misunderstanding, because the NEDO documents are process documents.

MEMBER BLEY: Every one I have seen is a process document.

MS. CUBBAGE: There are outputs of those documents that will be implemented later, and then there are --

MR. STATTEL: But they are actually -- they are implemented, and we are actually working to those procedures, those implementing procedures.

MEMBER BLEY: That's what we wanted to hear something about. Even though that's not what you're approving now, Amy, I understand what -- but we're --

MS. CUBBAGE: I understand.

MEMBER BLEY: At least I'm not completely comfortable with all of this being in the COL stage. And if it is going on, we wanted to see a little bit of it.

MR. KINSEY: This is Jim Kinsey. Could you just maybe clarify the discomfort, because, again, the process documents are the certification documents. So I guess I --

MEMBER BLEY: I understand that.

MR. KINSEY: -- I want to understand the context of the discomfort.

MEMBER CORRADINI: They want to peak under the hood of --

MR. KINSEY: The area under the hood I guess of the available --

MEMBER STETKAR: Let me give you a small analogy. We've seen now two very different, from my perspective, physical designs for a simple vacuum breaker isolation valve. That's a piece of hardware that has evolved quite rapidly, and we've seen nothing about any design of the control room, the entire control room, the entire human-machine interface.

We have seen nothing about this, and we have seen two -- according to the same design principles -- two very different designs of a piece of hardware. Same design principles, same criteria for isolating leakage, same design analyses, same criteria, and so forth, things have changed very rapidly in the hardware area, and we have seen nothing for the human-machine interface, except specifications.

MEMBER SIEBER: The problem with hardware in digital I&C is if you see something now and like it, by this time next year it will be obsolete and something else will be in there.

MS. CUBBAGE: And to follow up on that, the concept -- the design acceptance criteria or DAC approach that is approved by the Commission as Commission policy recognizes that fact of the evolving technology. So if a design certification applicant were to effectively lock in by rulemaking specific hardware technology in this area, the certification lasts for 15 years and then is renewable, and then the combined license applicants come in, get a license that -- and then they construct and then they operate.

By the time this hardware is installed, it could be 20 years from now. So it really wouldn't be appropriate to lock in on the specifics of the types of screens, etcetera, at this stage.

MR. STATTEL: And we have been working hard --

MEMBER BROWN: Hold on. I'm not dealing with that. I understand that fully when I made the comment. Okay? I have been through three different -- or four different in my past career of upgrades that we had to totally redesign everything due to technology movement.

And, but it's -- so I'm not talking about the specifics. I'm talking about the principles behind the application of the technologies. And if you --

MEMBER SIEBER: That's what's getting approved here.

MR. STATTEL: Well, it's the principles that we're trying to establish right now.

MEMBER BROWN: Yes, it's the principles that I'm interested in relative to the operator interfacing with the actual -- whatever the mechanism is he is going to command an action to be taken, as well as, subsequent to that, functionally the principles with which the rest of the control system goes out and tells the particular piece of equipment to do it. So there is a number of phases.

You've got the hardware aspects, which are constantly evolving. You've got the software aspects, which are constantly evolving. But there are principles that you can use that will ameliorate or make less difficult to deal with as you go -- go from plant to plant or from design to design. I mean, you'll be lucky once you've put one of these plants in. You build the next one five years later, you are going to have different hardware --

MR. STATTEL: But the principles will not change.

MEMBER BROWN: -- and different software, but the principles have to stay the same. And so what if anything, processes -- I like process. It's nice. Human factors engineering is valuable. But the process has to be based on some set of principles that you are going to use. And that's what --

MR. STATTEL: In fact, the process development is what --

MEMBER BROWN: As I see these, that is what I would be asking about as we go through this.

MR. STATTEL: Yes. In fact, the process does facilitate developing those principles. And, for example, one of those products is the style guide, and that's kind of what would ultimately dictate how -- you know, the colors of valves and how we indicate to the operator the concurrent status of a pump or a valve or some component.

And that's really -- we are doing that, and we don't -- we certainly don't intend to keep you locked out of what is under the hood, because we want you to see the activities that we have going on right now, because this is really a decisionmaking process that we're going through right now.

And we've had a lot of discussion in the last month over the DAC, the design acceptance criteria, and trying to establish the line of what needs to be DAC and what would be, you know, construction ITAAC items for the --

MEMBER BROWN: What is DAC again?

MR. STATTEL: It is design acceptance criteria.

MEMBER BROWN: Okay. Sorry to be ignorant.

MR. STATTEL: Well, it's basically the design certification would be based on the process for developing the design rather than the completed design itself, since we are kind of doing these HFE activities up front, that will develop the principles that you have mentioned.

And, really, you know, we understand your charter as far as issuing the design certification and having to make that safety evaluation call, and the importance of that. So we want you to have the right information, and we want you to understand that we are following the process that is outlined in the NUREG.

And we have a high degree of confidence that our products that we are creating here, as far as the principles of design -- not specific design -- will be acceptable, right? Now, we have an upcoming audit that we propose. We are kind of working out the logistics of that now.

But I believe that will take place next month, and part of that audit will be opening up our implementing procedures and allowing the NRC to come in and actually participate and see what it is that we do in these task analysis meetings and when we are performing these functional requirements analysis activities.

So that is the place where the decisions are being made about principle, and we want the NRC to see that process in action, because we are in the middle of doing that right now.

CHAIRMAN SHACK: Mike, we are running about half an hour late already.

MEMBER CORRADINI: I know.

CHAIRMAN SHACK: How much longer can you -- do you think you're going to take?

MR. STATTEL: Oh, I'll just be a couple more --

MEMBER CORRADINI: It's out fault, but I will --

MS. CUBBAGE: I mean, we can -- the staff can or cannot present. It's your choice. And we can do it in one slide.

MEMBER CORRADINI: I think we want to have them finish and have the staff conclude, though. I think we'll finish in a few minutes.

Dr. Shack? I don't want to be schedule-driven. Sorry, I had to do that.

(Laughter.)

CHAIRMAN SHACK: We'll be here Saturday morning. Thank you, Mike.

MEMBER CORRADINI: Well done. I deserved that. Thank you.

MR. STATTEL: Okay. Well, I'll just finish up fairly quickly here. The current status is that Chapter 18 is -- the update to that will be submitted of course with the Revision 5 of the DCD. And we have answered all of the RAIs that were associated with that, and I think some new ones just came in, however.

We have -- as mentioned, we have a dozen LTRs, which are also being updated, mostly in response to the RAIs that we have received and answered.

And we have established what we call a HFEITS system, which is a -- the issue tracking system for the HFE issues. And we use this as part of our process in order to collect data and also provide feedback to the design engineering groups, so this is what's driving the design of these systems. And the systems engineers also participate in all of our HFE activities as well.

Okay? We spent a lot of our time analyzing OE, operating experience, and we have brought a lot of operating experience just from the staff that we have hired in. And an example, we have people from the fossil plant that have fossil plant experience, nuclear experience of course from both sides -- boiling water and pressurized water reactors. And also we have -- we have the experience from the Lungman projects and the predecessor designs.

And we are constantly -- well, we are getting up to speed with the OE, and we are also getting on board with just distributing that OE to the design engineers, so they incorporate that into their design as they create it.

CHAIRMAN SHACK: Of course, Lungman isn't operating yet, but --

MR. STATTEL: That's correct. That's correct. But they -- we do share resources, so we keep abreast -- well, they are actually part of our team. We have several people from the Lungman project who participate.

As I mentioned, the functional requirements analysis, task analysis activities, are currently being performed and will be open to the audit. And the program elements are being prototyped through procedures and training that we are offering.

Okay. And that's all I have. That has pretty much hit the high points of what was discussed at the Subcommittee meeting.

MEMBER CORRADINI: Thank you very much.

MR. STATTEL: Thank you.

MR. GALVIN: Okay. We are just going to go through the final summary slide.

MEMBER CORRADINI: Good.

(Laughter.)

MR. GALVIN: It has already been discussed. My name is Dennis Galvin. I'm the Project Manager for Chapter 18, and we have also with us our Technical Lead, James Bongarra.

What we are reviewing is a process. We don't have a design before us. That picture they keep showing is not in the design certification as their concept.

We have made considerable progress in addressing the issues. That is our first bullet.

There is some level of details remaining for some of the implementation plans, which describe the process. Essentially, in some areas they have repeated back our guidance to us. That is really not what we were looking for.

They have cited, well, the actual methodologies in these procedures, so we've -- we're working out logistics of a -- when we're looking at those procedures. I guess based on what we've seen to date, we don't expect any major obstacles.

Jim, did you want to add anything? I think that's the message we have. You've sort of covered most of the points with GEH.

MEMBER BLEY: I would just say one thing. I am -- I am glad to hear what you just said, because this is a process that looks very good. It has been laid out. But becoming convinced that the process is leading to what it is intended to lead to seems to me something we don't want to wait until it's all done about, and it sounds like you're doing that, so I'm pleased to hear you are going --

MS. CUBBAGE: Right. And I will also add that the DAC process -- DAC are implemented through verification of ITAAC, but these are special ITAAC. They are ITAAC that verify the design has been completed in accordance with the process that the staff will approve in the certification. And we will be inspecting and verifying that the design comports with the process before they actually install hardware in the plant.

So there is that checkpoint long before the plant is actually going to be constructed and ready to receive authorization to operate.

MEMBER SIEBER: But the approval is done by inspection, right?

MS. CUBBAGE: The approval of the design and conformance with the process is done through our inspection program.

MEMBER SIEBER: Right.

MS. CUBBAGE: So in that case, we need to make sure, through the review process, that these -- the processes are detailed enough, such that they -- it would be repeatable. If multiple people were to try to implement this process, they would achieve acceptable results.

MR. GALVIN: They have also estimated that, you know, some percentage of the DAC -- design acceptance criteria closure process will involve some level of technical review. So the more technical aspects could involve the technical staff at some level.

MS. CUBBAGE: You are absolutely right, Dennis. I mean, by inspection, we mean that's the process we are in, but it would involve HFE experts here at headquarters and consultants as necessary.

MEMBER BROWN: What do you mean by "inspection"?

MS. CUBBAGE: It's --

MEMBER BROWN: Like in a paper or --

MS. CUBBAGE: Both.

MEMBER SIEBER: Both.

MS. CUBBAGE: The design prior to installation, and then the actual --

MEMBER BROWN: The actual hardware.

MS. CUBBAGE: -- the actual as-built hardware after.

MEMBER SIEBER: The hardware in plants that --

MEMBER BROWN: Yes, okay.

MS. CUBBAGE: And all of that has to be completed and verified prior to the Commission granting authorization for the applicant -- the COL licensees to load fuel.

MEMBER BROWN: Can I make one additional comment?

MEMBER CORRADINI: Sure.

MEMBER BROWN: One of the benefits -- there is downsides to all of this digital-type stuff as well as the upside. And one of the downsides is you can present so much information to people that they lose track of what's valuable and useful in their evaluation of what -- the plant conditions and what they ought to do next.

And I guess -- somebody can correct me if I'm wrong, because it has been 28 years since I looked at it, but when TMI occurred one of the fallouts of that was data overload and the wrong data that -- which the operators had access to. And they were distracted from some of the indications that would have given -- possibly, never say for sure, but possibly given them a clue as to what was going on.

And I've thought through this, and as we developed all of the microprocessor and computer-based systems for the nuclear Navy, and we had -- the laboratories love to present tons of information to the operators, and headquarters was always taking it off the screen and putting just the stuff for certain operations that the operators needed to make sure the plant -- they could control it and make sure that the plant was being operated satisfactorily.

So that's what I will -- you know, I'm not trying to control everything. I just -- since I'm the newbie, I just thought I'd speak up. But that's the kind of stuff I think about in terms of how we apply this. It's good stuff, very reliable stuff. It operates better and more consistently than the -- a lot of the older analog stuff, much cooler and less subject to other drift problems, and everything else.

But you've got to make sure you don't fall into the trap of -- I think we've got all of this good information, and we have just got to get it out there.

So that's just -- it's some input to the thought process. That's all.

MR. BONGARRA: I feel compelled, as the staff lead, to say at least a word. I certainly -- the staff certainly shares your concern, sir, about the potential pitfalls you just identified. And having worked with this process that Rich outlined just a minute ago here for some time, the staff is also confident that the process that we have in place to look at an overall human factors engineering program -- again, at a methodological level -- is a pretty solid one.

We have applied the process to our four previous design certifications, and we continue to apply it certainly to ESBWR and future designs. I'm sorry that I don't have an opportunity here to go into a little bit more detail, to try and really address some of the concerns that I am very pleased to see that the ACRS members have raised on -- Dr. Bley, your question about the details.

We are certainly concerned about the details as well. No question there. And we are, as has been mentioned, planning on yet another technical review of more detailed work instructions where some of these principles that were identified already hopefully will be available to us to scrutinize.

Again, I'd like to talk further about it, but I won't. The verification-validation process where I think a number of issues that have been raised by the Committee may be addressed, and I'd like to talk at some point, if possible, about that. But at this point, I realize we are overdue.

And if the Committee would like to hear more about the staff's efforts to review human factors engineering in Chapter 18, and procedures as well which are part of Chapter 13, and the principles that support those -- the development of those procedures, we would be more than happy to come back and talk with you.

Thank you.

MEMBER CORRADINI: Thank you.

Other comments by the members?

(No response.)

Thank you to GEH and the staff.

And, Mr. Chairman, on time, on budget.

(Laughter.)

CHAIRMAN SHACK: We are on break until 11:30.

(Whereupon, the proceedings in the foregoing matter went off the record at 11:15 a.m. and went back on the record at 11:30 a.m.)

Insights from PHEBUS - FP Tests

CHAIR SHACK: Our next topic are insights from Phebus-FP tests. Again, these are integral tests with application from severe accident Source Term and some very interesting results that they have recently obtained on containment iodine behavior. And Dr. Lee, I assume you will be leading us through this.

MR. LEE: Thank you, Mr. Chairman. It is almost a year ago we came before this committee and reviewed on Phebus. And Bernard Clement was with me at that time and we are pleased to have him back today. Last time, he didn't talk about it. So, at this meeting he is going to give the French view of what the lessons learned from the Phebus tests. So, we are going to let him go first and then the staff would like to share with you what our findings from NRC perspective and what we need to do for the rest, at least two years from now. So, Bernard.

MR. CLEMENT: Thank you, Richard. So I am Bernard Clement from the French Institute for Radiological Protection and Nuclear Safety. So, we are making the Phebus-FP program. And my position in this program is tat I am the scientific project leader.

In this program and also in the following program that is the International Source Term Program. And so as Richard said, I will try to provide you with our main findings, view from ourselves, from IRSN.

Some main lessons learned from Phebus-FP concern fuel degradation, efficient product and material release, their transport in the reactor cooling system, the thermal-hydraulics in the containment building and also aerosol behavior in the containment building, and iodine chemistry. After we will have some words about the status of knowledge and implications and what is the Phebus following program that is International Source Term Program, which a general objective and the different studies, different experimental studies that we are performing now.

So, for the fuel degradation, I am sorry we have not made the introduction from our Phebus but if you have questions about that, you can ask during the presentation.

Our first thing on fuel degradation we have looked at in the Phebus experiments are small fuel benders, one meter long, is the cladding oxidation. Well, when we performed the first experiment, FPT-0, without surprises, we have made pretest calculations and we observed a much more violent than expected cladding oxidation runaway, as can be seen here on this kind of graph, cladding oxidation runaway. And in fact, in all of our correlations for use for calculating that were validated on the different experiments. And we went out of the validation range over what was expected.

So, we have revised these correlations and now we are able to have correct predictions of cladding oxidation for different kinds of transients you can see under here, three different slopes or three different Phebus-FP tests. And this is important not only for hydrogen production totally alone but for hydrogen production rate. Because depending on the hydrogen production rate, if it is true important, you may have difficulties with recombiners of fuel vapors. That was the first point.

For the fuel degradation, we have had some surprises again at the beginning of the program. We have observed that fuel liquefaction and more precisely transition from rod-like geometry to molten pool at temperatures that we are far below their true melting point of pure uranium. So, something like 500 Kelvin or Celsius or below.

Well, in fact, the calculation codes are able to take this into account, adjust say calculate the fuel when you reach such a level of temperature, you have to relocate downwards from a melting places of first. While this works well to reproduce what is done.

MEMBER ABDEL-KHALIK: I'm sorry, in the previous slide, what is the difference between these three, FPT-0, FPT-1, FPT-2?

MR. CLEMENT: The differences are mainly the steam fluid in the bender. In the first experiment, FPT-0, the steam fluid coming into the bender is large. Okay? And not all the steam is consumed. You are always in excess of steam in this experiment. So that, the excess of steam is not the limitation for the cladding oxidation. Why FPT-1 there is less steam than in FPT-2, there is even less steam and there is a steam starvation. You consume all of the steam and the steam amount is the limiting factor.

And in fact, I would say that experiments, the correlations have been validated first on the experiment site in conditions more like these ones such as, for instance, a PBF experiment in the past. And when we have applied that to experiment where there was no limitation on steam, it didn't work.

MEMBER ABDEL-KHALIK: Can you scale these results then, based on steam flow? In other words, if I give you the results of FPT-2, can you predict the results of FPT-1 --

MR. CLEMENT: Yes. All these three are -- yes, you can predict all these three with the new correlations, no problem.

MEMBER ABDEL-KHALIK: Okay. Thank you.

MR. CLEMENT: Predict all this range.

Okay. As I said, it is possible to reproduce the fuel degradation at low temperature. But it is also important to understand why. While there were quite recent measurements of a fuel temperature, high isometric fuel temperature. And from this point and from oxidation measurements and thermodynamic calculations, in this Phebus experiment, you probably have this high burst stoichiometry. And then from this one, we arrive at recent measurements that have been performed on high burst stoichiometric hues. So we can explain from that and different kind of interactions this temperature level.

Just to give you an example of what codes have been calculated, we have adjusted the fuel relocation temperature. On this graph, this is the elevation of the bundle. So initially, the mass distribution in the bundle was a straight line like that but is measured here, distribution is a black solid line. This is a measurement. So you have here fuel that has disappeared and fuel that has been relocated here in the molten pool. And you can see the curves here, calculations. So the gray line is the total mass. But you can just reproduce it without any trouble.

FPT-1, this is also the case with MELCOR with the same kind of assumptions.

Coming to Source Term, we come to the fission product releases. While in Phebus experiments, as we go up to very large degradation with the molten pool, where the volatile fission products are nearly totally released. How volatile our fission gases are iridium, caesium, and so on.

While in general, the total amount of volatile is well calculated by the codes, with some differences, there are some codes that do not take into account the fuel oxygen potential on fission product release. Sorry to be so technical.

But the case, for instance, with the CORSOR approach. In that case even CORSOR approach is using MELCOR, for instance, even if the total amount of volatile is well calculated, the kinetics are all resonated at the beginning of the transient. That is because they don't take into account the progress of oxidation of the fuel that increases at issue in coefficients.

Okay, and this is what I called semi-empirical models. You can eyeball to do well for that.

For less volatile for which chemistry plays an important role, the situation is I will say more contrasted. I don't have, I think -- no, I don't have them. For instance, there were some difficulties at the beginning through calculate molybdenum that was generally underestimated by the calculation codes. Now, it is better. But this is not the case for all of the models, I would say. Because for to calculate that well, you have to take into account chemistry within the fuel and also outside of the fuel. And this, to do that, we use in fact mechanistic codes, without describing the reparation of fission products in different phases of the fuel and their changes with temperature and stoichiometry.

There is also a coupling between fission product release and fuel degradation. A good example is the barium. The barium release is much smaller in Phebus than in separate-effect experiment. Separate-effect experiments are experiments performed on the irradiated fuel. And in these experiments, there is a large release of barium. You can see there is a low release of barium. We have looked at that, made the thermodynamic calculations and so on, looked at the interactions between fuel and cladding material and this is reduced in barium volatility because of interaction between true and oxidized zirconium. So, that is important to take into account.

And also we observed this was not a surprise for us that there was a low release from the molten pool.

What is important and you will see that afterwards, it is for iodine chemistry are the release of silver-indium-cadmium control rod. Why? Because all of these elements may react with iodine to form metal iodides. And especially in the containment, silver reacts with iodine, it can trap iodine in the sump water and it is no more available.

So, it is important to know how much the silver, indium and cadmium will come out from your core. While it is quite easy while governing phenomena, you just need to calculate the vapor pressure of these elements above a complicated mixture, but this is physical. So governing phenomena are well understood, but there are some coupling between the degradation processes of the control rod and vaporization of the material. And here some modeling effort is still needed.

Okay, coming up to the transport in the reactor cooling system, in contrary to what was generally assumed by everybody for the first experiments, in the hot leg of the Phebus to start with that is at about 700 Celsius, iodine and cadmium were the only non-condensed elements of the first true test of the program. That simply means that the caesium hydroxides, caesium, CsOH, was not the dominant species for caesium transport, as assumed by everybody before.

While this made some code calculations, in fact these are thermodynamic calculations taking into account the ground release of the elements and for that we calculate caesium molybdate. And if you look at volatility of caesium molybdate, it is consistent with caesium being condensed in the hot leg of Phebus, FPT-1. So, this shows that the volatility of caesium previously assumed was not correct.

Iodine was observed to be transported partly as a gas and partly as metal-iodides. While we did not measure in Phebus direct association of metal iodides, while metal iodides can be caesium iodides, silver iodide, cadmium iodide and others, what we can say is that caesium iodide is not the only species for iodine transport as a vapor and as an aerosol.

This is quite a complicated point. In here you have got a line with a high temperature here, a low temperature here. And what is released from the core enters this line and there is a thermal gradient. What is in pink is the deposition of caesium. What is in red is the deposition of iodine. This part here, you can see only iodine, without any caesium. This means that this species was not caesium iodide.

In this part, it was at another time of

the experiments, you have got a pink peak and a green peak in the same place. So this is likely to have been caesium iodide. And the other peaks was not caesium iodide. And as I say, caesium iodide is not the only species.

But for other points about depositions, I am just speaking here about some differences between what have been seen and what is calculated by codes, we over just above the core, the bundle, vertical section where the temperature drops down to 700 Celsius, so from say 1500 to 700 Celsius. And the high deposition in this part was underestimated by all calculation codes.

We have looked at that and in fact, it is simply because we are not in a developed flow nor hydrogen and this can be explained by that. So that is the first one.

Then we in the circuit, we have a high temperature gradient in a portion of the circuit simulating a steam generator in here. The main deposition mechanism is due to the gradient between the flow and the walls. And this is thermophoresis of aerosols and this is overestimated by codes. Here, the question is not sold. Some partners of the Phebus FP program, mainly Swiss, are looking at things about interaction between turbulence and aerosol particles. So, they needed promising wait.

But overall, in the Phebus-FP experiments, you have overall 50 percent of retention of what is emitted from the core in the circuit, or something like that.

CHAIR SHACK: Fifty percent?

MR. CLEMENT: Fifty. Fifty percent. Not for all the species. But okay, if you take an example for the steam generator, and you have 20, 25 percent.

MEMBER POWERS: I just need to interject, it is no more wildly different than what is assumed or what comes out of an accident calculation. And the details of where it is accruing are different.

I will also comment that the speciation has profound ramifications on how you treat iodine in the reactor containment and we will talk more about that as the day goes on. But the fact, for instance, the caesium hydroxide is not coming out, then you get to count on caesium hydroxide keeping some basics so you don't get iodine partitioning in a pool. Well, that is just not happening.

MR. CLEMENT: I have tried to be rather brief here. We also simulate what are the thermal hydraulics in the containment. Well, it is quite simple. It simply a closed volume without compartments and so on. And the thermal hydraulics are governed by the balance between the incoming steam in the containment and the condensation indoors. And of course, we have no problem to calculate that. The models are okay.

The aerosol depletion inside this containment are mainly due to gravitational settling and diffusiophoresis. That means entrainment by the condensing steam onto the condensing surfaces. This is also generally well calculated by models implemented in the calculation codes with some smaller detail here. But this is probably not fully typical of the reactor so skip it to save some time.

This is to show you the general evolution of the gaseous iodine in the containment wall. We have all of these points, I would say, measurements, of gaseous iodine. This is for the second experiment, FPT-1. This is for the first experiment, FPT-0.

While you have got here different phases of the experiment, the fuel degradation phases here, so they are short few hours, and they were short, this is a few days. Okay? And then after the fuel degradation is stopped for this period, we have aerosol settling and so on. And then we let the thing evolve during different periods here.

So what is interesting to notice, first of all, that is that this is a very strict schedule and over two days from here to the top. The first point is that you observe an early presence of gaseous iodine in the containment. And this, if we look at our models of gaseous iodine of iodine chemistry in the containment, we cannot explain this early presence of gaseous iodine in the containment by, for instance, prediction of volatility in the containment coming from the same portal or whether it is this or other things. So, this cannot be explained by that.

Then we have a decrease. This decrease is quite important, very important for the first experiment, less for the second one but still important, as it is a logarithmic scale. And then what is interesting is that we arrive in a sustained level here.

A little comment about this second experiment, FPT-1, well, first a sustained level here and then a second sustained level here. What happens at that time is that there were some aerosol particles deposited on the bottom of our vessel, invading the containment. At that time, you have washed this aerosol in order to put everything in the sump water.

That means that we have iodides, aerosols containing iodine deposited here that we have washed down and left them in the sump water. Then we have a small jump. And then we get a sustained level.

So, that is important to remember we don't always obtain this different sustained levels.

CHAIR SHACK: Now, I can't recall. Do the tests have the same sump pH?

MR. CLEMENT: Yes.

CHAIR SHACK: These are both high pH?

MR. CLEMENT: No, it's pH-5.

CHAIR SHACK: Oh, it's pH-5, acid.

MR. CLEMENT: Acidic. But it is pH-5 but you will see afterwards there is some silver in the sump. I think it is after a while.

Yes, some comments first on this first part, this early presence. Okay? Well, as I said before, it is likely to have been formed in the primary circuit of gaseous iodine. And when we make thermodynamic calculations in the reactor cooling system at equilibrium, we don't find this gaseous iodine. So, we think this is linked with non-equilibrium chemical effects. That means we are not a thermodynamic equilibrium.

This assumption is supported by the

existence of a sharp and large temperature gradient. And you are at high temperature. At high temperature, you are very likely to be a thermodynamic equilibrium. Then you make some quenching of your system. So you may keep one part of the species that are stable high temperature. You may keep part of them at low temperature and then release free iodine. This is the affect of this gradient such as chemical quenching. And this is also fully compatible with the difference between the two first tests.

MEMBER ABDEL-KHALIK: Now, the initial iodine inventory in both of these cases is the same.

MR. CLEMENT: No.

MEMBER ABDEL-KHALIK: It is not?

MR. CLEMENT: Yes.

MEMBER ABDEL-KHALIK: The initial in the bundle.

MR. CLEMENT: Yes, it is not the same.

MEMBER ABDEL-KHALIK: It is not the same.

MR. CLEMENT: Yes. And just because of this is a trace irradiated fuel.

MEMBER ABDEL-KHALIK: This is what?

MR. CLEMENT: This is trace irradiated fuel with a very low burner. Okay? So, here, reactivity is the same but the number of modes in this experiment is much lower than in this one. This is expressed in a fraction of the initial inventory. This is a fraction of the initial inventory. But I would like to point out is that it is much higher for this one with a low number of iodine modes. And this is consistent with the assumption of non-equilibrium chemical effects. Because if you have a small amount of modes, it is much more difficult to, I mean the kinetics of the chemical reactions are slower.

Okay, there was in the last experiment performed a fraction. We will come to this probably later on.

Okay, this comes to your question what was the sump. So, the sump pH in these two experiments was acidic but in fact, we have released the silver from the full bundle. And in these two experiments, iodine has reached with silver in the sump water to form non-soluble species of silver iodide. And then once it is non-soluble, this is inhibiting gaseous iodine production by radiolytic processes, despite the acidic pH. So, most of the iodine will instruct into the sump water.

For the third experiment, we have used an alkaline pH. So, it is well known with pH and it is high for iodine remain trapped.

MEMBER ABDEL-KHALIK: In a hypothetical accident, what would be the timeline for failure or melting of the fuel versus melting of the control rods?

MR. CLEMENT: Oh well you have got the starting of I would say release from the control rod and start of iodine release all the same. So, I mean, the control rod in the silver-indium-cadmium will fail, anyway it will fail at stainless steel melting temperature because the cladding is stainless steel.

So anyway, at 1400 Celsius, you will have some free silver that is molten, at that time, that is in contact with the atmosphere of the reactor cooling system. So you will have some release of the vapors and the iodine release will start later on. And then you have gotten the core and it is quite high, a progression of this degradation of the control rod later. So you would have a quite continual release of this silver.

But this kind of reaction here, in fact, iodides in the containment reaction, reaction of silver with iodine. And in fact, well, it is a little bit complicated but silver metal -- metal silver is reacting with iodine. But if silver is oxidized, reactions are faster.

MEMBER ABDEL-KHALIK: Right.

MR. CLEMENT: And in a containment, silver aerosols are partly oxidized due to radiolysis products and things like unless the reaction is faster and is more efficient.

Now, the other experiment, FPT-2, we used an alkaline pH. So, in that case it is normal. And FPT-3, we will come back later on.

Here I want to note that for these two tests, an efficient trapping of iodine by silver requires an excess of silver as compared with iodine. Just because silver iodides is decomposed under radiation, even if there is an excess of the composition and then you have that reaction of silver with iodine. But if you have no excess, it will not be 100 percent efficient.

CHAIR SHACK: Now, is Phebus prototypical on the relative amounts of iodine and silver?

MR. CLEMENT: Well, it is not fully prototypical but fairly well. I would say, in fact, we have one bundle of 20 rods and we add one control rod. Okay? We cannot add one get one and two. In fact, we are in between the amount that is corresponding through an assembly with control rods and an assembly without control rods. We are in between them. We are already in excess of silver as compared with iodine, as compared to reactor situation and it is not so bad.

Okay, what we have observed also in the Phebus is that the volatile iodine concentration is mostly determined by gas phase chemistry. This is just because in the sump water we have different mechanism that traps the iodine.

I have already spoken about the importance of gaseous iodine injection from the RCS, that is the early presence of gaseous iodine. What is important now is to look back at these sustained levels here. Here are some trapping mechanisms just because the concentration of gaseous iodine phases and then we have a sustained level.

So, why do we have a sustained level? Well, this is just because we have got an equilibrium between iodine formation and destruction processes. And also we know that iodine can be absorbed on the surfaces and can be also desorbed from the surfaces. So, there is an equilibrium between all of these processes that are sources and sinks in this yield a steady-state concentration in the long-term. That is quite --

MEMBER ABDEL-KHALIK: Wouldn't that lead

to a steady-state concentration that is constant on an absolute level rather than a relative level, based on the initial concentration?

MR. CLEMENT: My feeling is that while the initial concentration, if you look at the number of modes, there was a large difference. And there is not a lot of difference here. But we can show you this --

MEMBER POWERS: To answer your question, it is in principle yes, but what you will see is one of the things that controls is with the magnitude of your sink. And the sink depends on how much steam you have. At FPT-0 they had a lot more steam. And so it is almost coincidental to end up about the same in relative amounts. But if you were to put up FPT-2, you would see it stabilized at a different level.

MR. CLEMENT: At a different level, yes.

MEMBER POWERS: But it is over -- the really interesting thing is it always stabilizes a level. And if you look at the timeline, that is days. That is just a little period of time. That is days. So, it is a very robust stabilization. It looks a lot like a steady-state. Now, there is a difference in opinion whether it is actually a steady-state or not but it is a, whatever it is, it is very stable. And we will show you some data eventually that says you can manipulate it by manipulating the sink drum.

MEMBER ABDEL-KHALIK: So this five percent is fortuitous that the two of them ended up at the same --

MEMBER POWERS: Well, if you look in detail, they are not quite the same but they -- the problem with any kind of stability there depends on the source and depends on the sink.

MEMBER ABDEL-KHALIK: Yes.

MEMBER POWERS: And I can manipulate the stability by manipulating either one of them. It turns out in the operational claim is that what they tended to manipulate was the sink.

MR. CLEMENT: At a sustained level, probably it will reach a steady-state but just factually --

MEMBER POWERS: Well, I mean, it could be a steady-state or it could just be a continuous --

MR. CLEMENT: Yes.

MEMBER POWERS: -- release that depends more on your inventory and whatnot. And there are different views on that and you can never sort it out based on the steady-state level, until you deplete how much you have as your source.

MR. CLEMENT: Another fact is that the

first two experiments, most of the gaseous iodine was organic in the long-term. This, we have not observed in the last two experiments. I don't understand why. We have to reconcile things. I don't understand why right now.

Also, another point is that we are absolutely to take into account interaction between gaseous iodine and aero-radiolysis products because this is one of the possibility to destruct the gaseous iodine to form in fact iodine oxides that will become particulate. Okay? And we need absolutely to take this into account.

Well, this is just some words about that. These are conclusions mostly from IRSN, I would say. We think that we have seen a number of things that were either unexpected or that are badly quantified phenomena. These of course, have been identified in Phebus-FP but also in other experimental programs.

First point, these are just examples, okay? Fraction of iodine entering the containment as a gas and not as an aerosol. While we know our safety studies we take five percent in our safety studies, that is exactly the same values that the NUREG-1460 shows them, not for the same reasons but it is the same value. But you will see now the experiments we are far more. That is okay. This was just an example.

And with all of these uncertainties, we have looked at the impact on the iodine Source Term assessment studies. You have two kinds of Source Term assessment studies where the classical probabilistic safety assessment level two studies. And also you would say what is I would say not an envelope but a pessimistic Source Term that are studied. And this Source Term is used for checking the adequacy of emergency planning measures. And from that, we have seen that these uncertainties are important.

Again, just to try to reduce these uncertainties, we have set up a Phebus follow-up program to provide a set of experimental data to allow to improve the models.

Okay, maybe a little bit faster right now. Well, what we have seen from our safety studies -- okay, that is obviously risk dominant in the short term. And also what is important is a partition of airborne iodide in the containment between particulate, organic and inorganic gases.

When I said that part of the iodine at the break in the gaseous is a fraction of a gaseous, it is badly quantified. This is Phebus use for FPT-1, four percent, FPT-2, 0.6 percent, and the last test, FPT-3, 85 percent. Okay? The difference between these tests and this test is that in these two tests, control rod was in silver, indium and cadmium, whereas in this one, it was it was in boron carbide. Well, is there a reason? Maybe.

Then in our safety studies, we are looking, when you have got I_2 inorganic iodine in the containment, we have looked at the possibility of this I_2 inorganic iodine to be converted into organic iodine, probably methyl iodide. So this is what this graph is what has been done by our people in charge of safety studies. They have looked at all of the experiments that were available at that time worldwide and they have translated the experimental results in a very simple number that is a conversion fraction of I_2 into organic iodide by interactions with paint. And then they have plotted a distribution function like that.

Okay, of all the experiments, this is the number of cases. You see the scale here is 90. So they are part of that. What is important is to see the scattering. So, there is a factor of two between the median values -- factor of ten between the median values of this distribution and the 90 percentile -- I don't know how to say that in English. Factor of ten between this and this. Okay?

So, that is a large uncertainty in interactions between iodine and paints. Just to give you an idea of the kind of uncertainties that we are looking at, if we had found a factor of two or three, well, not so important.

Okay, other point, I was already saying that gaseous iodine reacts with air radiolysis products that are mainly ozone an nitrogen oxides to form less volatile species. So that is good news for safety that the species are less volatile. While the fate of the species, what happened to them once formed from small particulate, is badly known. We have to look at that.

Okay. Maybe it would actually be faster -- I don't know.

MR. LEE: I think you wanted to talk about, briefly, on each of these programs.

MR. CLEMENT: Yes, briefly. Briefly.

MR. LEE: Because Dr. Shack asked about it, what is the follow-on program.

MR. CLEMENT: Okay. So for the follow-on program, we studied in the iodine chemistry in the reactor cooling system with two objectives. To confirm and quantify the amount of gaseous iodine in the reactor cooling system. We have seen that in Phebus. We have just seen that and better measure it in small scale experiments. That is the sketch of the experiment. And also, as we suspect that there are chemical kinetic effects, we need to measure kinetic data for modeling. And this is how it is done with a high temperature mass spectrometer.

This is just a sketch of the first part. It is just a simple tube in which to inject a number of species of conditions which represent hot leg, and cold leg, and you measure what is happening with gaseous iodine.

This is more complicated. This is a system to measure kinetic data. So it is really a chemical reactor, where you introduce different species. There is cracking of the species, then recombination and then you go to a mass spectrometer.

And this operates, it is quite complicated. And this is what we will do. These are pre-test calculations of these experiments, with a simple system here with few reactions between hydrogen, oxygen, iodine. These are a number of reactions. We calculate what happens with equilibrium. These are the plain lines, solid lines, and what happens with a chemical kinetics, kinetic limitations. Those are the dots. These are pre-test calculations of those experiments that are not yet performed.

MEMBER ABDEL-KHALIK: So the data shows considerably less sensitivity to changes in temperature.

MR. CLEMENT: Yes. That is just because here is a logarithmic scale again. Here is one indicator. It is in two lines. See it? So it is largest in deviations. Okay?

MEMBER ABDEL-KHALIK: Right but looking at a specific set of data, variation with temperature.

MR. CLEMENT: Yes.

MEMBER ABDEL-KHALIK: Vis-a-vis the model predictions.

MR. CLEMENT: Yes?

MEMBER ABDEL-KHALIK: We are talking about in some cases differences as much as three orders of magnitude.

MR. CLEMENT: Yes, that is true. But in fact, not all of the -- well, here is our pre-test calculations. We have modeled a system with a simplified set of reactions. This is a simplified set of reactions with the three elements. Okay? This is a simplified set. Then we calculate all of them.

But you see here, what happens for these reactions is probably not so important to the amount that are formed are much less than from these ones, for instance. But we need to look at all of that because all of the kinetic constants for all of these reactions have potentially an impact on the results. So we need to model all of that.

So, it is just to give you -- the intention in showing you that was just to give you a flavor on the complication of the system for the treatment of such data.

Okay. Then for the containment, we have the EPICUR facility, where we look at the kinetics of organic iodides formation through reaction with paints. We have already discussed about the importance. More generally, the kinetics of reactions in gas phase and also some compliments about the kinetics of formation of volatile iodine in liquid phase. So what is EPICUR? Just a simple here vessel, a few liters, in which we can put whatever we want. Not exactly whatever but we can put water, paint of coupons, things like that and then we irradiate it. We irradiate it and then on-line we measure by here specific apparatus, selective filter for gaseous iodine, we measure what is produced as gaseous iodine. And here we have a separation between particulate, inorganic, and organic iodine. This is measured on-line. And this is an example of the kind of measurement you get. This was an example of an on-line measurement for liquid phase chemistry. So this is what is on this date for inorganic -- for gaseous inorganic iodine here. Here the measurements are made by gamma spectrometry in here. You can see just a statistical of the measurement.

And this is just an example. Some small experiments have been performed already about interactions between iodine, surfaces, and air radiolysis products. This was realized in Germany upon funding by us. And this was realized in a sealed flask where you have put different atmosphere representative of different conditions, different surfaces, and so on, iodine and irradiated that and measured what you got after that.

Here maybe it is more interesting for you. I said previously this is the difference between the last two experiments, FPT-2 and FPT-3 that were performed exactly in the same conditions, except the nature of the control rod. In this one it was silver-indium-cadmium. In this one, this was boron carbide.

So, I showed on a previous graph the difference in gaseous iodine coming from the reactor cooling system between these two experiments. And this is reflected in these two peaks here, where measurements of points and calculations, positive calculations are solid lines. You can see this large difference, again, between these two experiments. Okay?

What is interesting is that despite this large difference early in the transient, while you have got similar on some threshold level at the end of the transient, I said similar because it is logarithmic scale. And if you look into more detail and the scattering within the points, the calculations will, I said, similar. Okay?

And then we have performed these calculations with our code that is model of the access system level code. And we have introduced in this code the interaction of iodine with paint. This was already introduced and we have also introduced what is due to aero-radiolysis products interaction with iodine. And we have introduced, we have already the models. We have introduced what was gained from the various other experiments. And from our calculations, we are not so bad as compared from the general tendencies. What are used are not exactly the same but okay, we obtained also this very large decrease due to mainly interaction with paints and to destructions by aero-radiolysis products. For this, maybe it was an answer to a previous question showing that even with a very higher iodine from the reactor cooling system, maybe we are lucky and it is, in the long-term, decreased. But this needs to be confirmed and further analyzed.

Okay, maybe we will go further quickly now. As for boron carbide, while there could be a possible impact of boron carbide degradation product here on fuel degradation. While indeed the all of the codes are not able to reproduce the fuel degradation in the FPT-3 experiment, we have some early fuel degradation that is not reproduced by that. Maybe it is due to these products.

And also, there could be a possible impact on fission product chemistry. Could this be an explanation for this very high fraction of gaseous iodine at the break in FPT-3? I don't know. Maybe it is because there was no silver-indium-cadmium. Maybe it is because there was some carbonated fissions in this boric acid. This will be tested in one of our problems.

What will be also tested are, I would say what has been tested is the oxidation of boron carbide in liquid and also will have degradation in oxidation of 30 centimeter boron carbide was.

This is just an example of oxidation of liquid boron carbide stainless steel mixture. This is the hydrogen production. And this is done. But this was an horizontal furnace. Here was the mixture here, at an angle. What you see here are projections of bubbles. So it is not a gentle oxidation. You produce gases or vapors within this liquid that produces these bubbles. And you can see this is not a gentle oxidation and so on.

And what we suspect is that these projections can go to the surrounding walls of the control rod. And these mixtures containing boron, carbons, iron and so on would probably have interaction with the zircaloy cladding and all this mixture would probably have interaction and distribution over proportion of the fuel, of the O2, of the neighboring rods. So that is probably this come from would be a local effect, probably, not generalized but a local effect. And this will probably will made rather low melting point temperature always going down. And this would contain uranium. This is what we have observed in this PT-3.

For last point, this is something that we have not tested in Phebus. We had intention to make an air ingress experiment in Phebus. This was not the case. Experiment was cancelled.

What air ingress, you probably know that air may be in contact with degrading fuel for different several accidents, reactor accident scenarios, and also of coolant in shutdown situation, after melt-through of reactor pressure vessel and so on.

What is known is that under very oxidizing conditions with air ingress, ruthenium is largely released. This has been studied in detail by our Canadian colleagues. Just because air ingress was a designed by this accident for Canadian reactors, so they looked at that in detail. And ruthenium acts as a volatile fission product and is largely released from fuel.

What happens after that? What has been seen is for ruthenium transport in the reactor cooling system. There have been two sets of experiments performed in Hungary and in Finland. And they both show that part of this ruthenium is transported as a gaseous ruthenium tetroxide in the reactor cooling system and not only as aerosol particles. That means that what will arise in the containment will be in majority are also particles but also gaseous ruthenium, ruthenium tetroxide. And radio-toxicity of ruthenium is very high, comparable to that of iodine in short-term and in caesium in mid-term. So, we have to look at that in detail.

Another point is not for reactor accidents but for spent fuel storage pool accidents. So, if we have a fast cladding oxidation in spent fuel storage pool accident, temperature may increase to a level sufficiently high to have a fuel degradation and fission predictability. So that is another topic.

Well, once we have looked at IRSN, is ruthenium behavior in the containment. As we know, the way to get ruthenium tetroxide inside the containment, we say well, what will happen?

First thing, we have to do tests on the ruthenium tetroxide absorption and desorption on surfaces in the containment better than with painted and steel surfaces. We made test without radiation with a cements with ozone that also with an irradiator. And we have tested deposition, destruction of ruthenium tetroxide and also oxidation of deposits of ruthenium dioxide that also deposit from surfaces.

From all of that, again, it is question of destruction and production processes. Destruction processes, destruction of ruthenium tetroxide. And creation is oxidation of deposits of ruthenium dioxide.

For the first series of tests we show that a significant fraction of ruthenium remains gaseous. Part of the ruthenium gaseous items is destroyed. A significant fraction remain gaseous. We are not looking at what is an implication for a real containment. Because when I say fraction, it depends whether it is important or not, of course.

Okay. There are some tests from liquid phase from re-vaporization from liquid phase that are still under way.

This is an example of the experiments. This is an experiment. This is a small coupons. The black deposit is deposit of ruthenium dioxide. This is before it being in contact with ozone during one day and here is after changed color because that ruthenium dioxide had been oxidized. And this is what is done in the flask used for irradiation. Same kind of experiment but with irradiation.

Last point is cladding oxidation by air. So that is for different kinds of accidents. We have looked at different alloys, zircaloy, M5, zirlo cladding for different regimes and conditions. What we wanted to look at not only that but looking at the determination of the kinetic transition between the different regimes and also look at the role of nitrogen.

Just here to give you an example of what kind of thing we obtained. At the beginning, we have a protective dense oxide layer. That is here. In that case, what controls the rate of oxidation if the diffusion of oxygen within that layer and you have got a parabolic low for the oxidation. Here you have got cracks. And below the cracks here is development of a porous layer. And then diffusion here of course, is no more controlling the process.

What is also interesting is to see these yellow nodules here. Those are zirconium nitrites that form at interface between the oxide layer and the nickel. And that probably have an effect on the nickel stresses in this layer and so on.

This image here, what is brilliant is a zone that is very porous. So here, nothing in here. And much more within the nitrite. In fact, when we are in this regime here, the kinetics is no more parabolic. It is more linear and even sometimes accelerated. So, faster oxidation. And we want to know that in order to know if I would say how much time we have in case of an accident, for instance for management measures.

Okay. The last point is some additional experiments on fission product release studies. There are existing data from small scale and integral experiments, while the measured release is strongly dependent upon temperature and oxygen potential and not only on temperature. That is an important lesson.

We saw that there was a need to extend the data to high burn-up and MOX fuels. And also what we are doing is we tried to create and to elaborate predictive models, not only correlations, in order to be able to make predictions even for small fuel evolutions. Not for revolutions but for small fuel evolutions which predictive models we can probably tell what will happen.

Okay. And also what we will do is we will look at what was happening in past experiments. These are two views of two pellets having experienced true unyielding release experiments in the reactor program a few years ago.

We will now try to look at where are the fission products inside these pellets, in which phases they are. These are the waters remaining in the metallic phases and things like that because we have models to that but they are not validated. So we tried to validate our model.

Okay. And also --

CHAIR SHACK: These VERCORS tests are very old, aren't they?

MR. CLEMENT: Yes, sure.

CHAIR SHACK: Okay, you are just getting round to --

MR. CLEMENT: Sure, sure. Okay, just from conclusions. We have seen that there were some unexpected phenomena for severe core meltdown accidents. They are coming from Phebus but also observations from other programs. Some of the phenomena are still misunderstood or badly quantified. Badly quantified is just because Phebus has been an integral experiment. So you observed the things, you measure the things. But if you want to validate or build models, you need more precise measurements. Okay?

So with that, we have already a large level of uncertainties and what we have observed in our institute is that at least is that this has an impact on the results of Source Terms assessment studies.

So that is why this Phebus program has been built, in order to reduce this level of uncertainties.

So maybe I was too long.

CHAIR SHACK: Are there any questions?

MEMBER ABDEL-KHALIK: If there are 85 percent fraction of gaseous iodine that you have with boron carbide control rods is real, --

MR. CLEMENT: Likely not.

MEMBER ABDEL-KHALIK: -- what would be the implication?

MR. CLEMENT: Well likely not.

MEMBER ABDEL-KHALIK: Likely not?

MR. CLEMENT: Gaseous iodine interaction. You mean gaseous iodine interaction with boron carbide?

MEMBER ABDEL-KHALIK: No. If the difference that you got, the 85 percent versus a few percent in the two cases, is real, --

MR. CLEMENT: Yes.

MEMBER ABDEL-KHALIK: -- what would be the implication of that?

MR. CLEMENT: Well, first of all, we have to understand why. Okay? It may be that iodine released from the fuel in the FPT-3 experiment did not have silver to react with, did not have cadmium to react with, did not have indium to react with.

It may well be also that all caesium had reacted with boric acid to form caesium boride. So it may well be that it did not have caesium to react with. But this, we are not sure. In that case, it would be simply because iodine was left alone.

But this is just a speculation. This is just speculation. We have to look at it in more detail.

MR. LEE: I think the NRC view is that the boron react with water create boric acid. The acid capture all the cations. So, to rephrase what he said is that here are no sites for the iodine to combine with. So you will have -- that is why in FPT-3, you see an 85 percent gaseous iodine release for that test.

But remember that this is a very small bundle, representing a very small part of the core experience and conditions where you have a before C-rod that is controlling the chemistry. But in the big core, you will have different conditions in the core. So, the iodine gaseous fraction going up into a containment should be the mixtures that you encounter in the upper plenum and mix and then go out. So this so called 85 percent, you cannot take it directly and translate it to the reactor case.

That is why we need to have models to understand what Phebus is doing. And then you can extrapolate it to the containment. So we cannot say that his 85 percent has any occasion to our 1465 where we said you should assume five percent. In our new Source Term we said five percent gaseous iodine. That 85 percent, we are not concerned with that, at this time.

MEMBER BROWN: But can you ignore it, once you have it? I mean, it's a big number.

MR. LEE: The thing is that it happens. We understand why it happens. So, in order for the reactor condition, you have to calculate the entire core behavior and determine what is the gaseous fraction in the containment. So it is not the 85 percent. It has to be less than that.

CHAIR SHACK: Yes, but if you have done that calculation, I guess, is the question.

MEMBER POWERS: I mean, there is more, the scale was fairly abbreviated on that.

Two things to recognize is the boron carbide used in the experiment was not representative in configuration with like the a rod blade in a BWR. We have done that experiment with a control rod blade and we know that because of the high steel fraction, you don't expose boron carbide to steam the way they did in the experiment here, which resulted in a lot of boric oxide being released.

That leads to the argument that the boric oxide really likes to combine with metals, so it sucked up all of the counter ions that would ordinarily react with iodine. They are just gone. They are tied up as borates and allow gaseous iodine to come into containment.

This particular is still undergoing analysis but was also observed, and I think this was explained in the previous visit, is that the gaseous iodide decayed in the containment at a rate that was actually faster than the aerosol decay rate. So it is going into the solution. It is doing stuff. Once it gets into the solution, then we start to handle it.

And it came down and it established this nice steady state again we observed in all of the other tests, which is really remarkable. You put in 85 percent, you still end up with the same steady state. And that is why the steady state has to be understood.

What the ramification is that gee, assumed in the past we could get by with kind of a crude analysis of how boron carbide control blades behave in reactor accidents and now we say, well, we can't be crude. You have to got be fairly sophisticated. Just like we always silver-indium-cadmium control rods could be treated with a simple failure temperature. Now, we can do that. So we have to be a little more sophisticated in our treatment of control rods across the board here.

CHAIR SHACK: Richard, do you want to proceed?

MR. LEE: Okay. Our presentation is going to focus mostly on the iodine behavior itself. Because last time I talk about the RCS behavior that Clement mentioned here. But the start thing to talk about is the expectation of what the iodine behavior in the containment and what are the Phebus findings with respect to our expectations. And what are we going to do about it in the near terms in one or two years to address the difference in expectation versus findings and how we are going to scale it to the reactor conditions.

And I also want to say that we have been working NRI for the past decades now since the inception of participation with Phebus. So, our user office are well aware of the findings from Phebus and especially what it meant for the designed based accidents analysis.

Just to remind you there is about 750 million Curies of iodine in the typical core. Mostly all of our, nearly all of our reactors are licensed under the old Source Term, the TID-14844, which is promulgated in 1962. And you can see that very large gaseous releases versus the particulate which is only like one percent. Okay?

Following the TMI, if you used that TID-1484, we know that we didn't see those gaseous iodine from the TMI accident. There was a lot of experiments, separate effects experiment conducted over the world. And from the experiment, we will still see that there are gaseous iodine still appearing.

So, in the subsequent, in 1995, when we go to the alternative Source Term, NUREG-1465, we set you will assume for you analysis, on the alternative Source Term, you should assume at least five percent in gaseous iodine. And the iodine, five percent gaseous iodine you, you see the molecular iodine or organic iodine. Basically, they combine to add up to be five percent. And the other 95 percent is aerosol.

And I only -- there are actually four periods of releases. It is no longer a constant like the TID-1465 is a constant source, you have a different type of releases. You have a gap releases, you have an in-vessel releases. And then when the lower have failed, you have an ex-vessel releases and then, subsequently, there is a late in-vessel releases. But for the DBA analysis, we only use the first two in terms of the percentage of the inventory that come out. We use that fraction to look at the outside dose, the teddy for the boundaries and so forth.

Now, in the iodine, the caesium -- the form for the iodine is assumed to be CSI and I think Bernard has discussed that in Phebus is that we are finding it differently.

Now, what happens to the iodine in the containment? Of course, we know that the aerosol gravitational settling, and these are the phenomena that we postulated for the behavior for how the aerosol can be removed by natural processes and also by safety system like the spray or the suppression pool or the ice bath and so forth.

But during that period for the two

decades, they are persistent, constant research in Europe, especially in UK, France, and other place continue to study the gaseous iodine behavior. That is not so new. Most of our studies were in the early 90s, basically mostly at Oakridge, Tom Crest and Company. Usually, NRC tasks them to look at certain things. And they finish and we start. But there is a lot of studies still going on in Europe. So, we have to be mindful of what the findings are from those. And those are being factored into our understanding as of today.

But we also know that the iodine chemistry is very complicated and especially in the aqueous phase. The iodine has about eight oxidation state, which most elements you don't find those. So, we know that some people spend entire career from the day they get their Ph. to the day they die still working on it and it is never finished. But for our use, we need to find out what are the important things.

And in early 1997, we have commissioned Oakridge to look at the iodide chemical form on the severe accident. The iodine evolution and pH control, we like to know what does the pH control do to the gaseous iodine partitioning between the water pool and the atmosphere. And those have been studied and that actually formulates the basis for the 1995 Source Term that the staff have published.

Now what are expectations? We said all the particulate and gaseous iodine released in the containment usually end up in the sump. And then we said, if the sump remained inclined, that is when the pH is greater than seven or more, these iodine should not be coming up. Okay?

We also know that there are other processes that create acid, cable installation and all of those things. We also studied that one, too, experimentally at Oakridge and we published some reports in that area. But if the sump become acid, then we know that these volatile iodide will come back out. And we also, I think there was a model that we developed to look at what type of materials get released from the reactor under certain accidents and how much quantities you need to change the pH up. We have such models developed. Tom Crest has done that for us, too.

Now, just to remind you long time ago, NRC funded a lot of experiment like the thing Bernard mentioned about these taking different elements and combine them and study them, radiate them at different temperature and find out what the products are. We have done many of those experiments. This is not to put you down but we found out that there are so many infinite combinations that you need to do and the time and the money will be enormous. So we abandon that one.

The second thing we did is said we wanted to develop a vittorial code to look at all the thermodynamics in the program. And once you run the code, you find out that you get infinite amounts of species coming out, which we don't know which one is correct or not. So, Phebus came along and we said, this experiment at least will give us some guidance of what are the things that we really need to model. This is where we are today.

If you look at this here, I think you mentioned that this has a control rod that is different. And this has an alkaline in the sump. And then does the temperature also control that they do here is basically it is condensing but later time, it starts evaporating in the sump. We also know from separate facts that if you evaporate the sump, you know that mass transfer, you should have more iodine coming out from the sump into the atmosphere.

We have done those separate effect tests, though. These are the things that we like to see what is people telling you but not also tell you that these are the period of time that the releases phase. You have the first aerosol phase, second aerosol phase. This tells you the time for these four tests. There is actually another test we have done but has no relevancy. There is no containment involved so we didn't put this on.

And then you have the chemistry phase here. And these are the washing phase that Bernard mentioned about is to wash down the those aerosol that settle down at the bottom part of the containment and wash it into the sump. And then this is the chemist's completion of the chemistry phase that takes actually days. And for these two tests, you see that they changed the condition from condensing to evaporation.

The next view basically summarize what the durations of the release phase and the aerosol phase, and the chemistry phase for all these four tests that we have just shown here.

Now, Phebus containment is quite large, in the sense of fission product experiment. There is about ten meter cube of volume in there. And these are the treatment then so he is talking about. There is one dry part and there is a wet part. They have to have a dry part in order to control the condensation on the condenser. It is a requirement. And the fission products and the gas particulate are released into the containment here and they have a sump, simulating the sump of the containment.

And when we look at the aerosol sedimentations, we found that the time it takes half of the aerosol to disappear for all of these tests is around 1.5 hours.

And then what happened in the gaseous phase is basically these are the reactions you can expect that the I_2, the ozone generated in the air due to irradiation put this I_2 into this IO_x and then it will diffuse into this condenser or into the sump. So the breakdown of these parts are around 15 percent to the condenser and about 85 percent to the sump.

Now of course, the water chemistry is very complicated. This is only a few examples to show you is that it can have three different things happen. You can have molecular iodine come up from the sump. You can have stable AgI. Stable and it was retained and stayed in the sump, it would not come out or you can have organic gaseous iodide coming up.

MEMBER ABDEL-KHALIK: Doesn't the deposition process depend on the length scale of the experiment?

MEMBER POWERS: In a -- it's treated as a well-mixed environment. You can have a length scale in there but it is not very important.

MEMBER ABDEL-KHALIK: He's talking about deposition of condensers by diffusiophoresis. Doesn't that depend on the length scale? I doesn't?

MEMBER POWERS: It depends on the condensation rate.

MEMBER ABDEL-KHALIK: Now, condensation rate can depend on the length scale.

MEMBER POWERS: I depends on the kind -- the aerosol physics is all modeled in terms of the condensation rate.

MEMBER ABDEL-KHALIK: So the fact that this sort of vessel is not really prototypical in terms of length or volume scales, does that --

MR. LEE: With the results. If you say that this is ten meter cube so basically, if you compared it with the last containment, the surface would be distorted. It is much larger than the real one, of course. But they did try to scale it to sort of resemblance to a 900 megawatt electric, you know, the French PWR and leaving it in the containment part of it.

MEMBER POWERS: The only reason that you

would worry about the scaling here is if the phenomena varied with scale. Well, I tell you, they vary. And there is no evidence of that.

I mean, you get condensation on surfaces. If you get gravitational settling. Bigger scale, you get condensation on surfaces, you get gravitational settling.

MEMBER ABDEL-KHALIK: I mean, they are coming up with time scales for these processes.

MEMBER POWERS: Oh, yes, don't count that. The time scale will change.

MEMBER ABDEL-KHALIK: Right. How much do I believe that? I mean, how relevant is that?

MEMBER POWERS: For a reactor accident, it will be different. They are all different. It depends on the sequence and everything.

MR. LEE: Now, we also have experiment to show that the sensitivity to pH, what are the predominant iodine forms in the sump? Okay, and you can see that at high pH, this will be your dominant form so you will have the molecular iodine coming off. But on this side here, it is not. It has become a stable form. So, these are some of the things that we have studied previously.

Now, let's look at the results here.

Okay, here we have already discussed about what the C rod did to the fractional iodide in the containment. We discussed that.

The aerosol sedimentation we said is around 1.5. This one is a little bit large. We are not quite sure. This data is still preliminary.

Now, you look at the silver and iodine concentration in the sump, these are very large number. So is this one. So you expect that this AgI should dominate, so it should be stable. There should be no gaseous iodine coming out into the containment. So, our expectation for this case, this case, this case is that all of these should be no. There should be no gaseous iodine in the atmosphere because you should form a stable silver-indium iodide in the precipitate out. Here, you don't expect it because you have an alkaline sump. You should be controlling because that is what we said. Keep the pH seven or higher, then you should not have any gaseous iodine in the containment.

MEMBER ABDEL-KHALIK: When you say no, that means zero, absolute zero on the first row?

MR. LEE: On those two, yes. It should not be coming up from the sump. This is what the model says.

MEMBER POWERS: At worst, what you should see is as the gaseous iodine comes in and starts at some level, it should come off about the way the aerosol does. And it should just keep going towards zero.

MEMBER ABDEL-KHALIK: Nothing at all?

MEMBER POWERS: No iodine, eventually.

MR. LEE: Look at here. Right? You see the gaseous iodine decay is much much faster than the aerosol. This is a more simplified one that Bernard showed earlier on FPT-1. We show you two. So you see that in the degradation phases came in and it decayed. It decayed very fast.

And look at the aerosol here. This is the AgI. You can see the slope here. This is the washing phase. It went back up, as Bernard mentioned earlier, and then it decayed back again.

Now, let me go back to the conclusion on what we are understanding from Phebus. Related to the aerosol, we think that in Phebus experiments show that our understanding of aerosol is very good, except of course, where the CSI is the only one exists or not is different. The other forms, we recognize that you have these types of other, other than CSI for iodine form. And in other words, these are quite good in our understanding.

Now, as I told you right before, we have done some other tests related to the increasing in the sump. When it is condensing, you are supposed to remove the iodine in the atmosphere into the sump. When you are evaporating, the mass transfer will promote the iodine from in the solution to come out into gas. But this is what you show from Phebus. When the sump is condensing, it is higher and when it is evaporating, it is lower. This is not what we expect from our model.

MEMBER POWERS: That is not what you expect, it is precisely the opposite.

MR. LEE: Now, in the third bullet, we also found that the switching between molecular iodine and the organic iodine is also quite complex as not mentioned in the FTP-1. It started with organic iodine and this gets replaced by molecular iodine. But in other subsequent tests, that was not the case. The molecular iodine was the predominant one. The organic iodine is only like 20 percent of it throughout the entire tests.

So, what we conclude now is that is the sump pH controlled is whether the AgI is there or not is really not the driving force for what we observe in the iodine chemistry, gaseous iodine in the atmosphere of the Phebus containment. We believe that the painted condenser played a key role in the evolution of the iodine in what we observed in Phebus.

So, you can say that the condenser actually really simulates some of the cold surfaces you see in our reactor because they are colder surfaces as the steam comes out it condenses on different surfaces. And those surfaces are not the ones that we can do pH control.

MEMBER SIEBER: What is the products of the paint interaction with iodine?

MR. LEE: That is what we are trying to sort out now.

MEMBER SIEBER: I mean, that is an important consideration, to my mind.

MR. LEE: That is the key things that we are studying in EPICUR and these follow-on programs.

So, what are the hypothesized mechanism? First, is the gaseous iodine and the aerosol gets swept onto the painted condenser by steam condensation or even by other means, diffusions, not just by steam.

Then, it has to be dissolved very rapidly onto the painted surfaces. Because in Phebus, they do drain. They collect the condensate and then they drain it out into the sump. So the question raised is that how does the iodine dissolve into this paint? And there are different views about the paint, the paint surfaces because the paint surfaces are not even. So there are some water pools trapped into it like shown here.

So we can go into here, so the chemistry can develop in those pools or does the chemical or the iodine react with the polymers on the paint? And then also, paints, as you know, is a polymer mixtures with solvents. So basically maybe they didn't finish reacting, then iodine may react with some of those. Because you can see that the organic iodine, the big sources of organic iodine coming from Phebus is really from the paint. That is why, that is where the carbon is.

And then the next thing that you have to find is that after it gets absorbed, we need to find out how does it come out? Gas in what form? Was it molecular or was it organic iodine?

And then also Bernard mentioned in the atmosphere, you have radiation, so these things get reduced and there is a destruction part related to the forming, they may form some very fine particles and these so-called iodine particles could also migrate to the condenser or settle. And these are the two last points that we talked about.

So this is a pictorial of the view of what how iodine interacts with the paint, deposition as well as it coming out. So these are very complicated pictures and models that we can develop. But the problem is that we need to have data to support it one way or the other, even though you can hypothesize many mechanisms, whether you have it or not needs to be supported by data.

Another thing has to do with the Phebus scaling aspect because it is distorted. I cannot take the data from there and say that this has happened in the real reactor. So, we have to have models to do the extrapolation. But in order to validate our model, we need the data.

So that is why, we think there are about six mechanisms that at least we should evaluate. The first one is that Phebus remember, it is not a concrete containment. They use steel as containment because from test to test you have to clean it up and then prepare for the next test. So, one can argue that the source of our iodine is not from the paint, but from the steel. But this can be sorted out very easily. In EPICUR they are going to conduct a test to see which one is right. But we still think the steel does not play a big role in terms of the iodine behavior in the containment.

The second hypothesis is to understand what is happening on the pain film itself. And we also want to know how much is the quantity you can absorb onto the paint? The amount if important. If you get only like one or two percent, we think that it is not going to be much of importance for the regulatory aspect, from the regulatory point. The reason is that you see that every time in Phebus you perturb some of the system, you can see the iodine either decrease or come up but you go back to another state, to the steady-state condition again.

So, in other words, in the containment, in our containment, if there is a leakage, you are diluting the atmosphere. But once you dilute the atmosphere, the paint can be a source of iodine so you couldn't put it back out into the gas phase. So basically, over a long time, you are going to pump out all of the iodine that was absorbed onto the paint.

So what we would like to know is what it the amount got onto the paint because if you get an amount larger than the five percent that we said in the alternative Source Term, we will need to address it. So, that is another thing that we need to look at.

But this is too complicated study for the EPICUR to look at but we are also participating in the Canadian program that is just started in Chalk River in the study of iodine chemistry. And over there, we are going to ask them to characterize much more deeper than what EPICUR can do.

Now, there is also different aspects related to the paint reactions with iodine. As you remember, there is a dry part, there is a wet part. And this -- okay, so basically the EPICUR is going to look at the coupons. That is what Bernard is talking about. This is basically for the looking at the dry condition. What we want to concentrate, look at, is the wet condition. So we will be -- I think the Canadian is going to do that and not you. Right?

The hypothesis number four is that we would like to see is that the formation of the so-called fine particles in the atmosphere that the French believe is the one that they need to study and in the study of these fine particles, I think we pointed out that they are looking for instrumentation for looking at these particles. And Dana pointed out that the oceanographers have done a lot of work in that area. They have produced some very good instrumentation of doing that and perhaps they can adopt it to study that aspect.

So basically, it is try to find out in the atmosphere, in the radiation field, you can form these fine particles. How does it form? Does it even form at all?

And then the fifth hypothesis is basically, if it is formed, how does it get settled? Does it go to the condenser or was it settled by gravity.

The sixth has to do with the paint, the aged paint because you know in our reactors the paints are not band new. So we need to address the applicability to this aspect. They are two different opposing view. The Canadian view was the first one and they said the interaction has to do with, involved with the residual solvents. But that is not the case. The French view was different. That's the second point but they need to test out which one is correct.

So, in other words, beyond the EPICUR program that Bernard had mentioned to you, we are participating in the Canadian one as well. And these are the areas that we mentioned earlier, that these are the points that we need to address.

And the characterization of the surface is very important because you need to understand what you have first before you and see what comes out and what reacted.

So, what is NRC plan for the next one or two years?

CHAIR SHACK: That was quick. So the debris that you are really worried about is paint chips? That is your primary concern?

MEMBER SIEBER: Or paint on the wall.

MR. LEE: Not necessarily, no.

MEMBER POWERS: In fact, it is likely to be rust.

CHAIR SHACK: Just because there is lots of it.

MEMBER POWERS: Well not -- because rust is fairly famous for being able to absorb iodides. Most things don't absorb iodides but rust does. And what we have supplied with the Canadians is what you guys used for your filter blocking? We just got the material from you and we are asking, put all kinds of junk in the water, find out what goes where, and then sort out which one is the important thing.

So, we are going to put a junk pile in the water with some tag stuff. My guess is that the bust will be as important as paint. Zinc would be very important. Zinc oxide really likes to suck up iodine. That is my guess. But is easier just to put everything in there, tag it, and go find out what absorbed where.

MR. LEE: You know, most of the water chemistry that we did were with distilled water. So, pure water does nothing else in there. You have a radiation field, you have certain things you put in and you look at what is in liquid and what came out. So, it is actually a controlled study.

And the reason that, what Dana mentioned that we asked the Canadian's to look at all of the zinc oxide and rust, the motivation for that is that you know that the PBI is moving from silver-indium-cadmium rod to propulsion rod. So you will not have the Ag to bind the iodine in the sump. So we are looking for alternative mechanism. We think the iodine will go to the other things. And that is what we say, but you need to have data to back you up. So we are asking the Canadians to do it.

We are also asking the Canadians to look at the iodine binding with the insulation which has huge surface area in the reactor case. And we think iodine also go there and bind it and it could be very stable. So you will not have these iodine coming back out. So we are looking at many problems.

So basically, the last one is that the models have been assembled, I believe, but we need the data to validate it. Between the French and the Canadian, we think within the next two years, we will get all of the data needed to validate the models. Using the same model, you need to understand the prediction under the Phebus case and we know that the model can predict that. And then we use the model to extrapolate to the PWR case so we can say what is the gaseous iodine fraction you will find in the containment, under basically all we are looking at is a DBA. Really, we are not interested in severe accident stuff. This has nothing to do with severe accident.

And after that, we are going to publish and peer review it. And then at that time we are going to decide whether we are going to get rid of the pH control that we put into the 1465. Because there is a pH control requirement written in it, if they are going to use the revised Source Term.

So, that is all --

MEMBER MAYNARD: What kind of time frame are you looking at? You say looking at it, you may have been able to redefine pH requirements or something. Are we talking like five years from now, are we talking a year? What are we talking about?

MR. LEE: We are talking about roughly two years or so. Of course, they have degenerated data, so we have no control over that part because we can ask them but it doesn't mean that it will happen because this is a joint model. They have their own things that they need to do to address the French regulatory aspect of it.

There is other partners in the French program. The same thing with the CS and the behavioral iodine project.

MEMBER MAYNARD: It just, this seems to be an important aspect dealing with a situation that may not address it again in a timely manner could have more consequence than her. So, that is -- I would urge you to move along quickly on that.

MR. LEE: Thank you.

MEMBER SIEBER: From what you have learned so far from the Phebus program, does that alter your confidence in the alternate Source Term?

MR. LEE: Yes.

MEMBER SIEBER: And what -- could you explain that a little more? Does it change your confidence?

MR. LEE: The aerosol behavior are fine. And then we need to make sure that that five percent -- if you look at this, basically there is nothing in the gaseous iodine fraction, basically remains around under the five percent. Okay? Forget about FPT-3, which has 85 percent. But you can see that even that one decayed very fast. If you look at the chemistry phase, it is still very low.

MEMBER SIEBER: Right.

MR. LEE: Okay? That 85 percent is during the release degradation phase, not the long-term behavior. The long-term behavior for all these tests are low, lower than the five percent that we are prescribed in the institution goals.

Now, the only thing we have that remains that we would like to address is that we need to, whether we should have the pH control captured there or not.

We also need -- paint has become an important role. We would like to know how much got into the paint because it is only if it is less than five percent, we really don't care. If it is more than five percent, we have to do something but this is too early a time to postulate.

MEMBER ABDEL-KHALIK: Reaching that sort of steady-state takes a long time. So, early containment failure, you know, these large numbers, may have relevance.

MR. LEE: But early containment failure is not a DBA.

MEMBER POWERS: Well the point is, we have reached that stage very quickly.

MEMBER ABDEL-KHALIK: I thought it is days, based on --

MEMBER POWERS: No, it's just a few hours.

MEMBER ABDEL-KHALIK: Oh, it was faster than that, wasn't it?

MEMBER POWERS: As soon as the degradation phase is over, it is essentially three hours.

MR. LEE: Well look at the bundle inventory. In our alternative Source Term, we have five percent. This is the whole five percent.

MEMBER ABDEL-KHALIK: The other concern I have is, you know, the word validation of models. There are so many complicated interacting phenomena and with a limited set of experiments to actually be able to identify these separate effects. I'm not sure that that is possible.

MR. LEE: But I think that is the reason

from the Phebus and effects experiments, to give you some guidance what you think are the important chemical species that you need to worry about. So, the separate effects from the target, it is that part of it.

MEMBER ABDEL-KHALIK: You are not trying to validate models qualitatively. You are trying to validate models quantitatively.

MR. LEE: Right. The simpler effects are supposed to be more quantity, quantification, yes.

MEMBER SIEBER: You might be better off after you are done than you were before.

CHAIR SHACK: It's very interesting. Does the committee have any more questions?

MEMBER SIEBER: I would like to comment a little bit to Jack on his question because I think it was a good question. If I were to characterize the Phebus program, I would say it has substantiated a lot of the judgmental points that had to be made at the time the alternate Source Term was formulated. That was formulated based on a lot of computer code runs and separate effects tests that were had. And I would say it was well done. Dr. Reid was one of the people authoring it.

And when it came down to gaseous iodine, at the time there was a huge pressure to claim that all of the iodine coming into the containment was just particulate but they had this nagging occasional experiment. It would show a little gaseous iodine and nobody knew really what to do with it. And so they stood out on a limb a little bit and they took a lot of static from it by putting in that five percent gaseous iodine.

And so the one area where the Phebus experiments have come back and said well, you were right in doing that but you were wrong about the mechanism. And that is what they are trying to sort out now, that here is a gaseous iodine compound cornered. It is behaving differently than we thought at the time and they are trying to sort it out.

And it really is a question of magnitudes here. If we are dealing with five percent of the inventory engaging in this gaseous iodine concentration, that's quantity. It doesn't change the regulatory stance at all. If it is 20 percent of the inventory, then you have got a big problem. Okay? Because there are just so many Curies of iodine in these first -- I mean, after 30 days, you don't care but in the first two weeks, you have got a real problem.

But in general, I would say the Phebus tests have come back and said yes, you are about right in your ultimate. So when you are about right in things, there are lots of details that we like to agonize over. You know, like we tend to under predict the deposition immediately above the core and over predict it in the steam generator. The net result is we get it about right in the containment. So, you are worried about those sorts of things.

CHAIR SHACK: That's comforting.

MEMBER POWERS: And the chemical speciation and whatnot is fluctuating. And some of these things have ramifications and some of them you don't care. I mean, the chemists go crazy but nobody else cares.

CHAIR SHACK: Thank you.

MEMBER MAYNARD: I have got a question as far as relative risk to the entire fleet right now. We have some information that may indicate that pH doesn't have the effect on the iodine as what we had originally thought. Right now, we know that a lot of the buffering that is used in the plants do cause problems. So we have a known problem. We have a potential not -- we have a potential solution and maybe that buffering is not needed.

Really, for the next couple of years here, are we better off with the buffering waiting for the results of these tests or are we better off to remove the requirement for the buffering? If the tests don't confirm it, then we can address do we need to put a backer on it. We have a known problem now that we can deal with.

MR. LEE: I think in an hour, we will answer that question.

MR. SCOTT: Mike Scott, NRR and responsible for the resolution of Generic Safety Issue 191. We do not believe that it is a slam dunk to say that removing a buffer is the net right thing to do, based on the information available at this time.

We encourage and support the Office of Research in investigating this issue. However, we do not think it is ripe for a rapid regulatory change to remove the buffers at this tie. As you all are aware, the licensees are taking actions to deal with the buffer situations that are out there now. And the situation is variable, very much, depending on the materials that are in the containment and the buffer that is chosen.

So again, while we are very aware of what is going on here, we don't think a precipitate action here to remove the buffers is appropriate at this point.

MR. LEE: In our overall evaluation, we also need to address the spray. Because the question raises that in the spray we know that if you have a high pH in the water, it will spray down and it will capture the iodine. So the question raised is even if you take off the pH earlier, do we need to reintroduce a pH at a later time.

MR. SCOTT: Yes, the other thing is that is that --

MR. LEE: Another thing you have to address. It is not just one thing.

MR. SCOTT: -- our friends in the Division of Component Integrity NRR tell us that removing the buffer will not remove chemical effects. It will change the chemical effects.

MR. LEE: Yes, there are some other chemical effects that doesn't go away.

MEMBER MAYNARD: I understand all that. And I understand that we are no where close to having a slam dunk. But at some point, you have to weigh your relative risk and decide what is best right now. If it was 50/50, I could understand that but if it 90/10 or 80/20, I would tend to want to lean towards what I thought was the safest right now. So that is all I am asking is some consideration of --

MEMBER BROWN: Maybe Mike, you can elaborate a little bit why, that statement you made. I mean, yes, there are the chemical effects, certainly but maybe there are a few other reasons why what you are saying is --

MR. SCOTT: As an example, and this doesn't directly relate to the PWR buffers, but it is sort of a similar subject. We just went on a trip to Japan to discuss some testing results with Japan Nuclear Energy Safety Organization. And they had done some testing in a Japanese BWR representative environment. And they did see some chemical effects with the iron oxide. And I am certainly not an expert enough to talk about those in detail. And we are looking into that situation as to whether it applies to the U.S. BWRs and we don't know.

But the point is, is that they observed some chemical effects in a buffer free environment. So again, we don't want to jump into this. At the same time, we are not ignoring it and we are encouraging research to move forward on it but that we have had discussions about just the very same thing that you mentioned. You know, should we take immediate action to change course with regard to Generic Safety Issue 191 because of the preliminary information from Phebus and the answer was, based on where we stand now, no we should not.

But we will get smarter. As you all fully know, we have been getting smarter and smarter on GSI 191 and so, something may come up that changes that situation. And so we are going to keep our eyes open. We are also not holding up the other corrective actions that the licensees are being asked to take to deal with chemical effects waiting on this issue to be resolved because we think that would be imprudent as well.

CHAIR SHACK: Okay. Gentlemen, I hate to interrupt this discussion but some of you may want to go eat lunch before the cafeteria closes.

(Whereupon, at 1:18 p.m., a lunch recess was taken.)

CHAIRMAN SHACK: Our afternoon session begins a little behind schedule but not too far. Our next topic is a draft NUREG on PRA methods for digital systems. And George is going to be leading us through that.

MEMBER APOSTOLAKIS: Thank you, Bill.

4) DRAFT NUREG/CR REPORT ON PRA METHODS

FOR DIGITAL SYSTEMS

4.1) REMARKS BY THE SUBCOMMITTEE CHAIRMAN

MEMBER APOSTOLAKIS: We met with the staff, the Subcommittee on Digital I&C met with the staff, and researchers from Brookhaven National Laboratory on April 17th doing this report, which is a main product of the project that focused on additional PRA methods. I think that is an important thing to remind the Committee.

First of all, this is part of the work that the agency is doing on digital I&C because many tasks that the senior-level committee would have been briefed on and so on, but when it came to risk and reliability, the agency had two projects. One was the one we will be reviewing today.

The purpose was to look at so-called traditional PRA methods, event trees, fault trees, and they lumped Markov models in the two, and see how useful or whether they are capable, to begin with to deal with digital I&C systems.

And the second effort, which is not part of today's meeting, has been reviewed by this Committee. That was when representatives from Ohio State University came here. And the task there was to use so-called advanced methods, which really meant simulation methods, but methods that are not user-friendly PRAs.

So this two-pronged approach has been implemented by the staff. I think the major conclusion of the work that these guys are going to present is that the traditional methods by themselves are not sufficient. So they borrow a little bit from simulation.

And, without further revealing anything else so that Alan will just get up and leave, I will turn it over to Mr. Kuritzky unless somebody else wants to talk first.

MR. KURITZKY: Okay. Thank you very much, Dr. Apostolakis.

4.2 BRIEFING BY AND DISCUSSIONS WITH REPRESENTATIVES OF THE NRC STAFF AND BROOKHAVEN NATIONAL LABORATORY

MR. KURITZKY: I am Alan Kuritzky with the Office of Research, Division of Risk Analysis. And with me today is Louis Chu from Brookhaven National Laboratory. He's the principal investigator for this work up at BNL.

Also involved with this work are Gerardo Martinez-Guiridi and Man Gua, neither of which could be with us today. I will go ahead and do the presentation on the work that we have done so far on this project, as Dr. Apostolakis mentioned. And Louis is here to help out with any detailed questions that the Committee members may have.

As Dr. Apostolakis mentioned, we briefed the Subcommittee in an all-day meeting a few weeks ago and delved into a number of the details on this project. What I am going to go through today is just to identify the objective of the work and where we stand with the project.

The primary focus of my discussion today will be on the contents of NUREG/CR-6962, which in draft version was provided to the Subcommittee. Also, we are going to talk a little bit about the insights that we have from the first benchmark study.

As soon as the draft NUREG was completed back last year, we nearly started going forward with the first benchmark study. And while we don't have anything documented yet on that work, we do have some important insights to share with you.

Also, based on the feedback we received from the Subcommittee a few weeks ago, we have identified a few changes to the work. And I will discuss our response or the staff's response to that feedback in this presentation. Lastly, I will just wrap up with the remaining steps of the project.

Okay. The objective of this work, echoing to some extent what Dr. Apostolakis said, is to determine the existing capabilities and limitations of traditional PRA methods for modeling digital systems.

And in defining traditional methods for this project, we mean methods that are well-established, well-used in the community, but specifically methods that do not exclusively account for or address the interactions between the digital system being modeled and the plant physical processes. Some of the other methods that you heard previously from Ohio State University and others do address those specific types of interactions.

The ultimate goal for this work is to support the development of risk-informed decision-making and review guidance for digital system models and also guidance for including such models into plant PRAs.

As I mentioned, NUREG/CR-6962 documents the work we have done on the initial activities in this project. This project is going to involve a series of NUREGs, NUREG/CRs, this initial one as well as one each for the two benchmark studies that we are going to be undertaking. Those benchmark studies involve a digital feedwater control system and a reactor protection system.

The contents of NUREG/CR-6962 include the set of what we now call desirable characteristics for reliability models. We have identified a number of aspects that we believe would be things we would be looking for in an ideal digital system reliability model.

The NUREG also documents the process that we are using to apply the event tree/fault tree and Markov methods to the first benchmark system. It also identifies some of the limitations and capabilities of the traditional methods and gives some preliminary areas for additional research.

One thing I do want to be very clear on is that this project is examining the existing capabilities and limitations of these methods. So it's not intended to advance the state-of-the-art. Therefore, things such as the quantification of software reliability are not within the scope of this project.

Lastly, as I mentioned before, we have gone forward with the first benchmark. It's nearing completion. And we will also talk a little bit about that later.

Okay. In the NUREG/CR, chapter 2 goes

over this list of desirable characteristics to include in a digital system model. These characteristics were grouped together in about nine different bins. They address most of the key aspects that you would expect to see in a digital system reliability model. It covers things such as level of detail, dependencies between the systems or within the system, better modes identification, documentation, all the general aspects.

This list of characteristics was put together based on the knowledge and experience of the study team members, who are experienced in both the PRA and have a fair amount of experience looking through digital systems. They also involved a literature review of things such as journal articles on probabilistically modeling digital systems, NRC reports related to digital systems, and the new reactor PRAs, which do include models of digital systems.

The initial list of these characteristics was the subject of an external peer review panel meeting that was held up at Brookhaven last spring. And in that meeting, there were quite a few practitioners from the areas of PRA, people with familiarity in digital systems, people from national laboratories, from the industry, from international organizations.

And that review meeting resulted in a number of substantial changes to the list of desirable characteristics or, as they referred to back then, it was evaluation criteria. I will get into the semantics change a little bit later.

After that set of characteristics was revised, it was then included in the draft NUREG/CR, which then in itself was subjected to a fairly substantial review process.

Both user offices, the Office of New Reactors and the Office of Nuclear Reactor Regulation, reviewed this, the draft report. They have people from their PRA groups as well as their engineering groups looked at it.

It was also subjected to review by a selected panel of peer reviewers, which again included industry folks, national lab folks, and international regulators. It was also subjected to a public review comment period. And so we received a number of additional comments from stakeholders.

As a result of all that review effort, we had to tweak the set of criteria a little bit more, not nearly as extensively as in the first go-around.

And that final set of criteria or, actually, we call them now characteristics has provided input to the interim staff guidance for digital system modeling in new reactor PRAs, as this Committee was briefed on a few weeks ago.

It also has been used as part of the planning of an international activity, a nuclear energy agency meeting under WG-Risk, working group risk, that is going to look at a lot of these same topics that are covered in the NUREG/CR.

MEMBER APOSTOLAKIS: When is this meeting going to take place?

MR. KURITZKY: Again, that meeting was supposed to be held this past April. There were some problems with our international partners as far as scheduling. And we are now in the process of trying to reschedule that. And we are hoping to have it in the fall.

MEMBER APOSTOLAKIS: Where?

MR. KURITZKY: Well, that was one of the issues we mentioned last time. It was originally scheduled for --

MEMBER APOSTOLAKIS: Probably Paris?

MR. KURITZKY: Well, it was originally scheduled for Brookhaven. And a lot of the international partners didn't like that idea, so now maybe Honolulu. No. I don't know.

MEMBER APOSTOLAKIS: Oh, it would be in the United States?

MR. KURITZKY: Well, from our point of view, it would be easier, but that we don't know yet. That is just what we have to -- the international partners didn't like the fact that we were kind of specifying it was going to be in the United States.

So, in any case, we are still working on that. And we don't know where it will be, but we do hope it will be in the fall.

MEMBER APOSTOLAKIS: Okay. Before Alan goes on, I want to emphasize something for the benefit of the Committee. Everything he will talk about, failure modes and this and that, refers to hardware parts of the digital system.

Software failures they did not analyze. All the stuff we've been talking about, design and specification, requirements, faults and all that, they did not do it. This is hardware. That's important.

MR. KURITZKY: And, just to further expand on what Dr. Apostolakis said, the failures that are quantified or the failures that are modeled in the Markov and event and fault tree models that we have in this project only include hardware failure modes.

However, software is included. The normal behavior software is included in the sense that in order to determine what were the failure paths that would lead to system failure, we had to consider the software of the system.

And we also consider a few hardware-software interactions in the models. But, as Dr. Apostolakis mentioned, we do not in these models quantify software reliability.

MEMBER APOSTOLAKIS: And the important thing is -- we are jumping a little bit ahead, but this is an ACRS meeting.

(Laughter.)

MEMBER APOSTOLAKIS: Even though they focused on the hardware parts, the traditional methods did not prove to be sufficient. These are very important. This is very good insight or conclusion from their work.

They still have to go to simulation, as Alan just said. In other words, you still have software left. We are not considering their failure modes, but they are part of the system. So they have to go and simulate it.

MEMBER STETKAR: To follow up on that, do you consider, does the staff consider traditional event tree/fault tree reliability methods adequate to evaluate traditional analog reactor protection control systems? That's a "Yes" or a "No" answer I would like on that. I would like a "Yes" or a "No."

MR. KURITZKY: I can only give you my opinion on that. My opinion is yes to a degree.

(Laughter.)

MEMBER STETKAR: Do you feel that simulation is necessary for analog hardware systems?

MR. KURITZKY: I have only looked at one PRA that actually broke down the details of the analog protection systems. And no simulation was necessary for that to develop those fault trees.

Now, whether a simulation would have made a better fault tree or a more complete or accurate fault tree, I can't tell you, but in that particular case, simulation was not necessary.

MEMBER STETKAR: Okay. Thanks. I'm just going to try to keep that in the back of my mind.

MEMBER BLEY: Keep it in mind, but the thing Alan said about they looked at the proper functioning of the software, it's the software interacting with the hardware failure modes and the timing that happen is where they use the simulation.

MEMBER STETKAR: Okay.

MEMBER BLEY: So it's how things happen in the software given a hardware failure curve. And the random things of that is where they needed it. You will tell us about that, right?

MR. KURITZKY: Yes. Also I have to take some exception to what Dr. Apostolakis had a --

(Laughter.)

MR. KURITZKY: I would say that the traditional methods are not capable of modeling.

MEMBER APOSTOLAKIS: Sufficiently I said.

MR. KURITZKY: Well, right now we are not in a position to make that claim. I think that the traditional methods themselves are very powerful, as I will talk about later, and can account for many features of digital systems.

I think where we run into some problems is with the supporting analyses for the modeling. In other words, quantifying software reliability for --

MEMBER APOSTOLAKIS: Even identifying the --

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: So that's what I meant.

MR. KURITZKY: Right, right. But that's

not really a limitation of a traditional method. Almost any reliability modeling method will need to have those same types of supporting analyses, will need to have the failure mode identification, will need to have --

MEMBER APOSTOLAKIS: But in the failure mode identification arena, I cannot do it using only event trees and fault trees.

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: And that's a conclusion from your standpoint.

MR. KURITZKY: Right, right. But we don't see that --

MEMBER APOSTOLAKIS: That's all I said, I think. If I didn't, this is what I'm saying now.

MR. KURITZKY: Okay.

MEMBER APOSTOLAKIS: But this is a very important conclusion. It is a very important conclusion because the other "advanced methods" were really on simulation. So what these guys are finding is that you can't really have two separate approaches. I mean, one has to borrow from the other.

MR. KURITZKY: Right, but the difference being at the simulation or the scale --

MEMBER APOSTOLAKIS: The degree you

simulate.

MR. KURITZKY: In our case, the simulation was more -- it wasn't an area that wasn't well-understood. It was just almost an advanced bookkeeping technique; --

MEMBER APOSTOLAKIS: Right.

MR. KURITZKY: -- whereas, the simulation for the advanced methods is, of course, more involved and --

MEMBER APOSTOLAKIS: Because they also buy the good software.

MR. KURITZKY: And the interaction with the --

MEMBER APOSTOLAKIS: Yes. This is not intended to be, you know, this method is better than the other.

MR. KURITZKY: Right.

MEMBER APOSTOLAKIS: But I think it is very important to appreciate that this separation, which may be at the time that the staff decided to have, made sense really ultimately does not make sense. You really have to try to come up with something integrated in my view without saying "My scope does not include this" or "does not include that."

See, the difference is that, you know, when Louis deals with something, sponsorship, he has specific objectives that he has to meet. This Committee is not bound by those.

MR. KURITZKY: Has no borders.

MEMBER APOSTOLAKIS: Yes.

MR. KURITZKY: Well, but the point that I just --

CHAIRMAN SHACK: It has no restraint.

(Laughter.)

MR. KURITZKY: I think the important thing, though, I do want to make clear is that while we recognize that the traditional methods, as we defined and are using them, do have what I will refer to as supporting analyses that need additional work, we are not there right now.

MEMBER APOSTOLAKIS: Yes.

MR. KURITZKY: But the other more advanced methods go beyond just the things that we are kind of --

MEMBER APOSTOLAKIS: No question about it.

MR. KURITZKY: It's not clear whether we necessarily need to or don't need to address some of the aspects that the advanced methods address. We don't know whether we will or not. We just don't know at this point.

MEMBER APOSTOLAKIS: In any case, this is a fairly artificial separation. When you do research, you do research. You are not saying, "Oh, they told me not to touch this method."

I think this is getting off on a tangent now. So why don't you continue?

MR. KURITZKY: Okay.

MEMBER APOSTOLAKIS: But I just wanted the members to be fully aware of this factor. They are using some simulation. And also on there it says the relative failure of most of the components. Bear in mind that errors in the software logic are not included. They mean the hardware components.

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: Very good.

MR. KURITZKY: Okay. So in applying these methods for this study, the first thing that the study team obviously had to do was get intimately familiar with the digital feedwater control system going through detailed design diagrams and documentation, how the system works, understanding its function, its digitally unique features and capabilities and tendencies.

After undertaking that effort, that put

the team in position to develop a failure modes and effects analysis. In that case, they had to identify exactly what types of component failure modes could occur and how they would impact the system function.

Using that information, they could then go and develop the Markov and function models for the DFWCS. Once those models are put together, obviously with the intent of trying to quantify these models, we need to have data. So we investigated what type of data was available in the public arena to do quantification.

As I will discuss a little bit later, there wasn't a lot there. The Subcommittee is quite aware of that fact, let us know about it last time.

MEMBER APOSTOLAKIS: You were not surprised, though.

MR. KURITZKY: No.

MEMBER APOSTOLAKIS: I mean, you yourselves don't seem to think much of these data.

MR. KURITZKY: No. We are in full agreement there, full agreement.

Again, the point is the last bullet on this slide goes back to what Dr. Apostolakis wanted to make clear, that these models do not address quantitative software reliability in terms of actually modeling and quantifying failures for the system we are focusing, specifically on the hardware in this case.

Also, human reliability analysis, which would be required if we wanted to integrate the digital system model into a full plan PRA, is not part of this work right now. But, again, in the ultimate goal, we will have to move forward in that area, too, because there was a lot of human interaction with the digital system.

Okay. What we did find out were a number of capabilities and limitations of these two models, at least initially, because, remember, this work is in the NUREG, which was before we actually did the first benchmark.

But our initial understanding of the capabilities and limitations of these methods is that both of them are very powerful methods. They are capable of addressing many features that I mentioned in digital systems.

Where the trouble really tends to come in is in the supporting analyses. And by "supporting analyses," I'm identifying things such as the failure mode identification, database development, dealing with the contribution of software failures. In my mind, those are supporting analyses, and that is where the weakness occurs.

But that weakness doesn't occur just for these traditional methods. Those weaknesses will tend to be problems for most any reliability modeling method: existing or advanced.

One advantage, of course, of event tree/fault tree models is that they are integrated into a plant PRA, which is one of the goals of the work.

One specific advantage of the Markov method over the event tree/fault tree method is that it is capable of treating some time dependencies and, more specifically, the ordering of failures. I will talk a little bit about that in another slide.

MEMBER APOSTOLAKIS: Are you coming back to the Markov?

MR. KURITZKY: Yes. Well, I am going to come back to the ordering of the failures.

MEMBER APOSTOLAKIS: No. The ordering I like. The Markov I think raises another point that needs to be made clear. And the reason why the Markov matrices can be solved in this case is because they don't consider any repair or restoration of failure components.

In essence, what the Markov model becomes is an event tree. You are just advancing in time.

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: So they consulted. So it is really a very limiting case of a Markov. If you have a Markov model that results in a matrix, transition matrix, that has, you know --

MR. KURITZKY: Feedback.

MEMBER APOSTOLAKIS: -- below diagonal members are all zero or they are above -- it depends on how you put it -- then it is easy to solve. So by assuming that there is no repair, which may be a reasonable assumption in this case -- I don't know -- although some of our consultants were troubled by it, but then you consult. Okay?

So it's not just Markov models. It's a very special case of Markov models.

MR. KURITZKY: Right. I think, to underscore that goes back to the -- as Dr. Apostolakis mentioned, the beginning of the two projects that the staff is working on in this area, the more advanced methods and the traditional methods. And if you recall from the presentations on the advanced methods, Markov models, more advanced version of the Markov models, including cell-to-cell mapping techniques, were considered to be an advanced method.

What we have pulled into the traditional methods is a more simplified Markov model, which, again, does not have the repair. So, as Dr. Apostolakis mentioned, we can solve it using differential equations. And it doesn't have to be numerically solved. And we don't subject ourselves to state explosion or other problems that may --

MEMBER APOSTOLAKIS: Right.

MR. KURITZKY: -- occur in more complicated models.

MR. CHU: I would like to explain a little bit this. The model, the system that we model, we don't need to consider it here because it is our understanding, you know, it cannot be repaired offline. That makes solving the model analytically possible. So it is relatively easy to devise an analytical solution. But when it comes to, say, situations where repair is needed, it doesn't necessarily mean that -- probably an analytical solution cannot be devised.

But as long as we look at single failure, double failure, triple failures, if we look at it this way, say you had a triple failure and it's recognize the first failure occurs and it's reparable, you can put that into a model, too. So it's not necessarily the problem will be unsolvable.

Of course, the current system doesn't require that.

MR. KURITZKY: Okay. Let's see. Some of the limitations --

MEMBER APOSTOLAKIS: One other thing. I am trying to bring up the members who were not at the Subcommittee meeting up to speed.

One criticism, if you will, of the Subcommittee members present -- by the way, it was Dennis Bley, Jack Sieber, and -- were you there? I don't know.

MEMBER STETKAR: I was not.

MEMBER APOSTOLAKIS: Mario Bonaca. For those who were not there, one criticism of the Subcommittee members was that the assumptions that we mentioned so far, namely no software failures, is simplification of the Markov. They were not made clear enough up front in the report. You really have to read 96 pages before you realize the software failures are not modeled. And the Subcommittee members felt that it should not be so.

That is not a major plan to fix that, but, I mean, that is what we saw at the time and also the

Markov thing. I mean, it really dawned on us after quite a while when I think Louis was talking that the Markov matrix was really very simple because of these assumptions. The comment was bring all this stuff up front so the reader knows.

In fact, do you think the title of your report is accurate?

MR. KURITZKY: I think so. Is that a "Yes" or "No" question?

MEMBER APOSTOLAKIS: I don't think. Well, you answered "Yes" or "No," and I didn't even put that constraint.

It says, "Approaches for Using Traditional Probabilistic Risk Assessment Methods." But you do do some simulation.

MR. KURITZKY: Right. And that's where, again, I go to the degrees of simulation. In my mind, having -- and we're jumping a little bit because we are going to get to the idea of the simulation in a slide or two.

But using that simulation to get over the hump of what I would call bookkeeping is not the same as the --

MEMBER APOSTOLAKIS: It's not the same, but it's still simulation.

MR. KURITZKY: But I wouldn't say that that is necessarily not a traditional method. That's where I -- the definition of traditional methods for us really hinges on whether or not you account for the plant physical processes and interactions with the system.

MEMBER APOSTOLAKIS: Do you do that?

MR. KURITZKY: We do not.

MEMBER APOSTOLAKIS: That's another thing I forgot to mention.

(Laughter.)

MEMBER APOSTOLAKIS: So there are three fairly significant assumptions. I mean, we all make assumptions when we do work, but there are three significant assumptions that should be really way up front there so that the reader knows what follows. The interaction with the process variables is not. Okay.

MEMBER BROWN: Can I ask a question?

MEMBER APOSTOLAKIS: Sure.

MEMBER BROWN: You made the assumption that you have a failure and then you just keep on operating and then you have another failure and then you keep on operating and then you have another failure and then what plant operates like that. Have

I missed something?

MEMBER STETKAR: Yes.

MEMBER BROWN: Okay. Then tell me.

MEMBER STETKAR: This is a three-element feedwater controller. And they assumed any failure gave you a loss of feedwater.

MEMBER BROWN: Well, then you've got to fix it.

MEMBER STETKAR: Well, no. But their failure was loss of feedwater. So it is a really simplified model of a simplified control system such that any failure gave you the undesired result. Therefore, they didn't need to look at repairs.

MEMBER APOSTOLAKIS: Basically, what they are doing is this. They go through what Alan said when they identify failure modes of individual elements, hardware. And they say, now, if this failure occurs, what is the impact on the system? And that's where they need to do this simulation on the side.

And they have a number of such single failures. And they conclude yes, this is a failure of the system, this is not a failure. When they take them three at a time, when they go to three at a time, they reach more than a million rounds.

MR. KURITZKY: You are stealing my thunder again. You are stealing my thunder again.

MEMBER APOSTOLAKIS: Well. So that's really all they do. They don't ask, but, you know, if I have two failures, you know, maybe there's something that will happen and they will stop the system from operating, right? You just go underway to see the impact.

MR. KURITZKY: Right. But it does consider the fact that -- and that's where the ordering the failures comes in to be important, that you could have a failure that, in and of itself, if it occurs first, will result in the undesired outcome, the system failure, as we define it.

And then you could also have cases where some other failure occurs first which doesn't fail the system. And then the failure occurs that previously would have failed the system had it occurred first, but occurring second, it does not fail the system. So it does account for that.

So it's not like it's just accumulative. The ordering of the failures is accounted for because it does make a difference for these types of systems.

MEMBER APOSTOLAKIS: But I think the important thing is that they assume that if a failure of an individual part occurs, the plant does not know about it.

MR. KURITZKY: Right. There are fault-tolerant features in the system, which we do model, you know, self-correcting features. But we do not account for any operator or human intervention to correct something that was failed.

MR. CHU: It is our understanding that when a failure occurs, it is not possible physically to go in and change a part without interruption to feedwater control. Therefore, for practical purposes, we cannot repair it.

MEMBER BROWN: But if the failure itself stops the feedwater, then it doesn't make any difference. You have to --

MR. CHU: Not every failure, not every individual failure, mode causes a failure. That's why we went all the way to consider triple failures. See, for a triple failure to fail the system, after the first or the second failure, the system is still working.

But the plan may recognize, there may be some indications that show something as strong. But in order to repair it, you have to shut down the system. That defeats the purpose. Therefore, it's a reasonable assumption that failures are not reparable.

MR. KURITZKY: Now, to get over, your point would be when we do the second benchmark system and look at a protection system, where there are multiple redundant channels, there is a case where things could fail and those failures may be identified and repair may take place.

So that's going to become more of an issue when we look at a protection system versus in this case, which is a control system, where as soon as you have the failure, you have the undesired outcome. So repair is after the fact, essentially.

MEMBER APOSTOLAKIS: Wouldn't it have been easier to do the reactor protection system first?

MR. KURITZKY: We would have loved to do the reactor protection system first. That was, in fact, what the plan of the whole project was. And we couldn't get the system in-house.

My understanding was that the feedwater control system, we were able to get -- this was tied into the work that was done at University of Virginia for Fulton-Jackson, which supported the Ohio State work and also our work. And while the intention was to do the RPS first, we just couldn't get the system available.

And it is unfortunate because had we done the reactor protection system first, I think it would have been a little more -- we have learned more appropriate things for our concerns. That way it may not have been as important to do a second system necessarily had we looked at the protection system, but that's just not the way it worked out.

MEMBER APOSTOLAKIS: Okay.

MR. KURITZKY: Okay. Again, this is one of the items that Dr. Apostolakis already mentioned. By definition, the traditional methods do not explicitly account for the interactions between the plant process parameters and the system being modeled or the timing of these interactions. And that is the big definition to differentiate between what we call traditional methods and what we call dynamic methods or advanced methods.

Additional limitations of the methods themselves, the event tree/fault tree method, cannot account for the order of the failures. And, as we just discussed a moment ago, that is something that is important for these types of systems.

One potential limitation of the Markov method, even the simplified Markov method, as Dr. Apostolakis pointed out, we have a much more simplified version than what would be done in the dynamic approach, but even in our simplified method, we are subject to state explosion, where we could have so many system states that developing and solving the model, the Markov model, is not practical.

And, as you will see when we talk in a few minutes about the simulation, we do end up with quite a large model, even with our simplifying the assumptions. But we do manage to steer clear of state explosion or at least be able to address it.

The NUREG/CR also identifies, as I mentioned before, some candidate areas for further research. One of the important things that has come out of this work and has been discussed in front of the Committee and elsewhere with other digital activities in the agency is the need to identify the failure modes of the digital system components. Failure mode identification is a very important underpinning to all of this.

And while we have developed an FMEA for this project, for the proof-of-concept project, that we undertook, we recognize that the completeness of that set of failure modes is obviously there is a fair amount of uncertainty associated with that.

And so further work into getting a more

complete picture of the different types of failure modes that can occur for digital system components is clearly an area that we would benefit from.

Also, determining the effects of component failures on the system, both single failures and combinations of failures, as was already discussed a little bit, that is where we had to bring in the simulation model. And, again, I will talk about that in the next slide. But it is very difficult to do that manually, so to speak. And the systems are too complicated such that automatic tools are typically necessary to support that effort.

MEMBER STETKAR: Is this control system any more complicated than a normal analog three-element feedwater control or --

MR. KURITZKY: I couldn't tell you.

MEMBER SIEBER: A little bit.

MEMBER APOSTOLAKIS: Is more complicated you're saying?

MEMBER SIEBER: More interaction between --

MEMBER STETKAR: Comparison.

MEMBER SIEBER: Simple systems.

MR. KURITZKY: Simple until you try to do the model for it.

MEMBER SIEBER: Right.

MR. KURITZKY: Also, as you mentioned, there is a need to get better data, find better data, classify better data, assemble better data, particularly in the public arena.

Vendors and manufacturers have their own proprietary databases. And those may, in fact, be more complete. They may be of better quality. We don't know. We are not privy to them. But certainly in the public arena, there is a tremendous scarcity of data.

MEMBER BROWN: What kind of data are you looking for?

MR. KURITZKY: We are looking for failure rates.

MEMBER BROWN: Failure rates, like how many resistors I've had fail or how many integrated circuits I've had fail out of the number produced or how many --

MR. KURITZKY: How many have failed per either cycle operation of the component or per hour, per time. So we are looking for failure rates or failure probabilities as well as a breakdown of failure mode distributions for a particular multiflex or whatever component we're looking at that can fail in different ways.

And we need to know, even if we have a failure rate for the component as a whole, what fraction of that failure rate was this type of failure mode versus another type of failure mode. And so it's really failure rates and probabilities and a breakdown of failure mode distributions.

And it is an important thing to identify because a lot of data collection efforts that exist, both in the nuclear field and elsewhere, focus on just looking at the actual operational experience and looking at the data and studying it. And that gives a wealth of information. It goes directly towards the first bullet of helping to identify failure modes. But it doesn't give us the pieces we need to stick into a probabilistic model and come up with a number.

Another again item is clearly the quantitative software reliability model or at least some means of addressing the contribution for software failures in the model. That is something that clearly we don't have in this. Currently the state-of-the-art doesn't support it right now. And that is something that obviously is a prime candidate for further research.

Treatment of uncertainties, again, in this case particularly modeling uncertainty and completeness uncertainty are two thorns in the side that we really need to address.

And, lastly, human reliability analysis, again, taking it step one further in implementing or integrating the digital system model into a plant PRA, we need to address the human reliability analysis aspects of interacting with the system as well as the human-system interfaces that occur in digital control rooms.

Okay. That pretty much wraps up what is in NUREG/CR-6962 in a nutshell. Like I said, there was a lot more detail we went into at the previous Subcommittee meeting. We don't have the time to go into all of it now. We can address any questions on any of those topics that you would like. But that essentially is what is in the existing NUREG/CR.

We have moved forward on the first benchmark study. And we have identified already a number of insights or obtained a number of insights that are making us rethink a little bit about what was in the initial NUREG.

One of the biggest things that we have come across -- and it has been mentioned already a number of times this afternoon -- is that a level of detail for these system models that we feel you need to go to to be able to identify all the aspects of a digital system that could impact its reliability, you end up with such large and complex models that it is virtually impossible to use the traditional fault tree or the Markov model method to identify all the failure paths or failure combinations that lead to system failure. And it is because of that reason, particularly with the digital feedwater control system, where not only was it combinations of failures were impossible to track to determine if the system failed but even some individual failures, which is very difficult to determine whether or not the system function would be compromised.

And because of that, Brookhaven put together a simulation tool that takes as input -- well, first of all, it's based actually on the source code of the system itself.

So it's built on the software, and it takes as input the various failure modes that were identified in the FMEA. And it uses some analyst-specified rules for determining what qualifies or constitutes a system failure.

And then it goes and it looks through. As Dr. Apostolakis mentioned, it will go through each individual failure. And it will crank it through and determine whether or not the system has failed. If it does, it is retained as a single failure of the system.

All those that do not cause system failure are thrown back in the pool to be looked in pairs. And then, again, those pairs that cause system failure are retained. Those that do not go and look at the triple combinations.

In addition, because the order of the failures, as we mentioned, is important, it tracks the orders, too. So whereas ABC may not be a failure, you know, BAC may be a failure. So it keeps track of that and ultimately gives the combinations of component failure modes that lead to system failure, essentially the same as the cut-sets you would get from quantifying or from solving a fault tree except that you have the order of the failures included.

Again, when we went and used the simulation tool for the digital feedwater control system, we came up, as Dr. Apostolakis mentioned, a few hundred single failures, many thousands of double failures, and millions of triple failures. We stopped after coming up with the triple failures.

Using the questionable data that we had

available but in quantifying it, we saw that, as expected, as you got to the higher order failure combinations, the contribution to the system failure probability was decreasing rapidly. So we felt no need to go beyond the triple combinations.

I shudder to think how many quadruples we would have come up with.

MEMBER STETKAR: Alan?

MR. KURITZKY: Yes?

MEMBER STETKAR: Let me stop you right there for a second. These numbers of very large combinations of failures are impressive. How much are they affected by the level of detail that you felt was necessary to model a component?

For example, one can model a relay by looking at probably 30 different subcomponents of a relay. And that will lead me to many thousands of combinations of three relays failing. On the other hand, most people model a relay as a relay.

MR. KURITZKY: Right.

MEMBER STETKAR: So the degree of detail that you felt is necessary to model these systems perhaps affects your perceived complexity. Is that correct?

MR. KURITZKY: Right, exactly. If you

look at the top line right there, the reason we are running this problem is because at the level of detail that we felt was necessary to take the model to, we ran into this problem.

MEMBER STETKAR: I'll come back to my original question, then. Twenty-five years ago, when we first started to model reactor protection and control systems for analog types of signals, we could have given this same presentation, that they're so complicated that it's not possible to model them. And, yet, we have been doing that, using these same methods, for the last 25 years, apparently to the acceptability of all of those concerned.

Why do we need to examine these methods to model the digital hardware? Because we have used them to model extremely complex analog hardware that has exactly these same problems.

MR. KURITZKY: Okay. I have some answers for you here.

MEMBER STETKAR: Okay.

MR. KURITZKY: And, again, I think the example that we used at the Subcommittee meeting was diesel generators, in fact; whereas, we essentially can have diesel generators fail to start, fail to run.

I personally have been involved in doing a fault tree of a diesel generator that broke it down to the components of the air start system, the little boil system, all of it. We don't include that in a PRA model.

MEMBER STETKAR: And the reason you don't is if you try to, you will come to the conclusion that the diesel will never work.

MR. KURITZKY: Well, there is the problem that summation of the parts exceeds the whole.

MEMBER STETKAR: That's right.

MR. KURITZKY: But that --

MEMBER STETKAR: No. It's true. I participated in a reactor protection analysis 25 years ago that took 9 months to develop a fault tree, had the same problems, was too detailed, could not find data.

And the conclusion once they finally solved it was that the reactor would fail to trip once in every five demands, something obviously wrong because the plant had operated many, many years with many, many successful reactor trips.

So the perceived level of detail necessary is very important. The level of detail compatible with available data is very important.

MR. KURITZKY: Right. And the point that

I will make is that in the diesel generator example, the reason that we don't model to that level. It's not because that the calculator probability would then exceed reality.

It's the fact that the key to what level of detail you need to go to, there are two major factors. One is get the level we have data. And we have data for diesel at the higher level.

The second is we have to identify the dependencies between that component or system and other components and systems in the plant. And we don't necessarily need to go to that level of modeling to do that either.

For this particular case, the reason we felt that we needed to go to this level of detail -- and, for the record, we're not saying that that is necessarily the level of detail that models in the future have to go to. It's what level we took it for in this proof-of-concept study because we suspected that it may be necessary. Time will bear out whether that makes a difference or not.

But the reason that we went to that level is twofold: one, because what little data was available in the public arena was at this. It's called basic, generic component level. And so we went to go down to that level to get data.

The second reason is that going to that lower level, we are able to identify certain potential failure modes of the system that would not necessarily be picked up at a higher level.

MEMBER STETKAR: Now, that's important. That is important.

MR. KURITZKY: Right. And we did identify a couple of cases there of things that are unlikely that even the designers may not be aware of that would result in system failure.

Now, whether those failures are at all probabilistically significant is another matter. So we wanted to go to that level of detail because we felt that was necessary.

Now, time will tell whether you do need to go to that level of detail, but at this point we did. And that is the reason we have all of these failures. If we didn't go to that level of detail, you are right. We wouldn't need the simulation model. You probably could do things at a much easier way.

MEMBER BLEY: I think you just hit on something that is important. And I'm not sure the report said this itself, that this report wasn't written to be a model for how to do this kind of analysis but as a proof-of-concept study to see what you could get at.

I don't think that's at all clear from the text in the report.

MR. KURITZKY: Okay. We will go look at that, make clear that it is or beef it up.

MEMBER SIEBER: It certainly is an opportunity for confusion.

MEMBER APOSTOLAKIS: all right.

MR. KURITZKY: all right. Okay. Where did I leave off? Okay. So the simulation tool itself helped solve a problem for us because obviously we had way more combinations that we could deal with and we were not able to identify which ones cause system failure. And it got us over the hump.

However, it still took quite a bit of time to run the simulation due to the sheer number of failure combinations that we had to encounter.

MEMBER APOSTOLAKIS: So let me understand this. When you say, "simulation," you had simulated the whole system, right?

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: Now, when you postulate that one or two --

MR. KURITZKY: Component failures.

MEMBER APOSTOLAKIS: -- failures, do you go back and look at the simulation to make sure that it's running, I mean, it's still running?

MR. KURITZKY: The simulation actually has it automated. Louis, you can jump in, but we feed, actually, the whole set of individual failure components, you know, the basic failures. And it goes and takes upon them actually one by one. Then it takes them in pairs. Then it takes them in --

MEMBER APOSTOLAKIS: But would the software be affected by these individual failures? Do you see what I'm saying? Maybe something that was a subgrouping or something would not work because you would have lost a couple of components, hardware components.

MR. KURITZKY: Right; in other words, the software in the actual digital feedwater control system.

MEMBER APOSTOLAKIS: Yes, yes.

MR. KURITZKY: We don't have that type of failure in there, right?

MR. CHU: We are basically running the actual fault tree. So when we postulate a failure mode, it's a fact it's reflected in the signal of the variables processed by the software. And that has --

you know, if you feed certain input to it and the software processes it and gives you a surprising result, then you question how long could it be the design of the hardware or it could be the design of the software itself.

MEMBER APOSTOLAKIS: You just said the variables. So the actual software of the system works with the physical variables of being modeled, correct?

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: You are assuming in your simulation that these are nominal?

MR. KURITZKY: Some fixed variables, yes.

MEMBER BLEY: I am not sure it's clear from what you said here, that you talked about in the Subcommittee meeting. When they do this simulation, they are actually running the software that runs in these machines in a test machine against each other.

So they're introducing these failures, letting the software run, letting it interact with the next processor's software, and seeing how that timing works out, right?

MR. CHU: Yes, as compared to, say, the dynamic method. The dynamic method developed a model of the software while we actually run the software.

MEMBER APOSTOLAKIS: Right.

MR. CHU: Of course, our running of the software still is in approximation to the real system.

MR. KURITZKY: Okay. I have a couple of slides now that I want to just address. This is based on the -- now it gets interesting.

MEMBER APOSTOLAKIS: Yes.

MR. KURITZKY: This is the feedback we received from the Subcommittee a few weeks ago. I broke it down into two separate camps. Comments or recommendations that involve programmatic issues in my definition are things that actually exceed the scope of this project, the stuff that is covered in the NUREG/CR. And then the other camp is actual comments and recommendations that apply to the information provided in the NUREG/CR.

I am not going to go over individually every one of the items that are this list, but the gist of these items was -- and I think Dr. Apostolakis I think has already mentioned to some degree the issue -- is that, as opposed to looking at the system, looking at the hardware part of the system, and then recognizing that the software part needs further work and dealing with that separately, that the staff should go and look at an integrated model, a probabilistic model, look at the software and hardware together, to see how it can be modeled and, as a first step in doing that, actually to look and see whether or not from a philosophical point of view the fundamental concept of probabilistically modeling software or using, considering software failure in a PRA context, is, in fact, appropriate and practical. And I think that was the essence of what we took away from a programmatic --

MEMBER APOSTOLAKIS: Yes, because depending on what conclusions you reach there, you may want to investigate a few things and not bother with others.

MR. KURITZKY: Exactly.

MEMBER APOSTOLAKIS: It's not just that you want to do some philosophy.

MR. KURITZKY: Right.

MEMBER APOSTOLAKIS: In fact, Louis I think last Subcommittee meeting did mention why a failure rate makes sense. If you have context, things happen.

MR. KURITZKY: Right.

MEMBER APOSTOLAKIS: I mean, if we agree with all this stuff, then I think that would be fine. But we do need that because it's not like we are modeling a new piece of hardware. Well, we more or less know there would be a failure rate or read the failure for demand. So that's really the reason to guide the further investigations.

Where did you get these, by the way, from the transcript?

MR. KURITZKY: Well, actually, based on the notes that we took.

MEMBER APOSTOLAKIS: Oh, you took notes?

MR. KURITZKY: Right.

(Laughter.)

MR. KURITZKY: And we did get the transcript later to look through it, but that didn't really change any of the -- in any way, so that's the general gist, as Dr. Apostolakis mentioned, is looking to see whether it makes sense to go ahead and model the software and the hardware together.

MEMBER APOSTOLAKIS: I don't understand. When you say there, "The staff should explore," I thought they reviewed that thing. I mean, you guys wrote it. Appendix C?

MR. KURITZKY: Well, that comes in the third bullet. The first bullet in my mind was actually broader. Appendix C is a first step to that. In the next slice, they give you the answers -- well, not really the answers but will give you some --

MEMBER APOSTOLAKIS: I am really curious. I don't remember if you answered. Why did you remove it from the report? You felt --

MR. KURITZKY: Okay. Then we're going to go to the next slide. Okay. Staff response --

MEMBER APOSTOLAKIS: Yes.

MR. KURITZKY: -- to these recommendations. Okay. The staff is undertaking or will soon undertake the following activities. The first thing is reviewing the draft former appendix C that was in the report that we took out.

The reason that that appendix was taken out of the report was primarily twofold. One is because it dealt with advancing the analysis and evaluation of software failure reliability, which was not part of the scope of the current project. And, remember, I am speaking to you as a project manager, not as an unbounded ACRS member. Okay? So that was --

MEMBER APOSTOLAKIS: So when you say, "The staff is undertaking reviewing," you mean you?

MR. KURITZKY: No.

MEMBER APOSTOLAKIS: Not Louis?

MR. KURITZKY: That's right, right, staff, but particularly I mean to say not me, not under this project, under a separate activity but in the --

MEMBER APOSTOLAKIS: But it would be NRC staff, not a contractor?

MR. KURITZKY: That's right. NRC staff. Now, we may involve contractors in that, but --

MEMBER APOSTOLAKIS: Not this guy, no, because he wrote it.

MR. KURITZKY: Well, I didn't just which contacts. I said we may --

(Laughter.)

MEMBER APOSTOLAKIS: I mean, we are supposed to review our own work. You should not be --

MR. KURITZKY: No. They will not be involved in reviewing their own work. However, they may be involved in explaining it, but they are not going to be involved in reviewing it.

MEMBER APOSTOLAKIS: That's fine. In any case, so it was out of scope. So the software is out of scope. The second thing is this was work that was done actually a few years ago under a separate part of the contract.

And it was before the program was redirected. And so it fell off the edge of the table. And so now, again, we are looking at it. The staff is re-looking at it under a separate activity to see whether there is stuff there that is of value.

There are some interesting concepts that may be worth further pursuing. And it's what we are looking at right now but just not as part of this direct project.

Okay. Secondly, the staff is also looking at data. Well, I guess the Committee is aware right now, obviously, that the staff and industry are looking at nuclear digital system component failure data right now to derive insights as to failure modes.

But what we are also doing is looking at some non-nuclear. There's a lot of digital experience in non-nuclear industries. And so we have an effort underway to look at some of that data also.

MEMBER APOSTOLAKIS: I have a question about that. I don't remember if it was in this report or another report where it is stated repeatedly that each software system is unique and that you cannot take experience from this system and use it for another. Is that in your report?

MR. KURITZKY: I don't know the specific words, but it generally exists in our report and everywhere else.

MEMBER APOSTOLAKIS: So why did you experience with other systems, especially in non-nuclear industries, where, you know, the quality assurance may be not as rigorous and so on and so on? Why did you do it?

MR. KURITZKY: Because, again, we go to the difference between identifying failure modes and understanding failure modes versus quantifying.

MEMBER APOSTOLAKIS: To see what happened.

MR. KURITZKY: That's right. We're not looking at that data in order to plug it in to our model as in numbers but, rather, to see what you can learn about how the digital systems and their components fail.

MEMBER APOSTOLAKIS: You heard EPRI's presentation to the Subcommittee.

MR. KURITZKY: Right.

MEMBER APOSTOLAKIS: You were aware of it before?

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: Is that changing anything from what you are doing? I mean, they seem to be collecting a lot of information.

MR. KURITZKY: Well, it is not really impacting what I am doing in this project. Again, there were other NRC projects. In fact, the Committee was briefed on an in-house or staff effort looking at nuclear and digital system failure experience.

And I think there was even some talk. And whether the industry or the staff mentioned in the last briefing that there has been some cross-checking or, you know, one said that they had looked at the others and got some information that they couldn't find, et cetera. So both of those efforts are going along I guess independently, but there is crossover there. It's not part of this project, but that is something that is going forward.

And so both of those efforts -- I mean, we can't lose from any of them. The more data we look at, the more things we see and understand that better our knowledge. But it's just not part of this project. But, again, it is an activity that the staff is undertaking to increase our understanding of failure modes of digital system components.

We also are planning to conduct internal discussions on the fundamental aspects of software failure modeling. And that is to get at the issue as to whether or not it makes sense, whether it is appropriate or makes sense to be able to model software failures in a probabilistic sense.

MEMBER APOSTOLAKIS: What brought this about, the conduct of internal discussions?

MR. KURITZKY: What is the title of that slide?

MEMBER APOSTOLAKIS: Yes. But, I mean, we asked you to do this two years ago.

MR. KURITZKY: But you just asked me this year.

(Laughter.)

MEMBER APOSTOLAKIS: Good answer.

(Laughter.)

MR. KURITZKY: I think the staff has been working on these ideas. It's just we're more formalizing them now as we're getting more input and moving further along.

MEMBER APOSTOLAKIS: Well, it's also the time scale of response.

MR. KURITZKY: I can't answer that.

MEMBER APOSTOLAKIS: But I know from experience.

MR. KURITZKY: Okay.

MEMBER APOSTOLAKIS: It's not on the order of months.

MR. KURITZKY: In any case, the results of these various efforts as well as the other recommendations, the programmatic recommendations, will be all folded together. And that will be reflected as we update the five-year digital I&C research plan, which the Committee will be briefed on later in the year.

MEMBER APOSTOLAKIS: And this is happening in the fall sometime?

MR. KURITZKY: I don't know what the exact schedule is, but --

MEMBER APOSTOLAKIS: This is very important, by the way. I think the Committee will have a major role there. Now we are all wiser after all of these preliminary steps.

MR. KURITZKY: Right. And last item I just want to mention, a specific recommendation that was made by the ACRS was that one task in the current project or the last task of the current project, in fact, was to integrate the models that we come up with into an actual plant PRA to make sure that it would flow smoothly.

And the recommendation, which we have taken to heart, is that we should hold off on trying to implement that until after we have more complete and integrated models; in other words, that have software failures if it's going to be possible in the model itself. And it would be premature to try and essentially put the hardware model into the PRA. So we are holding off on that task.

This slide brings up a few of the specific recommendations that we heard in April from the Subcommittee on the information in the report itself. The first item was the fact that the staff should continue to focus on failure mode identification and the effects of those failure modes. And, in fact, that is still a major portion or a major part of the report.

And the staff should also explore the theoretical basis for evaluating individual systems in the traditional probabilistic model, the emphasis there really on the software aspects.

Secondly, because some of the criteria at that time, what were called evaluation criteria in section 2, address issues for which there are not currently methods available or there is no consensus on for accomplishing, it was felt that -- and some other ones were relatively vague and not very specific as to what a practitioner would have to do. The staff should go revisit those criteria.

MEMBER APOSTOLAKIS: How many did you have?

MR. KURITZKY: Fifty-two.

MEMBER APOSTOLAKIS: How many are you

going to end up with?

MR. KURITZKY: Fifty-four.

MEMBER APOSTOLAKIS: You start with 54 and you end up with 54?

MR. KURITZKY: We didn't add any.

(Laughter.)

MR. KURITZKY: The next slide I'll show you will respond to that comment.

Okay. Another item that we have that we have to deal with is the fact that we have very poor-quality data in the public arena and the issue that is not meaningful to quantify when you have such poor data.

The recommendation we got from the ACRS was either to heavily caveat that.

MEMBER APOSTOLAKIS: The ACRS Subcommittee.

MR. KURITZKY: Subcommittee. Sorry.

-- Subcommittee was to heavily caveat the tables that have that data so that they could not be separated out and used for nefarious purposes.

MEMBER CORRADINI: He said, "Nefarious"?

MEMBER BLEY: That's an appropriate word. It's very appropriate for this circumstance.

MR. KURITZKY: And also again, -- and Dr.

Apostolakis already mentioned this -- the fact that the report does not address software failures should be made more prominent right up front.

MEMBER APOSTOLAKIS: Right. That was already discussed.

MR. KURITZKY: Right. Right. Okay. So this is what we are planning to do in the report to address these recommendations. The work on failure modes still does comprise a significant portion of the report. And that clearly is an important aspect. And we agree with that comment.

The issue about coming up with a theoretical basis for evaluating software failures, that's something that we felt, again, was out of the scope of the current project. So we weren't going to address it substantially in this NUREG/CR.

In chapter 6 under the Markov models, we talk a little bit about including software failures into a model. But the bulk of what that really addresses, this topic was in the appendix C, which, as we mentioned before, is not going to be included in the final NUREG/CR but is being looked at further by the staff as a separate activity.

The criteria in section 2, we went and looked at them. We made some minor changes to the wording of some of the criteria. However, the big change I think that really gets at addressing the Subcommittee's concern is just the name.

We call them "evaluation criteria." And that implied a certain amount of regulatory impetus and implied that people who were to come in with models would have to meet these criteria. There would have to be some level of acceptance, some acceptance criteria they would have to meet in order to be so-called adequate or sufficient.

That really was not the intention of that list. And so what we have done is we have actually renamed them as desirable characteristics of a digital system model because, really, what they are is a list of the things that we feel an ideal model should contain.

Whether or not all or some or none of those things would ultimately be required of a model from a regulatory perspective that remains to be determined. As of right now, all they are is essentially a wish list of what we think should be in a model.

And we want that list because when we go do our benchmark studies and we are going to compare them against that list, we want to see how well we can match up with current state-of-the-art in meeting these various characteristics that we think are desirable. And that is going to help us identify where we feel there may be additional areas that may need additional research and development. Okay?

But they're not intended to have any regulatory implication at this point. And so I think that terminology change actually is very important because it gives a whole different meaning to what that list involves.

Also, the report, we are still after much debate deciding to take all the numbers out of the report or heavily caveat them. We have decided to leave the numbers in the report. We are going to make it clear throughout the text that there is great limitation to these numbers and caveat them quite substantially. All tables that provide NRC-generated numbers are going to have a caveat on every page of the table so no one can yank it out.

But the reason we decided to go ahead and keep those numbers in the report or essentially to keep quantifying as part of the project was because that as part of the proof-of-concept study, we want to be able to demonstrate what these methods could accomplish.

We're not going to put any value on what we calculate. We're not going to draw any conclusions on what failure modes are more important than other failure modes or what the final system failure probability means.

We want to be able to just go through the exercise to demonstrate the capabilities of these models and what you can use them for. So that is the reason we have kept the numbers in. But, again, like I said, we were heavily caveating them and making sure that they are not going to be misused.

MEMBER STETKAR: We all know the fault trees can add and multiply. So showing that a fault tree can add and multiply is showing a proof of concept for a fault tree model. Why do you need models to show that proof of concept?

I know the fault tree can add and multiply. We have been doing that for a long time.

MR. KURITZKY: We understand that. I guess, you said that you had identified for demonstrating them?

MR. CHU: Yes, to start with, since the order in which failure occur affects the result. So in our quantification, we need to account for the order in which failure occurs. And the Markov model is an actual model that allows you to account for it.

When you come to fault trees, you know, you use the standard fault tree computation method. It just multiplies the ability that you are not quite accounting for that.

So fault tree is not a very good quantification tool for the model as far as --

MEMBER STETKAR: Correct, but we also know that without actually running the numbers through. I don't see the benefit from publishing tables of numbers and running the numbers through the models because everything you have said we know.

MR. KURITZKY: But, again, what we are trying to do is demonstrate in this proof-of-concept study as to what is potentially capable using these models. In other words, we would intend if the data were good, if the various lists of things that we now have as rough spots can somehow be polished over, that this is what we would use these models for.

And that would include quantification. And it would include things like make a determination as to which failure modes are more important, which design features and, therefore, which design features you may want to look at more carefully, may want to redesign or --

MEMBER STETKAR: Did you have a question that fault trees could not do that or that Markov models could not do that? In other words, this proof of concept of a methodology I don't understand why we need to quantify at this particular moment for identifying failure modes.

Process you used, I think that's important for looking at the timing of failures to identify the fact that that may be important for identifying specific combinations of failures within a specific sequence, that Markov models can do that.

They have that capability for which fault trees do not. That is important. And demonstrating that that methodology can do that is important. And why that might be important in this context, where it might not be so important for other more traditional things I think is important.

But actually turning the crank and churning out numbers --

MR. KURITZKY: Right. Again, the numbers in question are not the output numbers. It's the input numbers. It was the idea of --

MEMBER STETKAR: Except that the input numbers are the numbers you have absolutely no confidence in.

MR. KURITZKY: Right.

MEMBER STETKAR: What I'm concerned about -- and I wasn't at the Subcommittee meeting. So I don't have the benefit of the discussions there. We have suffered in the PRA community for years and years and years of NUREG reports being published with tables of numbers in them, in many cases with very large caveats on those tables, caveats notwithstanding those numbers are extracted as NRC-sanctioned numbers. And they are used.

And especially if there are delays in completion of this project to actually integrate the software analysis, those numbers will take on a life of their own with people who are being charged now to develop PRAs for design certification, for COL applications that are going to be coming in over the next two to five years, while you are still working on integrating the software analysis.

That is the primary concern that I have about the numbers in there. And I don't see the benefit of having quantification in there as a demonstration --

MR. KURITZKY: I am very sensitive -- because, I mean, I understand. I am very sensitive to that concern. I think our reasoning was really to demonstrate that the hierarchical vision method that was used with those numbers, which we feel may be a valid method to use if you had better numbers to stick into the sausage grinder, would, in fact -- that is what we are kind of demonstrating, that you can use this method. And it would work.

I am sensitive to the concern over those numbers. I guess we just have to rethink whether, actually, the final numbers -- I mean, we want to demonstrate some of the numbers that are out there just as part of the discussion on what's available. But as far as the tables at the end that we have come up with, the numbers, we will take a re-look at that to see whether it is true value to that and whether the risk outweighs any potential benefit.

MEMBER BLEY: Just on the last thing you said, Alan, the NRC has published the handbook for parameter estimation for PRAs. And that walks you all through doing the hierarchical phase and how it works.

So I guess it seems the only benefit is to say, "Well, we did a lot of this. And, here, look. We did it," rather than some real technical value coming from it.

MR. KURITZKY: Okay. Do you want anything?

MR. CHU: The hierarchical base analysis we did basically was to capture the population variability of different sources of data. It's questionable whether or not those data are good or applicable to the specific application.

But this kind of population variability curve in general represents a prior distribution of a single basing analysis. So if you want to have better data, you need to collect application-specific data and then use it to update this population variability. Of course, in our study, we don't have any specific data to use.

Going back to in technical availability modeling, often you ask typical questions, like, say in the case of digital systems, there are digital design features that we know. See, use a watch-stop timer.

Then it's reasonable to ask, you know, what is the benefit of having this watch-stop timer or in the case of this system, there are two micro processes that are redundant. So what is the benefit of having the redundancy?

So when we developed this model and using our model, we can answer that kind of question. So we can perform with numbers, with data. And we can perform sensitivity calculations to provide answers to these kinds of questions. This kind of illustrate the usefulness of the model.

MEMBER STETKAR: But as a proof of concept of a methodology, we know we can do that because we do that all the time in terms of developing risk importance measures and evaluating the effects of redundancy and the effects of common cause failures.

I come back to the purpose of this exercise. And that purpose, as I understood it, was to examine the benefits and the limitations of existing analysis methods, not numbers, just analysis methods, with respect to digital I&C systems.

We know we can do sensitivity studies. We know we can evaluate importance measures. We know that if, indeed, the conclusion is that the fault tree methodology is applicable, we know what we can do with that. That's close to 35 years of experience doing fault tree analysis.

If there are unique features of a particular methodology, like we discussed earlier, that's important. But, again, I don't see where the actual quantification at this stage, given the paucity of applicable data, is a real net benefit and not only is a real net benefit, it could be a detraction from what you're trying to accomplish here.

MEMBER APOSTOLAKIS: How would it hurt your message if you remove the tables?

MR. KURITZKY: And that's a valid point. We'll re-look at that. I think, like anything else, when you give an example, sometimes it helps make things clearer in the reader's mind. But in this particular case, with the risk of misuse being as potentially high as it may be --

MEMBER APOSTOLAKIS: Well, yes.

MR. KURITZKY: -- it may be that we can suffice with sufficient explanation on the concept that you don't necessarily need to show an example or numbers to make a point. So, I mean --

MEMBER APOSTOLAKIS: Bayesian method there. What do we call the hierarchical?

MR. KURITZKY: Hierarchical.

MEMBER APOSTOLAKIS: I can see practitioners saying, "This number is good because it was derived by a sophisticated method by the NRC or its contractors."

MEMBER STETKAR: Using data derived from --

MEMBER APOSTOLAKIS: Using data --

MEMBER STETKAR: -- Oak Ridge National

Laboratory and --

MR. KURITZKY: Now, also, to be fair, too, I am very sensitive to the concern and will rethink it. But, I mean, anybody can take anything out of context. They read the Washington Post.

(Laughter.)

MEMBER STETKAR: They will. They will. And you have to recognize we're dealing with --

MEMBER APOSTOLAKIS: I like the Post.

MR. KURITZKY: I like it, too. I read it every day.

MEMBER STETKAR: In this Committee, we are dealing with submittals from design certifications that are I'm assuming struggling with the issues of trying to develop PRA models for their digital I&C systems in real time, I mean, now. And those people given the opportunity to use numbers that are published in a NUREG report, NUREG/CR, whatever, we will certainly refer to them.

MR. KURITZKY: Now, that's a very good --

MEMBER SIEBER: They don't have better numbers.

MEMBER STETKAR: And they don't. And they don't have any better numbers.

MEMBER APOSTOLAKIS: It is such a nice

phrase, "hierarchical Bayes."

MR. KURITZKY: One thing I wanted to point out I did not point out previously was while we had to use hierarchical Bayesian method, we demonstrated one because of the fact we had very poor data from various sources, the applicant may, in fact -- we don't know, but applicant may have a very good manufacturer vendor database. They may not use that method. They may use some totally other method. We don't know.

MEMBER STETKAR: They won't have one.

MR. KURITZKY: They might.

MEMBER STETKAR: They won't.

MEMBER APOSTOLAKIS: Three hundred? That really jumps up off the page, an error factor of that magnitude. I really think the message from all of this discussion is you really ought to think very hard about numbers.

And let's move on to your 15.

MR. KURITZKY: Yes. Okay. Oh, wait. The last bullet because there is one very dear to you.

MEMBER APOSTOLAKIS: Yes. You agree. Okay.

MR. KURITZKY: Yes. And now it's in the abstract --

MEMBER APOSTOLAKIS: Just say we agree.

MR. KURITZKY: -- and section 1.2 in objectives and section 1.2 in scope. We have it --

MEMBER APOSTOLAKIS: The abstract, yes.

MR. KURITZKY: The abstract says it. Everywhere says it. No one can read this report and not realize it now.

MEMBER APOSTOLAKIS: Front cover?

MR. KURITZKY: It wasn't on the front cover. I will take that back.

MEMBER APOSTOLAKIS: Okay.

VICE CHAIRMAN BONACA: It was in section 1.

MR. KURITZKY: What's that?

VICE CHAIRMAN BONACA: It was in section 1.

MR. KURITZKY: It was in section 1.3 in the scope, I know. It was as an example, but it was there. But now it's really there. Not it's really there.

MEMBER APOSTOLAKIS: Very good. So next steps.

MR. KURITZKY: Okay. Next steps in the project. Again, we will finish applying the two methods in the first benchmark studies, the digital feedwater control system, which will help us gain further insights into the reliability modeling of systems.

The idea of major contributors to the system is obviously not so much now that we are de-emphasizing quantification of the numbers, but from a quantitative sense, we can get some further insights there.

Also, we will further determine the existing capabilities limitations of the methods as we complete the study. We will attempt to compare the results of insights from this study to the sister studies. These are parallel studies being done with dynamic methods on the same system. Again, there is some substantial difference in boundary conditions for --

MEMBER APOSTOLAKIS: What is the sister study?

MR. KURITZKY: The dynamic, the dynamic method studies. There are some substantial differences in boundary conditions between those two studies. So it's going to be somewhat limited of a comparison, but we will do the best that we can. And the NUREG/CR on the first benchmark is due to come out in a couple of months. So we will have more information then.

Then the next step will be to go ahead and apply these methods to the second benchmark study, which would be a protection system, you know, safety rate system.

We will use a reactor protection system, the TELEPERM system, the design requirements for safety relates. This is obviously much different than for a control system. And so we have expected the modeling issues may be different also.

In one specific case, in looking at the protection system, we no longer have to deal with the complexity of the feedback that you get with a control system, but we will have to look at other things, such as the communication and synchronization between redundant channels and other aspects of protection system failures.

MEMBER APOSTOLAKIS: What are the two traditional methods?

MR. KURITZKY: The fault tree method and the Markov method.

MEMBER BLEY: You are only going to look at the reactor trip function under the TELEPERM?

MR. KURITZKY: We will be looking at reactor trip function.

MEMBER APOSTOLAKIS: Are you done?

MR. KURITZKY: I am done.

MEMBER APOSTOLAKIS: Any other comments or questions from the Committee?

MEMBER MAYNARD: I've got a couple.

MEMBER APOSTOLAKIS: Sure.

MEMBER MAYNARD: Before I start, though, you have done a lot of good work here. And my comment, the couple of concerns I have, really aren't negative about the effort and stuff that you have done.

One concern is a couple of times we talked about what's desirable and an ideal system. And I really think that what the NRC needs to be developing right now is what is the optimum for the state of knowledge and where we are at right now.

I am a little concerned that we might be encouraging a higher level of detail and more complexity than what is achievable and what would be practical and what is really needed for assuring adequate protection of health and safety of the public, so a little concern on my part as to, are we trying to say that the ideal one is where we need to be right now and is that really too complex? So that's just one comment I have.

And the other gets into timing in that

with the way we are going, we could easily be working on a number of these things well after we have submittals that we are having to review and evaluate right now. That's why I get back to more of what is the optimum from a regulatory standpoint that is needed based on what is the current state of knowledge and availability of information.

You know, we do have applications coming in. Some are going to be coming in here real soon that we need to be evaluating now and can't be waiting for a number of years down the road?

Those are my two comments/concerns. And that's not derogatory in any way on your stuff.

MEMBER APOSTOLAKIS: And I think those comments would be even more important when the new plant comes.

MEMBER STETKAR: Just remember even Duke is looking at replacing their entire reactor protection safeguards, actually, now.

MR. KURITZKY: I don't think they are using any risk --

MEMBER STETKAR: They are not using risk guard.

MR. KURITZKY: Right, right. Based on the --

MEMBER APOSTOLAKIS: They are already an approved platform, right?

MR. KURITZKY: That's right.

MEMBER APOSTOLAKIS: Without any risk.

MEMBER STETKAR: They are not using risk guard.

MEMBER APOSTOLAKIS: Okay.

MR. KURITZKY: But let me just talk to Dr. Maynard's comments. I agree we have to make sure that the report is clear that this is not what we expect from an applicant. This is doing the research to help us learn what we feel should go in the system, then determine what is minimally necessary.

The second one versus timing, that is an important concept, but I think that is one that has been addressed by the fact that in the steering committee, there is a task working group that is looking at risk-informed applications of digital I&C.

And they have an interim staff guidance document. It is out in draft. I mean, it is close to becoming final now. And that is specifically for new reactors. In other words, what is acceptable under a part 52 PRA for digital system modeling there?

The staff has made a clear distinction that there is a certain level that you need to go to to demonstrate that you don't have any -- you don't challenge the safety goal, there are no significant vulnerabilities that may be out there in the system design. Okay? And you may only have to do a certain level of pedigree of model to be able to establish that; whereas, what we are looking at under this work is trying to get the basis for a more robust risk-informed framework that would allow a lot of decisions, going back to the idea of Oconee.

If Oconee had, in fact, come in with a risk-informed submittal, how could we evaluate that? That is where we would want to dive into more of the detail.

So the staff really is going on a two-pronged approach, where we have existing guidance that is almost final now that is going to be the guidance for something coming with a new application. And what we are really pursuing is the longer-term, more robust risk-informed framework.

MEMBER MAYNARD: And I understand all that. And I appreciate your comments there. If 90 percent of the stuff is already done by the time we get there, what is the value-added to that thing?

MEMBER SIEBER: It seems to me that you are going to very great lengths to form this probabilistic analysis for digital instrument system. And if I compare that to what is done with analog instruments, it would seem to me that the answer is going to come out that the failure rates for digital instruments just because of all this detail, it is going to be higher than for analog instruments. Is that a true fact?

MR. KURITZKY: Well, I can't tell you whether that is going to be a true fact. I mean, that goes back to the same comment that we had before. When you break something down to a lot of little parts and then add up all the failure probabilities, you know, you end up with something that is not realistic.

MEMBER SIEBER: It could be 10 times, 100 times different.

MR. KURITZKY: But, again, you know, we don't know. And, again, like we said, we don't know what level of detail ultimately we would find acceptable for some type of a method, too. But that obviously --

MEMBER SIEBER: If that happens, then I have to ask, why are we doing this? Because it probably is not correct.

MR. KURITZKY: Right. But if we knew that right now, you're right. We wouldn't be doing this.

But I think we're not in a position yet that we know that.

MEMBER STETKAR: It is really the compatibility with the data. I mean, you cannot separate the modeling from the data compatibility. That has always been the problem.

MEMBER SIEBER: Yes, but you can look at it as a system function.

MEMBER STETKAR: That's right. I mean, you have to have that higher --

MEMBER SIEBER: Techniques to making the software actually work properly, as opposed to figuring out how often it is going to fail.

MR. KURITZKY: But we are in the PRA group. So we have to figure out how often it is going to fail.

MEMBER BROWN: John's point is very much on the number. Okay? I mean, there is a vast population of operational digital applications today. They are vastly more reliable and online and more failure-resistant than the analog systems.

They don't drift. So you don't have to deal with that. They self-check themselves from an alignment standpoint. They tell you when something is -- you know, put a little test resistor in and say, "What temperature am I getting?"

You're running against it. Is the thing working? They just work so well. Yet, by the time you stack up the numbers, you are applying a level that we used to apply to the analog systems down here. Now you are putting the bar up here for the digital systems to apply.

And I can only speak from experience that you will get far more value-added out of making sure the software that you apply in these digital systems is controlled, is simple, easy to make sure it works, as opposed to that's kind of a -- I mean, I'm just giving you an opinion based on the approach of doing about six or seven different or eight designs that all went on the micro processor, digital-based system. And stuff came out much, much better.

And this stuff is what you want to put in the new plants. You do not want to go back and force us to stick with analog stuff that nobody builds anymore.

MEMBER SIEBER: You probably can't because a lot of those vendors don't make those anymore.

MEMBER BROWN: Yes. There's nobody that comes out and does any analogs other than little parts of front ends for A to D conversion. And after that, it is all digital.

MEMBER SIEBER: Well, your background is Navy. And I have some of that in me, too. And that philosophy was simplicity in the design of this.

MEMBER BROWN: And that is the whole argument that I would try to bring to this. If I were brought in for anything, that is the argument I would bring in. Simple software. If you get complex software, you get more than 15-20 thousand lines of code, you are lost.

MR. KURITZKY: I don't think anybody is disagreeing with that.

MEMBER BROWN: Even if you have that much, you don't need it most --

MR. KURITZKY: We're getting crossed goals here. The goal of this work is to look at calculating the probability of the failure. The fact that it makes sense to have simpler and more effective and higher reliability software or in digital systems to put in place the practice that will ensure that, we're all for it. But that is not the goal of --

MEMBER BROWN: No. I understand. But you are trying to put the PRA emphasis. The licensee and/or whoever designs the stuff has to do that work. That costs money. And that takes time. Software takes time.

MR. KURITZKY: Right. But the reliability, what we're talking about is not -- we're not requiring anybody who even wants to apply to backfit or to replace or to upgrade a digital system to do this. Rather, they can go right now with the existing deterministic framework and make that change.

What we are just saying is if the licensee would like to make a risk-informed argument as to why they are replacing system X with system, digital system, Y, then we would ultimately have to review that argument. So we are trying to get in place the guidance that we would give to the person in NRR or NRO, whoever has to go ahead and do that review. So that is where the goal is of this work.

We certainly agree with everything you said about improving the reliability of the systems and making them simpler. It's just that that is not the goal that we're focusing on right now. That is not our charter, so to speak.

We're trying to lay the framework and the groundwork for someone if they did want to come in with the risk-informed argument. That's optional.

MEMBER APOSTOLAKIS: Any other comments from anyone?

(No response.)

MEMBER APOSTOLAKIS: Well, thank you very much, gentlemen. Back to you, Mr. Chairman.

CHAIRMAN SHACK: Okay. I think that you're on schedule. Take a 15-minute break, gentlemen. No more transcript.

(Whereupon, the foregoing matter was concluded at 3:31 p.m.)