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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

493rd MEETING

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THURSDAY, JUNE 6, 2002

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS PRESENT:

GEORGE E. APOSTOLAKIS	Chairman
MARIO V. BONACA	Vice Chairman
F. PETER FORD	Member
GRAHAM M. LEITCH	Member
DANA A. POWERS	Member
VICTOR H. RANSOM	Member
STEPHEN L. ROSEN	Member
WILLIAM J. SHACK	Member
JOHN D. SIEBER	Member
GRAHAM B. WALLIS	Member

ACRS STAFF PRESENT:

JOHN T. LARKINS	Executive Director
SHER BAHADUR	Associate Director
SAM DURAISWAMY	Technical Assistant
TIMOTHY KOBETZ	Cognizant Engineer
HOWARD J. LARSON	Special Assistant
MAGGALEAN W. WESTON	Staff Engineer

NRC STAFF PRESENT:

PATRICK BARANOWSKY	NRR
BILL BASEMAN	NRR
STEVE BLOUR	NRR
THOMAS BOYCE	NRR
ART BUSLIK	NRR
JOSE CALVO	NRR
CYNTHIA CARPENTER	NRR
KEN CHANG	NRR
STEPHANIE COFFIN	NRR
MARY DROUIN	NRR
RON FRAHM	NRR
MIKE FRANOVICH	NRR
D.E. HICKMAN	NRR
ALLEN HISER	NRR
MICHAEL JOHNSON	NRR
IAN JUNG	NRR
PETER KANS	NRR

NRC STAFF PRESENT:

P.T. KUN	NRR
ANDREA LEE	NRR
SAM LEE	NRR
W. LIU	NRR
TONY MARKLEY	NRR
MICHAEL MARSHALL	NRR
MIKE MAYFIELD	NRR
SCOTT NEWBERRY	NRR
ALLEN NOTAFRANCESCO	NRR
BOB PALLA	NRR
RICHARD PUDLEY	NRR
JACK ROSENTHAL	NRR
MARK RUBIN	NRR
MARK SATURIUS	NRR
PAUL SHAMANSKI	NRR
MIKE SNODDERLY	NRR
DWIGHT SNOWBERGER	NRR
ASHOK THADANI	NRR
JOHN THOMPSON	NRR
KEITH WICHMAN	NRR
CHARLES ADER	RES
SATISH AGGARWAL	RES
NILESH CHOKSHI	RES
FAROUK ELTAWILA	RES

NRC STAFF PRESENT:

SIDNEY FELD	RES
ED HACKETT	RES
HOSSEIN HAMZEHEE	RES
ASIMIOS MALLIAKOS	RES
JOHN RIDGEBY	RES
ALAN RUBIN	RES
HAROLD VANDERMOLN	RES
JACK GROBE	NAC/Riii
ALAN LEVIN	OCM
SUSAN UTTAL	OGC

ALSO PRESENT:

MIKE BARRETT
CHARLES BRINKMAN
BOB BRYAN
KURT COZENS
DAVID DELLANO
STEVE EIDY
STEPHEN FYFITCH
PAUL GUNTER
ANN HARRIS
TOM HENRY
JOHN HINKLING
PHIL HOLZMAN
BILL HORIN

ALSO PRESENT:

DANIEL HORNER
TOM HOUGHTON
STEVE HUNT
ROGER HUSTON
DICK LABOTT
JOHN LEHNER
DAVID LOCKBAUM
STEVE LOEHLEIN
ALEX MARLION
PATRICK McCLOSKEY
MARK McLAUGHLIN
JIM MEYER
JIM POWERS
DEANN RALEIGH
PETE RICCARDELLA
JACK ROE
JOHN RYCYNA
ROBERT SCHRAUDER
KEVEIN SPENCER
TUNG TSE TSENG
BOB YOUNGBLOOD

I N D E X

P R O C E E D I N G S

8:31 a.m.

CHAIRMAN APOSTOLAKIS: The meeting will now come to order. This is the first day of the 493rd meeting of the Advisory Committee on Reactor Safeguards. During today's meeting, the Committee will consider the following: CRDM Cracking of Vessel Head Penetrations and Vessel Head Degradation; Technical Assessment Generic Safety Issue (GSI)-189, "Susceptibility of Ice Condenser and Mark III Containments to Early Failure from Hydrogen Combustion During a Severe Accident"; Technical Assessment of GSI-168, Environmental Chylifaction of Low-Voltage Instrumentation and Control Cables; Development of Reliability/Availability Performance Indicators and Industry Trends; Technical and Policy Issues Related to Advanced Reactors; and Proposed ACRS Reports.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Mr. John T. Larkins is a designated federal official for the initial portion of the meeting.

We have received no written comments from members of the public regarding today's sessions. We have received requests from Ms. Ann Harris, a member of the public, and David Lockbaum, Union of Concern Scientist for time to make oral statements regarding GSI-189.

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

I don't have any special comments. Do any of you Members want to say anything before we start?

MR. LARKINS: Mr. Chairman?

CHAIRMAN APOSTOLAKIS: Yes.

MR. LARKINS: I think we also received a letter from Mr. Ken Bergeron regarding GSI.

CHAIRMAN APOSTOLAKIS: Yes.

MR. LARKINS: 189.

CHAIRMAN APOSTOLAKIS: Yes, we did.

MR. LARKINS: Which we will enter into the record.

MEMBER KRESS: And I understand Mr. Lockbaum will speak to that letter.

CHAIRMAN APOSTOLAKIS: Yes. The first item on the agenda is the CRDM Cracking of Vessel Head Penetrations and Vessel Head Degradation. The cognizant member is Dr. Ford. Please.

MEMBER FORD: Thank you. The Metallurgy and Plant Operations Subcommittees had an extended meeting being briefed on the CDRM housing cracking and pressure vessel head degradation issues. We purposefully did not dwell on safety culture and reactor oversight process issues since these are being dealt with separately.

All the ACRS Members, apart from Dr. Powers, were present at the Subcommittee meeting. The staff have requested a letter from us, commenting on the technical aspects of these degradation programs.

I'd like to proceed with the first presentation by Jim Powers, I understand from FENOC.

MR. POWERS: Good morning. I'm Jim Powers, the Director of Engineering for First Energy at the Davis-Besse Nuclear Plant and we're going to review the -- briefly, the presentation that we did yesterday to the Subcommittee and I brought with me once again Mark McLaughlin, who is our field team lead for work on the reactor head at Davis-Besse; Bob Schrauder who is the Director of Life Cycle Management for First Energy. He's responsible for the procuring and installing a replacement head from the Midland Plant which is now our preferred approach to recovering the head at Davis-Besse. And Steve

Loehlein will talk briefly on the root cause, any updates and questions there may be on that. So Mark, why don't you go ahead.

MR. McLAUGHLIN: All right, thank you, Jim. Since you all have seen these pictures, I will be brief. Next slide, please.

(Slide change.)

MR. McLAUGHLIN: Keep on going. Next one. Okay, this first picture is abrasive water jet cutting machine that we used. This particular picture is on a one to the mockups. We did mockup this process twice prior to performing it on the reactor pressure vessel head at Davis-Besse.

Next slide.

(Slide change.)

MR. McLAUGHLIN: This next picture is a picture of the cutout on the actual head at Davis-Besse.

Next slide.

(Slide change.)

MR. McLAUGHLIN: This is a picture underneath the head at Davis-Besse using a remote camera and it's the same cutout.

Next slide, please.

(Slide change.)

MR. McLAUGHLIN: This is a picture of the cavity that has been removed and I'll talk about on the next slide. We had three phases of samples that we're going to do analyses for. Phase 1 was boron samples from various location son the head. Those -- we do have a draft report with the results of those samples. Just briefly, we did five boron, iron and lithium which is to be expected, as well as nickel and chromium in those samples.

Phase 2 samples --

MEMBER SHACK: Excuse me. You're looking at analysis techniques that will tell you more than just the chemical composition. We're going to know the actual bores?

MR. McLAUGHLIN: That's correct, yes. We do have -- they had the forms.

MEMBER SHACK: Right, you're not a mineralogy, so --

MR. McLAUGHLIN: That's correct.

MEMBER SHACK: That's not your concern, but that information will be available?

MR. McLAUGHLIN: Yes, it will. We would expect to have that report issued to the staff within the next two weeks.

Phase 2 will be essentially the same type of analysis. The Phase 2 has the samples that were taken when we removed nozzle number 2, so there should be some boron from the annular space and should hopefully that will help us with some of the chemistry questions that we have in the annular space.

And then Phase 3 is the actual nozzles 2 and 3 that were removed as well as the cavity and we're working with the staff on determining exactly which tests to perform on that. Right now, all three of these samples are in Lynchburg, Virginia and we have meetings scheduled within the next two weeks with the staff to go down there and discuss what type of analysis because the next step will be -- will require some destruction of the samples.

MEMBER WALLIS: It seems to me that there's a lot of clue in the shape of the cavity as to what happened. I hope you're really careful to get all the information you possibly can out of it before it is destroyed or turned into something else.

MR. McLAUGHLIN: What we're doing is we're going to take extensive photographs of the cavity in its present condition, as well as take a lot of measurements so we can gain as much information prior to doing any destruction of the sample.

MEMBER WALLIS: I would suggest that

people some hypotheses before they start doing this so they know what they're looking for, so they know what's required in order to verify or challenge the hypotheses.

MR. LOEHLEIN: Yes, we in root cause have been advising from several months ago what sorts of things we were looking for that might give us evidence of different types of mechanisms, whether they be flow induced, impingement, corrosion, what have you.

In this cavity, we were unable to in situ take any kind of impression like we were able to do at Nozzle 2. There are areas, a lot to do yet --

MEMBER WALLIS: You can take impressions of that.

MR. LOEHLEIN: We couldn't while it was on the head.

MEMBER WALLIS: You can now though.

MR. LOEHLEIN: Now we can do a lot of things and Tod Plune is back at the site that's working on the lead as far as what we do with these samples.

MEMBER WALLIS: Okay.

MR. McLAUGHLIN: Yes, we also have a person who will be down there in Lynchburg with us, with the staff is Mr. Steve Fyritch. He's on the Root

Cause Team for the Davis-Besse Root Cause. So we're keeping the root cause personnel tied into this process.

And this picture is a picture of the actual cavity. You can see into the underhung area after it was removed. And then the last picture shows the side view of the sample that was removed. You can see the J-groove weld around Nozzle 11 and the last time we were here there was some discussion about maybe a possible detachment or corrosion between the stainless steel liner and the base material. We did perform a visual inspection. We can't do any dye penetrant because the surface is too rough to do that and there was no evidence of any cladding detachment.

That's all I have. If there's any questions -- all right. I'd like to turn it over to Bob Schraider who is the Director of Life Cycle Management for First Energy Nuclear Operating Company. And he's the senior person in charge of head replacement.

MR. SCHRAUDER: Good morning. As Mark and Jim indicated, while we went down the repair path, I in parallel was looking at the ability to procure, transport and install a replacement reactor vessel head at Davis-Besse.

Our search included looking at accelerating a schedule for manufacture of a brand new head for Davis-Besse and also looking at existing heads in the industry.

We were unable to significantly accelerate the schedule for our new head which is scheduled to be delivered during the first quarter of 2004. We did find two compatible heads with Davis-Besse existing in the industry. One was at a checkdown plant in California, the Rancho Secho Plant. The other was the unfinished plant up in Midland, Michigan which was also a Babcock & Wilcox design. We quickly narrowed our view down and decided to purchase the Midland head. It had several advantages to us. It was very close to us, one state away and it was not contaminated, so any work that we had to do on it and transportation was significantly easier with an uncontaminated head than it was a contaminated one.

I'll talk a little bit about the similarities on this head to the Davis-Besse design. It was fabricated by Babcock and Wilcox to the same code and addenda as the Davis-Besse reactor vessel head was. We have records on this head, indicating that it was accepted by Consumers Power. And it was signed off by an authorized nuclear inspector as an acceptable ASME component.

We also have records indicating that this head was hydrostatically tested prior to its shipment to the Midland site.

Now our approach to procuring this -- well, one thing I should say is that the Midland Plant was canceled back in the 1980s. Since that time this reactor vessel head has been sitting on the head stand within the containment at the Midland site.

We chose Framatome to work with us because of their expertise, technical expertise and their access to the records on this head. They actually purchased a head from Consumers for us as a basic component. They're compiling the code data package or pulling that out of the records, compiling it for us and they will disposition any nonconformances due to the storage of that head in the containment.

They will also reconcile the Midland head for the design at Midland to the design at Davis-Besse and I'll show those design requirements in just a minute and of course they do have a quality assurance program there at Framatome and they will be doing this in accordance with their quality assurance program, including Part 21 reporting on requirements. Then they will sell that head to First Energy as the component, basic component.

The next slide shows that the material of construction and this head is virtually identical to that of the Davis-Besse design, even that material for the closure head flanges, in fact, the same material has all the same material properties. The design, you see, this head and vessel was designed to the same pressure and temperature as the Davis-Besse design requirement.

We did take a look at the nozzles on this head and the material of those nozzles. They are the same nozzle material as the Davis-Besse with a different heat number and those two heat numbers are identified on this slide. All but one are from a single heat. Neither of these two heats has any industry experience. Their qualities and their yield stress we have found to be in the middle of the range of the heats that have some industry experience.

And of course, the alignment of the control rods is the same on this head as it was for the Davis-Besse design.

This picture shows what's known as the key-way. There are four of these key-ways on the head that precisely align this head to your vessel and each is somewhat custom fit to the vessel. They are in nearly the same position but the times are mils off. There are eight surfaces on these four key-ways, the inner and the outer. Four of those eight surfaces needed to have some slight machining to precisely fit this head to the Davis-Besse head. And the control rod drive mechanism flange indexing, where the control rod drive mechanism comes on to the nozzle has an indexing pin for proper alignment and there are two locations that you can align from on this. The Davis-Besse design is on the opposite one that Midland was set up for and therefore those indexing holes, there's a plug that needs to be taken out of the existing hole on the Midland head and moved to the other side so that we have the proper indexing location for our control rods.

MEMBER KRESS: Is the plug welded in?

MR. SCHRAUDER: No, it's not.

MEMBER KRESS: Just forced in?

MR. SCHRAUDER: That's correct. The other difference on this head is the O-ring design. The O-ring has the groove in the O-ring itself is slightly smaller on the Midland head and that is consistent with the rest of the head, the Davis-Besse had somewhat of a unique difference. We had a .5 inch small diameter in our O-ring. We have analytically shown that the smaller O-ring will seal effectively in the groove in our vessel and of course, we'll test that as we bring this vessel and head up to pressure.

We will manufacture and install new O-rings on to the Midland head.

MEMBER KRESS: How did you assure yourself that the O-rings would seal sufficiently?

MR. SCHRAUDER: We have the precise dimensions of the location of the grooves on the Midland --

MEMBER KRESS: Was it dimensional?

MR. SCHRAUDER: That's correct. And there is a leak off system between those seals that we'll be able to verify that the seals -- we see no problem. We have very good crush on --

MEMBER KRESS: Are those the same seals that were leaking in the regional vessel?

MR. SCHRAUDER: No, those seals, I believe were the control rod drive mechanism.

MEMBER KRESS: That's not the seals you're talking about?

MR. SCHRAUDER: No, this is the head to vessel flange seating surface.

MEMBER KRESS: Okay.

MR. McLAUGHLIN: As a matter of fact, if you want to --

MEMBER KRESS: It would be right here.

MR. McLAUGHLIN: Right here, the O-ring grooves are here.

MEMBER KRESS: That's a big O-ring that goes all the way around?

MR. McLAUGHLIN: That's correct, a set of two of them.

MR. SCHRAUDER: And the gaskets you were talking about are up here.

MEMBER WALLIS: Do those O-rings move once the system is pressurized?

MR. McLAUGHLIN: I suppose they could a little bit. There's clips that hold the O-rings in place. However, the clips are slotted.

MEMBER WALLIS: You're essentially relying on the crush to hold them in place?

MR. McLAUGHLIN: Correct.

MEMBER WALLIS: And that seals -- they're not supposed to move the way the rubber ones do.

MEMBER SIEBER: Not from side to side, but when you pressurize the vessel, it moves a little bit. There's tension in the studs. The compression of the O-ring reduces slightly.

MR. SCHRAUDER: This next pictorial, if you will, is useful in looking at the next few slides that I'll discuss the examinations that we'll do on this head to verify its suitability for use at Davis-Besse.

We're doing three different sets of examinations. One is to supplement the Code Data Package. One is our pre-service inspections and another is just additional, nondestructive exams that we'll do to verify that there's been no deleterious effects due to this long-term storage that this had at the Midland containment.

You see to supplement the code data package we'll be doing visual examinations, looking for any obvious signs and in particularly looking to verify that there are no arc strikes on the head which may indicate unauthorized welding on the head.

We're going to radiograph and actually we've already completed the radiograph of the flange to dome weld. This head, like the Davis-Besse head was forged in two pieces, the dome and then the flange and then there's a large weld on that. We've completed a radiograph on that weld and they've shown it to be a good weld.

We got about a 96 percent coverage due to the lifting lugs that prevented 100 percent radiography on that. We do, however, have records that indicate that there was 100 percent radiograph successfully done on that head in the past.

We do intend to do a radiograph on all the nozzle to flange welds for the control rod drive connection and then we will do a dye penetrant exam of the J-groove welds on the nozzles underneath the vessel.

The pre-service inspections are shown on the next page, the magnetic particle again on the flange to dome weld. We'll do an ultra sonic on that same weld and then we'll do a liquid penetrant exam of the peripheral control rod drive mechanism, nozzle to flange, and that is required by code and we will meet the code on that. Our expectation, our intent is that we will actually get to all of those nozzle to flange welds. We believe we had adequate access --

MEMBER WALLIS: So now we have some theory about the rate of crack growth, you have some idea about how big a crack you need to detect, then you CRDM nozzle and its environment, in order to predict what will happen, say in the next 10 years?

MR. McLAUGHLIN: The next slide, I think we'll describe what we're going to do.

MEMBER WALLIS: I just want to be sure that what you're doing here is going to detect what you need to detect in order to predict what's going to happen, let's say during 20 years or whatever. I didn't ask you that yesterday, but it occurred to me you can match -- that the kind of techniques you're using here on the precision to what you need to know. I didn't ask that, but I'd like to some assurance that you've done that.

MR. McLAUGHLIN: Okay.

MR. SCHRAUDER: The non-destructive exams, the additional exams that we'll do, many of these are to get that base line and to fully understand what -- if there are any existing flaws or cracks.

MEMBER WALLIS: Well, you can't detect below a certain size.

MR. McLAUGHLIN: What we're doing is we're going to do the eddie current of the inside diameter of the nozzles, so that we can detect any surface flaws so that would be a crack initiation spot and then we're also going to do the ultra sonic examination to make sure there are no cracks present.

MEMBER WALLIS: No cracks.

MR. POWERS: To make sure we understand any indications.

MEMBER WALLIS: Well, you never detect nothing. You detect up to above a certain size and I just wondered if that precision is good enough. This isn't my field, so someone else should be asking it.

MR. POWERS: This is the same equipment we're going to be using for the in-service inspection. So this will be a baseline of --

MEMBER WALLIS: Yes.

MR. POWERS: The condition of the nozzles.

MR. McLAUGHLIN: Our expectation --

MEMBER WALLIS: I guess you didn't give me a quantitative answer though.

MR. POWERS: Steve Fyfitch, would you please?

MR. FYFITCH: Steve Fyfitch for Framatone. It's not my field either. I'm not a UT, eddy current specialist. But if memory is correct, the eddy current can see a flaw in the surface that's approximately 2 mils in depth and the UT can see something a little bit larger than that.

MEMBER WALLIS: And within how many years would that be expected to grow to a point where you worry about it?

MR. FYFITCH: If you go by industry experience, we've had vessels in-service, so we've done eddie current inspections on, that have been in service for 20 years and we haven't seen indications on some of those.

MEMBER WALLIS: I was thinking of using all those wonder DADTs we saw yesterday.

MR. FYFITCH: Well, that's -- you know --

MEMBER WALLIS: Maybe we can ask the DADT father there.

MR. FYFITCH: The cracked growth curves, yes.

Do you have anything to say on that, John?

MR. HICKLING: John Hickling, EPRI. As I pointed out yesterday, the DADT curves have been evaluated or derived to evaluate relatively large flaws in their further growth. The industry experience of stress corrosion cracking is that the initial phases of growth are very small flaws or defects is very, very slow indeed and takes up the large majority of life. So it's difficult to make a quantitative prediction in that area because the DADT curves do not apply to those very slow early stages of growth.

MEMBER WALLIS: So it's a qualitative judgment, really.

Thank you.

MR. SCHRAUDER: Let me -- I probably should have said this earlier. Let me state that our intent with this head is not that it will be a permanent replacement, but rather we intend to put this head in now and we are continuing with the procurement of our new head with the new material and our expectation is that we'll install that head on our vessel around the Year 2010 or 2012 when we replace our steam generator. So this vessel will be, or this head will be in service for 8 to 10 years. And I believe that is not very many thru-wall cracks, certainly have identified themselves within that time period.

MEMBER WALLIS: You might have to face this question if you actually started detecting cracks in this Midland head.

MR. SCHRAUDER: Yes sir.

MEMBER KRESS: Why not keep it permanently?

MR. SCHRAUDER: Say again, sir?

MEMBER KRESS: Why not keep the head permanently?

MR. SCHRAUDER: We think that the new material in the new head would be a better option for us and the inspections and the exposure from the inspections on this would still make it a better choice to replace the head with the new material.

This head, as I said, is within the containment at Midland. And that head will not fit for the equipment hatch at Midland, nor will it fit within the equipment hatch at the Davis-Besse plant, so both of those containment structures will need to be temporarily opened and then restored in order to get the heads in and out.

MEMBER SHACK: Will you be left with an equipment hatch so you could bring the next new head through?

MR. SCHRAUDER: No, we will not. The design and the time required to put a new equipment hatch in it's really quite significant. So we'll evaluate when we put the steam generators in whether we want to add a larger hatch at that time, but we're not doing it for this. We'll restore the containment as we find it now.

MEMBER RANSOM: Is the Midland containment going to be restored?

MR. SCHRAUDER: The Midland containment will not be restored to nuclear design. It will be restored for basically weather protection and that's in accordance with consumers' desires.

We will prepare our head for moving outside of the containment also and we'll take the necessary radiological controls to temporarily store that head at the site. Our intent at this time, if it categorizes this low-level waste, we would like to dispose of it now rather than use permanent storage at the Davis-Besse site.

We are going to transfer our service structure and work platform from our existing head to this head. We are doing the modification on the lower portion of the skirt on the Midland head which will remain and we're putting in the inspection ports there to make it accessible for inspection and any cleaning that might be necessary.

We are re-using as I said earlier, I believe, the control rod drive mechanisms from the Davis-Besse head on this head also. As we did look to the repair and had to cut out a couple of the nozzles on the old head, we had to redesign our control rod locations. We will revert back to the original control rod configuration for this new head.

And we'll do a couple of really serviceability modifications to this to the split nut rings to make them easier to get on and off as we go into outages. We also are putting the upgraded gasket design onto these nozzles as we had the Davis-Besse head.

And that's all I have on the head replacement, unless there are additional questions.

MEMBER LEITCH: When you go back in service will you have modified the so-called mouse holes, if that's the right terminology, to improve --

MR. SCHRAUDER: That's what I was referring to. We don't actually modify the mouse holes. The new inspection ports go up a little bit higher than those, but they will have the larger inspection ports.

MEMBER LEITCH: Okay, so that's what that bullet refers to?

MR. SCHRAUDER: Yes sir.

MEMBER LEITCH: Thank you.

MR. SCHRAUDER: Okay, with that, I'll turn it over to Steve Loehlein who has the lead on our root cause investigation team.

MR. LOEHLEIN: All right, the root cause report has been an issue as of about 7 weeks ago and I understand the ACRS members are familiar with it, so we have a brief slides here in the way of summary. I ask that we move ahead to the conclusions as a means of remembrance here.

The key conclusions that we had out of our root cause investigation were that the degradation to the Davis-Besse reactor head was caused initially by primary water stress corrosion cracking which led to nozzle leaks which were undetected which then allowed boric acid corrosion to occur over an extended period of time.

We also concluded that the existing guides and knowledge was adequate to have prevented this damage from occurring.

We also included in today's presentation the time line, just in case Members have questions.

MEMBER FORD: Just for the record, I want to be sure that we understand that we knew physically what occurred, but we don't know in terms of predictions since the specific mechanisms and thereby we cannot tell whether this is, in fact, just a leader of the fleet or that it really is an isolated occurrence. For instance, we don't know the specific mechanism by which you can get 1-inch per year. You don't know the specific design operational criteria that would give you that in any, not just Davis-Besse, but in any reactor of this particular design.

Do you agree?

MR. LOEHLEIN: I think what the report

clearly shows is that there's a lot of evidence that substantiates that the corrosion took at least four years in that area, four to six, that even over that period of time it is still a significant corrosion rate for the cavity size that's there.

We also determined through comparison of testing that's been done historically that under the right conditions, rates like that can be created, but I think what you're saying is a question in which we do not have data for is what does it take to get to that point where that type of rate gets established and in this particular degradation issue here, Davis-Besse, we don't have any new evidence that tells us anything more about that. All we know is what we see there and the evidence we do have available is consistent with what we wrote in the report is that if you have a small crack and things go undetected that can go into a leak which through some slow corrosion mechanisms slowly open up the annulus and once there is the ability for communication of air, oxygen with just the right amount of moisture available to keep local temperatures low, these high corrosion rates then become possible.

MEMBER FORD: Again, for the record, it's our understanding that the MRP is considering the conditions that need to be evaluated and then we'll evaluate those conditions which will give us the prediction capability for this particular degradation mechanism.

MR. LOEHLEIN: I hate to speak for them. I can tell you we're working with them and the work that I've seen is in line with what you're expecting.

MEMBER FORD: I just hate to think that this root cause analysis, this document is the end of this whole process. It is not.

MR. LOEHLEIN: And of course, from our perspective and what we had available to us in terms of evidence at the time, there's only so many conclusions that we can draw in looking back from the 1996 to 1998 time frame. We really don't have evidence to look prior to that and draw conclusions from it. You have to use the existing industry body of knowledge to predict what happened prior to that. So all I can say is we uncovered no evidence of anything new. What we don't have, probably, and many people feel we should have a better understanding of these early stages than we have had up until now.

MEMBER FORD: Okay, but you can't say, for instance, you can't disprove a hypothesis that the cavity grew slowly and then grew maybe at 4 inches a year in its final year.

MR. LOEHLEIN: As a matter of fact, that's a good point. It's the reason why we said as a bounding assumption that if you look at the other industry data, a rate is highest at the end with what we would consider to be a bounding assumption, would have been 4 inches which of course means that we would consider that to be kind of a linear assumption than it was maybe one inch per year in 1998.

MEMBER FORD: Right. The one inch a year, taking the one inch a year as being what's going to happen, in another situation, there could be another event where the hole actually closed faster at some stage.

MR. LOEHLEIN: What we can say is that what happened at nozzle 3 in the physical evidence that we have, it appears as though that cavity grew at newly ideal conditions. The right balance of a leak rate with forecast and availability. In actuality, if you have leak rates lower and probably significantly higher, the corrosion rates, we expect would be lower. One case you don't have enough moisture to get the ideal conditions and in the other, you get enough moisture that you get a dilution effect and you don't have as high a concentration of boric acid.

So the combination of a situation where a cavity region was growing at the top of the head, where the boric acid had accumulated could remain there to be constantly available for concentrating mechanism, all these things that build a case that this was a nearly ideal corrosion --

MEMBER FORD: For making a cavity. Now if you have a big leak, you might make a canyon rather than a cavity, it seems to me. That's the flow going down the head.

MR. LOEHLEIN: There's a lot of things that could be speculated as to what would happen in a higher flow rate. Certainly, higher flow rates would show up more readily on RCS than identified leakage as well, probably other things, maybe containment, humidity and so forth.

I guess lots of variations could be conceptualized.

MEMBER FORD: Could you comment on the nondestructive testing techniques that could be used which would be able to size the amount of this degradation, this particular degradation phenomenon?

MR. LOEHLEIN: Do you mean in terms of how large the cavity --

MEMBER FORD: We're hearing that we will

be talking about managing all of these degradation issues in terms of visual inspection as appropriate. But what is the capability of nondestructive testing as used in the plant to size a corrosion?

MR. McLAUGHLIN: I'll talk to that.

MR. LOEHLEIN: Yes, I'm no expert in that area.

MR. McLAUGHLIN: What we found is if you look at the ultra sonic testing results and I believe we presented those to you guys the last time we were here, you could see on both nozzles 2 and 3 a couple of clues that something was going on. One, you could see where a normal plot of ultra sonic data, you can see the top of the head. And the location of both of these cavities, you could not see the top of the head. You could also see a location that was obvious that there was no contact between the outside diameter of the nozzle material and any base material. You could see that on the ultra sonic. Now the ultra sonics will not tell you the depth, so you don't know whether it's two mils or six inches. But we did have a clue that something was going on and that's why in our repair process we chose to repair nozzles 2 and 3 first because we did feel that there was some anomaly.

The other thing I would say that from the inspections that we did on say nozzle 2, I believe that you would pick up the area on top of the head, so if you're doing a visual inspection and you had the cameras that we're using now, that you would see that area of corrosion on top of the head. So from a visual standpoint, I believe you would see it. Definitely from an ultra sonics will pick that up.

MEMBER FORD: But it would be by inference in terms of the sizing capability, looking at the top of the head and the amount of boric acid you see on the head, top of the head, it will be by inference?

MR. McLAUGHLIN: That's correct.

MEMBER FORD: If you've got a problem, it would tell you nothing at all, of any of your inspection, kinds of inspection, nondestructive inspection techniques, any way of sizing the amount of that degradation.

MR. McLAUGHLIN: That's correct. I think that you have to have both. You have to use the ultra sonics as well as the visual, if you want to get the size of any type of corroded area.

MEMBER SHACK: Your through the vessel wall for sonic measurement, was that able to size that the minor degradation that you saw at nozzle 2?

MR. McLAUGHLIN: No, what happens is the

J-groove weld comes down and you can't do ultra sonics from underneath the head going up.

MEMBER SHACK: That would almost set a limit. If it was any deeper than say one inch or something then I would see it with the through-wall.

MR. McLAUGHLIN: That's correct. You could pick it up then and we did do some ultra sonic tests.

MEMBER SHACK: So that would sort of set a minimize size of a cavity I could detect with the through-wall ultra sonic if I had a shadow on the through nozzle ultra sonic that I wanted to see how big the cavity was behind it, I could say if I didn't see anything on the through-wall it would be less than one inch or something like that.

MR. McLAUGHLIN: That's correct.

MR. SCHRAUDER: But Mark, I think the other thing, maybe it's not noticed here, is that when you have through-wall leak and all the evidence of that and the UTs that show where the cracks are, in the repair process of grinding those out, you automatically expose the area and as a matter of fact, that's how we knew that there was a small cavity region, also two, pretty early, as I understand it because of that, we machined that out. Or is that not true?

MR. McLAUGHLIN: That's true. I mean when you machine the bottom of the nozzle, you specifically machine up above any cracks that are there so you can get all the cracks out and the corroded area should start either at or just above. I think we saw it started just above the cracks, so you know, I would expect during the repair process you would discover that, but --

MR. SCHRAUDER: One thing is clear. The boric acid deposits that appear on the head by the time even at that stage, where it's only 3/8ths inches deep, there is a significant amount of boric acid that's going to escape and it's going to have some rust colorization with it as well. That's consistent with what EPRI saw in its test of an annular. Once you have corrosion by products, they'll be evident in what's expelled out of the annulus.

I think in our figure we have in the root cause report, the cavity region does extend to the top of the head.

MEMBER FORD: Thank you. Unless there's any other --

MEMBER SIEBER: One quick question. On your bar chart of unidentified leakage there, if I look at that through about the second quarter of 1998, leakage was pretty low.

MR. SCHRAUDER: Right.

MEMBER SIEBER: Then you developed a pressurized relief valve leak and it looks like you shut down, repaired that, started it up again, but leakage was now up. Have you drawn any conclusion as to what that additional leakage, after 1999, said quarter, was?

MR. SCHRAUDER: Certainly. At this time we believe that some of it was due to the development of the leakage at nozzle 3. But as it is with unidentified leakage rates, since this leakage that was ultimately repaired went on for some months, that masking and then that loss of time frame, the staff -- the site staff wasn't able to determine the source of the changes and of course, they could have been attributed to other possible leak sources and there were attempts to look for them, but they never found them.

MEMBER SIEBER: Okay, thank you.

VICE CHAIRMAN BONACA: Just one comment I have. Although the problem may have developed in the last four years, in looking at the root cause, I think you have to look before. Root cause does that. It goes with the early 1990s because although by 1996 you had all the flanges were not leaking any more, but there was a certain mindset in the people from previous outages that you have leakage from the flanges and you can live with it and I think the mindset, it's important to understand. I understand the code allows for leakage to occur from those flanges to some degree. And the question then has to be also is the code proper or adequate because I mean clearly there is a history, if I look at the root cause, it covers about 12 years, that in which there's a certain mentality there that may not be unique to Davis-Besse.

MR. McLAUGHLIN: What you're saying is is from a management standpoint back in the early 1990s with some of the decisions that we made, we set the standard at Davis-Besse before that.

VICE CHAIRMAN BONACA: Right. And I don't want to speculate. I'm not part of the root cause, but I think it's important to see this ingrained thinking because I think it's associated with an interprotectional code and it could be further than simply Davis-Besse.

MR. POWERS: And that's a good point and this is a picture of the technical aspects of the problem that we're resolving at Davis-Besse, but there are larger issues on how this was allowed to occur in the areas of decision making, ownership, oversight standards is where we're driving to resolve the bigger issues in the organizational performance. They got us here, we'll be working with that under the 350 inspection manual chapter process as part of the plant recovery sets of major activities that will be discussed elsewhere.

MEMBER FORD: I'd like to move on at this stage unless there are any other questions for this particular team.

Thank you very much and we appreciate it.

We'd like to move on to presentations by the MRP, Larry Matthews.

MR. MATTHEWS: I'm Larry Matthews. I work for Southern Nuclear and I'm the chairman of the Alloy 600 Issues Task Group of the Materials Reliability Program.

MEMBER KRESS: Those were cedar shakes on that roof.

MEMBER FORD: That's your house, Tom.

MEMBER KRESS: Yeah, that's my house.

(Laughter.)

MR. MATTHEWS: We had quite extensive

presentations yesterday with a lot of data and what I propose to do today is try and quickly go through some of the summary conclusions information.

First thing we did was introduce -- not really introduce, but reorient our thinking on how we categorize plants and rank plants to something called effective degradation years where we don't use a reference of some significant degradation like Oconee 3, but we just measure effective degradation years for each plant, which is the same thing as the effective full power years normalized to 600. And this is just a simple chart that shows the ranking of the units and their inspection results to date as a function of where they were in effective degradation years.

The date of the EDY, if you will, was a year ago. We're going to update these to the exact effective degradation time at the time they did the inspections.

(Slide change.)

MR. MATTHEWS: Then John Hickling got up and gave a significant discussion where the expert panel was on coming up with recommended crack growth rate curve. If you recall, the expert panel had narrowed the data base down to 26 heats of material from lots of material suppliers and product forms with the number of data points for each heat ranging from 1 to I guess to 32 for one heat. The method used was to assume a shape of the curve versus stress intensity factor and then to normalize the magnitude of the crack growth rate for each heat to the best fit to that heat data. That's the numbers in the column here. And then plot those and sort those and plot those and fit that with a log normal distribution.

The recommended crack growth curve we've come up with is one based on the parameter that go through the 75th percentile of the heat data.

(Slide change.)

MR. MATTHEWS: This is the data base, all the 158 data points that we have and the dark curve is the 75th percentile of the heat data. If you go back one, basically each one of these points on this curve could be represented as a curve parallel to the MRP curve or the Scott curve on this curve, plot, and then the black MRP curve would indeed be above 75 percent of all those family of curves.

(Slide change.)

MR. MATTHEWS: The application of this recommended curve is intended for the disposition of PWSCC flaws that are detected in the field in thick-walled Alloy 600 components. We don't disposition. We repair through-wall flaws, so we're talking about flaws that are axial ID flaws that are shallow or flaws that may be detected below the J-groove welds.

This crack growth rate curve would be used to determine the crack growth between time of detection and the next inspection interval to decide if it's okay to run for one more cycle or one more operating internal before that flaw is repaired or inspected again or not. And if it's not, then it would have to be repaired at that point in time.

The last two bullets, John pointed out yesterday, were that there's essentially very little or no data on our data base below, approximately 15 megapascals root meter, but for all practical purposes by the time a crack is detected the K would be above that value. So it doesn't really effect the actual use of the curve.

(Slide change.)

MR. MATTHEWS: Then we had Dr. Pete Riccardella, got up and made his presentation on the probabilistic fracture mechanics analysis that's being performed by his company for the MRP. The point in this is to try and determine the risk of rod ejection as a function of time for the units and for the fleet.

A model is being constructed and using that model, if we go to the time that Oconee 3 detected their first large leak, they were at approximately 20.1 effective full power years. That would translate to slightly over 21 effective degradation years.

The prediction at the top line is what is the probability they would have detected their first leak at that point and it's over 90 percent. The thick line at the bottom is what is the probability they would have one large Circ. flaw and that's about 12 percent, if you look at this for the B & W fleet, that's close to how many what the fraction of the plants that have detected large Circ. flaws and then the probability of net section collapse is fairly small still, but net section collapse being equivalent to a rod or nozzle ejection.

This model then was used to help us construct a technical basis for the proposed inspection plan that we had come up with. We analyzed plants at various head temperatures and the model hasn't been fully constructed at this point for CE and Westinghouse design, so all this work was basically done with a Westinghouse -- I mean with the B & W geometry but at different head temperatures.

Then we set the risk categories based on the probability of net section collapse per year and also based on accumulative leakage probability. We used both of those and you'll see in the next slide or two that they pretty much parallel each other.

And then set the inspection intervals based on the effect of various inspections on the probability of net section collapse.

(Slide change.)

MR. MATTHEWS: This is a little bit different way of plotting it, but I think it's instructive. The horizontal axis is simply that each individual plant's current head temperature of left axis is the equivalent effective full power years, not degradation years, but effective full power years, normalized to their current head temperature. And for many plants, their current temperature is the temperature they've had for the life of their plant, but there are a few that made modifications to their internal package that has made a significant difference at some point in the life of the plant. These two points, right here being in particular at early in their life they were operating at a significantly higher temperature accumulated quite a number of effective full power years when you normalize it to their current temperature after their modifications and so they -- even though they're now a cold head plant, they had accumulated a significant amount of degradation, if you will, before they made that modification and this methodology that we have of now trying to capture effective degradation years captures that and doesn't then look at then how slow that plant would progress which would be very slow from between 1080 watts and 1580 watts, would take a significant amount of time.

MEMBER SHACK: They must have been a very hot head plant though?

MR. MATTHEWS: They were -- in fact, they may have been over 600. For a Westinghouse unit later design that was perhaps rather unique. I'm not exactly sure. I think they were well over 590 and then dropped their -- they did a significant modification to their upper internals to get their upper head temperature --

MEMBER SHACK: But I mean Davis-Besse and Oconee run over 600 and they're way down at 18 years.

MR. MATTHEWS: Well, they're down at 18 effective full power years at 600. They're actually 20 something effective degradation years, if you will, whereas this plant is only slightly over 10 effective degradation years. Got it?

MEMBER SHACK: Yes, I keep getting -- between EDY and EFPY.

MR. HISER: Bill, this is Allen Hiser from NRR. That plant was operating initially at 601 and dropped to about 561 after their steam generator replacement and other related mods.

MR. MATTHEWS: From our kind of generic analysis, we pulled off the function of temperature here the effective full power years at that temperature at which the plant would reach net section collapse probability of 1 times 10^{-3} and 1 time 10^{-4} and those are the two chain link curves here and then we also pulled off the probability of leak being 75 percent and 20 percent and those are the dark solid blue line here and the gold colored line here. You'll note they very closely parallel the curves for the net section collapse probability at 10^{-3} and 1 time 10^{-4} and then we also just plot and this is a fairly simple plot to do, the effective degradation years on where a five effective degradation years would be in terms of EPFY, 10, 15 and 18.

In the upper set that we talked about, tends to be very close to the 18 effective degradation years, the 10^{-4} on that section collapses very close to the 10 effective degradation years. And so for the purposes of our inspection plan, the initial inspection plant. We had proposed that everything above 18 effective degradation years be classified in the high susceptibility or high risk category, between 10 and 18 be moderate, and below 10 be classified as low, and then come up with a graded inspection approach as a function of which category the plant was in as a function of time.

(Slide change.)

MR. MATTHEWS: We also looked at the impact of the inspections that could be done but bare metal visual and NDE. For the bare metal visual we assumed a fairly low probability of detection in today's world of .6 and then we also -- if a flaw is missed, in other words, if there is a leaking penetration that's not detected by the bare metal visual and it's in that .4 that's missed the first time you do the inspection after that leak develops, the next time that one is inspected, we knock it down, for that nozzle, down to .2, so -- I mean .2 times .6, so there's only about a 12 percent probability that that would be detected in subsequent cycles. So that's the kind of credit we're taking for the visual inspections and then for nondestructive examinations under the head, there was a POD curve from an EPRI report based on size that was used and then we knocked that down by 80 percent.

(Slide change.)

MR. MATTHEWS: If you look at the effect of the inspections, the blue line is the probability of net section collapse. These calculations, I believe, were run at 600, so EPFY would be the same as EDY. The probability of net section collapse with no inspection would be the blue line. And the effect of doing a bare metal visual, the recommendation for a moderate plant which is 1 over 10 EDY, doing that every 2 EDY would that knock down on the probability of -- and you only have a 12 percent probability of picking it up later. It initially has the significant impact on the probability of net section collapse, but then that tends to go back up over time because of the low probability of detection over time.

Recall that at this point while we're still below 3 times 10^{-4} on the probability here, we would move that plant into at 18 EDY, we'd move it into the high susceptibility category and impose a different frequency on these inspections.

The effect of NDE with the PODs that we had assumed in these models is significantly more and because of that better inspection capability keeps that probability of net section collapse down all the way out until the plant moves into -- and even though it's on a lower frequency, it keeps it down as you move on down, out.

(Slide change.)

MR. MATTHEWS: After that --

MEMBER WALLIS: Before you go on to this,c an we go back to your Figure 6?

MR. MATTHEWS: Yes.

MEMBER WALLIS: Because we've had some time to think about it.

MR. MATTHEWS: This one?

MEMBER WALLIS: Figure 6, next one.

MR. MATTHEWS: Yes.

MEMBER WALLIS: I'm trying to think about what it means. The Scott curve is a curve fit to some data for a steam generator experience and it has three constants in it, alpha, beta and 9; 9 has been chosen not to change. Data is 1.16. You assume it's the same as the steam generator experience.

MR. MATTHEWS: Right.

MEMBER WALLIS: So the only coefficient in this equation that's been tweaked is alpha.

MR. MATTHEWS: Correct.

MEMBER WALLIS: And alpha is tweaked by

means of a method which you use for Figure 6. There's a cumulative distribution function. Essentially what's happened it's a way of getting a mean alpha for all the heat, right?

MR. MATTHEWS: Correct.

MEMBER WALLIS: So once that has been done, you've determined your Scott equation and all you've done is found an alpha. What's the best alpha to describe this huge amount of data.

MR. MATTHEWS: Exactly.

MEMBER WALLIS: On average, right?

MR. MATTHEWS: Exactly.

MEMBER WALLIS: And then Figure 6 then, nothing has been derived from Figure 6. Figure 6, you're simply saying given that you've made this decision to choose this alpha, which is the only parameter you've derived from the data, the only parameter, very gross thing, here's the curve and here's the data and it's not a surprise it goes to the data because it was derived from mean alpha for the data.

And so looking at it, what are we supposed to conclude? I guess we conclude that there's an enormous amount of scatter. That's about all we can conclude from this figure. It's not a derivation of anything. It's just a comparison between a curve and data which is all over the map. That's all we can conclude from this figure, right?

So I'm just wondering what I ought to conclude, since I think I now understand what you've done.

MR. MATTHEWS: Okay. Well, what we're proposing to do is use this as an estimate of the crack growth rate to be used if we have a flaw that is detected in the field.

MEMBER WALLIS: Right.

MR. MATTHEWS: To determine the crack growth rate to assess whether or not that flaw could be left in service for some period of time.

MEMBER WALLIS: I guess I'm sort of familiar with science and engineering and I just wonder seeing this whether this gives me a good feeling, that we've got something reliable as a predictive tool.

If I saw this -- I would be very suspicious of this in any other context.

MEMBER SHACK: If you believe this was a fit to the data, you'd wonder why in the world they were fitting --

MEMBER WALLIS: They're not fitting this.

MEMBER SHACK: But they're not fitting it to the data and -- but you somehow look at it as though it is a fit.

MEMBER WALLIS: No, I look at it as a -- given that you've chosen this alpha to reach your conclusions and you've chosen to fix beta and 9, this is somehow telling me, well, I've made that assumption. How well does it compare with all the data I've got. This is what this is telling me.

Do I feel good about that? I don't know why I should feel good about that.

MEMBER SHACK: If you made each of those dots a different color to represent his 21 heats and then he plotted 21 curves, you would see that the curve is a reasonable representation of the data for a particular heat.

MEMBER WALLIS: You mean if you have different curves for each heat.

MEMBER SHACK: Yes.

MR. MATTHEWS: Yes, like I said if I take each point on this, that represents one heat.

MEMBER WALLIS: We haven't seen that. We haven't seen how well one of these alphas fits with a data where you've got say 26 points instead of 1.

MR. MATTHEWS: Right.

MEMBER WALLIS: And we haven't seen that.

MR. MATTHEWS: Each one of these would be a separate curve.

MEMBER WALLIS: You've got to sort of make a judgment about whether your method is appropriate as a reliable predictive tool.

MEMBER SHACK: No, clearly you can't have a predictive tool with a single curve with this much variability in the crack growth rate data.

MEMBER WALLIS: Right.

MEMBER SHACK: It's a hopeless task. It's an unreasonable thing to expect. Until you can come up with a predictive tool to tell me what alpha is for a given heat, but he has to make some -- you can argue whether his choice of a 75th percentile is appropriate as a way to --

MEMBER WALLIS: Well, I guess in a sense you've got a great deal of insecurity here. You've got to be very conservative is what I would conclude.

MR. MATTHEWS: Pete.

MR. RICCARDELLA: I'd just like to point out what you're focusing on now is really --

MR. MATTHEWS: Just state your name, please.

MR. RICCARDELLA: Pete Riccardella from

Structural Integrity Associates -- is really at the heart of the probabilistic fracture mechanics analysis because this huge scatter that you're seeing on this chart really dominates the results and the probabilities of getting a large crack.

You'll notice the horizontal line here at 1 millimeter per year and then if I go up an order of magnitude to where those higher data points are, that's 10 or actually more like 15 millimeters per year and in our Monte Carlo sampling in this probabilistic fracture mechanics, one out of every thousand points that we pick is way up there, that's over half an inch per year and of course those are the ones that lead to ultimately to the net section collapse if it's grown at that speed.

MEMBER WALLIS: So one could wonder if your tail is right -- I've got 6 points up there at the high end.

MR. RICCARDELLA: Yes.

MEMBER WALLIS: And I sort of wonder if cutting off the tail in the statistical way --

MR. RICCARDELLA: Well, but where I cut it off -- I've presented yesterday results where I did a log triangular and then also a log normal and show that that was about a factor of 2 difference on the probability of failures.

The log normal didn't cut off the tail.

MEMBER WALLIS: I think you did a splendid job with what was available.

(Laughter.)

MR. MATTHEWS: And that is what's available.

MEMBER WALLIS: But we've got to face up to the fact that there's a lot of insecurity about this and I agree, you have to do statistics, but then how you treat that tail up at the top there makes quite a difference.

MR. RICCARDELLA: Well, that's why I presented results from treating the tail in two different ways.

MEMBER WALLIS: I know.

MR. RICCARDELLA: To show what the effect was.

MR. MATTHEWS: The tail is a couple of the worst performing heats.

MEMBER WALLIS: It's actually about six of the worst performing heats.

MR. MATTHEWS: Above the 75th percentile, yes. It would be.

MEMBER RANSOM: Well, is the heat, for

example, a random parameter? It seems to be a more important variable than any of the rest?

MR. MATTHEWS: It is.

MEMBER RANSOM: Why are you focusing on that then?

MR. MATTHEWS: We don't know which heats a priori are going to be the ones that going to --

MEMBER RANSOM: If I were the general public I would say maybe you better take the worst heat.

MR. MATTHEWS: That's one approach that we could do. But the approach that we've proposed is to take a -- what we consider a fairly conservative estimate of what the crack growth rate might be for there. Certainly, it's not the ultimately bounding every data point that's ever been generated crack growth rate and then use that to make a best estimate of how far the crack would grow in the next interval and then tack margin on so that even if you're off some, you've set a limit. So even if you miss it, you're still not into any kind of catastrophe and even if we did miss it, and the crack did go through-wall, we're still well away from a net section collapse because you've still got time for that crack to then turn and grown circumferentially.

MEMBER RANSOM: Maybe I'm missing something, but do you drive on uncertainty to go along with this best estimate?

MR. MATTHEWS: No, no.

MEMBER RANSOM: But if you're going to use probabilistic methods it would seem like that would be the appropriate thing to do.

MR. MATTHEWS: In this right here, in the probabilistic methods, we didn't use a curve with an uncertainty. What we used -- well, I guess it might translate into that, but we used the whole scatter of the data base was put into the -- and sampled in the Monte Carlo analysis.

MEMBER KRESS: How long do you scatter above the 75 percent --

MR. MATTHEWS: Actually, the whole data base was used in the Monte Carlo. And like we said yesterday, we don't have any zero points in here. They weren't included --

MEMBER WALLIS: You see, your whole hypothesis is stress intensity factors and the main variable affecting crack growth rate and that isn't shown at all from this figure.

MEMBER SHACK: For a given heat.

MR. MATTHEWS: For a given heat it is.

And if I had plotted these so that you could tell these two and whatever the other points are for one heat and these down here are from another heat, you could say that well, okay, this shape is probably pretty good for a given heat. The heat gives us a sensitive parameter, but we don't know those parameters necessarily that's driving that for every heat out in the field.

MEMBER WALLIS: Well, we're not going to resolve this today.

MR. MATTHEWS: No, we're not.

MEMBER FORD: Hold on, there might be a -- John Hickling.

MR. HICKLING: John Hickling, EPRI. May I just remind you of two things I presented yesterday. I did, in fact, show two curves of the individual heats and at least in one of them you could see as Bill Shack says, the 50 is quite reasonable on a heat to heat basis, but let me remind you that all of the lab data does tend to be biased towards higher stress corrosion crack growth rates because a deliberate choice was made when many of the experiments were done to choose a heat which was known to be susceptible to cracking. And that's a bias which is in the laboratory data inevitably because the experimenter was desirous of obtaining a result in his test. And I fully -- I understand the problem that one has visually with this picture. I have it myself. There is that hidden bias in there which shouldn't be forgotten.

MEMBER FORD: Could I ask that we move on?

MEMBER SHACK: Since we're all talking about our warm and fuzzy feelings, my warm -- the problem where I don't have the warm and fuzzy feeling is in the K solutions yet. Until Pete explains to me why the zero degree nozzle one doesn't act like the way I expect it to act, that's really step one in this whole process. If I'm not warm and fuzzy up there, then I have a time following the chain down.

MEMBER SHACK: K is not the driver.

(Slide change.)

MR. MATTHEWS: Let's see, where was I? Then I was going to move into Glenn White from Dominion had gave a presentation on the work that Dominion Engineering is doing for the MRP relative to the progression or the possible scenarios for progression from a leak to a cavity and his work was trying to answer a couple of questions if there is a significant amount of head loss, would it be detectable visually? And I think his conclusion there is yes, the products that are going to be generated in that corrosion are going to be available on top of the head for detection and then is there a period of time following the initiation through-wall leak for which there is assurance that if we don't have unacceptable reactor vessel head corrosion and we believe, but we haven't finished the work yet, that there will be a significant period of time between the initiation of any corrosion and the time the cavity gets to be significant and the growth rate becomes significant.

(Slide change.)

MR. MATTHEWS: He looked at all the possible mechanisms and he characterized them as a function of the flow rate from 10^{-6} up to 1.0 gpm. He looked at the thermal-hydraulic environment, the chemical environment, properties of boric acid and their compounds and the relevant experimental results that are available.

His conclusion at that point was that the leak rate is expected to be the key parameter, primarily I think based on a couple of things. The expansion cooling at the leak rate increases, potentially could get to the point where a liquid film would be available and then it would be very easy to get some very high concentrations of boric acid at essentially saturation temperature and atmospheric pressure which are known to be highly corrosive. And then the increasing leak rates from higher velocities could get into erosion or flow accelerated corrosion mechanisms.

MEMBER FORD: Could you go back to that last slide? I want to be sure that we all realize that there's very, very little data to support this hypothesis as to the specific mechanism of degradation. That is reasonable. The hypothesis that the leak rate is a critical parameter is reasonable at this stage.

If subsequent experiments, which I hope there are subsequent experiments to prove this hypothesis, then it's going to be fairly obvious that current technical specification of one gallon per minute may have to be modified. Do you agree?

MR. MATTHEWS: I guess I'm not going to try to answer that right now. I don't know. One gallon per minute clearly -- I mean clearly Davis-Besse got into a situation where they eroded a cavity or corroded a cavity on their head with less than one gallon per minute leak.

If the purpose of the one gallon per minute tech spec is to try and prevent something like that, it doesn't do it. If that is not the purpose of the one gallon per minute tech spec, then maybe it doesn't and I'm not a tech spec guy. I'm not sure what the purpose of that 1.0 gpm was to start with.

MEMBER WALLIS: Okay.

MR. MATTHEWS: But if you're going to try and protect bio tech spec on unidentified leak rate, 1.0 gpm will not -- I mean it clearly did not stop what was going on at the Davis-Besse plant.

MEMBER WALLIS: Thank you.

(Slide change.)

MR. MATTHEWS: The leak rate also determines how much boric acid gets out of the system on to the top of the head or wherever else it goes and Glenn tried to use -- or I don't know that we've actually gotten to the point of trying to define a time line. I think he has looked at how much low alloy steel material might be lost versus the volume of boric acid and/or corrosion products that would be available for detection. He did not present anything on that. This was the basic result that he had going from a through-wall leak to the annulus that was not leaking to the top of the head because of being sealed off above the leak for some reason, having zero leakage up to .01, I mean .001 gpm, .01 and then the various increasing flow rates on up to greater than .1 gpm.

The types of flow, I mean the types of possible significant corrosion mechanisms or degradation mechanisms that would be taking place in each of those flow regimes and this seems to present a plausible progression from the through-wall crack in the nozzle or weld progressing to a larger flaw with a larger flow rate in the degradation progression as we go.

Almost all the other nozzles that have been detected with leaks in the U.S. industry, well, in the world, have been in this range here where there's been very, very little flow rate and very little boric acid accumulation on top of the head.

I guess we think that Davis-Besse had progressed further in that process and we're over into this range of degradation creating a larger cavity.

Glenn's not through with his work. It's labeled preliminary. When he gets through with that, we will find, I think we'll be putting more of a time line on this as best we can, but like we say, there's not a lot of work at these kinds of flow rates at this point and trying to do that we may wind up trying to spec tests that need to be done at these flow rates.

MEMBER WALLIS: This is very interesting preliminary work and I agree it presents plausible progression and we had some questions about some of the details yesterday which I don't want to get into.

I just wanted to ask that although this is preliminary, you are somehow using it in the guidance which we're going to get next and when to inspect. I mean what do you expect to happen physically and it's going to influence your strategy of inspection, it seems to me. Is this very preliminary work, being fed into the inspection strategy or not at all?

MR. MATTHEWS: I think it will be. Basically, if you recall from the presentation yesterday on the inspection plan, that initial proposed inspection plan did not take into account the wastage issue in any shape other than to assume that there would be some improvements in the boric acid control program that would prevent that issue from happening.

The staff gave us the comment. We need to marry these two issues and so we've taken that comment back and we're going to try and very rapidly come back with a modification --

MEMBER WALLIS: So you don't have an answer to my question yet.

MR. MATTHEWS: Well, the answer to your question is no, this was not taken into account because that program that we initially proposed --

MEMBER WALLIS: But you're thinking of taking it into account?

MR. MATTHEWS: Yes. This would have to be taken into account in response to the staff's request that we marry any inspection programs --

MEMBER WALLIS: Realizing that again this is not a very secure science.

MR. MATTHEWS: Right, it's plausible, but is it absolute, no, not yet.

MEMBER FORD: I'd point out for the record that corrosion science is one of the oldest sciences, in my own defense.

MR. MATTHEWS: Okay.

MEMBER FORD: I mean they all do. Science gets them all confused.

MR. MATTHEWS: Then we presented a presentation, Michael Lashley made this presentation on the proposed inspection plan that we had discussed with the staff on May 22nd and like we said that initial proposed inspection plan did not take into account on how to protect against the wastage issue. It was a nozzle ejection issue that that plan was trying to protect against.

We received significant comments from the staff that we should marry the plan with the wastage protection inspection plan and look at, like you say, the time frame for the wastage development, whether or not the tight nozzles will indeed leak because one of the basic tenets of the plan was that they would and that visual would be an adequate way to detect initial leakage in the plant.

And then the policy issue is that an acceptable way to detect when a plant initially has the problem by an initial leak and then we also did not address replacement heads because we recognize they would be of a different material, but they said the plan needs to at least put out some kind of inspection recommendations for the replacement head.

I've left out all the detail slides here, but just went straight to the flow chart.

(Slide change.)

MR. MATTHEWS: Like I showed earlier, categorized plants, that's low susceptibility, moderate susceptibility and high susceptibility based on their effective degradation years. A low susceptibility plant, we had recommended that they do 100 percent bare metal visual or alternatively if they chose or wanted to, 100 percent NDE. Do that once every 10 years after the plant has been operating for 20 years, some time in their third interval.

For a moderate susceptibility plant, we had recommended 100 percent bare metal visual. The first outage that they entered this category and then once every two effective degradation years after they get into that category. Put a cap on that of 5 effective full power years because some of the low temperature plants two effective degradation years could be a significant amount of time. If it's a high temperature plant, two effective degradation years is effectively going to be every refueling outage.

Alternatively, they could also perform the nonvisual NDE, the first outage, and then at half the frequency of the visual because the nonvisual NDE would detect cracks at a much earlier stage than the visual would.

The high susceptibility category, initially we were thinking about just doing bare metal visual, but could cover what we don't know. It was recommended that we include 100 percent NDE for those plants that are in the high susceptibility category and there was a time, a grace period because -- four years after NDE category or issuance of this plan and that was because there's a limited amount of tools out there and when the plan hits the street, there may not be enough tools to do all the plants that might be in that category the first time it's out there. But like I say, it's to cover what we don't know and we're requiring them to do that.

The bare metal visual would have to be performed every refueling outage or alternatively the nonvisual the first time in every four effective degradation years. And the four effective degradation years were based on how long the cracks would take to grow through-wall, etcetera.

MEMBER FORD: Again, just for the record, I think that's a very dangerous argument to make.

MR. MATTHEWS: Which one?

MEMBER FORD: Just because you don't have the tools, you're not going to inspect.

MR. MATTHEWS: The basic plan is based upon the visual and the NDE requirement that we're placing on the plants when they enter the high category is there, like I guess in the terms of my executive vice president, that's to cover what we don't know. We base the plan on what we think we know and that the visual was adequate to cover that. The nonvisual was there to cover what we don't know.

MEMBER FORD: I'm assuming that since this is on-going discussions with the staff --

MR. MATTHEWS: They're likely to have a different perspective.

(Laughter.)

MEMBER WALLIS: Could I ask, it is based on what you think you know and the arguments for what you think you know are overly -- have been quite good. But we've heard good arguments before Davis-Besse too.

MR. MATTHEWS: Yes.

MEMBER WALLIS: So once per 10 years seems as if you're really very, very confident that nothing surprising is going to happen in those 10 years.

MR. MATTHEWS: Like I said, this initial plan was based on just protecting against the next section collapse from PWSEC. As we go back and try to marry this inspection plan with something that's going to protect against the possibility of a wastage cavity. I suspect that several of these frequencies will have to be changed and possibly even the inspection techniques.

MEMBER FORD: Okay.

MR. MATTHEWS: Once you do the inspections what we had the plants do, if they detected a through-wall leak, the plant is reclassified as a high susceptibility plant and the only way to get out of that category then is to replace the head.

I guess theoretically you could replace al the nozzles and welds, but that would be prohibitive. We require them to -- they would be required to characterize the indication that they have that's generated the through-wall leak or through-wall crack or the leak. We can't run with that, so to prevent leaks in the future we'd have to pare that nozzle and then perform 100 percent NDE on the rest of the nozzles.

This was at the next refueling outage and I know this is one of the things we received comments on as allowing another cycle there. We'll have to look at that.

Basically, the logic behind that was you had performed some inspection that assured you that you had detected all of the leaks and you repaired all of the leaks. Agreed, there is some small probability that another leak might develop in the next cycle, but you're not sitting there with another nozzle that's been leaking for a number of years and growing a Circ. flaw because that would presumably have been detected in the other inspections. So that was the initial logic between doing that. The plant would then be reclassified and go back into the high susceptibility category.

If a low susceptibility plant detects any cracking, we're going to stick that plant into immediately into a moderate susceptibility cracking plant, unless it's through-wall and then they go to high. And then based on that crack and everything, they would have to determine their new inspection interval and what category they would be in.

But that's basically the initial plan. I can say we've received comments from the staff when we initially presented this. We're on a fast track to try and incorporate those comments and decide how we're going to modify our plan to address the issues that the staff raised and get back with them on another proposal.

MEMBER SHACK: Your temperature counts for one of the big variables that you're going to have in your susceptibility. The other one is the heat, the heat variation which we have no good way of handling. Have you looked to see with your current scheme what fraction of the heats you would be looking at in the high susceptibility category, that is, would you have captured a fair sample of the heats to assure yourself that you didn't have a moderate susceptibility plant based on temperature with a high susceptibility based on heat?

MR. MATTHEWS: We haven't done that, but I think we have the information that we could do that, that look. And that's something I think we ought to go back and take a look at.

MEMBER SHACK: It seems to me that somehow you ought to set this up so that your high susceptibility thing where you're going to be doing the nonvisual captures at least enough of the heats to give you a confidence that you've looked at those, even though they might be moderate susceptibility in terms of temperature.

MR. MATTHEWS: Pete, you want to say something?

MR. RICCARDELLA: Yes, I just wanted to -- this is Pete Riccardella from Structural Integrity. Remember that a big part of the categorization is based on the high susceptibility heats. Remember our time to leakage correlation which is that Weibull fit is strictly the B & W plants. So pretty much that part of the assessment is based on the higher susceptibility heats. And --

MEMBER SHACK: You did a triangular distribution, but your triangular distribution was only --

MR. RICCARDELLA: Only of the seven B & W plants which tended to be -- we believe, tends to be the higher susceptibility heats and don't forget we also correlated the crack growth to those as well.

MEMBER SHACK: You might get a certain amount of debate on that in terms of the heat basis.

MEMBER WALLIS: Yeah, I think so. They're high temperature plants. We don't know really know that they're the high susceptibility heats. There could be some other -- heat is such a mysterious thing that there could be other bad heats out there and I would really like to have a physical basis for making the difference, not some mysterious heat that no one knows what it is.

MEMBER FORD: I'd like to draw a close to this particular message. Any other questions.

MEMBER RANSOM: I'd like to make an observation or a comment that this may not apply to future things, but just the Davis-Besse observation of one of simply taking the massive material removed from the head and did a chemical analysis, you would have realized that the iron content, the amount of iron you're removing was significant.

And I'm wondering if a mass balance on the iron, I know that in a nuclear plant on any radioactive material there's a very detailed mass balance made. But even if you just took the material off the head at an inspection and analyzed it, you would realize whether you're removing grams, kilograms or what mass of iron is being removed and in fact, it might be worthwhile if the material has been preserved from the Davis-Besse head to estimate how much iron is actually in that.

MR. MATTHEWS: I'm not aware of how many barrels do you have locked up somewhere. None?

MEMBER SIEBER: Well, a lot of it stayed on the head, but some dripped down the sides. Some of it went into fan coolers, some of it is all over the containment.

MEMBER RANSOM: Sure, so that would only tell you that if you are removing significant iron in that, that I actually remove more than that.

MEMBER SIEBER: That would tell you --

MR. MATTHEWS: Probably not totally uniform in its constituency either.

MEMBER SIEBER: Right.

MR. MATTHEWS: Coming out in this amount versus that --

MEMBER RANSOM: Well, you've got to sample it, of course, than do a statistical.

MEMBER FORD: I'd like to bring this particular discussion to an end. Thank you very much, Larry.

MR. MATTHEWS: You're quite welcome.

MEMBER FORD: We'd like to call on the staff, Bill Bateman.

We'd like to ask Bill Bateman and his staff to make their presentations.

MR. BATEMAN: Good morning. I'm Bill Bateman, NRR, Chief of the Materials and Chemical Engineering Branch and with me at the table are Ed Hackett who is representing the Lessons Learned Task Force and Jack Grobe from Region III as a Division Director of Reactor Safety and also leading the 0350 Panel.

(Slide change.)

MR. BATEMAN: I've got one slide here and I'm going to try and go over quickly what the staff discussed yesterday. The first item is to update you on where we're at with respect to the status of the bulletins from the last time we briefed the full committee. I'll start with Bulletin 2001-01. As you may recollect, Bulletin 2001-01 was issued to address the concern with circumferential cracking and vessel head penetrations.

We emphasize with the bulletin that the high susceptibility plants had to inspect within a certain time frame and that was accomplished and we did identify, the plants did identify some cracking in VHP nozzles and those were repaired.

This most recent outage season, there were no other additional cracks identified as a result of inspections that were performed. So that gives us at this point some confidence in the susceptibility model. I know we've had discussions here about heats and their potential impact and I think there's definitely something we're going to look into, but at least at this point in time we haven't found anything as a result of the inspection data that would concern us that we are totally misled by the time and temperature susceptibility model. So that's kind of the status of where we're at with Bulletin 2001-01 at this point.

MEMBER LEITCH: I have a question that relates to BWRs. With respect to the CRDM cracking issue, the boron in the PWRs was an important indicator that we had some incipient through-wall cracks and the BWRs we don't have that obviously. And in the stub tube barriers, we have some of the same.

I mean it's difficult to inspect which might be analogous to the head of the PWRs. It's -- there's some tolerance perhaps for, in some plants, for a little bit of leakage down there. There are so many things that can possibly leak. It's not uncommon to have a few drips coming out of there which may be, in my mind analogous to the tolerance in the PWRs and the flange leaks and that's kind of clouding the picture.

Admittedly, you have a much lower temperature down there in the BWRs, but I guess my question is have you thought at all about whether there's applicability of this issue to the BWR stub tubes and other, CRBs and other instrumentation penetrations that are down there in the belly of the BWRs?

MR. BATEMAN: Yes. We have. As a matter of fact, there are at least two plants that come to mind that have had leaks in their stub-tube welds and we have allowed them to roll repair those stub tubes to stop the leak.

But the one thing that we do take some confidence in is the weld bead and how the stub tube is connected to the housing such that even if the weld were a through-wall crack you still have that weld bead around the OD of the stub tube that would prevent nozzle ejection.

MEMBER LEITCH: I guess I'm just not familiar enough with that design to quick picture what you're saying. Could you say that again?

MR. BATEMAN: You have the stub tube which comes through which you install the housing and then you basically weld the housing to the stub tubes. So if you picture a Philip weld in your mind, that Philip weld is attached to the housing and to the stub tube. If that crack, if that weld were to crack, you still have the Philip weld which acts as a blocker for that housing to go, move through the stub tube and out of the bottom of the vessel, where you don't have that situation here in the PWR design.

MEMBER LEITCH: So you could get a significant leak, but not a --

MR. BATEMAN: But not an ejection, right.

MEMBER LEITCH: Okay. And the temperature is --

MR. BATEMAN: Substantially lower, so you wouldn't expect there to be nearly the susceptibility.

We have seen some leaks at the older plants, Nine Mile and Oyster Creek have got some leaks. As I said and we have performed some role repairs as a temporary repair, but we're pushing for more permanent repairs. There is a recent code case that's provided an avenue for them to make a more permanent repair.

MEMBER ROSEN: What's the temperature at the stub tube, typically?

It seems lower.

MR. BATEMAN: Right off the top of my head -- what's the saturation temperature for --

MEMBER LEITCH: 545, I think.

MEMBER ROSEN: So it's in the range of the cold head plants, PWR cold head, even below that.

MR. BATEMAN: I'm not exactly sure either what the weld material is. I think it's -- and maybe some of my staff might know. I think it's a stainless steel weld as opposed to an alloy 600 weld.

MEMBER ROSEN: But a few degrees temperature difference is very significant. I mean this phenomenon is highly temperature dependent and what you would expect in the normal engineering disciplines to not matter, a few degrees Fahrenheit, it turns out to matter quite a bit.

MEMBER SHACK: Well, I'm not sure that's true in this case. You know the mechanism in the BWR is not PWSCC and I don't -- I was actually trying to think last night when Graham mentioned this to me, what we know about temperature dependence, but by and large the temperature dependence of the mechanism is likely to be operative in the BWR, I don't think will be as temperature sensitive as PWSCC is, although I don't think we have a whole lot of data on that although Peter would know that.

MEMBER FORD: I don't know if I can say anything because of a conflict of interest but I'm sure Dr. Hickling could address that issue.

MR. HICKLING: Just a brief, comment, John Hickling, EPRI. Bill Shack is, of course, completely right. It's a different mechanism in the BWR and the weld metals susceptibility, whether it be 182 or to a lesser extent 82, is well known, has been for many years. But it's not comparable, certainly not in terms of temperature dependance to the PWR situation.

MEMBER ROSEN: I got too far along there. Really, all I was trying to find is what is the temperature and I think the answer was 545 or something like that.

MEMBER WALLIS: In terms of a Scott curve you're probably below the magic number 9. It's not 9 in this material. But it's something.

MEMBER SHACK: No, no, no. Because your

activation energy is likely to be quite different and it's cold comfort farm. It might be cold --

MEMBER WALLIS: That doesn't help you?

MEMBER SHACK: That ain't buying nearly as much as it does in the PWR case, at least I believe that would be -- there's much sparser data.

MEMBER FORD: But if I could make a comment in relation to your concern which really comes down to is anything being done about assessing that particular phenomenon and yes, there's a tremendous amount of work being done, background work in the laboratory on cracking of 182, 82 and 600 in BWR environments.

It's not as though we're just sitting on our thumbs and doing nothing.

MR. HICKLING: John Hickling, EPRI. I had one comment. Of course, in the BWR, you have an effective mitigation technique by the use of hydro and water chemistry and one of the main driving forces behind hydro and water chemistry is to protect that sort of material down at the bottom of the head.

MEMBER LEITCH: Yeah, it's just there is a lot of history before some of these plants went to hydrogen water chemistry and some of that with relatively poor control of reactor water chemistry in the early years.

MR. BATEMAN: Okay, I'll move on to the status of Bulletin 2002-01 which was the bulletin we issued right after Davis-Besse head degradation was identified and that bulletin was issued to give the staff assurance that there were no other Davis-Besse's out there. And basically issued that bulletin requesting licensees to respond within 15 days and they did and we basically have reviewed all the responses and at least at this point in time have confidence that we don't have any other Davis-Besses out there.

We had some discussion yesterday, as you recall, about how do we gain that confidence and was basically based on the licensees' responses and subsequent phone calls by my staff to follow up on questions that arose from our review of their responses. It was not based on individual NRC observation of each reactor vessel head.

So anyway, that's where we're at with Bulletin 2002-01. When we did get the 60-day responses which asked for information on their boric acid inspection program. Those came in, I guess, last week and we're in the process of reviewing those. I think we got through about 20 percent of those. So that's where we stand on Bulletin 2002-01. Any questions on that?

Okay, the next item is we spent quite a bit of time yesterday listening to data analysis of crack growth rates and all that sort of thing and I think where it's all leading to is where do we go from now? I don't think any one of us wants another Davis-Besse head degradation type scenario. I don't think any of us wants any more circumferential cracking to the extent that we found at Oconee. So that's where our challenges are. What's the next step to go on from here?

And I think it's the inspection plan. I think that's where we're at. We've got to agree between the industry and ourselves what will be an effective inspection claim so that we don't have -- we won't have this kind of situation again and that's what we're working on right now. You heard the industry's presentation. We're basically at this stage working on a piece of generic correspondence to bridge the gap between now and the time we come to agreement with industry and then in some way codified either in the ASME code or through rulemaking and the regulations.

We haven't decided exactly what our

position is on that yet, but I can assure you that it will be in excess of what industry has proposed. Until we -- and then we'll back down from that over time, given that industry presents a technically sound argument to justify that.

MEMBER LEITCH: What's the time frame for this interim communication? Do you have a time in mind for that?

MR. BATEMAN: It's in draft right now and it's going to be moving pretty quickly, so I would say barring any unforeseen difficulties, I would say within the next month and a half.

MEMBER LEITCH: Before long, the fall outage seasons is going to be upon us.

MR. BATEMAN: Yes.

MEMBER LEITCH: And I'm sure that a lot of plants, if that impacts their inspection program in the fall, as I suspect it might, they need that information in a timely fashion.

MR. BATEMAN: Agreed. And we've had various licensees express that to us.

MEMBER SIEBER: Actually, if you wanted to hire technicians and rent inspection equipment, they ought to know now.

MR. BATEMAN: I think a smart licensee

would --

MEMBER SIEBER: Do it any way.

MR. BATEMAN: Do it any way. I mean if you're going to wait around for the regulator to tell you what to do, you may be caught between a rock and a half place when it comes to outage time.

MEMBER ROSEN: How are you going to impose the requirements of this new plant? What regulatory vehicle will you use?

MR. BATEMAN: What we're contemplating right now is a bulletin and a bulletin basically is not -- doesn't require licensees to do anything. We only have limited vehicles that require licensees to do anything, for example, orders. We're not contemplating orders at this time, but I think it will be based similar to the Bulletin 2001-01 where we'll ask the licensees what their plans are and we'll represent what we consider to be an acceptable answer to that question.

It would be undoubtedly based somewhere along -- something similar to what the licensees have presented for an inspection plan, but more than likely will have different intervals and frequency, different methods and frequencies.

Any other question son that? If not, I'd like to turn it over to Jack Grobe, to give you a brief update on the 0350 Panel.

MR. GROBE: Thanks, Bill. I apologize. I wasn't able to reduce it to one slide, but I do have a couple of slides, just summarizing what we talked about yesterday.

Following the discovery of the cavity in early March at Davis-Besse, the NRC chartered what's referred to an 0350 Panel. It's a more extensive oversight process for a plant that meets certain criteria and the bases for chartering that panel were that the head degradation issue at Davis-Besse certainly represented a complex and substantive technical issue, but also posed a number of complex regulatory issues and organizational issues for the NRC.

The plant has been in extended shutdown situation with a regulatory hold on that shutdown and that's through a confirmatory action letter. 0350 enhances our ability, as an agency, to define and communicate what we believe are necessary actions prior to restart and it also enhances our ability to coordinate the agency activities in response to the situation at Davis-Besse. So those are the bases for formation of the 0350 panel.

(Slide change.)

MR. GROBE: There's a number of goals that the panel has. The first of those is to ensure that we have a broad and integrated focus on assessment of the facility performance. For a normal plant in an operating configuration that assessment would be under the responsibility of the branch chief and the regional office and the inspection staff that feed into that. In a case like Davis-Besse, we want to have a much more substantive oversight process.

In addition to that, the 0350 panel insures that there's a shared understanding between both First Entergy, the licensee, the NRC and the public on the issues that need resolution prior to restart.

Also, the panel has the capability to break down organizational boundaries in the Agency. We have a number of staffs that are involved in response to this situation to ensure effective and efficient utilization of Agency resources and to minimize the impact on the licensee. The panel is able to bridge those organizational boundaries.

In addition, we've had extensive interface with concerned citizens in the area of the plant, concerned groups of citizens across the country, federal, state and local elected officials, as well as the media and the 0350 panel gives the agency a central focus for a single point of contact on consistent communication with the public.

Two other focus areas, the panel will provide restart -- excuse me, oversight following restart. During the course of an extended shutdown like this at Davis-Besse, part of our normal assessment program includes performance indicators and those performance indicators that are operationally focused will atrophy during the shutdown time frame. So the panel will continue to provide oversight after restart until it determines and recommends to senior agency management that the plant is ready to return to the routine reactor oversight process. And finally, one of the responsibilities of the panel is to create a compehrensive public record, publicly available record of decisions and activities that go into the Agency's actions.

MEMBER LEITCH: John, I'm still a little unclear. Whose approval of the NRC is required for the restart, is it this 0350 panel and the approval chain?

MR. GROBE: No. No. The panel is chartered by the regional administrator, Jim Dyer in

Region III. As far as a restart decision, the panel will go through a structured process to get to a recommendation for restart. That recommendation will be made to Jim Dyer and then Jim's responsibility is in with -- in coordination with Sam Collins, Director of NRR and Bill Kain and Bill Travers, the Deputy DDO and EDO. We'll make the final restart decision.

As far as return to service, excuse me, return to the routine reactor oversight program, again, that's a recommendation of the panel to Jim Dyer and he will coordinate with Sam Collins on that.

MEMBER LEITCH: Okay, thank you.

MR. GROBE: But Jim is the person that makes those decisions.

(Slide change.)

MR. GROBE: The licensee recently submitted on May 21st what they refer to as a return to service plan and that's available on our website. It contains six substantive building blocks. That's how the licensee refers to them. These building blocks form the major tenets of their return to service activities. First one, of course, is restoring the reactor head and they've chosen to replace it. Second is looking at inside containment at the effects of leakage and boric acid and that includes two areas of focus. One is the reactor coolant pressure boundary, the remainder of the reactor coolant pressure boundary beyond the reactor head and the second is other equipment inside containment that could have been affected by the atmosphere that existed in containment.

The third is a system health assurance plan. The focus of that is to examine risk significant systems that are important to plant safety and ensure that, in fact, their operability is where the licensee believes it is. Fourth is referred to as program technical compliance and what that means is are the programs functioning as expected and there's a number of focus areas here, one that the licensee has chosen is the boric acid corrosion management program, of course. Another one is the corrective action program. Both of those programs didn't function as expected, in this case, the design change process and there may be others.

The fifth area is management and human performance excellence plan and I would include organizational effectiveness in this. Clearly, there were some decisions made, judgments made, activities that occurred that involved human performance and that's an area that needs to be addressed. And finally, any necessary testing before restart and then after restart. So those -- hang on for just a second. Those are the six areas.

The NRC will be creating what's referred to as a restart checklist and that will be published, publicly available. The restart checklist will contain these activities and others that the NRC believes are necessary for resolution prior to restart. That would also include, for example, any licensing actions that are necessary or code exemptions and there may be sub-elements in these six areas. These six areas clearly capture the major flavors of what needs to be done before restart. And then our assessment in this context would be to ensure that we're comfortable with the licensee's assessment of root cause in each of these areas; ensure that there are detailed implementation of these activities is going to address those causal factors; and then examine their implementation, both by observing and evaluating what they do and then conducting independent inspections of other areas that they don't cover. And finally, ensuring that any deficiencies identified through the course of these activities are adequately resolved prior to restart, those that need to be resolved prior to restart.

(Slide change.)

MR. GROBE: My final slide is just simply to refresh your memory on what inspection activities are on-going right now. The augmented inspection team completed its work in April. The purpose of the augmented inspection team was a fact-finding mission. It did not put the results into a regulatory context. The AIT follow-up inspection which does that is on-going at this time. We've received substantive information from the licensee on the process they're going to go through to replace the head and we're crafting our inspection plan for that and staffing it right now.

And the extent of condition, these are the activities, the inspection activities that are on-going inside containment. That inspection is also under way.

Are there any questions that I can answer? We covered this in substantial detail yesterday.

Okay, thank you very much.

MR. HACKETT: I didn't get down to as efficient as Bill either, but I hope I can do this in three slides.

Davis-Besse Lessons Learned Task Force. I'm Ed Hackett. I'm the Assistant Team Leader.

Kicked off activities this week on Monday. I guess I'll start with the charter, again, like Jack said, we went into pretty good detail on this yesterday. There are five elements that are listed here. I won't go through those in detail. Only to mention that the focus will be primarily on the top two, the reactor oversight process and regulatory process issues. The team right now is consisting of nine staff from the NRC. It's a mix of managers, technical staff, also representation from all three major offices at the NRC and the regions.

Right now, we're looking at splitting the team two ways. Art Howell is the team leader and Art Howell and some of the regional folks on the team will head a group that will largely interface at the site and with the region and I will head a group here at headquarters that will deal with most of the headquarters' activities.

In terms of schedule, I think Dr. Apostolakis aid to me yesterday, when you're done in six months we'll have a good story. Unfortunately, we need to be done in three months. I think we're probably going to wish we had six months. But the bottom line is we're looking at having to complete this activity by September 3rd with finalization of a report. We're looking at doing it in two phases. As I mentioned, we've only just gotten the team together this week, so we're sort of in a preparation phase right now that includes putting together a lot of the processes and procedures for the group and just getting situated physically. That will probably take most of the month of June. After that, we'll be in a review phase and a report preparation phase that will extend from basically July into September.

A couple of things I mentioned along the way here, there are other activities going on that are related. There is a congressional investigation that's been organized through the Energy and Commerce Subcommittee, United States Congress. That will be going on while this activity is going on also. There's an NRC IG investigation also into certain aspects of the NRC decision making process related to the most recent outage and deferral of inspections at Davis-Besse. So those are going on also. There will be sensitivities and interfaces associated with that in the Davis-Besse task force. There may be things that the task force comes up with that need to get handed off, in particular, to Jack's panel, for instance.

In terms of status, sort of where we are right now, I think I mentioned the top two. Team members are here and physically located at headquarters now, including all the regional staff. There's going to be a lot of coming and going from the site. Team orientation, we had three days of briefings that just concluded yesterday and Jack briefed us for at least three hours, I believe, as part of what his group is doing yesterday. There was a preliminary Region III office visit scheduled for today. That is not happening since several of us are going to be out there next week. The fourth bullet down there, there is a site visit or what we've been calling a public entrance meeting in the site vicinity at Oak Harbor, Ohio. That's scheduled for June 12 and that will be in the morning of June 12. We're basically, we will do kind of what I'm doing here, inform the public and the folks in the vicinity of the plant, of what the task force activities are going to be.

As part of the process, we are conducting interviews with many of the NRC managers, the senior managers. Myself and Art Howell have done a number of those already and several others are in progress and the team right now is preparing detailed review plans.

The last thing I'll mention is to

supplement the meting we're going to be having out in the site area next week, we also plan a similar meeting here at headquarters. Right now, we're working towards having that on June 19 and members of the public are welcome and invited to come to that and we will be soliciting any comments on the team's charter at that point and also next week. So that's what I had in the way of status and I'd be glad to take any questions also.

MEMBER FORD: I'd like to thank you very much. I'd like to just say for the public record that yesterday we had a 10-hour meeting in which all of these topics which were covered in the last two hours were very fully discussed, so that will be in the public record.

MEMBER KRESS: One question before we close to the staff, is anybody perhaps in research working on an engineering chemical physical bottle for this wastage problem to try to see if they can predict by model?

MR. HACKETT: I'll go ahead and speak for the Research Office, since that's my home base. Bill Collins is probably the one. I don't know that he's here at the moment. Bill's got the lead for the NRC Research Office on doing exactly that and it's obviously the problem is defining the task and then getting it done and getting the right amount of resources applied to it I think is going to be one of the key issues.

I think one of the things that's been discussed is obviously a teaming with the MRP to look into doing some more detailed analyses on the cutout from the Davis-Besse head. There have been discussions of mockups for a variety of the mechanisms that have come up and have been discussed here with the Committee. All of that, as my understanding, plans for that are in progress. Bill's branch has put together a user request that's very comprehensive that's been sent to the Office of Research and has been iterated on several times. And again, our problem is going to be time and resources. There's a lot of work I think that needs to be done here and we'll probably be back talking to the Committee about that in the future, but the short answer is yes, that type of work is underway.

MEMBER KRESS: I'd be very interested in that because that's the kind of stuff I used to do, that kind of modeling.

MR. HACKETT: We have the advantage that a lot of folks want to work on this. It's technically exciting even though it isn't necessarily exciting in the right way for the NRC and the licensee and the public, but there's a lot of very interesting aspects of this technically, so there is going to be a lot of work.

MR. BATEMAN: I would just like to make a point and it's one I tried to make in my brief presentation. My hope is we never have to deal with this situation again and --

MEMBER KRESS: A good model might tell you whether you do or not.

MR. BATEMAN: I'm hoping that an aggressive inspection plan would preclude the need for any angst at all about whether or not this will ever happen in the future.

MEMBER KRESS: I think that would involve, if you saw any leakage at all, regardless how big it was, you have to go in and inspect to see if there's wastage associated with it.

MR. BATEMAN: Right.

MEMBER KRESS: Which may be the solution, you're right.

MR. HACKETT: I think I'd add one more comment just in closing. Allen Hiser yesterday had a presentation that got into discussion of management by leakage and I think we're starting to see that as a theme with some of these recent occurrences when you look back over this progression of D.C. Summer, Oconee, now Davis-Besse. I think some of the discussion yesterday went to the fact that these plants were designed in a very robust way, defense-in-depth, and so on. And for a long time, a lot of this type of situation has been managed through leakage fairly effectively.

What we're seeing now is erosion of these margins and that may not be the prime way of doing this in the future.

MEMBER KRESS: I think the purpose of the research and the model would be two things. One to tell you that you do have to have leakage that's observable in order to get the wastage. That's question one. Question two is how much does the leakage have to be an dhow fast does it progress and so that you can talk about scheduling inspections. I think those two things would be the purpose of developing a good physically based, chemically based model.

MR. BATEMAN: Just another point. I know you have read the root cause report and recognize that they characterize the root cause as a probable root cause with a causal factor being at the blanket of boric acid sitting on top of the head. At this point, we don't know how much of a contribution that blanket of boric acid, crystal sitting on top of the head actually contributed to the corrosion of Davis-Besse. Obviously, other plants had through-wall cracks and didn't have the same amount of wastage around the nozzles, but they also didn't have the blanket of boric acid on top of the head either.

MEMBER KRESS: I would personally think it's not very important but I have a mental model of what's going on.

MR. BATEMAN: Yes. I've talked to a number of people who feel that that blanket on top probably did contribute in some way to the wastage.

CHAIRMAN APOSTOLAKIS: Okay, thank you, gentlemen.

Please come to the microphone. Identify yourself first.

MR. GUNTER: Yes, Paul Gunter with Nuclear Information Resource Service.

A couple of questions. I noted that First Entergy said that they were collecting the boric deposits and they have the cutting of the wastage. Has staff made a request or is First Entergy offering samples of the cracks in the nozzles themselves? It seems like this would be worthwhile preserving as well and I'm wondering if, in fact, this kind of information is forthcoming.

MR. GROBE: Let me start the answer and then maybe Bill wants to supplement and if First Entergy has any contributions that would be fine, too.

First off, there's very limited amount of the boric acid on the head that was collected. At the same time, these repair activities were going on. The utility was cleaning the head and very little, if any, of the existing boric acid, boric oxide corrosion product blanket on the top of the head was collected. There were some materials collected from the crevice on penetration 2 when that penetration was removed.

By and large, the cracks have been ground out because that's part of the repair process, so they're ground away and there's very little data that can be gained from that. All of these materials have been transported to Lynchburg where they're going to be examined and I think Bill's staff is going to be involved in the decisions of what types of evaluations, destructive evaluations will be undertaken.

MR. BATEMAN: First Entergy has been

working very closely with us on the types of analyses, on what types of material to do, so the answer to your question is yes, we are working, First Entergy is working with the NRC to gather as much information as can be gathered from the samples.

MR. McLAUGHLIN: Paul, the process we've been using because all of this material is governed by our confirmatory action letter, there's a section in there addressing quarantine. All of these samples are being handled under the quarantine, so what we've done is we developed, in conjunction with the staff, as well as our root cause team, we develop a written action plan on what's going to be done with those samples and results will be shared with the staff as well as MRP and anyone else who wants those and that will be done, as I described earlier. Right now we have two nozzles in the cavity. We're going to actually make a trip down to Lynchburg, Virginia which is where those three pieces are stored right now and develop a written action plan on where to proceed as far as the testing that's going to be required to provide the industry as much information as we can.

MR. GUNTER: But I guess in gathering --

MEMBER FORD: Excuse me, could you just identify yourself?

MR. McLAUGHLIN: I'm sorry, Mark McLaughlin, First Entergy.

MR. GUNTER: I gather though that there is some concern with regard to sample size, that is currently available. As far as physical evidence that could be extrapolated further down the line. Am I correct? That --

MR. McLAUGHLIN: Well, the one piece of information that would have been nice and this is one thing that's kind of a thorn in my side because I was the project manager, but the one piece of information that looking back I wish we would have gathered is when we pulled nozzle number 3, the cavity was full of boron. If we had gotten some samples of boron out of that cavity it may have helped preclude some of the need for research as far as -- where there's some unusual chemical components that were at work there and it may have helped develop some of the corrosion rates.

MR. GUNTER: Okay, and just one final question. With regard to the cladding separation issue, I heard this morning that there was no evidence of separation, but that the dye penetrant test didn't do it or wasn't taken, so am I to believe then that the cladding separation issue is inconclusive?

MR. McLAUGHLIN: I've performed visual inspection and the reason that a dye penetrant test has not been done is because there will have to be some machine operation done on the outside diameter of that cavity sample and we will not do anything that would be considered destructive. It would be destructive to do that machining operation and we will not do anything destructive to that sample until a written sample plan has been issued and that's what we're going to be doing in the next two weeks. We're going to get with the staff and take a -- physically look at the cavity and that I would say that's going to be done of the tests that will be performed. However, we're not going to do anything that would destroy any evidence prior to everyone coming to a consensus on a written action plan to do those tests.

MR. GUNTER: Thank you.

MEMBER WALLIS: Now I'm curious. You said the cavity was full of solid material?

MR. McLAUGHLIN: When we pulled -- yeah, when we puzzled nozzle number 3, we had a camera that was underneath the head, so you could see when the nozzle was removed there was now we know it was a boron iron mixture. I guess what --

MEMBER WALLIS: I'm interested in how much water was in there.

MR. McLAUGHLIN: There wasn't anything that ran out. You couldn't tell that there was water there.

MEMBER WALLIS: It could have been --

MR. McLAUGHLIN: It maintained its shape.

MEMBER WALLIS: It could have been liquid boron, but then solidified, but it certainly wasn't in a liquid state at all. It was full of solid.

MR. McLAUGHLIN: That's correct. If you look at the video, it appears that it's carbon steel and you know, if you have an ant farm and you can see all the holes through the glass, that's what it appeared to be because there were so many little fissures and tunnels going through this boron that was -- and that was the pattern that we saw. I mean it really, from the camera view appeared to be carbon steel with some erosion.

MR. GROBE: I believe at that time you were 19 or 20 days after shut down. So for an extended period of time there had been no forcing function to force liquid into that area.

MR. McLAUGHLIN: Right.

MEMBER WALLIS: Yes, but it could have dried out or something.

MR. GROBE: Right, exactly.

CHAIRMAN APOSTOLAKIS: I think we have to move on. Are there any other comments from the public?

Yes sir?

MR. HORNER: Dan Horner from McGraw-Hill Nuclear Publications.

Yesterday, one of the EPRI representatives made the comment about, I think it was about GEL 8805, that it's a good plan if it's implemented properly. So in that context, I guess my question is as there's been quite a lot of discussion about the inspection plans that are being developed by the industry and NRC. Can someone say what discussion there has been about ensuring proper implementation of them and alternatively, is there consideration of a possibility that the current inspection regime is adequate on paper, but simply has to be implemented and enforced more effectively?

MR. GROBE: A number of responses. First off, as soon as the information notice was issued on precursors to this type of corrosion, specifically the containment air cooler cleanings and the rad monitor filter clogging, I can speak for Region 3. We went back and evaluated those issues at the plants in

Region 3. I believe the other regions also did, to confirm that there were no precursors that existed and that's consistent and in line with the activities that Bill Bateman's staff were doing following up Bulletin 2002-01.

Secondly, we talked about paper reviews. Our inspections do involve some paper reviews, but there's much in field activities and independent observations in the field, so it's not just a paper review, that the inspection program does. I believe part of the Lessons Learned Task Force and our Inspection Program Management Branch as well as the Lessons Learned Task Force is evaluating the appropriateness of our inspection activities in these areas and whether they need to be augmented. I don't know if either Ed or Bill want to talk to this.

MR. BATEMAN: The only other thing I'd like to add is that the 60-day response of the Bulletin 2002-01 asks the licensees to discuss their boric acid inspection program, so we do have those responses and are reviewing them at this time.

MR. HORNER: Thank you.

MR. MATTHEWS: This is Larry Matthews from the MRP. Also, the MRP is planning a workshop, I believe some time this summer to get together with all the utilities and look at best practices in the boric acid walkdown program and try and come up with what are the best ways to implement this type of program in the industry and that workshop will be taking place this summer.

CHAIRMAN APOSTOLAKIS: Any other questions or comments from members of the public?

Well, gentlemen, thank you again for coming here.

MR. GROBE: Thank you.

CHAIRMAN APOSTOLAKIS: We'll recess until 11:00.

(Whereupon, the proceedings went off the record at 10:44 a.m. and resumed at 11:02 a.m.)

CHAIRMAN APOSTOLAKIS: Okay. The next topic is technical assessment of Generic Safety Issue (GSI) 189, Susceptibility of Ice Condenser and Mark III Containments to Early Failure from Hydrogen Combustion During a Severe Accident.

Our leader on this subject is Dr. Kress. Tom?

MEMBER KRESS: Thank you, Mr. Chairman.

I remind the committee members that this issue has to do with ice condenser and Mark III containments that during a severe accident will effectively condense the steam and concentrate hydrogen. And in order to control the hydrogen concentrations so that you don't get detonable concentrations, these are -- these type of plants are provided with igniters located throughout the containment area outside the ice condenser chamber and in the drywell for Mark IIIs.

These igniters also have associated with them some fans to be sure you don't -- that the hydrogen can get to the igniters, and that you don't stratify and create pockets of high concentrations.

So the issue is, though, that one of the severe accidents that contributed a great deal to the risk is a station blackout. The igniters and the fans are powered by AC power, and in a station blackout you lose that power. So the issue before us is: should igniters and fans for ice condenser plants and Mark IIIs be equipped with backup power in the event of a station blackout accident.

And this -- if it were so required, this would constitute a backfit. And the staff is required to make a regulatory analysis for backfits. The research has done this, and this will -- what we'll hear about today is the regulatory analysis backfit for possibly some options on backup power.

I would want to point out that on this subject we have received comments from a member of the public, Ken Bergeron, and he couldn't be here today for other commitments, but I think David Lockbaum has agreed to speak to his comments.

And, in addition, we have comments from a member of the public living near Watts Bar, which is an ice condenser plant, Ms. Ann Harris. And I think there is a TVA employee -- I'm sure there is -- Bob Bryan, who would like to make a few comments. So we have a busy schedule ahead of us.

With that, I'll turn it over to the staff to give their presentation.

MR. NOTAFRANCESCO: Al Notafrancesco. I'm Task Manager for GSI-189. We are doing this in the Office of Research. I'm in the Safety Margins and Systems Analysis Branch.

Okay. GSI-189 has to do with Mark IIIs and ice condensers, as said earlier. Basically, in the process of risk informing 10 CFR 50.44, we had a series of Commission papers and gave us the status and the staff plans. We got an SRM December 31st, told us to resolve GSI-189 expeditiously. So that's what we plan to do.

In February 2002, this past February, it passed the generic issue screening process. We quickly generated a task action plan, and we are currently completing a technical assessment. And basically I'm going to present you an overview of the technical assessment.

Just to give a sense of what the population of plants we're talking about, PWRs with ice condenser containments, there's nine reactors, four dual units, one single unit. There's four BWR plants, four single units. In the 1980s, these plans were retrofitted with AC-powered igniters to mitigate the consequences of copious amounts of hydrogen as part of the post-TMI action.

So, but there has always been a long issue about the performance in station blackout, because they're not available, and that's where we're going.

This is just a schematic of the two types of plants. What they have in common -- their pressure suppression containments, their intermediate volumes between 1.2 and 1.5 million cubic feet. One uses ice, one uses water.

MEMBER KRESS: Would you point to where the igniters are likely to be located in those?

MR. NOTAFRANCESCO: Okay. The igniters are judiciously located pretty much everywhere except the ice chest and the lower plenum here. Everywhere else there is igniters. For the Mark III, there's more igniters, so they're pretty much particularly below the ACU floor where there's potential for hydrogen buildup.

Okay. The objective of this work was to justify if a backup power supply is warranted. Two aspects we looked at -- cost benefit guided by the NRC-prescribed methods.

MEMBER WALLIS: Excuse me. You said just the igniters. How about these fans, which may be a pointed issue?

MR. NOTAFRANCESCO: It's included in here.

MEMBER WALLIS: Do you mean igniters and fans or fans or both or either or --

MR. NOTAFRANCESCO: Well, we've considered the fans, and we feel --

MEMBER WALLIS: You've already discarded them as a need?

MR. NOTAFRANCESCO: Well, I --

MEMBER WALLIS: This just says igniters.

MR. NOTAFRANCESCO: As part of our analysis, we pretty much discarded them.

MEMBER WALLIS: Okay.

MR. NOTAFRANCESCO: We did consider them.

CHAIRMAN APOSTOLAKIS: So the power supply will be to igniters only.

MR. NOTAFRANCESCO: That's the bottom-line recommendation.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER ROSEN: And you will explain to us why the fans are not needed to --

MR. NOTAFRANCESCO: And we'll get to that. And that's why I have it here. Cost-benefit analysis guided -- based on looking at fans, not --

MEMBER ROSEN: Pardon me. But it's a little bit unclear from that statement that you --

MR. NOTAFRANCESCO: Okay. But here. For ice condensers, perform an updated severe accident analysis demonstrating igniters alone are adequate. I didn't get to that line yet.

MEMBER WALLIS: So your purpose there -- you don't say anything about fans here at all. It looks as if you've already decided --

MR. NOTAFRANCESCO: Fans are imbedded in here.

MEMBER WALLIS: They are? Okay.

MR. NOTAFRANCESCO: But we -- we'll get to it. I'm just trying to walk you through the history a little bit, too, of the action plan. We didn't discard it at the beginning, but as time went on -- okay. So then we executed the task action plan, and then briefing the committee, and we want to send our findings to --

MEMBER WALLIS: It's a poor objective. I mean, it looks as if you're asked to prove that igniters alone are adequate. It's just a poor starting point. It's almost that you start with -- that igniters alone are adequate.

CHAIRMAN APOSTOLAKIS: Well, that was not part of the original objective, I hope.

MR. NOTAFRANCESCO: Well, we've got to understand this is melted with the Mark IIIs, and the fans aren't an issue with that. So the fans are a little issue with ice condensers but not for the Mark III. So we've got to put it in perspective. It's a larger -- dealing with two different classes of containments.

Okay. Our approach for expeditious resolution was to use existing studies and to assemble a support team with contractor assistance. We supplied you about three or four weeks ago a package, and each of the contractors provided a report. And one component is the cost analysis, the benefits analysis, and the plant analysis, specifically on the fan performance and the igniters alone aspects of it.

MEMBER WALLIS: But, again, you say use existing studies. You've got to determine that they're adequate first.

MR. NOTAFRANCESCO: Well, what I -- I'll get to it and try to differentiate. There's some ongoing work. But before I get to the analysis, I'll get to some of the preliminary -- the aspects related to the cost analysis first.

CHAIRMAN APOSTOLAKIS: Now, what percentage of the large early release frequency does the SBO contribute to? Is it one of the major contributors?

MR. NOTAFRANCESCO: Well, hopefully, our benefits analysis will quantify that.

CHAIRMAN APOSTOLAKIS: Well, you'll probably lift it from existing studies. You're not going to do it yourself. That's part of the --

MR. LEHNER: In the --

CHAIRMAN APOSTOLAKIS: Who are you?

MR. LEHNER: John Lehner from Brookhaven National Lab. In the March 3 analysis, which was based on the -- on NUREG-1150, the SBO was 90-some percent of the total core damage frequency. In the ice condensers, it varies, but it's still a significant part of the total core damage frequency.

CHAIRMAN APOSTOLAKIS: But you are not dealing with core damage frequency here. You are really producing LERF.

MEMBER KRESS: That's part of it. Core damage frequency is --

CHAIRMAN APOSTOLAKIS: Yes, but, I mean --

MEMBER KRESS: -- a component of LERF.

CHAIRMAN APOSTOLAKIS: I know. But what was the percentage to LERF?

MR. LEHNER: Well, if you -- for Catawba, the conditional containment failure probability was about .3. So probably about 30 percent of that's SBO frequency.

MEMBER KRESS: Yes, that's not a conditional early, but --

MR. LEHNER: Conditional SBO.

MEMBER KRESS: Yes. But conditional early is a little lower than that, but it's a substantial contribution of the LERF.

MR. LEHNER: Okay. Thanks.

MR. NOTAFRANCESCO: Okay. As part of the cost benefit, we are trying to get a handle of what the cost is and what kind of configuration can one construct that would enhance plant capability. And we've concentrated on a pre-staged design, which is a stationary diesel that could be hooked up when needed, and then we also looked at an off-the-shelf option where a portable generator is put in place with minimum plant modifications. So we're trying to run a gamic of what is an optimal arrangement considering cost.

MEMBER WALLIS: What's the difference? They're both going to be there all the time. It's just that one is cheaper than the other.

MR. NOTAFRANCESCO: Right. But that is needed to --

MEMBER WALLIS: You're not going to move the portable diesel generator around.

MR. NOTAFRANCESCO: Well, the portable diesel generator is hopefully small enough that there will be more of them, and they'll be available --

MEMBER WALLIS: This is one you can buy in a hardware store or something, instead of going to some nuclear supplier.

MR. NOTAFRANCESCO: Right. They will be more of them, more diverse places. There will be more --

MEMBER SIEBER: Does that mean somebody has to go out and buy these things? Here's an accident. Will you send a clerk down to the store and say, "Get me one of these"?

MR. NOTAFRANCESCO: Well, that's -- they're small. They're about 5 KV generators for igniters.

VICE CHAIRMAN BONACA: Well, I think if I can offer a suggestion, I mean, looking ahead to your slides 14 and 15, they really provide answers to all the questions you are getting right now. I would suggest that you go through this analysis first, and then we'll understand why you're making certain equipment choices.

You know, you have presented some options. It seems to me that those two slides explain why you, for example, feel that igniters alone are effective. And then, in that case --

MR. NOTAFRANCESCO: Well, again, we're isolating on ice condensers. We'll looking to try and do both classes of plants. I'm trying to walk through this.

VICE CHAIRMAN BONACA: All right. I just -- all right. That's fine.

MR. NOTAFRANCESCO: Again, there's the cost-benefit component that's necessary to meet --

VICE CHAIRMAN BONACA: Okay.

MR. NOTAFRANCESCO: -- to promote any sort of backfits. I wanted to just -- I'll quickly go through this thing and --

VICE CHAIRMAN BONACA: Sure.

CHAIRMAN APOSTOLAKIS: So why is the low-cost option more reliable during an earthquake?

MR. NOTAFRANCESCO: Well, okay, that's my next slide. There's some judgment in this. The pre-staged design, if it's designed for external events, clearly, the costs start to skyrocket. We do expect some survivability even -- or a subset of the external events. So it's not going to be 100 percent qualified, but it does provide us some capability.

CHAIRMAN APOSTOLAKIS: So, again, now we're bringing up the issue of external events. How much is -- are these contributing to station blackout?

MR. NOTAFRANCESCO: They could be about a half. External blackouts could contribute roughly a half, I think we assume.

MR. LEHNER: Yes. For the ice condensers, the external core damage -- the external SBO frequency was about two-thirds of the internal station blackout frequency.

CHAIRMAN APOSTOLAKIS: When you say "external," do you mean earthquakes primarily?

MR. LEHNER: Primarily earthquakes, but I think there is also some high winds. Yes, but it's primarily earthquakes, I believe.

MR. NOTAFRANCESCO: Again, this judgment on the low-cost, no permanent structure, and setup would occur after the initial impact of the external event. Portable diesel may come from multiple diverse locations. Attributes may --

CHAIRMAN APOSTOLAKIS: I don't understand that sentence. Is that clear? No permanent structure, setup would occur?

MR. NOTAFRANCESCO: Well, there's a -- since this option --

CHAIRMAN APOSTOLAKIS: Do you mean damage?

MR. NOTAFRANCESCO: Well, in the pre-staged design, there is the assumption of having a concrete pad and having a small doghouse off the aux building. So it's a permanent structure.

CHAIRMAN APOSTOLAKIS: Oh, I see.

MEMBER ROSEN: The setup would occur after --

CHAIRMAN APOSTOLAKIS: There would be no permanent structure, and the setup would occur after the initial --

MR. NOTAFRANCESCO: Right.

CHAIRMAN APOSTOLAKIS: Oh. See, I'm thinking sometimes --

MR. NOTAFRANCESCO: Well, I'm --

MEMBER WALLIS: The difference is build a building or just wheel up a generator and hitch it down.

MR. NOTAFRANCESCO: Right. I mean, that's what this was. Use of portable with minimum permanent modifications.

Okay. Putting numbers to this concept, we --

CHAIRMAN APOSTOLAKIS: Well, let's understand this a little bit, though. You are saying it would occur after the initial impact of the external events. So we presume that the humans will perform as anticipated, as expected, after a major earthquake? Or you didn't address that issue?

MR. NOTAFRANCESCO: Well, we assumed there will be an army of guys trying to recover from the damage, so --

CHAIRMAN APOSTOLAKIS: And those guys have not been affected by the fact that they have just been through a major earthquake.

MR. NOTAFRANCESCO: Well, you know, we're not saying it's going to be 100 percent effective through all the credible earthquakes, but at least a significant fraction.

CHAIRMAN APOSTOLAKIS: But you have some human reliability numbers in the calculations? Because, I mean, in the one instance you assume that the earthquake will affect the pre-staged design --

MR. NOTAFRANCESCO: Well --

CHAIRMAN APOSTOLAKIS: -- which is reasonable. But then, you know --

MR. NOTAFRANCESCO: Well, we -- in the numbers we do say the reliability of the portable setup is a little less than the pre-staged setup. But we also use judgment to say it may be compensated by the fact that the off-the-shelf approach is more versatility to respond to external events and may compensate for that negative in which --

CHAIRMAN APOSTOLAKIS: Well, there is more versatility, but we are relying now on the crew.

MEMBER LEITCH: You have some considerable time to do this.

MR. NOTAFRANCESCO: Two, three hours, several hours.

CHAIRMAN APOSTOLAKIS: Oh, you do?

MR. NOTAFRANCESCO: Yes. At least several. It depends on your sequence.

MEMBER LEITCH: I thought I remember seeing 48.

MR. NOTAFRANCESCO: Well, we wanted the --

CHAIRMAN APOSTOLAKIS: Wait a minute. What happens during those 48 hours?

MR. NOTAFRANCESCO: The 48 hours are used as an assumption --

CHAIRMAN APOSTOLAKIS: Are you also in a state of damage to the core? Has the core been damaged?

MR. NOTAFRANCESCO: In these cases they are, because you're trying to deal with hydrogen. You're trying to get the igniters powered.

CHAIRMAN APOSTOLAKIS: Okay. Sure. So the fact that I have 48 hours by itself doesn't --

MR. NOTAFRANCESCO: No, I'm not saying that's --

CHAIRMAN APOSTOLAKIS: -- help me very much because I have a core damage event. So --

MR. NOTAFRANCESCO: You don't have 48 hours. The 48-hour number had to deal with the length of time of putting the diesel in a tank. It was just part of the estimate of having them working for 48 hours after setup. That's where the 48 hours comes in.

CHAIRMAN APOSTOLAKIS: Okay.

MR. NOTAFRANCESCO: But you're in a degraded core -- core melt sequence. You have time to -- to set this up before you -- the hydrogen is generated. That's the concept of --

MEMBER KRESS: There's a station blackout rule that requires the plants to have backup diesels already. These are big diesels to power safety-related equipment. Why can't the igniters and fans be hooked to those diesels?

MR. NOTAFRANCESCO: That could be possible. That could be --

MEMBER KRESS: Was that an option that was --

MR. NOTAFRANCESCO: That could be an option for the utility, clearly. We just crossed it out based on an independent backup.

MEMBER KRESS: An independent backup.

MR. NOTAFRANCESCO: Right. There's other demands on other things. I don't know if we could --

MEMBER ROSEN: The problem, Tom, is if you hook them to the station's safety-related diesels, you're assuming those diesels are not functional in station blackouts.

CHAIRMAN APOSTOLAKIS: Right. They're

out.

MEMBER ROSEN: That is the assumption. Station blackout means you don't have AC power either offsite or onsite.

VICE CHAIRMAN BONACA: So you have the station blackout, and now you have core damage, and you have hydrogen.

MEMBER ROSEN: Now, the question is: why would you assume, given that, that these would work? I mean, don't you then say it'll be -- there's another layer through --

VICE CHAIRMAN BONACA: Right.

MEMBER ROSEN: -- but it -- one says with the assumption of station blackout it means you don't have AC power. And here you say, okay, we're going to provide AC power.

VICE CHAIRMAN BONACA: Well, I mean, do you have a redundant system, an additional system? I mean, how many layers are you going to --

MEMBER ROSEN: I understand. I understand that this is --

CHAIRMAN APOSTOLAKIS: No. But, I mean, the reason why you are in an SBO situation is that something very dramatic has happened.

MEMBER ROSEN: Exactly.

CHAIRMAN APOSTOLAKIS: And I think the question, you know, why should these additional diesels survive, then, is a good one.

MEMBER ROSEN: Well, and I think the focus on earthquakes is completely wrong. I mean, the issue is not really earthquakes, although that's one of the ways you could get to station blackout. But, you know, high winds and flood are -- seem to me also very important.

CHAIRMAN APOSTOLAKIS: Yes. They mentioned that they are -- those are --

MEMBER WALLIS: I have another question. Why does the diesel have to run the 48 hours? Because the igniters are only used once, aren't they? You need a certain amount of --

CHAIRMAN APOSTOLAKIS: Well, no, no, no.

MEMBER WALLIS: -- energy, or do you keep them clicking away all the time?

CHAIRMAN APOSTOLAKIS: That's not what he said. He said you have 48 hours to connect to diesel.

MEMBER ROSEN: Allen, do you want to try again?

MEMBER WALLIS: He needs a tank. He's going to --

MR. NOTAFRANCESCO: The tank of 48 hours

was just an assumption just to come up with an estimate. It could be even less than that. But the costs associated with a tank covering 48 hours or 24 hours is quite small.

MEMBER WALLIS: It reminds me of something that goes off all the time.

MR. NOTAFRANCESCO: That continuous hot points --

MEMBER WALLIS: Continuous operation. Okay. Okay. It's not something that senses --

CHAIRMAN APOSTOLAKIS: Anyway, can we go back to seven, because I don't think I got an answer to my question. This seven. You have in there the study that you guys did has some probabilities that a setup would not be correctly done?

MR. NOTAFRANCESCO: Yes.

MR. ROSENTHAL: Can we just play -- this is Jack Rosenthal. You or I -- I think we need, just so everybody is clear, at time T zero you have Hurricane Andrew hit, or you have an earthquake hit, etcetera, real events that cause loss of offsite power. You hypothesize common mode failure of the diesel generators. The source of the power would be diverse, not subject to that common mode which would dominate the event.

Given blackout, either several hours will go by in which you live off your batteries, your station battery, six, eight hours, with supplying water to your steam generators from your steam driven auxiliary feedwater pumps, or sometimes people will postulate failure of that steam driven pump which moves the sequence up in time.

At some point, so many hours into the event, you start uncovering the core, heating the core, generating hydrogen. You'd like the igniters to be continuously powered, so that they can burn off the hydrogen in small amounts over a period of hours that's being created. And the emission time for this whole process that was assumed -- that's the 48 hours that he's talking about in which -- during which, you know, it's -- one could be -- so we -- I --

CHAIRMAN APOSTOLAKIS: I understand that.

MR. ROSENTHAL: -- I just wanted some clarity on the sequence.

CHAIRMAN APOSTOLAKIS: How much time do I have?

MR. ROSENTHAL: To start.

CHAIRMAN APOSTOLAKIS: To start.

MR. ROSENTHAL: Well, if the batteries are running and the auxiliary feedwater pump is running, then things shouldn't get bad for, let's say, eight hours.

CHAIRMAN APOSTOLAKIS: So I can stop having those --

MR. ROSENTHAL: But that's not to say that the station crew would be dedicating its resources to getting this little generator connected up. I would think that they would be dedicating their resources to getting the main power back on. So at some point in the process, the tech support center, the coping crew, makes the decision that they have to divert resources to get out to do these heroic actions and somehow get this alternate source connected. I think that a .8 was assumed.

MR. MEYER: Yes. Jim Meyer from ISL. The low-cost option has some down sides, and the functional reliability we're assuming for that was about .8. The majority of --

CHAIRMAN APOSTOLAKIS: And .8 is the probability that they will do it successfully.

MR. MEYER: Yes. It would be the non --

CHAIRMAN APOSTOLAKIS: Within whatever, four, five, six hours.

MR. MEYER: Within the required period of time, which we were given guidance on as being between two and four hours. The --

MEMBER ROSEN: How does that compare to the higher cost option?

MR. MEYER: Yes. The pre-staged we were assuming a reliability of about 90 percent. And the difference between the 90 percent and the 80 percent is basically the human reliability issue because the pre-staged is a matter of -- of everything is set up ahead of time.

You really have to initiate the start of the generator and hook up to the igniters, whereas the low-cost option you have to actually move the generator to the place where it's to be hooked up to the igniters and then power the igniters. So we were assuming --

CHAIRMAN APOSTOLAKIS: You didn't do any uncertainty analysis? I mean, it was a point estimate based --

MR. MEYER: We didn't do any uncertainty analysis.

VICE CHAIRMAN BONACA: Would you have better survivability for the low cost, given that you can utilize protected areas to maintain it rather than the installed one, which is going to be installed in some area where, as you are saying, because of cost reasons you are not protecting it as well. I'm just asking if the protection issue is considered here.

MR. MEYER: Well, you're talking now about external events?

VICE CHAIRMAN BONACA: Yes.

MR. MEYER: The context of external events?

VICE CHAIRMAN BONACA: Yes.

MR. MEYER: Well, the pre-stage that we analyzed, we analyzed both assuming only internal events and then we considered the added cost of external events. For low cost we didn't do that type of direct analysis.

But these low-cost options have a history of being very robust and capable of accommodating, for example, vibrations from seismic events. So the expectation is a combination of robustness of the devices and their location would allow for accommodation of some external events that pre-stage wouldn't.

VICE CHAIRMAN BONACA: And so that's why I was asking the question, because I can imagine that when you were making a point in the pre-stage cannot be totally protected because the cost would be excessive, so you have -- a more costly option, however, is not fully protected.

And then that's why I was trying to understand the least expensive option, which is portable can be better protected because you can put it somewhere where you have protection. So it is an issue that is not reflected in the .8 -- or .9, is it?

MR. MEYER: The .8 and .9 were just assuming internal events.

VICE CHAIRMAN BONACA: Doesn't reflect that issue. Okay.

MEMBER WALLIS: .8 to .9 is just pulled out of the air? The actual reliability of the generator used in a construction trade is probably 99 percent.

MR. MEYER: The reliabilities of the actual generator are very high.

MEMBER WALLIS: Yes. Very, very high.

MR. MEYER: It's a combination of the reliability -- the unreliability, unavailability, and the human factors.

CHAIRMAN APOSTOLAKIS: The human factors.

MR. MEYER: The human factors drives both numbers.

CHAIRMAN APOSTOLAKIS: Now, why do you have to move it you say? I mean, why isn't it where it's supposed to be already?

MEMBER ROSEN: Well, that's one option, right?

MR. MEYER: No, this is the pre-staged --

CHAIRMAN APOSTOLAKIS: No, the portable.

MR. MEYER: Let me point out that we're not trying to do a future licensee's work in designing a system. We're just doing a feasibility study that said if you were to have a five, seven kilowatt pre-staged diesel in some sort of doghouse, or if one were to have a fancy Honda generator on the back of a pickup truck, what might it cost, and how efficacious might it be, with the details of the design left to the -- to some future licensee, should they be required to do this?

So, and what we recognized -- what it was -- I think that Honda generators, or whatever they are on the back of pickup trucks, are very reliable. They get bounced around all the time. The workman throws it off the back of the truck, drops it on the floor, pulls the ripcord, and the thing starts.

However, he's got to think to do it. He's got to divert scarce crew resources to take the action. He's got other parities to do. You've got to get this thing started, and then somehow you've got to get power -- some temporary rig of power onto the switch gear, which is going to the igniters. And it's all those human actions that would dominate.

CHAIRMAN APOSTOLAKIS: Okay. Let's move on.

MR. NOTAFRANCESCO: Here are the specific numbers of the low-cost option ice condenser, Mark III, pre-staged, and the difference here is basically to accommodate multi -- two-unit sites in which you could share some costs in the pre-staged. Again, Mark IIIs, they are only single-unit plants.

Also, give you a sensitivity if we were to make the pre-staged more robust to deal with external events. You can see the cost dramatically starts to go up.

MEMBER ROSEN: What does this "with ext-qual" stand for?

MR. NOTAFRANCESCO: External qualification.

MEMBER ROSEN: Qualification against external events.

MR. NOTAFRANCESCO: Right. It's just maybe several times a factor on the baseline cost.

MEMBER WALLIS: It's also the generator is only like $2K, I got from your report, so the rest of it is --

MR. NOTAFRANCESCO: Well, there's a lot of components to an engineering installation.

MEMBER WALLIS: So it's not just going to be driven off and take -- it's going to be --

VICE CHAIRMAN BONACA: I don't understand. You are showing there NRC?

MR. NOTAFRANCESCO: Yes, the NRC --

VICE CHAIRMAN BONACA: That's -- okay, that's --

MR. NOTAFRANCESCO: There's two components.

VICE CHAIRMAN BONACA: I understand now.

MR. NOTAFRANCESCO: Industry, of course, and it's in the document, and NRC. And the assumption here is that the rulemaking, of course, associated is minimal. But it's --

CHAIRMAN APOSTOLAKIS: So we do things that cost only $13,000. There are certain things we do that cost only $13,000?

MR. NOTAFRANCESCO: Well, that's why this is -- we're linking it on this.

MEMBER WALLIS: This is per installation. This is for the whole fleet.

MR. NOTAFRANCESCO: Per unit. This is per unit.

Okay. Now the benefits analysis on ice condensers and the Mark IIIs. This is the cost; this is the benefit component. What we did, again, to expedite this, we -- and to use existing information, we have -- the agency is required, as part of the license renewal, to have -- to look at severe accident mitigation alternatives.

And as coincidences the past few months took place, we understood that the Duke plants, McGuire and Catawba, came in with submittals. And one of the alternatives is looking at backup power to the igniters and fans. So we looked at their averted costs, and that's where I get this table from is that.

It's plant-specific based on the PRA. It was contrasted against an NRC or a Sandia report on using different containment conditional failure probabilities. And here's the sensitivity associated with it. These costs -- they look at discount rates. The base is seven percent. Three percent is the sensitivity, and looking at useful --

CHAIRMAN APOSTOLAKIS: What exactly are you calculating?

MR. NOTAFRANCESCO: You are converting the person rem of -- the averted person rem to a monetary cost.

CHAIRMAN APOSTOLAKIS: But in the report it also says that you are looking at land contamination.

MR. NOTAFRANCESCO: That's filtered into this, right?

MR. LEHNER: There are offsite property costs that are --

CHAIRMAN APOSTOLAKIS: No, no, no. You have to come up here. You have to go to a microphone somewhere.

MR. LEHNER: John Lehner from Brookhaven. There are offsite property costs that are in addition to the $2,000 per person rem calculation.

CHAIRMAN APOSTOLAKIS: Right. So these are here?

MR. LEHNER: These are included, yes.

CHAIRMAN APOSTOLAKIS: Okay.

MR. LEHNER: So it's both the $2,000 per person rem costs as well as the monetary costs for evacuation, cleanup, decontamination, whatever.

CHAIRMAN APOSTOLAKIS: So you assume a certain period of years that will be required to decontaminate some --

MR. LEHNER: Yes. Actually, those costs

are based on the consequence analyses that were done with NUREG-1150 for an ice condenser plant, and for -- well, in this case, for the ice condenser plant. Yes.

MEMBER KRESS: There's a NUREG document that tells how to -- gives real guidance on how to convert this cost and discount it for current worth. And we reviewed that one time and passed judgment and said we thought that was good guidance. And they followed that NUREG guidance.

CHAIRMAN APOSTOLAKIS: But did both the licensee's and the NRC's analysis consider the same kinds of costs? Because the difference is fairly large.

MR. NOTAFRANCESCO: This in here?

CHAIRMAN APOSTOLAKIS: McGuire in the NUREG, yes. Are you looking at the same --

MR. NOTAFRANCESCO: Well, this is a plant-specific, and this was a sensitivity that Duke did based on the conditional probabilities included in this NUREG.

CHAIRMAN APOSTOLAKIS: Sensitivity, where is it? No, it's discount rate.

MR. NOTAFRANCESCO: Well, the discount rate is based in here.

CHAIRMAN APOSTOLAKIS: The range. So even the high point, $248K, is significantly lower than the $678K.

MR. LEHNER: Can I maybe explain that?

CHAIRMAN APOSTOLAKIS: Yes.

MR. LEHNER: I think the -- what you're looking at in that table is -- both of those columns are the plant's calculations. Right, Allen?

MR. NOTAFRANCESCO: Right. Yes.

MR. LEHNER: No, both. The left and the right. The difference is that in the right column they use the failure -- the containment failure probabilities from NUREG/CR-6427. The NRC calculations actually -- or the calculations that were done for NRC by BNL are not shown there. They are similar to what on the right.

CHAIRMAN APOSTOLAKIS: Oh. So this is both for the licensees.

MR. LEHNER: Right. And the difference -- I think the main difference is that they used containment failure probabilities reported in NUREG-6427.

CHAIRMAN APOSTOLAKIS: And in the first one they use their own.

MR. LEHNER: Yes.

MR. NOTAFRANCESCO: But in your work you

confirm pretty much it's --

MR. LEHNER: Yes.

MR. NOTAFRANCESCO: -- high up there anyway, and that's what I said.

MR. LEHNER: It's pretty similar to that, yes.

MR. NOTAFRANCESCO: But it had nothing to do with the -- I mean, the variation has to do with discount rate.

MR. LEHNER: Right.

MR. ROSENTHAL: Excuse me. George, just to be absolutely sure, take the core damage frequency attributable to station blackout, multiply that by the delta change in containment failure attributed to whether you're going to have igniters or not, calculate the associated person rem for that event, and then convert that to dollars. So we're looking at averted person -- monetized averted person rem incremental.

CHAIRMAN APOSTOLAKIS: Plus contamination.

MR. ROSENTHAL: Yes.

CHAIRMAN APOSTOLAKIS: Yes.

MR. ROSENTHAL: Okay.

CHAIRMAN APOSTOLAKIS: Yes, I understand.

MR. NOTAFRANCESCO: That was the ice

condenser summary. This is the Mark III. Since we didn't have SAMAs and plant-specific numbers probably to work on, Brookhaven used the IPE specific to Grand Gulf, took the perspective and insights from 1150, and came up with a range of averted monetized costs.

CHAIRMAN APOSTOLAKIS: Now, give me an example of an early failure that is averted. You say all early failures are averted.

MR. NOTAFRANCESCO: Due to hydrogen combustion. Any --

CHAIRMAN APOSTOLAKIS: Yes. I mean, what kind of failures are we talking about? How they --

MR. NOTAFRANCESCO: Containment failures. That means they are early containment failures.

CHAIRMAN APOSTOLAKIS: Oh.

MR. NOTAFRANCESCO: They are early containment failures. Again, early failures are specific to the generic issues. The title of the generic issue is early --

CHAIRMAN APOSTOLAKIS: So you are eliminating early containment failure, right? That's what you're saying?

MR. NOTAFRANCESCO: Well, that's --

CHAIRMAN APOSTOLAKIS: From hydrogen combustion.

MR. NOTAFRANCESCO: Right.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER SIEBER: But if the igniters --

CHAIRMAN APOSTOLAKIS: So it's not all of them, just --

MR. MALLIAKOS: This is Asimios Malliakos from the staff, Research. We don't completely eliminate failures. I mean, we don't go completely down to zero. But let me give you an example. Let's say we have an RCS pressure at vessel break, lower RCS pressure. We can drive the probability from .2 to .01. So it doesn't go completely down to zero.

CHAIRMAN APOSTOLAKIS: And there is a rationale why you do that.

MR. MALLIAKOS: There is --

CHAIRMAN APOSTOLAKIS: Why is it .01? There must be some other possibility of failure, right? You are eliminating the failure -- you are reducing it by the probability of failure due to hydrogen.

MR. MALLIAKOS: Yes. Yes.

CHAIRMAN APOSTOLAKIS: So there are still other causes. That's what you're saying, and that's what --

MR. MALLIAKOS: That's right. We have

direct containment heating. We have other events that take --

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER KRESS: Okay. That's high pressure melt for --

CHAIRMAN APOSTOLAKIS: Not here.

MEMBER KRESS: Not very likely for Mark IIIs, but --

CHAIRMAN APOSTOLAKIS: Not in these containments, right? That was the whole point.

MEMBER KRESS: Well, yes, they are potential issues for both containments.

CHAIRMAN APOSTOLAKIS: Yes, John.

MR. LEHNER: Actually, let me make another clarification here. In the Mark IIIs, the igniters don't eliminate all early failures from hydrogen. In the high pressure scenarios, the vessel fails at high pressure. Then, at least according to the 1150 analysis, the igniters will not eliminate the --

CHAIRMAN APOSTOLAKIS: Do you still have high pressure scenarios?

MR. LEHNER: You still have high pressure scenarios, because in a -- you know, when you lose -- in a station blackout you will lose the ability to depressurize the vessel. And, therefore, you will have high pressure scenarios, in which case you have a whole bunch of other mechanisms that come in. One of them is DCH steam explosion.

CHAIRMAN APOSTOLAKIS: I thought that high pressure scenarios had been eliminated.

MR. LEHNER: Not for station blackout, because you eliminate -- you lose your ability to depressurize.

MEMBER WALLIS: This is something that hasn't been through a subcommittee?

MEMBER KRESS: No, we didn't have a subcommittee on this one.

MEMBER WALLIS: So no subgroup of the committee has had a chance to really dig into the rationale for all of these things?

MEMBER KRESS: Other than we were supplied with the documentation to read.

CHAIRMAN APOSTOLAKIS: So the dominant contributor is -- in station blackout is low pressure scenarios, but the others are not eliminated.

MR. MALLIAKOS: Yes. That's for the averted benefit. That's the low pressure.

CHAIRMAN APOSTOLAKIS: Okay.

MR. MALLIAKOS: The high pressure, it doesn't make much of a difference. There is no difference.

CHAIRMAN APOSTOLAKIS: But it's not a major contributor here on these containments.

MR. LEHNER: No, it is. I mean, one of the reasons why you see less of a benefit for the Mark IIIs is because the igniters will only help you in the low pressure scenarios, and the high pressure scenarios will not benefit from the igniters. That's why you see a much lower benefit here than you did for the ice condensers.

CHAIRMAN APOSTOLAKIS: It would have been nice to see some event trees here, you know? But it's too late now.

MEMBER KRESS: They're in the document.

MR. NOTAFRANCESCO: They're in the document.

CHAIRMAN APOSTOLAKIS: Well, this information is in the document, too, right? And yet it is also on slide 10.

MR. NOTAFRANCESCO: I'll talk to Asimios later.

I just want to give a sense of looking at other plant-specific parameters that are important to the values of monetized benefit, and looking at the other three Mark IIIs, give you a sense that Grand

Gulf is on the low range compared to these guys -- these other -- so we're looking at a plant-specific sample, but we're trying to look at the whole range of plans by something like this.

CHAIRMAN APOSTOLAKIS: What's the SBO frequency ratio?

MR. NOTAFRANCESCO: In relationship to Grand Gulf, since we did those calculations based on Grand Gulf, we wanted to see what other parameters will affect the monetized cost. And one of the things is the SBO ratio, and it's the population -- the difference in population and frequency will influence those numbers.

And on the cost-benefit analysis, this is many lines here. Basically, what I did here was put the benefits on top, the different ranges for the classes of plants. The relationship of the low cost and the pre-stage fix if one included external qualification of fans were more in this range. And this is why we gravitated to the low-cost option is there's margin related to the ice condenser, but it's marginal with the Mark IIIs, at least for some of them.

MEMBER WALLIS: What's the benefit to NUREG-6427? I don't understand that.

MR. NOTAFRANCESCO: Well, that's been quoted a lot, so I just put it in here as a sensitivity.

MEMBER ROSEN: Pardon me, but I'm used to benefit to cost ratios, where one has a number.

MEMBER KRESS: That's a ratio.

MEMBER ROSEN: This is incomprehensible to me, this slide. Is it two to one or three to one or four to one or some -- 10 to one?

MR. NOTAFRANCESCO: Well, we're trying to explain it as uncertainties here. There's uncertainties in how one could come up with this, uncertainties here. There's uncertainty in how this was derived.

CHAIRMAN APOSTOLAKIS: I guess if you look at it, you are comparing the upper --

MEMBER KRESS: The location of the upper with the lower.

MR. NOTAFRANCESCO: Right.

CHAIRMAN APOSTOLAKIS: So what you're saying is that the one that passes the test is the one where the lower part, the cost --

MEMBER KRESS: Is to the left.

CHAIRMAN APOSTOLAKIS: -- is to the left of the benefit.

MEMBER KRESS: Right.

CHAIRMAN APOSTOLAKIS: And the only one that does that is the low cost.

MEMBER KRESS: Right. The cost benefits, and then for ice condensers. It's marginal for Mark IIIs, but it's clear for ice condensers.

CHAIRMAN APOSTOLAKIS: But for Mark III even those still --

MEMBER KRESS: It's still -- they call it -- it depends on the range.

CHAIRMAN APOSTOLAKIS: But this range is only due to the range -- not the real uncertainties, is it?

MR. NOTAFRANCESCO: The range is due to the types of plants, the Grand Gulf --

CHAIRMAN APOSTOLAKIS: Oh.

MR. NOTAFRANCESCO: That was my previous slide, which I have the different factors involved. Those factors were the multipliers to the $40K, and that's how I get the close to 200-plus.

CHAIRMAN APOSTOLAKIS: How does that work, by the way? I mean, on a generic basis --

MEMBER KRESS: I would have gone ahead and added them up, and added up the cost for each one, and looked at the total sum.

CHAIRMAN APOSTOLAKIS: But is this cost-benefit analysis done on a generic basis or a plant-specific?

MEMBER KRESS: Well, it's -- they try to do it on plant-specific because you're going to have specific plants that this backfit will apply to. So you have to take into consideration those specific plants, but you try to do it for that group of plants in a generic sense.

CHAIRMAN APOSTOLAKIS: Yes?

MR. ROSENTHAL: Let me just try a little bit. What we tried to depict as a bar for the ice condenser plants is a range of initiating frequencies and associated consequences for the range of ice condenser plants. For this large bar, NUREG/CR-6427, there's a study that was done on direct containment heating.

And that used a range of initiating event frequencies extracted from the NUREG-1150. No, I'm sorry, from the NUREG-1150. The ice condenser bar is a range from their own IPEs or their own plant-specific estimates.

On the costs -- so it tries to consider the range as a function of the plant. On the cost side, it's very difficult to come up with -- on a plant-specific basis, one plant might be $60K, and another plant might be $80K. I think you're just tricking yourself. Nobody really -- you know, one could estimate the cost, but one full well knows that when you go build these things that the cost can have a considerable range.

And so what you'd like to believe is that the -- is that your decision is reasonably insensitive to the variability in the assumptions. And the argument is made that the low-cost option for a range of what you think the cost might be is less than the range of benefits that you think that you'd get -- than the range of benefit. That's all you're trying to say.

MEMBER KRESS: Now, would you explain the -- with the external qualification, or with fans, does the "with fans" mean the low-cost option?

MR. NOTAFRANCESCO: No, it's centered with the pre-stage. When fans are involved, you need much more power, and nobody is going to lug a portable diesel around. So it's tied to the pre-stage configuration.

MEMBER KRESS: If you had to supply power to the fans, you wouldn't use a portable is what you're saying.

MR. NOTAFRANCESCO: No, it's more -- a larger capacity diesel. I was just using this as a sensitivity in relationship to the other possible options here.

CHAIRMAN APOSTOLAKIS: Given the plant-to-plant variability, I want to understand that. Maybe you answered it, Jack, but when you -- if you guys decide that, yes, installing the low-cost option is cost beneficial on a generic basis, would there be some plants out there that would do the same analysis, and based on their numbers would show that it's not cost beneficial for them and they would be exempted, or that's not allowed?

MR. ROSENTHAL: It wouldn't be allowed. Number one, it wouldn't be allowed because it's a generic rule.

CHAIRMAN APOSTOLAKIS: It's a generic.

MR. ROSENTHAL: Okay. But now look at -- the bar on the ice condenser, okay, it's the range of ice condenser plants. And what we're arguing is that the low-cost option is by about a factor of three or four better --

CHAIRMAN APOSTOLAKIS: So you don't expect that to happen.

MR. ROSENTHAL: -- for the range of

plants.

CHAIRMAN APOSTOLAKIS: Right. So, okay. Right. Is that something you apply to all cost-benefit analyses or for a range of plans, whatever option you are considering must be clearly beneficial? What if it's beneficial for 60 percent of them? Then, you cannot do anything about it, right?

MR. ROSENTHAL: No. Then, one should do a regulatory analysis. Okay?

Allen, just leave it up for a second.

When we were discussing this -- okay. Cost-benefit analysis is clearly a risk-based exercise.

CHAIRMAN APOSTOLAKIS: And it's different from regulatory analysis.

MR. ROSENTHAL: We are supposed to be risk-informed.

CHAIRMAN APOSTOLAKIS: Right.

MR. ROSENTHAL: So one of the inputs to a risk-informed decision process that you would do in a reg analysis, okay, is you would say things -- okay, I have my cost benefit analysis. I have -- do I want some degree of regulatory clarity, regulatory coherence?

Does it make sense to have different

requirements for ice condensers in Mark IIIs given that the underlying issue is hydrogen generation? And so that a risk -- in our view a risk-informed decision would be to have a requirement for the Mark IIIs and the ice condensers.

One could argue that on a strictly risk-based basis you don't make the argument on the Mark IIIs.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER LEITCH: Can we talk a little bit about the fuel for this thing? Have we thought about fire hazards associated with that? I mean, I guess in the low-cost analysis we're picturing a doghouse someplace out in the field with this diesel on wheels, right, and probably a 55-gallon drum on wheels? Is that the picture? No additional fuel in the reactor building?

MR. NOTAFRANCESCO: I don't think we're specific on that. Are we?

MR. MEYER: We considered the fuel --

MR. NOTAFRANCESCO: This is the low-cost option.

MR. MEYER: We considered the fuel requirements for both the pre-stage and for the portable options, and, for example, chose the diesel as compared to gasoline type of generators because the plant would be familiar with the safety precautions associated with diesel.

MEMBER WALLIS: Is this winter diesel or summer diesel fuel?

MR. MEYER: I'm sorry?

MEMBER WALLIS: Is this winter diesel or summer diesel fuel? If you have a diesel machine, you have to change your fuel in the winter in certain parts of the country. Otherwise, it won't work.

MR. MEYER: Well, that -- we didn't take that into account.

MEMBER WALLIS: I mean, there are certain things associated with running a diesel machine, which give rise to extra costs, like changing of fuel every year and making sure it runs and maintaining it.

VICE CHAIRMAN BONACA: Would you have the procedures on how to connect it? I mean, I'm beginning to get concerned about, you know, pre-staging sounds like some kind of operation where it's wired and connected and there are procedures and switches. And this thing here is sitting out there on some kind of track, and somebody has to make a guess on what -- I mean, what do we mean it's not pre-staged?

MR. MEYER: Part of the cost analysis was to -- in addition to the implementation cost was to consider the operational costs to the industry, to the licensee, and that included maintenance costs, training, all that would go into maintaining the availability of that piece of equipment when it would be needed. So that was all folded into the analysis and is part of our report.

VICE CHAIRMAN BONACA: You know, if you have no procedures in place very specific, if you have no clear understanding of the fuel for summer, winter, all these kind of things, you know, I don't give you the .8 credit, because you may have a measured event out there that creates such a confusion that in addition to that we have to have people guessing on what they have to do or so -- I mean, sure, I am comfortable about the set of estimates that you are giving out.

MR. MEYER: Well, as I said earlier, there are definite down sides to the portable low-cost option. And it would have to be worked out through proper procedures to make sure that this was an effective alternative. The actual hookup to the igniters themselves isolating the 1E class system in an appropriate way, all that would be done and installed ahead of time. It would be the actual -- moving the portable diesel to the site and the hookup that would be part of the --

VICE CHAIRMAN BONACA: So you have a degree of pre-staging already. You have a location where you have to bring it.

MR. MEYER: Oh, yes.

VICE CHAIRMAN BONACA: So specifically -- okay. So that's --

MR. MEYER: And that's all been part of the cost analysis. That was included in the cost analysis.

VICE CHAIRMAN BONACA: I think it is an important element that you are not -- you have already pre-staging of a kind.

MR. MEYER: Yes. It would be semi pre-staged.

MEMBER LEITCH: You got off -- you were going to answer my fire question, I think, and you got kind of off that. In other words, tell me where this fuel is going to be stored in the low-cost option and in the pre-staged option.

MR. MEYER: Well, the pre-staged option, the -- what was envisioned would be a fuel storage tank right next to the actual steam -- the actual diesel generator. For the portable, it would have to --

MEMBER LEITCH: That would be in the reactor building? This one?

MR. MEYER: This would be in a separate -- it's been referred to as a doghouse, a separate facility located outside the auxiliary building or the reactor building.

MEMBER LEITCH: Okay.

MR. MEYER: For the portable, the fuel storage would -- we would envision it to be part of the normal diesel fuel storage, and have that diesel fuel available for the purposes intended, for use with the diesel.

MEMBER LEITCH: So you have this event, and then the -- you -- from the main diesel tank or the day take, or something like that for the main diesels, you fill up a 55-gallon drum and wheel it up to the location and wheel up this portable diesel to the location, and by a pre-established set of procedures you connect this to the fuel, you connect this --

MR. MEYER: Yes.

MEMBER LEITCH: -- to the electric somehow by -- you know, you know exactly what you're going to do, you've practiced this, you connect --

MR. MEYER: Our procedure is in having that part pre-staged you would have -- you would be able to hook up to the igniters and be consistent with conforming to the isolation of the 1E system. You know, that's an important part of that.

MEMBER LEITCH: And while this is actually in use, you would then have this 55-gallon drum, if you will, of fuel in the reactor building?

MR. MEYER: It depends on where you would have this hookup.

MEMBER LEITCH: Yes. But it's hard to imagine it being other than that.

MR. MEYER: That would be an issue -- an issue that would have to be contended with. That would be an important down side consideration.

MEMBER SIEBER: Sir, could you state your name and affiliation for the record?

MR. MEYER: Yes. Jim Meyer from ISL. I should comment, too, that at some sites these type of portable capabilities are already in place, and in other sites they will be implemented as part of license renewal considerations of the severe accident mitigation alternative fixes. So these type of considerations have been thought through before for licensees.

MR. NOTAFRANCESCO: This is a cost-benefit summary. The first bullet has to do with the ice condensers. Clearly, it's cost beneficial for the low cost and with potential attribute of having -- of better dealing with external events.

Mark IIIs, it's marginally cost beneficial. Some are more cost beneficial. Some plants -- some are close. Our recommendation was to send the issue over to NRR to pursue further regulatory action.

CHAIRMAN APOSTOLAKIS: What does that mean?

MR. NOTAFRANCESCO: As part of the generic issue process, we've done our technical assessment. It'll go over to NRR, and they may do a regulatory analysis, whatever.

MEMBER KRESS: This is the type of -- NRR can make a regulatory analysis of whether or not it complies with the rule.

Let me be clear. Your analysis shows that if you wanted to power fans as well as igniters, that you would have to use a more rugged pre-staged unit because the fans require a lot more power than the igniters do.

MR. NOTAFRANCESCO: Right. About five times more.

MEMBER KRESS: Yes. And that if you had had that option of those two together, it doesn't pass the cost-benefit test that you give it.

MR. NOTAFRANCESCO: Right.

MEMBER KRESS: Okay. Now, the other question I have is --

MR. NOTAFRANCESCO: It's illustrated here?

MEMBER KRESS: Yes. I don't know if you have a slide on it or not, but I would be interested in seeing the calculations -- I guess they are done with CONTAIN probably or MELCOR -- that shows the hydrogen concentrations in the various control volumes as a function of time for a station blackout event with the igniters operating.

MR. NOTAFRANCESCO: Right.

MEMBER KRESS: Okay. Do you have that anywhere, or do you --

MR. NOTAFRANCESCO: I could go through that. I'll be using the plots that are in your packet.

MEMBER KRESS: Yes.

MR. NOTAFRANCESCO: Well, before we go to that, how about let me give you some of the overview before --

MEMBER KRESS: Okay.

MR. NOTAFRANCESCO: There's only a few slides here.

MEMBER KRESS: Okay.

MR. NOTAFRANCESCO: And the third component, as I said, we're having Sandia using MELCOR to do the containment analysis aspects, igniters alone, igniters with fans. As part of the new 50.44 hydrogen source terms, we are feeding on this work in -- by looking at the containment response aspects of it. And as part of this, they're looking at different uncertainty studies on the hydrogen release rates and sequences.

MEMBER WALLIS: So this is a new study?

MR. NOTAFRANCESCO: Well, this study is within a year. It's still ongoing.

MEMBER WALLIS: And it replaces the 6427 containment study?

MR. NOTAFRANCESCO: Well, our MELCOR study effectively does that, right.

MEMBER WALLIS: It replaces it?

MR. NOTAFRANCESCO: It updates it with the latest hydrogen source terms and a more definitive containment analysis.

MEMBER WALLIS: It's a better nodalization, is it?

MR. NOTAFRANCESCO: Yes. There is better nodalization.

MEMBER POWERS: Mr. Chairman, I'd better recuse myself from the discussion of this MELCOR stuff. I will comment that it has not undergone an internal peer review at Sandia, and there are internal discussions about some of the results.

MR. NOTAFRANCESCO: Our study to date has shown that igniters alone are effective in controlling hydrogen buildup. There is marginal improvement if one air return fan is included. However, the down side is that it accelerates time of high-sped melt-out. We are continuing with the uncertainty study, looking at the variations of hydrogen source terms, we'll look at other sequences.

What we've looked at so far is a fast station blackout. We're going to look at a slow station blackout looking at burn propagation numbers.

Okay. I could go with the MELCOR, but since we were inspired by Ken Bergeron's letter, we have a quick response on that, if you would like to listen. Ken is a proponent of including the fans, and we looked at his basis, and he does push the envelope on what-ifs. And he uses limiting conditions and some of it seems extreme.

The ease in which DDT is discussed is not --

MEMBER ROSEN: Would you tell me what DDT is in this context?

MR. NOTAFRANCESCO: DDT?

MEMBER ROSEN: Yes, that's a pesticide, isn't it?

(Laughter.)

MR. NOTAFRANCESCO: It's deflagration to detonation transition.

MEMBER POWERS: Let me ask a question for my own interest. I've lost track of this field. What is the quality of our predictive capabilities of deflagration to detonation transitions?

MR. NOTAFRANCESCO: Well --

MEMBER POWERS: Isn't it true that we can't predict them at all?

MR. NOTAFRANCESCO: Well, part of it we're trying to predict the hydrogen concentrations and see what the menu is to make sure if there is a chance of DDTs.

Asimios, are you going to add something to this? He's a hydrogen expert.

MR. MALLIAKOS: This is Asimios Malliakos from the staff, Research. The question, what is our knowledge to be able to predict detonation from deflagration? The first thing -- I'm thinking and talking at the same time -- we need to have a very good understanding about the hydrogen distribution in the containment. We have performed quite a few experiments. We have developed some models for the deflagration to detonation transition.

I'm not really sure what we have done in the case of ice condensers. We need to have mixers at least above nine, 10 percent, to be able to have transition from deflagration to detonation. Only at higher temperatures we can go lower than that.

I'm not sure if I'm answering your question.

MEMBER POWERS: Well, the statement here seems to imply that someone can look at a geometry and say it is difficult to get a DDT or not, presumably based on something.

MR. MALLIAKOS: Yes.

MEMBER POWERS: There are a whole raft of experiments or some sort of a predictive --

MR. MALLIAKOS: The geometry has to do a lot with this. For example, if we have a geometry with obstacles --

MEMBER POWERS: I will grant you that. The question is: given a specific geometry with lots of obstacles in it, can anyone reliably predict whether there will be a DDT or not?

MR. MALLIAKOS: Based on if I have the hydrogen concentration? There are some areas that are kind of questionable.

MEMBER POWERS: We'll assume that you got up into the detonable range of hydrogen concentrations.

MR. MALLIAKOS: Yes. We do have models that with some reasonable assurance we can predict if it's going to happen or not, yes.

MEMBER POWERS: I'd like to see those.

MR. MALLIAKOS: Okay.

MEMBER WALLIS: There's something wrong with your bullet, though. It's not the job to show that there's ease of DDT. It's a job to show that with good confidence DDT will not occur. Isn't that what you're supposed to show? Not that it's easy to occur.

MR. NOTAFRANCESCO: Well, I was just commenting on the -- on the --

MEMBER WALLIS: Yes, but there's a

different objective altogether. Trying to rule something out is very different from trying to show that it might happen.

MR. NOTAFRANCESCO: I'm not going to rule it out based on this letter. I'm just saying the tone of it, I was trying to look at its basis.

MEMBER WALLIS: No. But he is claiming that you could have DDT. He doesn't have to show it's easy to -- for it to happen.

MR. NOTAFRANCESCO: Well, he's setting up sequences or scenarios in which we're going to get this 20 percent plus pocket throughout the whole ice condenser, and it would light off, and we would have a massive explosion. And I was trying to -- I was more pointed towards his postulation.

MEMBER WALLIS: Well, can you exclude it? Can you show that what he postulates is unlikely?

MR. NOTAFRANCESCO: Well, that's why we're continuing with this MELCOR work.

MEMBER WALLIS: Oh, you're continuing to work on it.

MR. NOTAFRANCESCO: We're continuing to work on it.

MEMBER WALLIS: Okay.

MEMBER POWERS: Dr. Wallis, again, I'm --

I confess ignorance in some areas. But in your considerable expertise in using control volume codes without momentum equations to predict hydrogen distributions, is that a well-developed field now?

MEMBER WALLIS: I don't know enough to say whether it's a well-developed field. It's difficult enough to predict without worrying about hydrogen concentrations what will happen in the containment in all the spaces.

MEMBER KRESS: I think you still have the problem of --

MEMBER WALLIS: Especially with condensation.

MEMBER KRESS: You still have the problem of numerical diffusion, and you have the problem of they don't treat the momentum effects very well with the control volumes.

But the question I had earlier was, given the MELCOR calculations, I'd like to see the results of hydrogen concentration versus time and the various control volumes that actually MELCOR predicts, regardless of whether it can predict those or not. Do you have that somewhere on a slide or --

MR. NOTAFRANCESCO: Yes, I'm building to it.

MEMBER KRESS: Oh, I'm sorry.

MR. NOTAFRANCESCO: But I'll pass this one up.

MEMBER KRESS: Okay.

MEMBER WALLIS: You have the steam concentrations, too?

MR. NOTAFRANCESCO: Yes.

MEMBER KRESS: And they're pretty low in --

MEMBER WALLIS: I don't think that was in our handout, was it, all the detail, all the stuff that came --

MR. NOTAFRANCESCO: Well, it was one of the attachments, but I -- I was given an hour and so many minutes. I have them as backup.

MR. TINKLER: Al, can I take a couple of your minutes? I wanted to respond to the questions about DDT. My name is Charles Tinkler from the Research staff.

Actually, there's been a great of work that's gone on, much of it centered in Germany and in Russia over the last 10 years to look at criteria for the transition to detonation. These are criteria for judging the potential for transition that focus on what is seen to be an intrinsic measure of the detonability of a mixture, the cell size of a mixture, which is mainly based on properties and characteristic dimensions of the geometry which confine the mixture.

Work done by the Russian Academy of Sciences, and in conjunction with work done at FCI, have developed correlations expressing the necessary ratio of characteristic dimensions to the cell size, correlations such as seven lambda and 13 lambda which give an indication of the measure of the likelihood that a mixture can undergo a detonation.

This doesn't speak to all irregular geometries, which can create local pockets of turbulence. But the state of the art for assessing detonability of mixtures is improved, and for certain kinds of geometries we think that those kinds of rough measures can give a picture of the detonability.

And I would also point out, too, that it is also -- the direction that you are concerned about, if you are concerned about circumferential propagation versus axial propagation in the ice bed, those are clearly things that we can make decisions on.

That's not to say that we have a rigorous first principles model for predicting transition to detonation. In that regard, it's clear that our ability to predict all of the contributors to irregular flow and transition do not exist. But methods have been developed, principally by FCK, for assessing detonability of mixtures.

So to simply -- and this is the point that we -- that the staff was making. To simply assert that because a mixture is richer in a region for some potential -- for some period of time, and that richer mixture presumably or a priori leads to a detonation, it simply isn't appropriate.

MEMBER POWERS: Let me come back to the correlation approach. The challenge one always faces with correlations is when you extrapolate them beyond the available database, this database that has been developed in Germany has no ice condensers is rich in ice condenser geometries?

MR. TINKLER: No. But much of the Russian data is quite large scale. And the issue of scale of experimental facilities for flame acceleration and transition to detonation is an important consideration. And the Russian data did fill a much-needed large-scale portion to the database and typically shows that mixture concentrations need to be quite high before there's a serious --

MEMBER POWERS: Well, I think that's -- before you're getting into any significant detonation, you're going to have to have a pretty rough mixture. There's no question about that.

I was struck by the numbers that you just threw out, the 11 lambda and seven lambda, because it was almost identical to the numbers for propagating from a large to -- from a small to a large channel.

MR. TINKLER: Yes, they are.

MEMBER POWERS: And that's remarkable because the physics there and the physics of the DDT are completely different.

MR. TINKLER: Well --

MEMBER POWERS: It shows you a certain universality, I suppose.

MEMBER WALLIS: Well, the bigger question is, isn't it -- it's what kind of hydrogen concentration is likely to occur with or without fans. Isn't that the issue that we're trying to address here?

MR. NOTAFRANCESCO: And that's what we're investigating.

MEMBER WALLIS: Are you going to show us that evidence, or are we going to have to go to lunch? Is there some evidence that's convincing that you don't need fans that you can show us?

MR. NOTAFRANCESCO: Well --

VICE CHAIRMAN BONACA: What concerns me, however, is that if fans -- if you show that fans are needed, then the backfit analysis says it cannot be justified. It seems to me that we are -- I don't know, we are selecting a solution and trying to justify it technically, because it's the only one we can afford. It's as if -- you know, if the only thing we can afford is a match.

MEMBER KRESS: Yes. But I think that judgment is made in the absence of a detonation in the ice chamber. If the fans could prevent a detonation in the ice chamber, then you would have a different cost-benefit ratio, I think.

That's one reason I wanted to see these concentrations and hear this discussion on why they think the potential -- or the detonation in the chamber itself is not very high. And I wanted to see the basis for that, and it has to do with the geometry of the chamber, plus the concentrations of hydrogen in there as a function of time.

CHAIRMAN APOSTOLAKIS: So detonation was not considered?

MEMBER KRESS: Not in the ice chamber.

MEMBER WALLIS: I don't understand why there's hydrogen in there at all. I mean, you've got an early accident, and there's a LOCA, and the steam rushes in and it drags in oxygen and nitrogen. It fills up with oxygen and nitrogen. Well, how does hydrogen get in there?

MEMBER KRESS: You make it out of the clad.

MEMBER WALLIS: How does it get into the ice condenser?

MEMBER KRESS: Well, the steam condenses.

MEMBER WALLIS: The steam is already condensed --

MEMBER KRESS: The steam --

MEMBER WALLIS: -- and dragged in a lot of non-condensables which are not combustible. So it's a long story. It's not a trivial thing.

MEMBER KRESS: Well, you always have an hour in there. The hour is --

MEMBER WALLIS: You see what I'm saying. In the early stages of the accident, you don't have hydrogen. You're going to fill the ice condenser up with a lot of non-hydrogen masses.

MEMBER KRESS: Well, you're making a speculation. MELCOR calculates that for you.

MEMBER WALLIS: I hope it does.

MEMBER KRESS: And that's what I want to

see. What does MELCOR tell us about that very thing?

MR. NOTAFRANCESCO: I'll give you a couple of samples of --

MEMBER KRESS: Okay.

MR. NOTAFRANCESCO: -- what we've done here.

MEMBER RANSOM: Well, the worrisome thing along that line, according to the document 1150, it doesn't account for the degradation of condensation in the ice condenser due to the presence of non-condensables.

MEMBER KRESS: Yes, it does -- it's in there. I don't know where that comes from.

MEMBER RANSOM: Well, it's in 1150.

MEMBER KRESS: Oh. Well --

MEMBER POWERS: Well, 1150 is -- the only MELCOR calculations that were done for 1150 are a pretty clear version of MELCOR.

MEMBER RANSOM: There is a discussion on the heat transfer modeling in there. It may be that that's not accurate.

MEMBER POWERS: Yes. You're talking about 12-year vintage modeling.

MEMBER SIEBER: I guess an associated question is, if you don't have fans, and you do have core damage that results in hydrogen, it also results in direct containment heating. And without fans, you aren't melting the ice.

MEMBER WALLIS: Can we go on with this now? Weren't there different ones maybe with different nodalization in the ice condenser? Or am I mistaken?

MR. NOTAFRANCESCO: Yes. In the report there is a sensitivity, but we so far gravitated to the 26-cell configuration.

MEMBER WALLIS: Okay. But there were tests -- there were ones made with --

MR. NOTAFRANCESCO: Yes. Less --

MEMBER WALLIS: -- more nodes than --

MR. NOTAFRANCESCO: Right, 38, something like that, and 15.

MEMBER WALLIS: But they were particularly in the condenser itself, I think.

MR. NOTAFRANCESCO: Right.

MEMBER WALLIS: I'm trying to remember, because I don't have this in front of me.

MR. NOTAFRANCESCO: Yes. The condenser was divided in four axial nodes.

MEMBER WALLIS: For this one.

MR. NOTAFRANCESCO: Right.

MEMBER WALLIS: Okay.

MR. NOTAFRANCESCO: The quick overview of what we've seen so far is that if I have fans, I have more oxygen.

MEMBER WALLIS: Where are the fans?

MR. NOTAFRANCESCO: It's an air return fan. It'll take air from above and force it down into the lower compartment. It's not here. So the idea is to -- it's replenishing the oxygen. Therefore, there's more burning in the lower compartment than without the fans, in which there -- and let me go through some of this and I'll --

MEMBER WALLIS: So you burn up the hydrogen before it can get to the ice condenser. Is that the idea?

MR. NOTAFRANCESCO: Well, that's what the fans do. But there's a distribution I'll show you.

I just wanted to give a sense of the fast SBO timing, because it's nice to know what drives this is what goes -- comes from the reactor vessel. So I just wanted to highlight a couple of areas.

This case is for Sequoyah. It has pump seal leakage, and hot leg fails at four hours. And I'll show you some of the -- this is the hydrogen source for the sequence. You can see core-in covers here, and you've got a couple of --

MEMBER WALLIS: Hydrogen is already being made when the hot leg fails?

MR. NOTAFRANCESCO: The hydrogen -- right.

MEMBER WALLIS: Okay. That makes a big difference, then. I'm sorry. I thought the hot leg was going to fail first.

MEMBER KRESS: And total hydrogen produced is about 500 kilograms there.

MEMBER WALLIS: A bit squirt of hydrogen comes out, then. Okay.

MR. NOTAFRANCESCO: For completeness, let me show you the profile for liquid water, since we have pump seals, the rates on this side, S rates.

MEMBER WALLIS: So there is steam that comes out earlier --

MR. NOTAFRANCESCO: Yes.

MEMBER WALLIS: -- from the ports.

MR. NOTAFRANCESCO: The ports and the hot water coming through the pump seals, and the hot leg breaks here. I think the seals fail about two hours --

MEMBER WALLIS: So there's a lot of steam in the containment for a long time before the hot leg fails. And it's being condensed in the ice condenser.

MR. NOTAFRANCESCO: Right. So you're affecting the ice bed geometry. The melting is going on already. And here's the -- that's the steam source rate, and it really pops out at the hot leg break. So the interest is between three and a half hours, four hours.

Before I show some curves, let me show you what the -- gets some of the difference here of a table of where the hydrogen is lit off. With the igniters only, there is less -- lower containment burns. You see with fans there's more -- it's more burn.

There is burning in the ice bed because there is upward and downward propagation, and that has happened a lot earlier. Then, you get a DDT issue.

MEMBER WALLIS: So it's burning there. It's not exploding. Is that the idea?

MR. NOTAFRANCESCO: Well, they are assumed to have deflagration-type burning, volumetric burning.

MEMBER WALLIS: This ice bed is dripping? All the -- there's water dripping from all these ice trays?

MR. NOTAFRANCESCO: Well, it's going to drip into the lower containments.

MEMBER WALLIS: Can you predict

deflagration and detonation in an ice bed with dripping -- full of droplets?

MR. NOTAFRANCESCO: Well, I don't -- I don't know if we can --

MEMBER WALLIS: Well, I think it would make quite a difference.

MR. TINKLER: We can predict deflagration behavior in simulated spray flow where we have droplet distributions that go from quite large to quite small, as well as in -- near supersaturated steam conditions, too. But that environment is a real -- acts to dampen the acceleration of combustion.

MEMBER WALLIS: Yes.

MR. TINKLER: That is a huge heat sink that works to slow down all combustion processes. That often is not fully appreciated.

MEMBER WALLIS: Well, I'm trying to appreciate it. What is --

MR. TINKLER: Well, I'm not suggesting that the committee doesn't appreciate it, but --

MEMBER WALLIS: What's the effect on detonation?

MEMBER KRESS: It doesn't have any effect on detonation.

MEMBER WALLIS: No effect on detonation?

MEMBER KRESS: No, because it takes place so fast that the heat sink doesn't matter. It's the geometry that --

MEMBER WALLIS: It might prevent it burning?

MEMBER KRESS: It might prevent an ignition, but --

MEMBER WALLIS: It wouldn't prevent a detonation. It might --

MEMBER KRESS: If you once started a detonation, it wouldn't have any effect.

MEMBER WALLIS: So the droplets might be bad because they prevented burning, and then we'd wait and wait and wait until it --

MEMBER KRESS: Until they build up in concentration. I still want to see the concentrations versus time.

MR. TINKLER: I think we would contend, though, that that environment would impact the likelihood that you could accelerate flame propagation and combustion, because it -- because of -- because the suspended water droplets will try to remove heat as that flame is -- as the flame propagates.

MEMBER KRESS: If you had suspended water droplets, but I doubt if you have any suspended droplets in there much. That kind of rundown --

MR. TINKLER: I think that looks like a rain forest in there.

MEMBER KRESS: Well --

MR. NOTAFRANCESCO: Let me offer you some -- I couldn't get a color one, but I'll -- it's not very simple to distinguish. This top here is steam, that's oxygen, and this is hydrogen. This is for the low containment in a particular compartment, nine. And this is the action area where the hydrogen is burning.

MEMBER KRESS: Okay. Now, do you have the same curve for a couple of the nodes in the ice chamber itself?

MR. NOTAFRANCESCO: Right. I'm going to get to that.

MEMBER WALLIS: What is the no dimension scale? That's very peculiar. It must mean something.

MR. NOTAFRANCESCO: It's mole fraction. That's all for --

MEMBER WALLIS: Okay.

MR. NOTAFRANCESCO: While I'm at it, this is the upper containment, and you can see it's about four percent. Okay.

MEMBER WALLIS: Someone is going to ask

you about the uncertainty in these predictions.

MR. NOTAFRANCESCO: Okay. The ice bed is over here. If you want to see --

MEMBER WALLIS: That's mole fraction of what?

MEMBER KRESS: Mole fraction of hydrogen.

MR. NOTAFRANCESCO: Here's hydrogen. Again, the peak is steam, and the hydrogen is the lower one, about here.

MEMBER KRESS: But for a period of about four hours, it looks like the hydrogen concentration in there with the power to igniters only is about 20 percent mole fraction. Is that -- am I interpreting that right? One of those nodes?

MEMBER WALLIS: Which one is the hydrogen? It's not clear to me which --

MEMBER KRESS: I was looking at that .2 line going across. That one. That's hydrogen in one of the nodes?

MR. NOTAFRANCESCO: That's steam. The higher peak is the steam. Right here is the hydrogen. It's under --

MEMBER WALLIS: Which one is -- which curve is the hydrogen?

MR. NOTAFRANCESCO: Right where I've got

the laser.

MEMBER WALLIS: In the beginning.

MEMBER ROSEN: Why don't you trace it from the beginning.

MR. NOTAFRANCESCO: Right here. Hydrogen.

MEMBER WALLIS: Oh, okay. It'll be low. Okay.

MR. NOTAFRANCESCO: Then it's here. There's a little blip because we got that big pulse, and then it goes back down. And it's --

MEMBER KRESS: And is that it continuing on after --

MR. NOTAFRANCESCO: Yes, this is --

MEMBER WALLIS: It's the fat line, isn't it? It's hard to see. So there's a time when it's up in the high teens?

MR. NOTAFRANCESCO: It may peak out briefly towards the high teens.

MEMBER WALLIS: And what's the uncertainty, you think, with this prediction --

MR. NOTAFRANCESCO: That's why we're looking at the uncertainty of the --

MEMBER WALLIS: You're looking at it now?

MR. NOTAFRANCESCO: -- of the source terms. It drives the containment analysis how good the source terms are, so we're going to look at the uncertainty of the --

MEMBER WALLIS: But you've reached a decision already on the regulatory action. And now you're looking at uncertainty in hydrogen concentration?

MR. NOTAFRANCESCO: Right. We're going to --

CHAIRMAN APOSTOLAKIS: Can we accelerate this a little bit?

MR. NOTAFRANCESCO: Well, that's all I had.

MEMBER KRESS: I think at this time on the agenda we have plans to hear from David Lockbaum. Is David here?

MR. LOCKBAUM: Good afternoon. I appreciate the opportunity to talk to you today on this subject. The reason I came today was Ken Bergeron contacted me last week. He was planning on submitting a letter, and he was concerned that merely submitting a letter might -- you guys get a lot of paperwork, and he was afraid it would just fall on a pile.

It's very obvious that it didn't just fall in a pile. It has been discussed, so I'm not going to spend a lot of time, because that the main reason for my coming here today was to call attention to Ken's issues, and they are clearly in play.

From the observations I heard of the staff's presentation this morning, there's a couple of things that I'm confused about. It's on slides 14 and 15, slide number -- pages 14 and 15 of their presentation, where they looked at -- for non-station blackout events, they assumed the igniters and the air return fans are functional. And for station blackout events they did a MELCOR study to show that igniters only are effective in controlling hydrogen burnup -- was the staff's conclusion.

That would lead one to believe that for non-station blackout events that you don't need to air return fans either. If the fans are effective, they're effective. And I assume that would then mean that the industry could make the air return fans non-safety grade or take them out altogether.

So it looks like it supports the statement on slide 15 that igniters alone are effective, and perhaps they don't need them for non-station blackout events either.

I think, more importantly, the concern that Ken has, that I echo, is that the low-cost estimate -- low-cost option that the staff is proposing, and I don't feel is sufficiently justified, may actually be setting the operators up for a worse accident than the one they are dealing with.

Three Mile Island and Chernobyl -- at Three Mile Island, the operators in training were stressed to avoid the pressurizer going solid, and that contributed them towards a path that wasn't as successful as it might have been otherwise. At Chernobyl, the operators were dealing with a situation where they thought it was getting out of hand, so they took action to shut down the plant with positive moderator coefficient, made things worse.

This low-cost option may be the cheapest way of setting the operators up for another bad accident, and we don't need to be doing that.

Unless a stronger justification is made for not including the air return fans in the station blackout provisions, we would oppose putting in just the igniters. That just doesn't seem -- and this bit with the 55-gallon drums of diesel generator on wheels just seems to make it a little bit easier for saboteurs to attack a plant without bringing their own explosives, and that may not be a good idea for a number of reasons.

That's all I had, since the Bergeron letter is already in play. Thank you.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER KRESS: Okay. I think at this time also we have on the schedule to hear from Ms. Ann Harris.

MS. HARRIS: Thank you. Mr. Chairman, members of the committee, my name is Ann Harris. I've traveled here today by my personal resources without benefit of taxpayer support or government payroll.

I appeared before this committee in November 1995 prior to your support to the Commission for the licensing of Watts Bar's nuclear plant -- TVA's Watts Bar nuclear plant. I moved out of the evacuation zone to a nearby area. The fact that we are all here again seven years later to hear staff's offering on the Generic Safety Issue 189, and NRC's recommendation, is evidence of how things work with staff and the industry.

The ice condenser issue may be a generic issue to you. But you should be aware that it's real people's lives you're talking about. This is not a generic issue to me. It's about the nuclear reactors just down the road from where I live and where members of my family and friends live.

I hope that you are as worried about the time factor as I am. I take it as a positive sign that at least something is going to be done, even if it's going to be just talk this time. But do we need more talk?

I was in this same room seven years ago arguing that Watts Bar was not ready for prime time. That didn't do any good since most of the problems were never fixed. They were just forgiven. Will we be back talking seven years from now when TVA and staff admit that safety is still not a prime factor? I think not.

TVA will be in the nuclear weapons production business at Watts Bar and Sequoyah because staff has never seen an industry license amendment request it did not like.

At the meeting in 1995, one of the subjects I heard about was whether the hydrogen igniters would work. My transcript of that meeting shows that Committee Member Ivan Catton tried to raise questions about hydrogen igniters and whether the igniters are Watts Bar were adequate to prevent the containment from leaking from hydrogen explosions.

In fact, he was asking questions about whether the igniters were located in the right locations in the containment, and now here you are seven years later talking about the same thing. These meetings are like seven-year locust visits; they just keep coming.

Committee members, talking just isn't good enough anymore. Your talking has put lives at stake. It appeared at that '95 meeting that Mr. Catton was truly interested in whether Watts Bar was safe enough, but he was cut off and shut up by the Chairman at that time.

What we did not know at that meeting was that the person at Watts Bar responsible for making sure the ice condenser was working correctly before startup had discovered that the screws holding the ice baskets up were defective. TVA devised a scheme to hide Curtis Overall's discovery, then get rid of him, therefore obtaining the Watts Bar license by lying to this committee and to the Commission.

After years of investigations and court proceedings, the NRC has been forced to levy a fine against TVA. TVA has had so many fines for employee abuse they shed them off like water off a duck's back. No big deal.

The most troubling fact is that inspections of the ice baskets that Overall wanted, and was abused for, were never done. We still don't know if they will stay put if there is an accident at the plant.

I've never told anyone that I'm an engineer, but I do have common sense. From what I understand, NRC seems to be finally facing up to the fact that ice condensers won't really work, won't protect the public during an accident. Their idea to fix the problem is to get a little portable generator from Home Depot or Lowe's, put it on a pickup truck, roll it up to containment, and plug it in.

I worked in TVA's nuclear program for 16 years, 14 of them at Watts Bar. I've seen some crazy, silly, childish, and outlandish things done in the name of safety. But I believe this one could take the blue ribbon.

I keep having this cartoon run through my head of what would be going on if this generator is needed. There is a hurricane, a severe lightning storm, a terrorist attack, a flood. It's dark, no lights, no backup power. Shift supervisor has just sent someone to the little shed out back containing the Honda generator with a copy of the combination to the padlock.

People living downstream are depending

upon this person to know the combination without hunting the paper it was written on. The rain is wetting the paper. His glasses are covered with water. The wind blows the paper away, and he starts back inside for another copy.

When he gets back, he unlocks the shed, rolls the generator to the containment building, plugs it in, proceeds to get it running. I think that our lives and our property values deserve a little more concern than this NRC proposal. Why are you only recommending this blue light special approach?

I feel that the people who live near these plants are getting short-changed, run over, and made expendable. The NRC recommendation seems to say the backup power doesn't have to work if the accident is caused by a flood or an earthquake or a terrorist attack. How do you think this kind of accident is going to happen? Merlin conjuring? Whoof.

Committee members, the people living in these communities are real-live people whose lives are being talked about here this morning, not just numbers and statistics. Those same people trust the NRC to protect their interest.

I wouldn't be surprised if NRC gets pressure from industry about making changes to the ice condensers to make them actually work. I imagine that you will be pushed to pick numbers, to redo your calculations, making it impossible to solve the problem that fixes the containment.

I'm speaking as much to licensing people in the audience as well as this committee and the Research staff, to keep in mind the interest of the real people living near these plants. Think twice about trying to make industry happy with an analysis that says they don't have to fix anything.

It is good that NRC has made a start, but so many times good starts end up as dead ends. I think you should be careful about plans to fix the ice condenser plants, depending upon the goodwill and good intentions of the plant owner.

Some of the proposed changes, like the cheap portable generator idea, seem to be planning on not having the inspections that you have for other safety equipment. I don't know about other utilities, but I know TVA well enough to know that if NRC leaves it all up to them the generator won't have a motor or a receptacle for the plug.

If there's neither inspection nor enforcement, that backup system is not going to be there when it's needed. You see, the bigger danger is to have a lot of back and forth talking, leading people to think that something has been done to fix the problem. But you and I know that's not true, and therein lies the problem. Misleading is worse than doing nothing.

I would ask that you recommend to the Commission that these ice condensers be fixed to protect the public now. You should advise the staff that they should be bending over backwards to protect the public safety, not bending over to avoid trouble from the industry.

Thank you.

MEMBER KRESS: Any comments or questions from the members? Seeing none, thank you, Ms. Harris.

And I'd like to turn the microphone over to Bob Bryan. I think he has a -- he's from TVA. He has a few words to say.

MR. BRYAN: Thank you. I just wanted to comment very briefly about the cost-benefit study. For TVA, which has the Sequoyah and Watts Bar nuclear plants, our igniter system is -- requires quite a bit more power than was considered in the cost-benefit study.

Our igniters are about 600 watts apiece, which would require a generator the size of about 21 kilowatts per train. This I think is outside the range of the four and a half or five kilowatt generator that was looked at in the low-cost option. So I think we're basically looking more at one that would be an agist of what was put together for the air return fan case.

This is just a quick look at the thing -- we're currently evaluating what the cost would be for us to install such a system with the cabling and tie-in to the 1E power system.

Thank you.

VICE CHAIRMAN BONACA: Are you considering powering also the air return fans?

MR. BRYAN: No, we're not. This was just -- the 21 kilowatts would be just for the igniters. If you powered the air return fans, depending on the unit, it would probably be between 50 to 75 kilowatts, depending on the plant.

VICE CHAIRMAN BONACA: Thank you.

MEMBER KRESS: Seeing how late it is, I guess I'll ask if there are any comments from the members that they want to make at this time, or any questions.

MEMBER RANSOM: I've got a comment. Mark I and Mark II containments are inerted. And in the material that was provided, it was indicated that this was the more or less ultimate solution. I'm wondering, I didn't hear anything this morning about inerting, you know, the Mark IIIs and the PWR ice container -- ice condenser containers.

MEMBER KRESS: They are not inerted. That's --

MEMBER RANSOM: Pardon?

MEMBER KRESS: They are not inerted.

MEMBER RANSOM: Right. But could you inert them?

MEMBER KRESS: I think that would be a much more expensive backfit.

MEMBER RANSOM: Has that been looked at?

MEMBER KRESS: I don't know if it has in the past or not.

MR. TINKLER: Following TMI, when we -- when we examined additional hydrogen control for all the plant designs, we did consider the feasibility of inerting ice condenser Mark IIIs. But they do require much more frequent access to portions of the containment.

Normal maintenance in the ice bed, and there's -- there are a lot of systems in Mark III where people are inside the plant. So limiting access so severely as a result of inerting the plants was judged to be overall detrimental to plant safety.

MEMBER RANSOM: Is that true of the Mark I and II? I mean --

MR. TINKLER: Well, the Is and IIs are small. So you can't go in the drywell of a Mark I when it's operating, if it was inerted or not inerted. The shine -- you know, the dose -- the received dose is just so large that you just couldn't stand it. So they are not -- you know, there are other reasons why you don't want to be in a -- in the drywell of a Mark I or II. But there are many portions of an ice condenser in Mark III where you can safely go into the plant.

MEMBER LEITCH: As I recall, all the hydraulic control units in a Mark III are inside containment, and they require frequent periodic maintenance it would be very difficult to do.

MEMBER KRESS: Would the staff care to make more comments before we --

MR. ADER: Tom, this is Charles Ader with the Research staff. I was just going to mention, because some of the discussion has kind of moved around on some topics. As Charlie Tinkler just said, the earlier studies on the 50.44 rule had looked at some of these things. As part of the IPE there was a look at the backup power for igniters, and at that time everybody was looking at having to power both fan coolers and igniters, and they've generally been found not to be cost beneficial.

This study, which was an expedited study, I think there was a view that you may be able to get by with the igniters. We were trying to expedite it through, so, really, the question is: does it appear to be prudent, cost beneficial, to proceed on with powering igniters with backup power?

Now, there is some ongoing work that will continue on with the staff. We think it will confirm the conclusions. But it was not a -- going back from square one and trying to revisit things that had already been determined not to be cost beneficial. So it's really that last piece of it that we've been looking at at this time.

CHAIRMAN APOSTOLAKIS: Thank you. Would someone from the staff comment on Ms. Harris' comment near the end of her presentation that -- regarding inspection of these diesels. I mean, are you going to require some sort of inspection, so that reliability will be maintained? Or it will not be a safety-related component, so what requirements are you going to impose, if any?

MR. ADER: At this point in time, the research study is looking to technical feasibility and the cost benefit. In the general process, if we conclude that it looks like we should go forward, it would be transferred to NRR, and they would look at the actual details of how it would be implemented, whether it would be --

CHAIRMAN APOSTOLAKIS: But wouldn't, though, your assumptions in the calculations depend on this? I mean, we were told earlier that the probability of installing it and starting it correctly would be .8. But it seems to me that that .8 would depend on a lot of things, part of which would be the inspections and possible tests. So I --

MEMBER ROSEN: I would second your comments, especially with regard to testing and demonstration that these things can, in fact, be done under adverse circumstances.

CHAIRMAN APOSTOLAKIS: Right. I mean, you know, the human factors is one element, but also, you know, other things are important. And regarding human factors, I mean, she has a pretty dramatic description here of what it would take to do. Is that what's going to happen? I mean, it's going to be a piece of paper or -- you know, sometimes these mundane things turn out to be very important. So that .8 probability probably needs to be scrutinized.

MEMBER ROSEN: You know, George, we have scientific words for what Ms. Harris described -- the aeroforcing context.

CHAIRMAN APOSTOLAKIS: That's right. The context, yes. It seems to me that deserves some serious consideration.

MEMBER KRESS: Well, you know at that .8 probability you are implying goes down, then this option gets closer and closer to telling the backfit analysis. So you're forcing the regulatory analysis to say this is not a viable option by forcing the reliability down.

CHAIRMAN APOSTOLAKIS: Well, then, we have to look at the other things, too. I mean, with LERF --

MEMBER ROSEN: I don't know where George is going with his comments, but I -- my comments are along the same lines. But they are that if you're going to rely on these devices, then I would need a showing that they will, in fact, work.

CHAIRMAN APOSTOLAKIS: Do what the intent is.

MEMBER ROSEN: Yes. That there's a fairly high likelihood that they will function as intended. And at the moment, it's unsatisfactory to me to have Research say, "Well, that will be determined by NRR." Part of my decisionmaking process here will be to know what the testing and inspection regimen will be.

MR. ADER: I didn't mean to leave that impression. I mean, in our analysis, we need to make a fair attempt at trying to quantify that before we transfer it over. The specific mechanism of implementation, where there would be rulemaking, plant-specific, it would be an NRR decision.

But you're correct. We should be trying to give the best analysis and most robust we could. Some of that I think had been put in number --

CHAIRMAN APOSTOLAKIS: Oh, I'm sorry. Go ahead.

MR. FELD: This is Sidney Feld with Research. One of the cost elements that we did include in our analysis was an industry operation cost, which included quarterly maintenance, surveillance, and testing of the diesel generator. And those costs were included in --

CHAIRMAN APOSTOLAKIS: That would be an important element, it seems to me, in the presentation.

MR. FELD: -- in the analysis.

CHAIRMAN APOSTOLAKIS: Yes, yes. That would be really an important element. But the other thing that strikes me as a little odd is the absence of an uncertainty analysis. I mean, would any of these conclusions change if one included the various uncertainties that are here?

How sensitive is the conclusion that the low-cost option is cost beneficial, if I consider all of the uncertainties? And how, you know, sensitive is the other conclusion that having qualifications, and so on, is not cost beneficial? I don't know.

I mean, when these reliabilities, and so on, are so uncertain, and what's going to happen -- it seems to me that would be one of the cases where you would try to look at the uncertainties.

MR. FELD: There is -- as I said, there is some additional work going on within staff on looking at some of the uncertainties, at least of the containment hydrogen analysis.

CHAIRMAN APOSTOLAKIS: Right.

MR. FELD: The feedback I've gotten is we think that will confirm -- you know, confirm the conclusions to proceed further.

CHAIRMAN APOSTOLAKIS: But if there is still work going on, why are we here today? I thought we were going to be presented with a technical analysis that would lead to some closure? And evidently there is --

MR. MEYER: Well, within the generic issue process described in the Management Directive 6.4, we would do technical work that would provide a basis for either dismissing the generic issue or deciding that it should move forward. And I think that we believe that we've done enough work to decide that it should move forward.

What we've tried to say is that for either the low-cost or the pre-stage option for the ice condenser plants, for a wide variety of assumed initiating event frequencies, and it -- that it makes sense to go forward. For the Mark IIIs, it's less clear that it's cost beneficial from a strictly risk standpoint, even for a range of initiating frequencies.

It seems to me that going from -- assuming that the thing is efficacious at .8 to .6, it isn't going to change the decision to move forward. The one area which is really a modeling issue -- and we're looking at the modeling issues in this -- is do you need the fans or not? That's going to dominate not differences as a factor of two in blackout frequency.

So -- and so we have an initial conclusion that we don't need the fans. That it would be efficacious without the fans. And then, we clearly say -- we go -- we've got to do some more work to pin this down, but that we've done enough that it pays to move forward.

MEMBER WALLIS: How about the comment that we heard that your estimates of the power requirement were way too low for this particular plant?

MR. MEYER: Jim Meyer again. Was the question on the -- in particular, the TVA issue with the added power requirements? We recognize that the -- the reason Catawba is our -- is kind of our base case plant, we recognize that for both Sequoyah and Watts Bar, that their igniters require considerably more power. And, in fact, it's about 520 watts per igniter compared to typically 133 watts per igniter for --

MEMBER WALLIS: I think we heard 800. Didn't we hear 800? 600.

MR. MEYER: Well, my information was 520, but we're in the same range. And so we went back and considered the implications of that, both for the pre-stage and for the off-the-shelf. And the conclusions we came to is that, yes, the cost would be higher because the diesel cost would be higher, and there would be some added engineering costs that would be higher.

But the diesel costs are only a small part of the overall costs, so the conclusion was that we still felt comfortable with our numbers.

MEMBER WALLIS: Well, his conclusion was that you couldn't get away with that portable generator. You had to go to the more expensive option.

MR. MEYER: Well, there are portable generators, and, in fact, portable generators up to 50 kilowatts. So there are such things as portable generators in that range. But I agree with you, you would move more towards the pre-stage with the TVA, because of the fact that you require considerably more kilowatts to operate the igniters. But we did take that into consideration.

CHAIRMAN APOSTOLAKIS: Any other --

MEMBER POWERS: A question was posed --

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER POWERS: A question was posed about whether what droplets would, in fact, be detonation propagation? And after horsing around with it a little bit, I have concluded that both Drs. Tinkler and Kress are correct. Dr. Kress said that large droplets dripping down from the ice bed would have no impact on the shock wave propagation. I think he's correct on that large droplets sparsely -- sparse numbers. The shock wave just doesn't even know they're there.

And then -- and Dr. Tinkler is correct that applying this to sub-500 micron particles just because of the momentum effect will inhibit the propagation of the --

MEMBER KRESS: Yes. And my comment was predicated on the fact I don't think you have that size droplets in there, those tiny --

MEMBER POWERS: Yes. I mean, that's when you guys are going to have to sort out -- but whichever way it is, you understand the detonation wave correctly.

MEMBER ROSEN: Geez. Between the two of you --

MEMBER WALLIS: It doesn't -- those droplets -- everything will be over by the time they're shattered, I would think.

MEMBER POWERS: You may be able to break

the big ones, but you --

MEMBER WALLIS: It will shatter them into pretty small pieces.

MEMBER POWERS: You won't break the little ones. They're -- there's surface tension there.

CHAIRMAN APOSTOLAKIS: Any other issues from the staff or members of the public?

MR. GUNTER: Yes, I'd like to --

CHAIRMAN APOSTOLAKIS: Please.

MR. GUNTER: -- if I can. Paul Gunter, Nuclear Information Research Service. I thought I heard, during the presentation, that the emergency -- that these portable generators would be fueled out of the common storage tanks. And I think that that ignores the issue of common mode failure and with contaminated fuel. So I just wanted to raise that issue as something I thought I heard and needs to be addressed.

CHAIRMAN APOSTOLAKIS: Any response?

Okay. We are running behind, so let's be back at 1:40. Thank you.

(Whereupon, at 1:04 p.m., the proceedings in the foregoing matter went off the record for a lunch recess.)

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

(1:42 p.m.)

CHAIRMAN APOSTOLAKIS: The next item is the technical assessment of Generic Safety Issue 168, Environmental Qualification of Low-Voltage Instrumentation and Control Cables.

Mr. Leitch is the cognizant member. Graham?

MEMBER LEITCH: As the Chairman has said, this is GSI-168 concerning the environmental qualification of low-voltage I&C cables. As we all recognize, these cables are very important in plant operation, since they can, if they fail, give misleading and confusing information to the operator.

We have some samples of cables that most the ACRS have seen previously, and they are identified to the tests, and so forth. These represent nothing that we have not already seen, except that some of the members of the ACRS are new since the last presentation, and they may be interested in seeing the samples. So we're not planning to pass them around, but they are here if you'd like to take a look at them. And they are all identified as to what they are.

CHAIRMAN APOSTOLAKIS: These are

artificially aged?

MR. AGGARWAL: Yes, sir. That is correct.

CHAIRMAN APOSTOLAKIS: It is correct.

MEMBER ROSEN: Have they been through a real LOCA?

(Laughter.)

MEMBER LEITCH: So at this time, then, I'd like to turn the presentation over to Mike Mayfield, who will introduce his presenters.

MR. MAYFIELD: Thank you. We are here this afternoon to talk to you about the technical assessment that we have completed and the transition from research/technical assessment to NRR's implementation phase. We have a panel of speakers this afternoon that will be headed by Nilesh Chokshi. Satish Aggarwal will be -- make the bulk of the technical presentation. Paul Shemanski will have a piece of this, and Art Buslik, who did the risk assessment.

So with that, Nilesh?

MR. CHOKSHI: Okay. I think this is, given the timeframe, we have got a pretty fairly high-level presentation. We came about a year and a half ago and talked about the results of the tests and research. So the purpose -- main purpose is now that the technical assessment is complete to summarize the technical assessment and discuss the -- our recommendation.

Paul, would you put that -- okay.

CHAIRMAN APOSTOLAKIS: Can you move it higher a little bit? All the way up there.

MR. CHOKSHI: Okay. As Mr. Mayfield mentioned, under the Management Directive 6.4, the operator research completes its technical assessment. The next step is it goes to the program office for consideration for the regulatory -- for the regulatory action.

A year and a half ago we talked about the test results. Since then, we have had some interactions with industry groups, and we have done a little bit more in the risk area. So I think at this point now the technical assessment is complete.

So the primary purpose today is to give you the results -- oral results of the technical assessment recommendation, and then get your comments, and, as the process requires, we will incorporate your comments before we transmit the final technical assessment to the NRR.

Our current plan is to --

MEMBER LEITCH: Let me just say that

originally there were 43 issues identified. And as I understand what happens, many of these issues were resolved from researching the literature. A number of them were felt not to require additional research. And that finally boiled down to a set of six issues that required additional research.

What we have today in the technical assessment is basically a report on the results of the research associated with those six issues. Is that a correct characterization?

MR. CHOKSHI: Yes, six. Right, there are six issues.

MEMBER LEITCH: Okay, good. Thank you.

MR. CHOKSHI: Those are the remaining ones.

MR. AGGARWAL: That is correct. However, when we interacted with the industry, as a byproduct of our research, several questions came. These were put to the industry, and we do intend to present to you the outcome of the discussions with industry as well.

MEMBER LEITCH: Okay. Thank you.

MR. CHOKSHI: So, yes, the two days -- we will talk about those six issues and seven questions, primarily findings from those.

So Mr. Aggarwal is going to do that now, give you an overview of the technical assessment. And in the end, I'll come back and talk about our final recommendation to move forward to -- this task to NRR.

So with that, Satish?

MR. AGGARWAL: Thank you.

As pointed out to you, Mr. Chairman, we met with you in October year 2000, and we presented the test results of all six LOCA tests, condition monitoring and assessment, and also we told you about the EQ literature review, the basic result being that we didn't want to reinvent the wheel. We wanted to see what industry had done so far and where we stood.

As pointed out by Graham, ultimately we narrowed it down to those six issues, and six LOCA tests had nothing to do -- there's no relationship one to one. But six tests were conducted and completed.

Subsequently, after meeting with you, we had numerous meetings with the nuclear industry and relayed many questions during those discussions, which I briefly will discuss.

One point I would like to point out, the criteria for qualification is based on zero failure, since we are only testing one single prototype. But please bear with me, and keep in mind a single prototype and the criteria is no failures.

Next.

And essentially, when you go for LOCA test, it is required that we bring that cable to the end of life condition. You had the 40 years or 50 years, and that is meaning thereby that we get thermal and radiation heating to bring the cables to that condition.

Then, we put the cable to a LOCA test sample, where either single peak or two peak. As required, in the original qualification, we go through the test procedure.

And, finally, we perform a post-LOCA test to demonstrate adequate margin by requiring the mechanical durability.

The underlying principle being that if you are part of the test, we feel that cables are so robust that we end up giving design basis even, those cables will perform their safety function.

Next.

MEMBER LEITCH: Now, the pre-aging is done by raising the temperature in accordance with the -- an iraneous relationship?

MR. AGGARWAL: That is correct. But the staff did not come out with any numbers. What we did was these cables were previously qualified by the manufacturers, and they have taken an iraneous equation, their design temperature. They came out with a number in terms of the hours and what degree of temperature and radiation. What we did in our test, we simply reproduced those numbers.

MEMBER LEITCH: Now, your technical assessment seems to suggest or flat out states that the iraneous methodology is conservative, yet Dr. Rosen was at a fire meeting -- and we have his report -- where it seems to suggest that the iraneous relation is non-conservative. Would you discuss that?

MR. AGGARWAL: Sure.

MEMBER ROSEN: This was the wire safety aging conference held here in Rockville several weeks ago that my trip report was about.

MR. AGGARWAL: I submit that both statements are correct. Let me bring to you --

(Laughter.)

That is the diplomatic response.

MEMBER ROSEN: I think he's qualified to be on the ACRS.

(Laughter.)

MR. AGGARWAL: There is no question in my mind and the industry that there are uncertainties in an iraneous equation. It has limitations, but this is the best we have.

CHAIRMAN APOSTOLAKIS: Well, I don't understand what it means that the equation is conservative. I mean, the equation has parameters. Wouldn't it depend on the values of the parameters, or whether --

MEMBER ROSEN: Let me see if I can reproduce what the issue was.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER ROSEN: From memory, because I didn't bring my report.

CHAIRMAN APOSTOLAKIS: Did you write it?

MEMBER ROSEN: Yes, I wrote it.

(Laughter.)

The aging -- according to the people in this conference -- is a phenomena that relies on oxygen -- that is caused by oxygen diffusing into the cable insulation. And when you do a test at higher temperature to simulate long life, you are exchanging temperature for time in the iraneous equation.

You do that -- you do it quickly, and the diffusion of oxygen into the cable insulation doesn't occur, because it's a time-limited phenomena. It takes time for the oxygen to get into the cable jacket. And so the -- what you get out of a simulation -- an aging -- accelerated aging test is a cable that is not as damaged as one that's naturally aged where there's lot of time.

It's a lower temperature in the normal environment, but there's lot of time for the oxygen to diffuse completely into the cable insulation material. And to me, when I heard that, either I got it wrong or it didn't square with what you're saying in --

CHAIRMAN APOSTOLAKIS: Microphone, Art.

MR. BUSLIK: There are two effects. One is diffusion-limited oxidation, which is what you're talking about. And in a sense, you luck out. The reason is that very frequently, if the material -- the material would become as brittle on the surface where the oxygen has a chance to diffuse, and very -- and very frequently, if it becomes brittle on the surface, you'll get a crack there which propagates throughout the depth of the cable insulation. So that, in a sense, you luck out because it's the properties at the surface which are important.

There's another effect which has to do with the fact that sometimes you don't have one rate-determining constant, let's say, in the kinetics. You may have two. And in this case, if -- if the arrhenious low with the activation energy determined from higher temperatures and accelerated aging, this will always be non-conservative.

It's just a simple equation. You have a linear combination of two arrhenious expressions, and you'll see that if -- that the one with the -- I think with the higher activity energy -- I may get a -- will dominate at the lower temperatures or -- I think that's right, or else vice versa. I'd have to figure it out.

(Laughter.)

But at any rate, that you always get a non-conservative thing. However, it is possible to verify using -- you're referring, actually, to Ken Goen's work. And it is possible to verify using oxidation -- ultra sensitive oxidation consumption methods what the aging is at much lower temperatures, closer to the ones that actually occur in a plant.

And, in some cases, you obtained the fact that there is really no -- no change in the activation energy. In other cases, though, I think it is really just true that we don't know. But I think that the results that -- Brookhaven also came up with using a method of verifying the activation energy for the cables in certain isolated cases, and he found that there was agreement there.

That was -- it's in -- what is it? NUREG/CR-6704, Volume 1, toward the back somewhere. But it's true, in general, you may not know.

MEMBER LEITCH: Thank you, Art.

MEMBER WALLIS: But doesn't it depend on the material of the cable? There may be some cables for which what you say is true, that there's a severe --

MR. BUSLIK: Yes, but it --

MEMBER WALLIS: -- at the surface governed by arrhenious, but maybe other materials, presumably other studies, that say that it's diffusion-limited, refer to something real, for which diffusion is an important phenomenon.

MR. MAYFIELD: This is Mike Mayfield from the staff. I've had the opportunity to spend some time talking with Dr. Gilland, and there are a couple of different classes of the materials. The bulk of the materials that he has tests fall into a class where the iraneous equation gives reasonable to somewhat conservative predictions of the actual aging that he sees.

There is another class of materials, and part of the work is to define what exactly -- how do you characterize that class, where the iraneous equation doesn't seem to work very well, and --

MR. BUSLIK: But it's not related to the diffusion-limited oxidation so much, I believe, as the -- I've forgotten what he calls it -- the chemical.

MR. MAYFIELD: That's correct. And so there are these two classes of materials, and part of the work that he is continuing is to better characterize the two classes. But for most of the materials that we've been talking about and for the insulation materials that I believe we've tested in this program, the iraneous approach gives you reasonable to somewhat conservative predictions of the aging.

We have also acquired -- I think in the previous briefings we've talked about some -- the limited amount of naturally aged cable that we could acquire. There's only so much of this stuff you can get, where we have then also had the archival unaged material that we then artificially age.

And within the uncertainties of the actual doses that the naturally aged materials received, and the variation in material properties that just naturally occur with these polymers, you are hard put to tell a difference within the extent that we can make these kind of measurements.

MR. BUSLIK: And referring to the question about the diffusion-limited oxidation, I think maybe perhaps in all cases what you're concerned about is the mechanical integrity of the insulation, which is related to its brittleness. And if it becomes brittle on the surface, I think the cracks will generally propagate throughout. So I think, in general, it turns out to be okay there.

MEMBER ROSEN: I'm a little bit concerned about the scope of coverage of the testing. Does the conclusion that you are offering that it is generally conservative to do the pre-aging as we have done it, apply to the kinds of safety-related cables, all safety-related cables in plants? I know "all" is a big word. But let me say the majority or in the main it applies to the cables? How broad is -- is it conservative to do this? It now depends upon the kind of cable.

MR. AGGARWAL: In our test program, we tested three types of the cable, which the majority of the plants used to the extent of 75 percent or 77 percent. It is our submission that these are the principal cables which are used in I&C applications in nuclear powerplants in the USA.

The second part is when we brought up a program, we were looking at it. We were not looking at the validity of iraneous oxygen diffusion. The technical issue before us was that when we do the testing, according to IPEEE Standards 323 and 383, you are required to create the cable.

And under certain exemptions, the manufacturers have come up with certain numbers in terms of temperature and the duration. Our goal was to provide some kind of judgment what industry did. Was it conservative? The only way to verify for us was it took naturally aged cable from the plants, and then we compared what we have done after excellent rating, and the staff concluded that the techniques we used in qualification, they seem to be conservative.

Now, with regard to iraneous -- the activation energy, in a separate study we also concluded that what the industry had used seemed to be reasonable and acceptable.

MEMBER ROSEN: So you don't feel that Gilland's results are inconsistent with that conclusion?

MR. AGGARWAL: No, I don't.

MR. BUSLIK: Well, no. I mean, I don't either. But you have to remember that sometimes it can be very sensitive to the material you have. For example, Gilland, in an old water reactor safety meeting paper, talked about a change in the activation energy for the ethylene propylene dyene monomer material. And I wrote him an e-mail about it, and it turns out that that was one used for seals, and it's mostly amorphous.

And even though it may be a problem there, it may very well not be a problem -- and probably the Brookhaven tests verify this -- for the ethylene propylene dyene monomer materials, which are used for insulation, which have a greater crystalline fraction.

MEMBER ROSEN: Okay. I'm not an expert on this. I just pointed out what appeared to me to be an inconsistency. And I just sat and listened.

MR. AGGARWAL: Thank you.

As we reported to you previously, there were failures of certain I&C cables in NRC tests, namely in LOCA test numbers 4, 5, and 6. Failures of single conductor bonded Okonite cables. Sampled more cables in test number 4, and eight out of 12 cables failed in LOCA test number 6 for 60 years.

We also found in our research that there is no single condition monitoring technique available which is effective to detect degradation. Probably combination of different techniques can be used, depending upon the type of insulation.

We also found that visual inspection can be useful in assessing the degradation of cable with time.

MEMBER POWERS: What do you mean? Clearly, if the degradation gets bad enough, I'd go in and I can see, "Yep, that cable is degraded." But it's a long time. I mean, it's -- it's visual inspection is not going to tell you anything about the level of degradation.

MR. AGGARWAL: You are correct. Again, as compared to doing nothing --

MEMBER POWERS: Ahh.

(Laughter.)

How about as compared to some of the instrumental techniques?

MR. AGGARWAL: We have discussed in our report and there are several which can be used -- elongation at the break is one which is universally used, but it is destructive. People use different matters -- the OIT, OITP, different techniques are available. And, again, each of them has limitations.

Our report, NUREG/CR, really provides that information, and we hope the industry will pick up and use it in a manner that will be useful to them.

MEMBER POWERS: Because what we were discussing earlier is you embrittle the surface, and then you get a crack, and that crack propagates through. So the embrittling of the surface presumably goes along at a nice arrhenious or quasi-arrhenious rate. But once it cracks, that's not going to be an arrhenious behavior.

MR. AGGARWAL: Correct.

MR. BUSLIK: But what is thought -- and, by the way, I think when they talk about visual inspections, they also pick up on the cable systems to see how flexible the cable is, and I guess whether there are --

MEMBER POWERS: Well, again, I mean, when -- if the damage has gone on far enough, yes, that works great. But by that time, you are in a severely damaged state.

MR. BUSLIK: That's true. But I think it's felt that if there's any -- practically any -- you'd have to speak to the people in industry. But if there's any flexibility left in the cable, or a certain amount, that the cable will survive a LOCA, at least at that time. And then you have to worry, I guess, about the rate of --

MR. AGGARWAL: The point I was trying to make was that licensees should know the environment and the reason cables are uprated.

MEMBER POWERS: Well, you've mentioned combined thermal and radiation doses. What kind of radiation doses are we talking about?

MR. AGGARWAL: We have taken 50 megarads total dose. And how much power?

MR. MAYFIELD: Basically, for EQ testing, we assume 50 megarads for the background radiation; that is, during the first 40 years. And then, typically, the accident dose is 150 megarads. So you get about 200 megarads would be the total integrated dose that the cable would be subjected to during a LOCA simulation test.

MEMBER POWERS: That does grievous damage to polybond chlorides.

MR. MAYFIELD: Yes. They are very susceptible to radiation, right.

MR. AGGARWAL: So the bottom line is that if you know the environments, some kind of visual inspections could be useful.

Next.

In the area of risk, as you must have noted with our -- in our report submitted to you, the state of the art incorporating cable failures into PRA is still evolving. We do not advance to all of them. But it may be noted the key assumption in PRA is that the operating environments are lower than or equal to what are presumed in the qualification test.

In other words, licensees know where the hardest parts are. That is the key assumption. And, of course, the uncertainties are in terms of the experiments, human failure rates, factors, and what not. And what we find, that if the -- if any requirements such as condition monitoring, and all of this, the benefits are zero to modest.

MR. BUSLIK: If you reduce the cable failure probabilities to zero, the benefits are modest. There are benefits. The benefits are not zero. But they're modest.

MEMBER ROSEN: When you say the state of the art of incorporating cable failures into PRA is evolving, I would wonder where. What was going on that I don't know happened?

MEMBER POWERS: Have we got a long time in this meeting?

(Laughter.)

MEMBER ROSEN: On this subject.

MEMBER POWERS: Oh, oh. Okay.

MR. BUSLIK: Well, first of all, what I did was I sort of took some data from Jacobus, which he had a certain number of failures and a certain number of tests, but it was on all different kinds of cables. And I used -- all I could do was take the fraction of failures over the total number of trials, basically, and get some sort of average probability of failure.

What you would like to be able to do is sharpen that for the particular type of cable. Also, I assume that the cables were essentially at their environmental qualification limit, because that's what was tested.

MEMBER ROSEN: Are you responding to the second bullet on this question -- on this chart? My question is: what's going on in PRA?

MR. BUSLIK: No, what are we doing now.

MEMBER ROSEN: In terms of incorporating the cable --

MR. BUSLIK: Well, we are doing something. We have a project, which instead of doing what I did will attempt to estimate, using the physics of the aging of the cables, of the cable insulation, the probability of failure of --

MEMBER ROSEN: Well, there's a research

project going on that might lead to some techniques that PRA practitioners could use. I don't know of any PRA practitioners in the utility industry that are incorporating cable failure probabilities.

CHAIRMAN APOSTOLAKIS: It depends on what you -- are you talking about LOCAs here?

MR. BUSLIK: Yes, yes. These are --

CHAIRMAN APOSTOLAKIS: Okay.

MR. BUSLIK: I'm sorry. These are -- the thing that is importance as far as cable failures is the possible common mode failure in the harsh environment of a LOCA.

CHAIRMAN APOSTOLAKIS: Because when you say that the results indicate that the benefits from reducing the cable failure probability is zero to modest, you don't include fires.

MR. AGGARWAL: Fire is out of the scope.

CHAIRMAN APOSTOLAKIS: Out of -- you eliminate the --

MEMBER ROSEN: No. Hot shorts or any of that, they're not --

CHAIRMAN APOSTOLAKIS: Nothing. Nothing.

MR. AGGARWAL: That's right.

MEMBER ROSEN: What you are talking about is just aging effects, I assume.

MR. AGGARWAL: That is right.

MR. BUSLIK: In fact, Steve Gullen pointed out that the -- that aging cables may actually behave better in a fire. There are less flammable, because the volatile materials come off.

MEMBER LEITCH: Could we talk about the tables that are on pages 45 and 46 in the technical assessment report?

MR. AGGARWAL: There are two tables.

MEMBER LEITCH: There are two tables, one on 44 concerning PWRs and one on 45 concerning BWRs. We need only talk about one of them. Let's talk about the one on 44. There is a core damage frequency there. Now that core damage frequency --

MR. BUSLIK: Is the reduction in the core damage frequency, if the cable failure probabilities were brought to zero from what it would be if -- if the -- if the cables had the failure probabilities that I estimated, assuming that industry essentially did nothing to try to reduce it.

But nevertheless --

MEMBER LEITCH: How could the probability be brought to zero if --

MR. BUSLIK: Well, what I'm saying is if you have really perfect condition monitoring, this is

-- then, the failure probabilities would be zero. It's a bounding case. Obviously, no condition monitoring technique is going to be perfect.

MEMBER LEITCH: Okay. Then, you give a certain credit for voluntary industry actions.

MR. BUSLIK: Right.

MEMBER LEITCH: And that --

MR. BUSLIK: And that I just reduce the values by 30 percent. This was the -- the voluntary industry actions I said were -- they were assumed to be limited to ensuring the cable environment is within the cable's environmental qualification envelope.

But actually I assume that for both cases, with respect to temperature and dose, and to inspecting cables visually, near their connections to a component, when maintenance on that component is performed. In other words, I didn't take any credit for a systematic walkdowns where there was tactical lifting of cable -- visual and tactical observations of the cables throughout the cable run. So it wasn't very much.

MEMBER LEITCH: So the first number, though, is the present state of things?

MR. BUSLIK: It's a conservative estimate of the present state of things, I would say. For one thing, all of the cables are not at their environmental qualification limits. But I don't know what the temperature and dose rate particular cables see in a plant. We have --

MEMBER LEITCH: I guess what I'm trying to do is get a feel for, where are we now in core damage frequency, where could we be with voluntary industry actions, and where could we be with a full-blown regulatory program?

MR. BUSLIK: All right.

MEMBER LEITCH: I only see two of those three numbers here. I guess that's what I'm --

MR. BUSLIK: Well, with the full-blown regulatory program, I didn't really intend to estimate it. It's bounded by the two times 10^{-5} per year reduction in core damage frequency. I mean, I don't really know how good condition monitoring could be. I don't know how accessible the cables are, things like that.

MR. AGGARWAL: Essentially, then, Table 1 tells you what the constant state is. Table 2 is telling you some allowance -- provisions for maintenance and related activities. And this is the difference.

MR. CHOKSHI: I think the most benefit you can get out is this two times 10^{-5}. So that is the upper limit of the benefit. That is this calculation.

MEMBER LEITCH: Two times 10^{-5}?

MR. CHOKSHI: That was the reduction in the core damage assuming zero probability of failure for cables.

MR. BUSLIK: And that was taken at -- between 30 years and 60 years, essentially. And before that it was zero assessment approximation.

MEMBER LEITCH: So there is -- reducing the cable failure probability to zero, the benefits are modest.

MR. BUSLIK: I think so, especially if you look at the costs. Basically, the averted costs from -- from averted accidents. They're not that high. What is it? $200,000 for a plant without license renewal or half a billion for a plant with license renewal. But those are bounding numbers.

MEMBER LEITCH: The benefits of industry actions are, then, even smaller than modest because you're getting all the way to zero.

MR. BUSLIK: That's right.

MR. AGGARWAL: Thank you. As I started earlier, that we had numerous meetings with industry. The bottom line in the discussion with industry was that followed the claim -- the industry claim that I&C cable has not experienced any significant aging. In limited cases -- and they know of the hot spots -- the licensees are exercising several options, such as early replacement, modification of the environment, or they do some kind of condition monitoring. Whether the old plants are doing it or not, we do not know.

Aging evaluations are ongoing throughout the plant life as a part of normal life.

Turning to the 60-year aging assessment, which was LOCA test number 6, in our test, eight out of 12 cables failed the post-LOCA test. And we have concluded that some of these cables may not have sufficient margin beyond the 40 years of the qualified life.

Again, if one can conclude the operating environments are less severe than what was assumed during the qualification, then margins can be used to extend the life.

MEMBER POWERS: Let me ask a question about that. When you test these cables, you take a cable and you age it, and then you run a test on it, and that cable is a cable.

MR. AGGARWAL: Yes, sir.

MEMBER POWERS: But in the real plant, the cable that's sitting there has all kinds of junk -- dirt, all kinds of contamination stuff, and things like that. Do we know what benign junk to get on these cables and what's deleterious junk to get on it? I mean, is there -- if we spill 40 weight motor oil on the cable, it doesn't make any difference; but if we spill glycerine on it, it does?

MR. AGGARWAL: Unfortunately, I don't have an answer to that. I have not studied the research program.

MEMBER POWERS: I mean, it seems to me it's what is missing from all of this, when you start saying you're conservative, is that there's another variable that the plant experiences that we really don't know anything about. I mean, what are cables getting contaminated with?

MR. AGGARWAL: That is correct.

MEMBER POWERS: What are they in contact with that -- maybe it's not a contamination. Maybe a little nickel metal does bad things to the cable insulation in a synergistic effect or something like that.

MR. MAYFIELD: This is Mike Mayfield from the staff. Keep in mind that most, if not all, of the cables have a protective jacket over the outside of the insulation.

MEMBER POWERS: That's true.

MR. MAYFIELD: And the jacket is what would see the spill, as opposed to the insulation itself.

MEMBER POWERS: You are right on that. Of course, the jacket itself may be the -- long-term incompatibility.

MR. MAYFIELD: It's a good question, and I don't have an answer for it. It's just that there is this other barrier between the insulation that we were concerned about --

MEMBER POWERS: No, you're right on that. You're right about that. But before I jumped and said I was conservative, I'd like to know a little more about that.

MR. MAYFIELD: Didn't say we were conservative. I simply said to keep in mind there's this other layer.

MEMBER POWERS: Yes.

MEMBER ROSEN: I'm less concerned, Dana, about spilling glycerine or motor oil on them than I am about such things that are much -- such things as humid or moist salt air.

MEMBER POWERS: Sure.

MEMBER ROSEN: So a lot of these are sea coast sites. How do your tests take that into account? Or isn't it necessary to do that kind of thing?

MR. AGGARWAL: The IEEE standard does not require any conservation. It simply has a LOCA test and the post-LOCA test. And if you pass it, then you're considered to have passed.

MR. CALVO: Excuse me. This is Jose Calvo from the NRR. Most of these cables are inside the containment, so I guess this portion to salt water -- it will not be seen there. So as long as you keep that salt -- with the water and the salt from the containment, you don't have to consider that part.

MR. MAYFIELD: This GSI is focused on cables in a harsh environment, which takes you inside containment by -- virtually by definition.

MR. AGGARWAL: The bottom line of the test is that knowledge of the environment for cables continues to be essential.

MEMBER POWERS: So let me understand that -- that you have told us that if you reduce the failure probability to zero, it has limited --

MR. MAYFIELD: Dana, she's asking you to use the microphone.

MEMBER POWERS: And I wouldn't want to get on the bad side of her, because she is behind me.

(Laughter.)

You said if I reduced the probability of cable failure to zero it does not have much impact on risk. How about the inverse problem? What's the kind -- how much risk do I gain if I raise the probability of cable failures up to one? I think that's what we usually do. Isn't it, George?

MR. BUSLIK: Let's see. I didn't bring it with me, but -- well, that would be the essentially similar -- that would be the Birnbaum importance of it. And those numbers are given here, but --

MEMBER POWERS: If I had looked hard enough, I would have found them.

MR. BUSLIK: That's right. And let me see if I can find --

MEMBER POWERS: But those are the numbers that lead you to say that it's essential.

MR. BUSLIK: Yes. I mean, roughly, I would say it could -- if you just change that in the PWR it could go up by maybe a factor -- I mean, it was a 15 percent probability of failure of instrument cables. And instrument cables were important at Surry. So it would go up by a factor of over six.

MEMBER WALLIS: We're talking about environment. You said they failed by a crack on the outside propagating through.

MR. BUSLIK: Right.

MEMBER WALLIS: This would seem to be influenced by bending of the cable --

MR. BUSLIK: Yes.

MEMBER WALLIS: -- around corners and --

MR. BUSLIK: Yes. In fact, you find that cables could be very brittle after the pre-aging -- the accelerated aging experiments. And yet they don't fail during the LOCA, because the LOCA simulation -- presumably, because they aren't moved there. And it does introduce an uncertainty because you don't really know for sure whether the cable will be subject to vibration or --

MEMBER WALLIS: No. I mean, I feel like in installing the cables they are stretched, aren't they?

MR. BUSLIK: I don't know --

MEMBER WALLIS: They couldn't be always straight.

MEMBER POWERS: Yes. But what they --

MR. MAYFIELD: This is Mike Mayfield from the staff. Let's be careful here. Cables are, of course, installed in the unaged condition. There are criteria on bend radii. There are criteria on pull forces. There are a number of things to look at exactly the issue you are raising, Mr. Wallis, that -- so there are criteria for this.

The issue is: if you had some mechanical vibration, some movement of the aged cable during the actual --

MEMBER POWERS: Well, like maybe in a main steam line break, or something like that.

MR. MAYFIELD: Could you get enough mechanical force to move the cables enough and --

MEMBER POWERS: Those kinds of questions.

MR. MAYFIELD: -- and that's an issue that we've talked about, but I don't think we have a good answer for it.

MEMBER POWERS: I mean, it -- when you mention that movement, of course, the thing that comes immediately to mind is the main steam line break, or even a steam generator tube break, because of the apparently -- the vigorous vibrations that we expect you get there. Maybe we should be looking at that.

MR. MAYFIELD: Again, that's something we've talked about a bit. But as Satish has pointed out, what we got to in this test program specific to this GSI -- well, it didn't take us there, but it's still a valid point. It's just we didn't get there, and I'm not quite sure how you'd address it in a sensible fashion.

I know that I can move the cable enough -- aged cable enough to damage it. Now, would I get that kind of movement depending on where it is inside containment during a steam line break?

MEMBER POWERS: You know, what we could do is we could take some of that money we have on heavy section steel and apply it to --

(Laughter.)

MR. MAYFIELD: But then we would miss vitally important information dealing with other critical systems.

MEMBER WALLIS: Going back to the radius of curvature and that sort of thing, these cables are installed by somebody. Someone is laying cable?

MR. MAYFIELD: Yes, sir.

MEMBER WALLIS: And I would think in handling the cable and manipulating it around corners, and so on, there is all kinds of bending that goes on, twisting, and so forth, which is not --

MR. MAYFIELD: In its unaged condition, this stuff is remarkably flexible. At the same time, there are criteria for how they handle it.

MEMBER WALLIS: Yes. But --

MEMBER POWERS: If you watch them pull cables nowadays, it just stuns me how careful they are about this stuff.

MEMBER WALLIS: So, well, they are in nuclear plants. They certainly aren't usually around universities where --

(Laughter.)

MR. MAYFIELD: I'm going to let that one go.

MEMBER POWERS: There's nothing critical at a university either.

(Laughter.)

MEMBER WALLIS: There are professors, and they -- they could complain.

(Laughter.)

MR. AGGARWAL: I would simply point out that in IEEE standards there is the test known as the Mandril test, that you take the cable and take so many times around it, and then test under the high voltage to show whether or not there are any cracks. So, indeed, that test gives you that kind of feeling that if anything like that happens in the life, in the operating plant, at the time of construction, then, if operating plant, at the time of construction, then, if a test passes, you will conclude that it would be capable of handling those inspections.

This cable is put all around, and this is roughly this diameter. In Mandril, it will bend around 20 times, but that's opposed to high voltage.

MEMBER LEITCH: I have a question concerning the second bullet there. Failure in NRC tests indicate that some cables did not meet qualification criteria in the margins that we set.

Now, in your technical assessment then, there's an overall conclusion on Page 57 that says, in part, that the EQ process is adequate for the EQ of low voltage cables and INC cables for the current license term of 40 years. How do those two statements square up? It seems on one hand you're saying the process is adequate, but here you've had some cable failures.

MR. AGGARWAL: My submission is that the process of qualifying cable is adequate. It presumes that the licensees know their environmental conditions and they are monitoring them. And if those conditions are lower than those during the qualification, then there is no problem. But if they do not know, of course there is a problem. This is how I will explain the failure.

MEMBER LEITCH: Now, you had some cables, I guess it was Samuel Moore cables that failed above 77 degrees at less than 40 years --

MR. AGGARWAL: Okonite cables.

MEMBER LEITCH: Okonite, was it? Yes. I'm sorry. Yes. That failed at less than 40 years service. So do we know that those -- that cables are not in the field and operating in those conditions?

MR. AGGARWAL: Okay. In a nutshell, the story about Okonite cables is that those cables originally qualified for 90 degrees C. And the manufacturer had never tested those cables in real life. He used a similar argument. Bigger cables were tested, and he applied that to the smaller cables. Now, when these cables failed in an RC test, the manufacturer named the Okonite and tested the cable themselves on their own initiative. And they concluded that their cables are only good for 77 degrees.

Now, NEI has done a survey and they indicated that probably four plants might have that problem but definitely one of them exceeded those conditions. And I do not know the name of the plant, and I do not know, you know, what the conditions are.

We do know that there is one plant which apparently has exceeded --

MR. CALVO: Excuse me. Let me augment this a little bit. Yes, we don't know whether one plant, we don't care to a certain degree, because the important part is that a new test has been done that demonstrates qualifications -- establish a new qualification threshold, which is at a lower temperature. One plant is very close to that, and you can say that where that plant may not reach the annual life of 40 years, but that's part of the Environmental Qualification Program. It's a lot of stuff out there that hasn't reached 40 years, and the Program requires that you replace them or you do some testing or you do some analysis.

So knowing the plant is not important. What is important is that the Okonite has informed all the licensees that report that kind of cable and told them, "This is a new threshold." Now, you look there pursuant to 10 CFR 50.49 was the EQ rule that's supposed to do whatever corrective action is necessary. And all that thing has been taken care of.

Now, the Okonite failure was not a safety significant failure, it was a very limited, very limited application on these cables. It was mostly a single conductor and it was very, very few of them, okay? So that one is not on the control. The licensees are being advised that corrective actions have been taken, pursuant to 10 CFR 50.49, so, presumably, that part is done.

MEMBER LEITCH: So that's what gives you the confidence then to say that the EQ process is okay? In other words, if the process is correctly followed --

MR. CALVO: Right.

MEMBER LEITCH: -- then -- so the 77 degrees is fed back to the licensee and he does all the right things and his plant environmental conditions are known and he factors that into the process, the process is okay.

MR. CALVO: Right.

MR. AGGARWAL: That's correct. And the bottom line, as you see, the knowledge off the operating environment is essential. The licensee, he should know where the hardest parts are.

MEMBER LEITCH: But the process is okay for 40 years.

MR. AGGARWAL: Correct.

MEMBER LEITCH: And what about for 60 years, is the process still okay, if he's still has all those things?

MR. AGGARWAL: Processes are still good as long as you know your environment.

MR. CALVO: If I may, the process is the same process. All you do when you reach in 40 years the question is being asked does this cable have sufficient life to go 20 more years? And what you do is you look at all the information that you collected over the previous years and you determine that the actual service conditions are sometimes much lower than the actual temperatures or radiation that this particular cable will qualify. So based on that, most of the cable that we see in the license renewal has been reanalyzed and concluded that because of the lower actual service conditions, you can extend it for 20 more years. So the process is the same process. It's a program that is still -- it's assumed that the cable -- the life is 40 years. You've got to make a decision to go beyond 40 years. Either replace the cable or you want to license it and you determine -- or test it or you determine what you're going to do with it. So the rule has those provisions built into it.

MEMBER LEITCH: So I think a lot of what our -- well, at least what my questions comes down to is not so much the research report but what is NRR going to do to implement that? And I guess we don't really have -- I mean this hasn't really been presented to NRR yet or it's just now being presented.

MR. CALVO: We've been working with research in these efforts, and we have reported the results. I guess the knowledge of the environment I think is necessary to ensure that the balance of the equipment within the qualified basis of the particular equipment. I think what is important knowing the environment is that's still to predict failures, but it should -- it verifies the fact that the equipment is within the tested parameters. It tells me that the equipment was qualified for these parameters, continues to be qualified. If it is not qualified, then the rule will come in, the process will tell you that you've got to do something about it. Something can very well be that it wasn't good for 40 years, maybe only good for 38 or 35. A decision has to be made when you reach that point there.

We know that knowing the environment it is important. It is necessary to establish that your equipment continues to be qualified. We know that they have done it, we know that we have done some inspections several years ago to verify some of that.

Then about three years ago we have done recently a programmatic evaluation of the program itself with some licensees. We verified that the program was adequately implemented as part of the license renewal. We're also doing some verifications right now to see that we can extend it for another 20 years. So we know the environment has been done. We see no smoking guns, that it will probably be the NRC or NRR to go there and do inspections at this time. We feel that they have done the correct thing up to now.

MEMBER LEITCH: So this will ultimately depend on voluntary industry actions rather than a big regulatory --

MR. CALVO: Well, no. It's an environment -- they've got to know what it is, because, you see, the rules say that equipment must be qualified and remain qualified for the life expectancy. So if the environment that you predicted changes, that means the qualification also has to change. So this is -- if they're meeting the rules, which I know they're meeting the rules, they've got to do these kind of things.

So they force them to do it. Just like any regulation, they've got to do it, because it's the only way that you ensure you do some maintenance, you