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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

MATERIALS AND METALLURGY AND PLANT OPERATIONS

SUBCOMMITTEE MEETING

VHP CRACKING AND RVP HEAD DEGRADATION

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TUESDAY, JUNE 1, 2004

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ROCKVILLE, MARYLAND

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The meeting came to order at 8:30 a.m., in Room T2B3 of Two White Flint North, F. P. Ford, Subcommittee Chairman, presiding.

SUBCOMMITTEE:

F. PETER FORD	Subcommittee Chairman
JOHN D. SIEBER	Subcommittee Vice Chairman
MARIO V. BONACA	Member
THOMAS S. KRESS	Member
STEPHEN L. ROSEN	Member
VICTOR H. RANSOM	Member
WILLIAM J. SHACK	Member
GRAHAM B. WALLIS	Member
M. W. WESTON	Staff Engineer

ACRS STAFF PRESENT:

Bill Bateman	NRR/DE/EMCB
Garesh Cheraventi	NRR/DE/EMCB
Jay Collins	NRR/DE/EMCB
Samantha Cane	RES/DET/MEB
Bill Cullen	RES/DET/MEB
Bart Fu	NRR/DE/EMCB
Rich Guzman	NRR/DLPM/PD1-1
Allen Hiser	RES/DET/MEB
Meena Khanna	NRR/DE/EMCB
Bill Koo	NRR/DE/EMCB
Todd Mintz	RES/DET/MEB
Matthew Mitchell	NRR/DE/EMCB
Wallace Norris	RES/DET/MEB
Eric Reichelt	NRR/DE/EMCB
Larry Rossbach	NRR/DLPM/PD3-2
Cayetano Santos	ACRS Staff NRR/DE/EMEB
David Terao	NRR/DE/EMEB

ALSO PRESENT:

Charles Brinkman	Westinghouse
Daniel Horner	McGraw Hill
Alex Marion	NEI
Larry Matthews	Southern Nuclear Op. Co.
Pete Riccardella	Structural Integrity
Jim Riley	NEI
Glen White	Dominion Engineering
Gery Wilkowski	Engineering Mech Corp of Col

AGENDA ITEMS PAGE

P R O C E E D I N G S

(8:32 a.m.)

CHAIRMAN FORD: The meeting will now come to order.

And the very first thing, I'd like to thank everybody for being here, having given up some of your Memorial Day.

This meeting is of the ACRS Joint Subcomittees on Materials and Metallurgy and on Plant Operations. I'm Peter Ford, Chairman of the Materials and Metallurgy Subcommittee and my Co-Chair is Jack Sieber, Chairman of the Plant Operations Subcommittee.

Other members in attendance are Mario Bonaca, Thomas Kress, Graham Leach, Victor Ransom, Steve Rosen, and Graham Wallace.

Bill Shack will be presenting and, therefore, is not participating as a member.

The purpose of this meeting is to discuss a generic communications regarding materials cracking and degradation issues.

Maggalean W. Weston is the cognizant ACRS Staff Engineer for this meeting.

The rules for participation in today's meeting have been announced as a part of the notice of this meeting published in the _Federal Register_ on May the 19th, 2004.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice.

It is requested that speakers use one of the microphones available, identify themselves, and speak with sufficient clarity and volume that they may be readily heard.

We have received no written comments from members of the public regarding today's meeting.

As a run-in to this meeting, as you know, for several years now, three years we've been having fairly regular meetings on the whole question of materials degradation problems with PWR primary site penetrations.

The last major meeting was in the spring of 2003 and you heard some very ambitious plans by both the staff and by industry.

Prior to this meeting, I've issued a list of topics that we'd like to have covered at this meeting shown on the slide right now. This is very much the schematic shown on the left-hand side of a penetration. It happens to be the vessel head penetration.

Showing in red and green are areas of

cracking, axial and circumferential cracking. And in the Cambridge blue is what I've called boric acid corrosion but general corrosion of the low alloy steel.

There are roughly seven areas that we've asked to be addressed in this meetings. First of all is the adequacy of the degradation algorithms for both cracking and boric acid corrosion, taking into account of the variables which would be of importance, the applicability of these degradation algorithms to different PWR primary site penetrations, not only the vessel head but also the bottom head penetrations in the pressure in the reactor vessel and the pressurizer, the impact of those two on inspection prioritization and periodicity.

And that relates to the FMEA part of the MRP plan, the risk analysis or the safety analysis part of the MRP plan, qualification and inspection techniques which were of great concern to us at the last meeting last year in the spring of 2003.

Another area we'd like to have tackled is the qualification of the repair or replacement options, and the last one is to hear what progress is being made by the industry on their proactive management approach.

Before we start, since there will be a presentation by Alex Marion on the MTAG efforts relating to Item 7 there, I would have to declare a conflict of interest. And, therefore, I won't participate in points of opinion.

Jack, do you have any comments before we start?

VICE CHAIRMAN SIEBER: No, I don't.

CHAIRMAN FORD: No? Bill, would like to make any overall comments before we start?

MR. BATEMAN: Yes, just a couple of opening remarks, if I may.

Just to reiterate basically what you said, Dr. Ford, this is another meeting in a series of meetings we've had in the past to present information to the ACRS as the state of our knowledge has increased. And I think today's presentations will indicate that the state of our knowledge continues to increase.

We basically had a meeting, we, the staff, had a meeting with industry, I guess it was probably about two or three weeks ago, whereat industry presented the safety assessment that they have developed and upon which they will base their inspection proposal for upper vessel heads.

That safety assessment was, I guess, about 1,100 or 1,200 pages in length. So it has an abundance of data, which I know the Material Subcommittee is looking for. We haven't had a chance to go through it all yet but -- and we're still awaiting the proposal for industry but there's certainly enough data there to --

CHAIRMAN FORD: Excellent.

MR. BATEMAN: -- for anybody to spend some time.

I guess the other thing I'd like to do at this point is to thank industry and the staff who are going to make presentations today. I can honestly say that we have been working together to try to solve these problems, come to grips and then come up with proper inspection schemes. And you'll hear about that today.

And I presume that we will have meetings to this one as we continue along our path to solving these issues.

CHAIRMAN FORD: Now, I understand that from your point of view, this is just an informational meeting. You're not requesting a letter, you're not scheduled to give talks. The full committee meeting is this week. Is that correct?

MR. BATEMAN: Yes. Okay. And with that, I'll turn it back to you, Dr. Ford.

CHAIRMAN FORD: Thanks very much, Bill.

Larry, let me pass it on to you.

MR. MATTHEWS: Good morning.

CHAIRMAN FORD: Thank you for coming.

MR. MATTHEWS: You're welcome. Thank you for inviting us the day after a holiday.

(Laughter.)

PARTICIPANT: Boy, that was a hidden dig.

CHAIRMAN FORD: You're welcome.

MR. MATTHEWS: My name is Larry Matthews. I work with Southern Nuclear. And I'm the Chairman of the Alloy 600 Issues Task Group of the Materials Reliability Program.

This is kind of the agenda we had laid out. I'll do a brief overview, present some of the conclusions up front. And then Glen White will go over the failure modes and effects analysis. And then Pete Riccardella will start on the probabilistic fracture mechanics analysis that we've got that is part of our basis for our safety assessment and will form the basis for our inspection program.

Kind of in the middle of his is about when I think we'll hit the brake. And then he'll come back and finish. Then I'll have a brief presentation on Alloy 690 and our technical basis for that material then the conclusions. And then Alex will come up and conclude the morning with a presentation on the materials initiative.

A little background, the industry events that have taken place have shaped our final safety assessment, which is MRP-110. MRP-110, itself, is not 1,200 pages long. But all the supporting documents that go along with it, if you add them all up, they're in the 1,100 to 1,200-page range.

Initially, the safety assessment methodology that the MRP was using was reactive to what was going on in the industry. After the North Anna 2 results, if you recall, there were circumferential flaws or certainly indications of circumferential flaws with no leakage on top of the head.

That caused us to reassess our approach. And we went back to Ground Zero and decided to do everything, starting with the failure modes and effects analysis to make sure we weren't overlooking something.

The purpose here is to discuss briefly our MRP-110 and some of the supporting work that goes into that. And then also a brief discussion on MRP-111, which is our survey or the assessment that we've done on the resistance to PWSCC for Alloy-690 in the weld metals.

Basically it's based primarily on known lab and field studies. But -- well, it includes the field work with the steam generators primarily.

And then an update on the status of where we're going with the inspection plan.

CHAIRMAN FORD: This MRP-110, is that the revision essentially of MRP-75 --

MR. MATTHEWS: No

CHAIRMAN FORD: -- which I understand there was a little bit of a hiatus about?

MR. MATTHEWS: No, MRP-110 and all its supporting documents are the technical basis that will be -- they form the technical basis for the revision to MRP-75. It will have a different number. It's going to be, I think, MRP-117.

But it -- that will be the inspection plan that we're going to put forth. And all of these documents form the technical basis for that.

We haven't completely gotten the inspection plan through our inspection plan through our review process. We hope to do that this summer.

CHAIRMAN FORD: When we had the meeting last spring, Larry, with all the comments made about MRP-75 as to whether that was now the document, whether it was -- everybody was agreed upon, it was a technical document. Where do we stand on that?

Is it -- if I pick up a document saying MRP-75, should I be reading it as the opinion of the MRP?

MR. MATTHEWS: No, no. We withdrew that document from consideration by the NRC after North Anna 2 because one of the primary bases for that inspection plan was that visual inspections were adequate.

CHAIRMAN FORD: Yes.

MR. MATTHEWS: And then because of the North Anna 2 results, we said whoa, we've got to relook. And --

CHAIRMAN FORD: Now just to remind us, North Anna 2, there was circumferential cracking but no boric acid crystals. That's the --

MR. MATTHEWS: There were --

CHAIRMAN FORD: -- simple reason why.

MR. MATTHEWS: Yes, there were some nozzles that had circumferential indications. And we've taken those nozzles out, sending some of them to hot cells to find out what was really there.

But that indicated there was circumferential cracking right at the root of the weld where there would not be much weld ligament left. But it didn't penetrate the annulus so there was no leakage on top of it.

CHAIRMAN FORD: Oh, I see. Okay. Okay.

MR. MATTHEWS: MRP-110 basically covers all of the CRDM, CEDM, and ICI-type nozzles that are attached with J-groove welds.

We haven't really addressed those few nozzles that are attached with butt welds. And from a technical standpoint, we really didn't do much analysis on the head vent nozzles. They're small and would be bounded by the analysis we've done on the CRDM nozzles.

There are a few nozzles in the plants that are attached with butt welds. They're either machined or forged nozzles on the low alloy steel and they're attached with butt welds. And those will be addressed more with the butt weld safety assessment.

Conclusions, axial nozzle cracking leading to nozzle rupture is not a credible failure mechanism in our mind. The critical crack length is much greater than the height of the nozzle region that's subject to welding residual stresses.

So the significant margin exists against nozzle ejection due to the amount of time required to grow the circumferential crack that you'd shown as one of the -- I guess that was the -- I can't remember, Peter, whether it was the top curve or the middle curve. But circumferential crack growth to nozzle ejection is one of the concerns. And we believe there's significant margin there.

Periodic bare metal visual examinations provide assurance against significant wastage of the low alloy steel head material. A program of non-visual NDE, FUT or EDICRUNT, and bare metal visual examinations at appropriate intervals, we feel provides adequate protection against safety-significant failures.

And, in addition, the probabilistic fracture mechanics analysis, which is documented in MRP-105, one of the supporting documents that Pete will discuss, shows a low probability of pressure boundary leakage, also with that same program of NDE and visual exams.

CHAIRMAN FORD: Now some of these conclusions, especially those that relate to the risk aspect, were spelled out in words back in the spring of 2003.

MR. MATTHEWS: Yes.

CHAIRMAN FORD: But there was no analysis shown to support them. I take it that the rest of this meeting, we're going to have that?

MR. MATTHEWS: Pete's analysis --

CHAIRMAN FORD: Oh, okay.

MR. MATTHEWS: -- will go into some detail on the PFM. Now we've had to cut out about 60 percent of the slides that he presented to the NRC just to squeeze it into our time slot.

CHAIRMAN FORD: I understand, I understand.

MR. MATTHEWS: But he does have some slides that show the risk associated with the -- or the probability of ejection, et cetera.

CHAIRMAN FORD: Okay. And as I understand it, Bill, that particular analysis, you have not given an opinion on yet? So it would be inappropriate for us to --

MR. BATEMAN: That's correct, that's correct. We have not.

CHAIRMAN FORD: It would be inappropriate for us to give opinions although we can comment.

MEMBER WALLIS: I guess we're going to get to this but this last bullet here, low probability pressure boundary. Presumably all these boric acid crystals and so on are evidence of leakage so it's happened. Did you mean a large leak? Or what do you mean by that?

MR. MATTHEWS: No, I mean going forward with the inspection program that we're going to lay out of non-visual and visual inspection program, especially the non-visual inspection program, if we implement that program, the risk analysis shows there's a lot probability of leakage.

MEMBER WALLIS: But that's sort of -- I'm tied up in the logic. But you're looking for a leak, you're looking at the crystals. And then you're saying there's a low probability of a leak. I don't understand what you mean.

MR. MATTHEWS: No, not the -- non-visual NDE is looking for cracks before they leak.

MEMBER WALLIS: Well, bare metal visual is looking for a leak.

MR. MATTHEWS: Visual is looking for leaks.

MEMBER WALLIS: And yet the next bullet says there's a low probability of it. So I don't quite understand how these two --

MEMBER SHACK: Because it's non-visual NDE.

MEMBER WALLIS: No, he's got bare metal visual, too.

MEMBER SHACK: He's got both.

MR. MATTHEWS: Yes, I'm doing both.

MEMBER WALLIS: But then why are you -- if there's such a low probability of it, why are you looking for it? I don't understand the logic.

MR. MATTHEWS: Defense in depth.

MEMBER WALLIS: No, but why do you have this thing, this last bullet? You've already -- we know it's likely because it's happened in many plants.

MR. MATTHEWS: It has.

MEMBER WALLIS: So I don't understand what you mean by the last bullet.

MR. MATTHEWS: The one --

MEMBER WALLIS: You mean a big leak? Or do you mean a trickle? Or what?

MR. MATTHEWS: I think -- no I mean any leak. I mean that if you implement the program of non-visual NDE, we calculate a low probability of leakage in the future. We didn't have that program of non-visual NDE in the past.

MEMBER WALLIS: An inspection is going to change the probability of leakage?

VICE CHAIRMAN SIEBER: That's because you find everything before they leak.

MR. MATTHEWS: Yes.

MEMBER SHACK: If you find cracks before they break through, then there's no leak.

MEMBER WALLIS: Oh, okay.

MEMBER SHACK: You fix them.

VICE CHAIRMAN SIEBER: That's what gives you the low probability.

MEMBER WALLIS: So that's the thing -- that's the thing that matters. The bare metal visual is just a backup.

MR. MATTHEWS: The bare metal visual does not contribute to the low probability of leakage.

MEMBER WALLIS: Oh, I see, okay. Now I'm beginning to get --

MR. MATTHEWS: So it's a defensive in depth for the wastage issue.

MEMBER WALLIS: So to be accurate and to allay grim things, you should put in the future at the end of that sentence. Your randy-dandy new process --

MR. MATTHEWS: Yes.

MEMBER WALLIS: -- says hey, we're not going to -- so you'll catch them before they leak is the idea.

MR. MATTHEWS: Yes.

MEMBER WALLIS: That's -- okay.

MEMBER SHACK: Yes, with a high probability.

MR. MATTHEWS: Yes.

MEMBER WALLIS: So what you mean is there is a probability of catching them before they leak, not an actual low probability by itself of the leakage. And then --

VICE CHAIRMAN SIEBER: Well, you may fail to detect some crack.

MR. MATTHEWS: Yes, you may fail to detect some but you take --

VICE CHAIRMAN SIEBER: You know, what they're trying to do is --

MEMBER WALLIS: There's some probability that some will get through --

VICE CHAIRMAN SIEBER: -- is to avoid the embarrassment of the NDE inspector saying, "Hey, it looks good to me with this big red tongue of rust coming out someplace."

MEMBER WALLIS: You have haven't changed the probability of cracking.

MEMBER SHACK: No, they haven't, that's

right.

MR. MATTHEWS: No, we haven't.

MEMBER WALLIS: You've changed the probability of leaking.

MR. MATTHEWS: Yes --

MEMBER WALLIS: Thank you.

MR. MATTHEWS: -- because we're going to catch it at that time. I should have said it that way.

MEMBER ROSEN: All this talk about low probability, I presume you're going to show us numbers with uncertainties at some point?

MR. MATTHEWS: I'm not sure how much -- Pete's done the uncertainty analysis and all. I'm not sure he's got those plotted on there. But he will show you numbers for the calculated probability.

MEMBER WALLIS: Bill Shack is going to present something with a range of 10^5 or something of uncertainty.

CHAIRMAN FORD: Larry, if you remember last spring at this corresponding meeting, we sure had a lot of concern about the ability of the various inspection techniques to detect damage in these large welded constructions.

MR. MATTHEWS: Yes.

CHAIRMAN FORD: I notice in this morning's presentations, there's nothing at all about probability of detection and inspection techniques. There's no discussion of those. Is it buried in here?

MR. MATTHEWS: On a couple of slides. We have a slide in there that impedes probabilistic fracture mechanics that plots them in the demonstration results and against the POD curve that he's using in his PFM work.

CHAIRMAN FORD: So there will be some data against the POD curve?

MR. MATTHEWS: Yes. Of course it's either detected or undetected. And we show the range.

CHAIRMAN FORD: Could you -- obviously it's going to be a very abbreviated discussion of the inspection techniques and their capabilities. Can you give us some idea of the extent of work that's going on in industry on that topic?

MR. MATTHEWS: We have numerous blind mockups that we discussed with you --

CHAIRMAN FORD: Yes.

MR. MATTHEWS: -- last year and those mockups have been available and the vendors, all of the vendors that are doing NDE on the heads have come in and performed demonstrations. We have the data for those demonstrations showing what's detected, what's not detected, the various locations of the flaws, how deep they were, et cetera.

And all of that data has been collected. There's just a limit to how much we can put into --

CHAIRMAN FORD: Sure.

MR. MATTHEWS: -- one morning. And, you know, we got it down on what slides, showing the UT results versus what Pete's using for his UT on the probability detection curve. But --

CHAIRMAN FORD: Now has this data -- has this -- all these detailed analyses of the various inspection techniques, et cetera, have they been shared with the staff?

MR. MATTHEWS: I'm not sure if we submitted that. They've certainly been available for them to look at.

MR. BATEMAN: I think it's been available but typically what we'll do if there's some new breakthrough, I'll send some of my staff down to the Upper NDE Center and we'll observe what's going on down there. But I think in terms of recent times, I don't think I've done that.

But I mean it is available to us if we --

CHAIRMAN FORD: It's just that I keep

hearing murmurs within the community that yes, we're doing all this fantastic work on PFM, et cetera, et cetera, even given the uncertainties about crack growth rates, et cetera. But we keep on coming across a huge barrier when it comes to the control aspect, the inspection aspect of this.

And we haven't been moving forward at all on this area. This is what I'm hearing within the industry.

Is that a fair comment?

MR. MATTHEWS: I wouldn't think so. I mean the inspections that are performed, when we do the volumetric inspection on these plants, I think they're pretty reliable. And they're picking up very small flaws and people are repairing flaws that are no where near leaking.

CHAIRMAN FORD: It would be really good for us to hear this because this whole committee, in our last letter, in fact, we -- in May of last year, we expressed a huge amount of concern about the capability of inspection techniques.

It would be good for us to hear those concerns are being addressed.

MR. MATTHEWS: And certainly I think we have confidence in the UT results that are coming out.

They look pretty good to us, I think. And they're picking up small flaws, et cetera, OD flaws, ID flaws.

CHAIRMAN FORD: Okay.

MR. MATTHEWS: Our inspection plan, which we will get developed fairly shortly, I hope, will define what those inspection intervals will be and what the details of the coverage and the characteristics.

The next part of the presentation, we want to walk through the development of the failure modes and effects analysis. And I'm going to have Glen White come up and do that.

CHAIRMAN FORD: Thanks.

MR. WHITE: All right. Good morning.

I have about 18 slides this morning. We have a 45-minute slot that's scheduled in the agenda. And I'm going to go through the failure mode and effect analysis. And this is documented in the MRP-110 safety assessment.

I'm the principal author of that document. And it was submitted to the NRC staff on April 14th. That document is the top level document. It references various other evaluations that have been submitted.

Pete Riccardella will be talking about the probabilistic fracture mechanics, which is in another report, MRP-105; 110 references that work. But 110 also includes some evaluations of its own. The failure mode and effect analysis is one and the wastage evaluations is another area where that information is included directly in 110.

All right, so in my talk here, I'll just briefly be introducing the concept of the FMEA, talk about why we performed this sort of analysis for the top head.

I'll go into the scope that is covered, what components are covered, what degradation mechanisms are covered.

And then I'll get into the heart of the FMEA, which is the failure path flow chart, which has been handed out. And so I'll discuss how that chart works and I'll discuss what information goes along with that chart in the MRP-110 report.

I'll give the conclusions and then I'll go through one example, what would we call a disposition path, a failure disposition path, and how that -- to illustrate how the flow chart works.

All right. FMEA is one of the total quality tools that's used often to ensure product reliability. Our goal here, our main goal here, is to ensure safety rather than just product reliability. So the way that we apply this has that in mind first.

But FMEAs do have a common structure of -- the purpose is to identify the various failure modes and their principal characteristics, one being the cause of each.

Two, what effects those modes can have, what are the consequences. The cause and effects, we're using the flow chart to illustrate those relationships.

Thirdly, we have the detectability of each mode and the frequency of occurrence. Those are the main parameters that one has to get after in a failure mode and effect analysis.

All right. The purpose here -- following the North Anna 2 experience, there was a renewed interest in trying to be proactive rather than being reactive to inspection results, to look at the component, and to postulate all the different ways that we could have a failure and without regard to the inspection results.

So this is wiping the sheet clean, thinking about all the different ways we could have a failure, and trying to anticipate all these types of failures and make sure that our inspections covered these types of failures even though we haven't seen them in the field yet.

Another purpose here is to direct technical evaluations. We want to make sure that we're doing the right sorts of evaluations in the right detail. And the FMEA makes sure -- points us in the direction of what detailed engineering evaluations need to be performed and what areas we need to collect additional laboratory or plant data.

We'll move on to the next slide here. We've brought forward the conclusions here before we get into the details. As we'll repeat later on, the main conclusion is that the FMEA confirms that nozzle ejection and head wastage are the two major potential safety concerns that we've already seen on a slide this morning.

And secondly, the FMEA helps define the inspection capabilities that are needed to detect a degradation before defense in depth is compromised.

There's a third concern. And that is the generation of loose parts. And that helps to set the required inspection area.

So it's not necessary to inspect inside the pressure boundary to provide assurance against nozzle ejection. But there's a concern for -- well, of course if cracks grow from outside the pressure boundary into the pressure boundary and up above, it could eventually lead to nozzle ejection.

But there is also a concern to inspect inside the pressure boundary to prevent generation of loose parts.

CHAIRMAN FORD: Can I ask a question of clarity? These FMEA results, are they all for just the vessel head penetration? There's no -- is it the same conclusions apply -- have you done the analysis and found the same conclusions apply, for instance, for pressurizer penetrations?

MR. WHITE: They would be --

CHAIRMAN FORD: Or bottom head penetrations?

MR. WHITE: They would be quite similar. There's separate work going on for other locations. I'm thinking of the bottom head, there are some different components. The consequences of having a break are different.

On the top head, we have control rods often that are in the penetrations. So it's a different situation from the bottom head. So there are some differences. Largely, they're the same. But there will be some differences.

CHAIRMAN FORD: Okay. I've got another question about scope.

At the last meeting in the spring of last year, they also raised a question about BWR bottom head penetrations. I recognize that this is not an MRP program, but within the EPRI family, is there a similar sort of effort going on for BWRs?

MR. MATTHEWS: Within the materials reliability program, this work is all in the pressurized water --

CHAIRMAN FORD: Yes, I recognize that. But within the whole --

MR. MATTHEWS: If there is, it would be in the purview of the BWR VIP. And I believe they've looked at the bottom penetrations but I don't know. It's just not something I've worked on. That's been the BWR VIP efforts.

CHAIRMAN FORD: Okay.

MEMBER ROSEN: And the answer to Dr. Ford's question was the work is going to be extended to include bottom heads as well as other RCS locations?

MR. WHITE: That's right. There's work in progress right now on the bottom head and in other locations. Specifically, there's an FMEA that's being worked on right now for the bottom head.

CHAIRMAN FORD: Now will it be confined just to the penetrations? One of the recent meetings that we had had with other people have indicated that there's a question of where you draw the line in terms of the scope of these.

For instance, the surge lines with respect to the pressurizer. Are you going to keep it strictly to PWR primary penetrations?

MR. MATTHEWS: There's also separate work being done. And I expect the assessment soon to be submitted followed by an inspection evaluation guideline for the butt welds that are throughout the primary system.

CHAIRMAN FORD: Right.

MR. MATTHEWS: So that work is underway and should be submitted shortly. And --

CHAIRMAN FORD: So this overall approach --

MR. MATTHEWS: -- so that's the butt welds. And then this is the top heads, the cooperative effort between MRP and owner's groups is working on the bottom heads. And I believe the Westinghouse Owners Group is primarily, but not solely, but primarily addressing the pressurizer situation.

CHAIRMAN FORD: So this methodology that you're talking about is being applied, obviously, to the vessel head. But you can look forward in the future to seeing a similar methodology applied to all of the PWR and by other people by the BWR components?

MR. WHITE: I thinks that's the general intention.

CHAIRMAN FORD: Is that?

MR. WHITE: Yes.

CHAIRMAN FORD: That's the wish. Okay. Good.

MR. MATTHEWS: I don't want to mislead you. I don't believe there's an FMEA like this as part of the butt weld safety assessment.

CHAIRMAN FORD: Okay.

MR. MATTHEWS: Because, you know, that's primarily a LOCA.

CHAIRMAN FORD: Yes.

MR. MATTHEWS: And a crack growing through the wall.

CHAIRMAN FORD: Could I ask another question which is, again, unfair maybe but we keep looking at these integrity of components to materials degradation under operational conditions. Very rarely do we see, unless it's a very unusual circumstance, as to the performance of degraded materials under severe accident conditions.

Has that got into your thinking as you move forward using this approach? Asking the question what if? What happened if we had a severe accident where you go outside the normal pressure temperature transients under a severe accident condition?

MR. WHITE: Well, we have -- there's been work done on consequential damage. So if we do have a pressure break, a pressure boundary break, what happens next. And that has been considered systematically.

CHAIRMAN FORD: Okay.

MR. WHITE: So -- but I think severe accidents should be -- are considered under that scenario.

CHAIRMAN FORD: But they're not considered about anything that you're -- in the scope of what you're talking about today --

MR. WHITE: No.

CHAIRMAN FORD: -- on primary or to side penetrations?

MR. WHITE: That's right.

CHAIRMAN FORD: Okay. Thank you.

MR. WHITE: Okay. So we'll step forward then to the next slide, which spells out the scope that we looked at.

We have the Alloy 60 nickel alloy nozzle material. The typical nozzle is a CRDM penetration nozzle, four inches in diameter, two and three-quarters inch ID is the nominal size for the typical nozzle.

And the area of the nozzle that's covered by the FMEA is the area in the region of the J-groove weld. The J-groove weld introduces high residual stresses and ovalization due to the weld shrinkage process. And that makes the Alloy 600 material susceptible to cracking, to have the tensile stresses.

So the area that's covered is in the region of the J-groove weld, including the Alloy 600 nozzle, the alloy, generally Alloy 182 weld metal that forms the J-groove weld, and also the weld metal buttering that's applied to the low alloy steel before the nozzle is installed.

So those are the components and materials that are within the FMEA. The FMEA does not cover, for example, the -- often there is a butt weld up above towards the CRDM housing. That would not be within the purview of this FMEA we're looking at of the region where there has been cracking or we know there's high stresses.

For each, this chart here identifies the plausible aging degradation mechanisms for each of these materials. These are the mechanisms and materials that are covered in detailed in the FMEA supporting technical discussions in MRP-110.

So starting with the nozzle, we have a primary water stress corrosion cracking, a low potential stress corrosion cracking-type mode that occur in pure water. We have also a potential concern for environmental fatigue if we have transient loadings.

With the caveat being we have the region above the weld on the OD of the nozzle, if we have leakage to that region, we can have concentration of primary coolant leading to a different chemical environment that primary water.

So that potentially can put us in a mode of cracking that is not classical for a water stress corrosion cracking potentially if we have an environment there that's enough off nominal. And then, again, environmental fatigue in that region, too.

When we look at the weld metal and the

weld buttering material, we also have -- we introduce another potential cracking mode that does not effect Alloy 600 and that's low temperature crack propagation that has been observed at elevated levels of hydrogen and relatively low temperature.

And there is a test program that the MRP has sponsored that's in progress looking at that potential for that type of cracking.

The preliminary results of that are that it's believed that the conditions that could lead to that sort of leak propagation are hard to come up with in practical plant conditions but it is being looked at more closely by the MRP. And it's also a crack propagation mode, not a crack initiation mode.

Then the last line --

CHAIRMAN FORD: I'm sorry. Could you repeat that last sentence?

MR. WHITE: It would have to require an existing flaw.

CHAIRMAN FORD: Okay. So it's propagation not --

MR. WHITE: It's a propagation, not initiation.

The last line in the table here refers to the concern for boric acid corrosion, the general corrosion of the low alloy steel material, and then we also have a concern for the potential mode of cracking for the low alloy steels is environmental fatigue.

Stress corrosion cracking is not a plausible degradation mode for the low alloy steel based on test data and plant experience.

MEMBER ROSEN: There's going to be fatigue of the vessel.

MR. WHITE: Well, we don't -- it is a -- you could come up with that. Under the right conditions, yes you can have environmental fatigue of the low alloy steel material.

MEMBER ROSEN: Whether or not these other things are doing anything? You have to worry about fatigue of a vessel? Does that apply to the whole vessel?

MR. WHITE: If you can imagine a crack propagating from the nozzle material.

MEMBER ROSEN: Or from somewhere else.

MR. WHITE: Right.

MEMBER ROSEN: Anywhere in the vessel?

MR. WHITE: Well, the flaw would come from the nozzle or from the weld. Obviously just the -- under without any flaws being introduced there, the head itself has been analyzed for fatigue as part of the design basis of the vessel. But now if we introduce new flaws, that may mean --

MEMBER ROSEN: Bigger flaws that aren't within their control.

MR. WHITE: Well, flaws that wouldn't be considered under the design basis of the vessel.

CHAIRMAN FORD: Glen, could you go back please one slide?

Okay. I just flipped through the rest of your slides and I'm assuming that you were to use this table, you've got to quantify the degree of degradation as a function of time for all the relevant system parameters. And yet in looking through here, I see no such algorithms.

Do you think the algorithms do exist? I mean we've heard about primary water sites just cracking until the cows come home almost. But some of the others -- we have boric acid corrosion. We have heard nothing at all about some qualitative arguments that you made about a year ago.

MR. WHITE: Yes.

CHAIRMAN FORD: About the degrade is important but nothing else quantified. Similarly, low temperature crack propagation and low temperature fracture toughness, I know of no algorithms that give how those arise as a function of time.

Environmental fatigue, yes, from work at Argon and other places. Presumably all these degradation modes are quantified as a function of system parameters.

MR. WHITE: Well, if we take them one at a time, the low temperature crack propagation, there's -- just in the last year, the MRP has initiated testing.

CHAIRMAN FORD: Yes.

MR. WHITE: And so we're moving towards understanding what conditions this type of crack propagation could occur. And so that work is not complete yet. But the preliminary conclusions are that it's very difficult to come up with these conditions in the practical plant experience.

CHAIRMAN FORD: And you can't bound them, like you say it's impossible to have such and such a system parameter like environment or whatever the environmental component is? You can't bound it? The probability that you could have degradation by that particular mechanism?

MR. WHITE: Well, we haven't done that. I'd have to say that that work is in progress.

CHAIRMAN FORD: Okay. Okay. I notice

also that these are all for old materials. There's no mention at all about 690, 52, 152, 172 --

MR. WHITE: Well, the --

CHAIRMAN FORD: -- the advanced materials.

MR. WHITE: -- the FMEA itself would largely, almost completely, apply what we've done to the 690 heads. The main difference is is that the 690 material is much more resistant to cracking, upper stress erosion cracking.

CHAIRMAN FORD: And we have data -- I recognize that.

MR. WHITE: Yes. So we --

CHAIRMAN FORD: But we have got solid propagation rate data to show that?

MR. MATTHEWS: Not propagation. We've got data on initiation. If you can't crack it, it's kind of hard to grow a crack. But we're working in the MRP to see if we can't come up with some crack propagation rates, which we believe will be orders of magnitude below the safe limits.

CHAIRMAN FORD: But -- well, let me give you a -- I can follow your argument assuming the crack initiates in the 690 or doesn't initiate. But how about crack initiating from a pre-defected 52 or 152 weld, which then hits the 690. And the question is does it propagate on through the 690.

That is a devil's advocate but potentially real situation.

MR. MATTHEWS: It certainly is. And we're trying to work to come up with what are realistic crack growth rates for Alloy 690 and the weld metals. And we have programs underway to do that. But it just takes a while. And we're not there yet.

But it certainly from an initiation standpoint. There's a lot of lab data out there and I'll talk about that later in the presentation.

CHAIRMAN FORD: As we go through this, Glen, you know we've talked about the boric acid, the quantification of boric acid. And that's presumably ongoing. You talked about the low temperature crack propagation and fracture toughness. And also the propagation rates for the alternate alloys.

And you say these are all being done. Is it fair to characterize where we're going on this FMEA approach right now as still in the preliminary stages?

MR. WHITE: No, I think the FMEA is complete in that it has pointed us to conclusions to support the safety assessment, what evaluations we need to do.

But we recognize that there's additional work, additional laboratory work that needs to be performed and is being performed. And then the MRP will follow these developments closely as we get data. And then always go back to the FMEA, back to the MRP-110 safety assessment to make sure that everything continues to be covered by the inspections that are being performed.

CHAIRMAN FORD: Okay. Can you give us some idea as to when some of these other activities will be finished? Is it two years? Five years?

MR. MATTHEWS: No, I think the boric acid corrosion work is supposed to be finished next year. There is a lot of the separate effects, et cetera, being done this year. And then the final mockups, I believe, are next year.

MR. WHITE: Yes, they'll begin early next year is the schedule and then to finish up by the end of 2006. So for the boric acid corrosion work, we're in an experimental program now. There's four different sets of tests that are being performed.

The first three sets of tests will be finished by the end of this year. And that helps to set up the design and configuration test matrix for the mockup tests that will be done in 2005 and 2006.

CHAIRMAN FORD: Okay. But in the mean

time, the thing that -- the thing of concern is that in the mean time, if before the end of 2006, we have another unfortunate incident, are we satisfied from the analyses that you and Dr. Riccardella have done, that we are not -- we do not have a huge risk of something happening between now and 2006 when we will have all the answers?

MR. WHITE: I am satisfied, I think yes we are satisfied. And if we look at the mode for plants that have a relatively high time and temperature is to do -- like right now, they're performing bare metal visual examinations every outage.

The work that we've done is making conservative assumptions on wastage rates and leak rates and so on, shows a very high degree of confidence that those sort of inspections would prevent any large amounts of wastage from occurring.

Then on top of that, we have the non-visual NDE inspections, looking for cracks. And those -- all the plants that have greater than 12 effective degradation years, that's the cutoff that the NRC has established for classifying a plant as high susceptibility.

All those plants have performed their baseline non-visual NDE examinations already. And the plants that are in the next category, the modern category will -- there's only a handful of plants that have not yet performed those inspections.

And they're fast -- over the next couple of outage seasons, they will all nearly performed those inspections. Given that, you know, I think that we have -- we do have this high confidence.

CHAIRMAN FORD: Okay. Thank you.

MEMBER RANSOM: Could I ask a question about where you have cladding, you know, protecting a base metal, do you get wastage if you have a failure in the cladding? And, you know, the wastage occurs from the inside out, in effect? It's a common failure of coatings.

And I'm wondering -- I've never heard anything about that failure mechanism. Is there a reason why that doesn't occur?

VICE CHAIRMAN SIEBER: No oxygen.

MR. MATTHEWS: Yes, lack of oxygen, you typically don't get much corrosion under that cladding without the oxygen present. And that's the disadvantage of the leak to the top head is that it's an open --

VICE CHAIRMAN SIEBER: Right.

MR. MATTHEWS: -- oxygenated environment.

Cracks in the cladding, even the VC, some are cracked through the cladding and hit the low alloy steel nozzle, blunted right at the nozzle and did not propagate into the nozzle.

VICE CHAIRMAN SIEBER: There are reactor vessels that have pieces of cladding missing on the inside that have been dispositioned and the corrosion rate is like a couple mils a year or something like that.

MEMBER RANSOM: Okay.

VICE CHAIRMAN SIEBER: And it's due to the lack of oxygen so they're in service.

MEMBER SHACK: And the low boric acid concentration.

VICE CHAIRMAN SIEBER: Yes, that's true. You know, there's no concentration mechanism.

MEMBER ROSEN: By low, you mean normal operating conditions?

VICE CHAIRMAN SIEBER: Yes, right.

MEMBER ROSEN: As opposed to concentrate?

MR. MATTHEWS: Yes, like when it goes to 22,000 or higher, means that the whole --

MEMBER ROSEN: The highest you're going to get in operation is 2,000 or 2,500 let's say.

VICE CHAIRMAN SIEBER: Right.

MEMBER ROSEN: DPM.

VICE CHAIRMAN SIEBER: Yes.

MEMBER RANSOM: What is the purpose of the cladding?

VICE CHAIRMAN SIEBER: To avoid the two mils per year corrosion. I mean you're generating a lot of corrosion products that are flying around in the cooling system. They're activated. They clog up stuff. They make hot spots. So the rad techs would just go bananas if you didn't have cladding.

MEMBER ROSEN: I think it's more about keeping their RCS clear than the corrosion rate.

VICE CHAIRMAN SIEBER: That's right.

MR. WHITE: Okay. Then why don't we move ahead to --

MR. MATTHEWS: You'd better. You've got about 15 minutes.

MR. WHITE: All right. At this point, I'll introduce the charts that were handed out. The first one -- one of the pages is the failure, the FMEA flow chart that's in the MRP-110 safety assessment. So this is right in the report in Section 2.

The other sheet we'll get to later. That's the example path that we'll follow.

So this chart was developed by the MRP to determine possible ways that these aging degradation mechanisms could lead to safety-significant failures. So the industry sat down and brain stormed, without regard to inspection results, all the different ways that we could postulate breaks of the pressure boundary and leakage.

We wrote down all these different mechanisms and then thought about all of the different operations, materials, fabrication issues that would impact the likelihood of these degradation modes occurring. And this is -- the chart is the end product of that work.

There are -- so Section 2 of MRP-110 summarizes the FMEA, presents the flow chart. And then two appendices in the back of the report provide detailed material that goes along with the flow chart.

Appendix B is a detailed table that goes on for about 30 pages. And it covers each failure path in this flow chart.

So as we move from the bottom of the chart, which covers operations and materials and fabrication-type issues, up towards the aging degradation modes, into leakage and up into wastage, loss of coolant accidents, and ultimately moving into the possibility of core damage, the safety-significant failure, the table covers all of these different paths that are connecting the different boxes.

So we have, you know, the chart itself shows the relationships. But it doesn't summarize what we've done to understand each of these failure paths. That's what's in Appendix B of the report.

MEMBER ROSEN: Do you have an example of what's in here that you can show us of that kind of thing? Maybe one or two of those entries from that table?

MR. WHITE: Well, we'll go through an example disposition path. I don't have any example slides from the table itself.

VICE CHAIRMAN SIEBER: Do you have data that shows the probability of achieving each one of these blocks? In other words, some of these are pretty improbable. And other ones have a greater probability of being the pathway.

MR. WHITE: Yes.

VICE CHAIRMAN SIEBER: And I was wondering if you comment on that work?

MR. WHITE: As we'll hear Pete Riccardella in the next talk, we'll talk about the calculations that show the probabilities.

CHAIRMAN FORD: But Peter's is primarily

just a stress corrosion cracking unless I'm wrong. And yet you've got a whole range of other possible degradation mechanisms. Is it fair to -- it's frustrating for us, I guess, because we haven't seen the report.

But in that report, are the conclusions relating to what Jack Sieber was asking, are they what you would call engineering judgment or is there some analysis based on data?

MR. WHITE: Well, yes, it's based on data and evaluations. The nozzle ejection -- what we calculate for the increase in core damage frequency due to nozzle ejection is the bounding mechanism, bounding failure path. Okay?

MEMBER ROSEN: Let me pick up on Jack Sieber's question about quantification of your chart. The interesting thing about this chart is if you turn it and hold it this way instead of the way you would expect to hold it vertically --

VICE CHAIRMAN SIEBER: I would have held it upside down.

MEMBER ROSEN: -- you have a -- and put the numbers at the branch points, what we call -- what us PRA guys call split fractions, you can calculate the likelihood of core damage frequency --

VICE CHAIRMAN SIEBER: Right.

MEMBER ROSEN: -- based on your knowledge, state of knowledge across the chart this way. And that would be the exercise that I would -- I think would be of high value. It tells me what the probability of large early release is.

Given all these mechanisms and all -- everything you know about all these things, these are just the sequences here listed this way. And across the chart it's an event tree waiting to happen, waiting to be filled in, I should say.

VICE CHAIRMAN SIEBER: Yes.

MR. WHITE: Well, we think we've done the appropriate quantitative evaluations to support the answer of what the appropriate inspections are.

MEMBER ROSEN: I'm looking forward to hearing about it.

VICE CHAIRMAN SIEBER: Yes, and we'll let you know whether you have or not.

(Laughter.)

MEMBER KRESS: Normally, failure modes and effects analysis do that adding up of the sequence events. They do that normally.

MR. WHITE: All right. And there is a second appendix that goes along with the flow chart.

And that's Appendix C. This is an expert elucidation of the issues associated with the chart, specifically the materials, fabrication, water chemistry, aging, degradation. So that is documented in Appendix C.

And here's a Table of Contents for Appendix C, materials and effects on cracking, fabrication and effects, water chemistry. And then how those issues flow into the degradation mechanisms of PWSEC, fatigue, the low temperature crack propagation.

And then there's a couple section -- or one section on nozzle reliability, repair reliability.

CHAIRMAN FORD: You mentioned expert elucidation.

MR. WHITE: Elucidation.

CHAIRMAN FORD: This gives the impression that all of these algorithms come out in this Appendix C are based on people talking?

MR. WHITE: No, this summarizes all the laboratory data and plant experience --

CHAIRMAN FORD: Okay.

MR. WHITE: -- that we have that sheds light on these various parameters.

CHAIRMAN FORD: Okay. I understand.

MR. WHITE: In other words, we know that

the material is susceptible to stress corrosion cracking. We know that that's been seen in the field. So it's a potentially an active degradation mode at each plant.

And the question becomes well what factors would increase the likelihood of cracking, crack initiation, and what factors would increase the crack growth rate, and how do those factors interrelate, and how does that move you along towards the potential failures.

CHAIRMAN FORD: Okay.

MR. WHITE: All right. Then the next two slides here show the plausible aging degradation mechanisms. And the key parameters that control those mechanisms. And these are based on usually laboratory experience that we know these things to be true.

For primary water stress corrosion cracking, material alloy composition, material structure, meaning the micro structure and presence of defects, and stress and temperature, those are the main parameters that can increase the likelihood of PWSEC.

If we look at the potentially for a non-primary water environment in that annulus above the top of the weld, then we add in pH, electrochemical potential, and the impurities that are present.

Environmental fatigue adds in cyclic stress range, cyclic stress rise time, and mean stress. So those are the transient loading factors that also much be considered when one tries to calculate an environmental fatigue rate.

Then for the low temperature crack propagation, the key there is -- the key parameter, of course, is dissolved hydrogen concentration. And then for boric acid corrosion, I've listed here many of the factors that would be expected to play a role.

We have mentioned already the oxygen concentration, but there are many other parameters here.

CHAIRMAN FORD: Now I'm sorry to keep asking this question, but the quantification of these -- the rate at which you degrade, obviously it's going to be a very complicated equation that takes into account all of those variables plus the secondary interactions between those variables.

MR. WHITE: Right.

CHAIRMAN FORD: Do you think that we have those qualified algorithms? Qualified against data, that is.

MR. WHITE: Well, we have our experimental data that sheds a lot of light on what we expect a process to be like. And we have some holes in that experimental data and that's the purpose of the present test program that's going forward.

Then you also have evaluations based on that test data, based on plant experience, that shows that we have confidence that this process takes time.

CHAIRMAN FORD: So if I looked up in MRP-110, I would see those, preliminarily at least, I would see those preliminary, at least, algorithms --

MR. WHITE: Yes.

CHAIRMAN FORD: -- for those various degradation plus the data to support those algorithms.

MR. WHITE: The models that we have, we tried to make conservative -- we have a probabilistic wastage model, this document in the MRP-110.

CHAIRMAN FORD: Okay.

MR. WHITE: We're not -- we don't have time today to get into all those details. We would --

CHAIRMAN FORD: But it's in MRP-110?

MR. WHITE: Yes.

CHAIRMAN FORD: I recognize you've got a time constraint right now. But --

MR. WHITE: Right. It's all there documented. We're not calculating the detailed, point by point water chemistry, boric acid concentration at each point in the grid, and calculating local corrosion rates. There isn't the data to support that sort of detailed evaluation.

So what we've done is we've linked leak rates to crack growth rates. And we've linked boric acid corrosion wastage rates with leak rates and then postulated an area over which this wastage is occurring to try to get an estimate of how fast this process could occur.

CHAIRMAN FORD: So when you take this FMEA approach, the output of it is damage by whatever the metrics are, degree of damage, as a function of all these variables. You're going to come up with very large uncertainty because many of those input key parameters you don't know what they are in any great detail.

So you're going to have large uncertainties to what that damage rate is. What damage rate do you put into your safety analysis, your next step? The worst case scenario? I recognize what you're doing in cracking.

But I'm talking about, for instance boric acid corrosion because it worries me -- well, it concerns me that we can have one nozzle with huge amount of boric acid corrosion and the very next nozzle to it has got a crack but you have no boric acid corrosion. As does your methodology predict that divergence of a response?

MR. WHITE: Yes, we do cover this in detail.

CHAIRMAN FORD: Okay.

MR. WHITE: It's not really part of the FMEA. But to answer your question, we've looked very carefully at the plant experience. There's been about 55 leaking CRDM nozzles. There's been 55 leaking CRDM nozzles. There's been other leaking nozzles in other locations.

Look carefully at this experience and carefully at the Davis-Besse experience where you do have the large cavity and look -- and try to explain the reasons for the difference and factor that into our modeling and our evaluations.

The main -- our best understanding of the main difference is the leak rate that occurred. And at the typical leak rate that's seen at most of these 55 leakers has been very low leak rates signified by small amounts of deposits.

For Davis-Besse, there is an extensive root cause evaluation report that was done by the utility. The best estimate is that the leak rate was upwards of .15 gallons per minute near the end of that process.

When we look at heat transfer calculations, that .15 gallons per minute is enough to cool the local area all the way down towards saturation temperature at atmospheric pressure. So we know that there's -- once you have that high leak rate, then there's the potential there for extensive local cooling, which allows liquid to exist all through the annulus and even on top of the head.

CHAIRMAN FORD: And there's data to support that?

MR. WHITE: Yes.

CHAIRMAN FORD: This expansion cooling? There's data to support the -- I realize you can do the analyses and the sums. But there is data to support the conclusions? Yes?

VICE CHAIRMAN SIEBER: Well all these leaks are progressive kinds of things. You start out with a very tiny leak in the geometry and temperatures are insufficient to create the chemistry along with oxygen to cause the corrosion.

And then you get finally, through an erosion or erosive mechanism, you finally get to a geometry where corrosion takes over and then everything goes pretty fast after that. Fast being measured in years. But it's still pretty fast.

MEMBER RANSOM: Well, that's a good point, Jack. It seemed like there is one other fact, which is the initial geometry.

VICE CHAIRMAN SIEBER: Yes.

MEMBER RANSOM: You know like Davis-Besse, it occurred up on top of the head and where it clearly, after you eroded a little away, you could retain, you know, the concentrated boric acid which then accelerated the corrosion.

And now I wonder if there are other places on a reactor geometry where a leak would actually result in a pool of boric acid that can concentrate?

VICE CHAIRMAN SIEBER: I think that could occur anyplace on the curvature of the head.

MR. MATTHEWS: Well, any horizontal surface or --

VICE CHAIRMAN SIEBER: It's just going to change the rate at which --

MR. MATTHEWS: -- or near horizontal.

VICE CHAIRMAN SIEBER: -- it happens.

MEMBER RANSOM: Is that a factor that should be considered in looking for this kind of thing?

MR. MATTHEWS: Yes. I think that's one of the things that we do look at in our walk downs and everything else.

The primary questions that we have if penetrating a horizontal surface is the reactor vessel hit. A lot of the others are on the bottom. But you can get wastage there, too, if the leak rate gets high enough.

Dr. Ford, we've used all of his time and we haven't gotten to the chart.

CHAIRMAN FORD: I recognize that.

MEMBER WALLIS: But can I ask a question? Effluence has no effect on any of this cracking?

VICE CHAIRMAN SIEBER: No.

MR. MATTHEWS: No, most of this stuff is into the very, very --

MEMBER WALLIS: Because there's so much water -- so much water --

MR. MATTHEWS: Yes.

MEMBER WALLIS: -- between the vessel -- the head and the core, is that what it is?

MR. MATTHEWS: Yes, it's far enough away that the effluence is going to be very low.

MEMBER WALLIS: Just -- it has been

evaluated then?

VICE CHAIRMAN SIEBER: Yes.

MR. MATTHEWS: I don't know that we did effluence calculations there as part of our work.

MEMBER WALLIS: So that there's no effect of the nuclear environment on this at all?

MR. MATTHEWS: I don't think so. Did you guys come up with that in the FMEA? I don't believe there is.

MEMBER KRESS: I want to ask another question about this flow rate as the indicator of whether you go to Davis-Besse or not, it appears to me like if you have that high of a flow rate cooling the top part of that head enough to allow a pool of water to exist, that the temperature is such that the rate of wastage ought to be very low.

MR. MATTHEWS: No, the wastage tends to be very high right at the boiling point of the water because you get this extreme concentration of the boric acid in an oxygenated environment.

VICE CHAIRMAN SIEBER: Right.

MR. MATTHEWS: And so you're going from -- all the way to 22,000 PPM or more, right at the metal surface as this water boils over.

MEMBER KRESS: How do you concentrate the boric acid at the boiling point of water?

VICE CHAIRMAN SIEBER: You boil off the water.

MR. MATTHEWS: Well, you're boiling off the water and leaving the acid.

MEMBER KRESS: You don't -- you're talking about atmospheric pressure boiling?

MR. MATTHEWS: Yes.

MEMBER KRESS: Mostly the boric acid goes with the steam then. And doesn't concentrate.

MR. MATTHEWS: There is some carry over but there's certainly some left behind, too.

MR. WHITE: Volatility is limited to ten percent.

MEMBER KRESS: Yes, I would worry about that analysis. I think it's harder, very hard to concentrate boric acid at atmospheric boiling of water. I think you concentrate it at higher pressures. Or the --

MR. WHITE: Well, certainly some of the wastage events that have taken place back in Florida at Turkey Point, you're dripping water onto hot metal. And as it goes from liquid to vapor, it leaves behind a very concentrated situation that erodes the head.

VICE CHAIRMAN SIEBER: That's right.

MEMBER KRESS: Well, I need to --

VICE CHAIRMAN SIEBER: Or to carbon steel.

MEMBER KRESS: -- look at the -- I need to look at my activity coefficients in chemistry a little closer. But it's been my impression that low pressure boiling of water to steam carries most of the boric acid with it. And it doesn't concentrate in the liquid phase.

MR. WHITE: I believe the volatility is limited to ten percent of --

MEMBER WALLIS: Well, it takes the oxygen with it, too, so you've got to look at how the oxygen gets to the surface.

MEMBER KRESS: Anyway, I worry about that Siegel criteria a little bit for Davis-Besse.

CHAIRMAN FORD: Now what is your plan here? You've got three more talks.

MR. MATTHEWS: I'll cut mine short.

CHAIRMAN FORD: Okay.

MR. MATTHEWS: But I think you want to hear this chart.

CHAIRMAN FORD: Yes, I think so.

MR. MATTHEWS: And then Pete's work on the PFM.

CHAIRMAN FORD: Well, so far it's all

words. I'd like to see something --

MR. MATTHEWS: Yes, put the chart up. Go to the chart.

MR. WHITE: Okay. We'll skip ahead. This just shows the different flaw geometries. And then here is the chart. We've already talked a little bit about it.

We have -- the yellow color signifies fabrication-type issue.

MEMBER WALLIS: Now each one of these boxes refers to equation so and so, which is how you calculate these things?

MR. WHITE: No.

MEMBER WALLIS: It doesn't? Well how do you do it then.

MR. WHITE: This is not intended to be an event tree.

MEMBER WALLIS: But eventually it has to be doesn't it?

MR. WHITE: Well, no, I think if we understand -- we understand it quantitatively enough so that we model the bounding events here.

MEMBER WALLIS: Okay. So there is an analytic tool used in the paths that matter, every box.

MR. WHITE: Yes, that's true.

MEMBER ROSEN: What are the circles with the alphabetic indications?

MR. MATTHEWS: Connection points.

VICE CHAIRMAN SIEBER: Right.

MEMBER ROSEN: So -- but --

MR. MATTHEWS: A goes to A, B goes to B.

MEMBER ROSEN: Okay. So I have to find -- you have to find the other one?

MR. MATTHEWS: Yes. It's a puzzle.

MR. WHITE: Except we do have a table that helps connect everything.

VICE CHAIRMAN SIEBER: If you didn't have those, that chart would be as big as this room.

MR. WHITE: And we'll -- why don't we move ahead towards the example.

VICE CHAIRMAN SIEBER: Yes, okay.

MEMBER RANSOM: I wonder, are there consequences of these boxes on the right at the top level that don't seem to lead to anything like prevent control rod drop, damage to fuel pins, damage to bottom reactor vessel. I mean some of those are kind of obvious that they may not relieve the consequences.

But like failure for control rods to drop must lead to an atlas or something.

MR. WHITE: For a single control rod drop, that's an analyzed event.

VICE CHAIRMAN SIEBER: Yes.

MR. WHITE: So that's within the design basis of the plant.

VICE CHAIRMAN SIEBER: And it's not particularly significant either.

MR. WHITE: But we have a box of prevent multiple control rod drops and that does lead to core damage.

VICE CHAIRMAN SIEBER: Well --

MR. WHITE: So that is specifically covered in the consequential damage part of our report where we look at postulating event with multiple rods not dropping.

MEMBER ROSEN: Okay so this prevent control rod drop is more like the single control one?

MR. WHITE: Yes.

MEMBER ROSEN: Failure to insert one control rod you mean?

VICE CHAIRMAN SIEBER: Yes.

MEMBER ROSEN: It can be read another way, which is the plant is designed to withstand and, in fact, preventing multiple control rod drops is called the scram if you read it the other way. You see what

I'm saying. It's just a little ambiguous language in that red block.

MR. WHITE: Okay. This slide here just goes over the different colors that are in the chart at fabrication plant operation, in the aging degradation modes, leakage, wastage, loose parts. And there are captured loose parts and released parts. And then the actual events. So that's how we systematically categorize everything.

We used different colors for the different failure paths. Red is reserved for those failure paths that we conclude are not credible based on a relatively high bar of evaluations.

MR. MATTHEWS: That's paths, not boxes, it's the path. If the arrow is red --

MR. WHITE: Right. And then the other failure paths we either categorized as not actionable or actionable. Not actionable means that this condition cannot be reliably detected. And, therefore, this failure path must be caught at a higher level in the chart.

And the green failure paths are actionable, meaning that the inspections that are required are designed to catch the process at those points.

MEMBER WALLIS: The coloring is backwards, isn't it? I mean you want to say green for okay. And red for bad. Green should be not credible.

MEMBER BONACA: These are not stop lights.

MEMBER WALLIS: It would be a good idea to go through the example then.

MR. WHITE: Yes.

CHAIRMAN FORD: Skip to the example.

MEMBER WALLIS: But the way we think about greens and reds in terms of the --

MR. WHITE: So the other chart --

MEMBER BONACA: Just reevaluate your thinking.

MR. WHITE: -- is the example. And this is grayed out everything that's not in the example path. So we still have material there but it's not being highlighted. And then we'll go through the four slides here -- or three slides to show the failure path.

What we assumed here is cracking that's occurring in the Alloy 600 nozzle tube. And what sort of process. We know because of all these nozzles are installed with the J-groove weld, so we know that there are high stresses. We know that it's Alloy 600. We know that we have high temperature water. So we're basically susceptible to this type of degradation.

But there are factors that can accelerate the time that initiation occurs at. And I've highlighted here a couple possibilities. One is nozzle roll straightening during material processing.

So you can imagine that perhaps some stresses, some residual stresses are introduced in the material as part of the nozzle manufacturing process.

MEMBER WALLIS: Well, this is an example. Did you put numbers on these boxes in some way?

VICE CHAIRMAN SIEBER: Yes.

MR. WHITE: Well, there's no -- we don't have ways of quantifying a lot of these fabrication --

MEMBER WALLIS: So why do it?

MR. WHITE: Is we want to understand what things can make things --

MEMBER KRESS: Usually in an FMEA, what they do is talk about high, medium, low probabilities, or non-credible. And take expert opinions rather than put numbers on them.

MEMBER WALLIS: But I think -- was it Lord Kelvin said if you don't put numbers on it, you don't understand it?

MEMBER KRESS: Well, they have sort of a range of numbers of probabilities in mind with these high, mediums, and lows. But they're a lot -- this is a loose kind of PRA. It's not as quantitative as a normal PRA.

VICE CHAIRMAN SIEBER: You can't really draw from this that if you cold work a nozzle, that sooner or later you may have to blow the sirens, right?

MEMBER WALLIS: Is what Dr. Kress is saying -- is that in fact how you have done this? I mean I'm looking at, for instance, just following surface cooled work.

And going up the tree to crack growth rate, we know from operating experience that you can increase the cracking susceptibility in terms of growth rates by a factor of 10, a factor of 100, all other things being equal just from that one thing.

So how do you put that observed fact into this decision? Is it surface cooled work and you say .9 going one way and .1 going the other way? Or .99 going one way?

VICE CHAIRMAN SIEBER: He just told you.

MR. WHITE: Well, and then we have these classified as blue, meaning that we can't do anything about the fabrication conditions that's in the plant.

MEMBER WALLIS: Oh, my sainted aunt.

MR. WHITE: That's history. So what we do is we want to make sure that our inspection is up above that catch the cracking are -- consider that the timing of those inspections are being done often enough so that we're making sure that we account for the possibility of these other factors.

Okay, so then the quantitative models, like we're going to hear from Pete Riccardella, and those models we want to make sure that they consider these factors down here. So this is a way to keep on top of all these different possibilities.

MEMBER ROSEN: So let me be sure I understand. Now Peter told me that a factor of ten cold working could increase the likelihood of cracking by a factor of ten. As a PRA guy, I would say that's a .9 split fraction one way. And .1 the other.

But you say no, we're going to put a one on there, 1.0 --

MR. WHITE: No, no.

MEMBER ROSEN: -- if you use -- cold work these machines, these tubes, and you do, if you fabricate them, you cold work them. That's a factor of one.

So these, in fact, doing it that way makes it conservative. It makes the answer conservative I think. A little bit. Not in that case but maybe more in others.

MR. WHITE: Well, I can be specific about the cold working. We recognize that that's an issue. We've written up the relevant laboratory studies in this document. But then we say well which of the nozzles that we've looked at for plant experience have been cold worked?

Well, we know that all of these nozzles are of such dimensions that they had to be machined. And we know, in fact, they were machined to final dimensions. So they all have cold working before the welding process. And we know that that's a bad condition.

So then we go and evaluate the inspection results and come up with statistical curves for our modeling efforts, then we know that we're considering that factor appropriately.

CHAIRMAN FORD: You know it would be tremendously useful once the staff have had their go at this, if we get involved so we can understand some of your rationale, both qualitative and quantitative rationales for going over this tree.

Let's move on.

VICE CHAIRMAN SIEBER: The inspection

process lies around Level 7 or Level 8. And if you do the inspections at the right rate and well enough, you never go any higher on this chart than that.

And so that's really the point of doing all of this is to say if I don't do anything at all for a long period of time, here are some of the things that could happen.

But I don't want a lot of those things to happen so I've got to stop the process, the degradation process somewhere along the line where I can do something about it.

And so that's where you put in your inspections and so forth. And that's the way I interpret rather than try to be real rigorous about it and say if I don't do anything, which would be a bad move, then I'm going to have this kind of an accident.

And I don't think that's what they're trying to show.

MR. MATTHEWS: And it really wasn't put together to be a full-blown risk assessment all the way to the top give nothing happening. I mean it was put together to help us make sure that the inspection regime that we come up with covers all of the possible degradation mechanisms that we know about that are credibly.

VICE CHAIRMAN SIEBER: Right.

CHAIRMAN FORD: Okay.

MR. WHITE: All right so if we just move on up to we've now assumed that we've initiated a flaw. Here we're looking at an OD flaw, axial flaw in the nozzle.

And so if I just quickly come out to -- so this would be a flaw. Like this one here. There would be a nozzle going up. So once it goes through the well zone and up to the annulus that we would have leakage occurring to the annulus.

And now that flaw can continue to grow upwards either by a like a stress corrosion cracking as the growth mechanism in this case. And potentially for coalescence with any existing fabrication defects for example. It would have to increase the effect of crack growth rate.

And then -- and now we move, once we reach the top of the weld, then we would have leakage occurring.

MEMBER ROSEN: Why don't you use the laser pointer so you can go up and down.

MR. WHITE: Okay, so that crack growing through the weld zone, reaching the top of the annulus produces leakage. And then that leakage, if that leak rate increases, you can have wastage concerns. So that's covered in this area of the chart.

VICE CHAIRMAN SIEBER: Right.

MR. WHITE: But here we're looking at the possibility of nozzle ejection so we would move -- we can move into initiation of or branching to an OD circ crack about the weld. So now we've created this wetting environment on the OD of the nozzle.

And so we could have branching or initiation of a new crack with the circumferential geometry. And if that circumferential crack grew to greater than about 95 percent of the wall cross section, then we would have the nozzle ejection event occurring.

MEMBER ROSEN: Now I understand your point about using this in a system way rather than as a PRA way. But once you've reduced it to the five or six blocks, it seems very easy to ask your experts what's the likelihood, given an RPV head leak, that we will have initiation of or branching to OD circ cracks.

And they will look at your data and so oh, it's about a third, one in three. And you can easily put that on the chart.

MR. MATTHEWS: And I think Pete's work tries to conservatively bound this growth, circ flaw growth up through nozzle ejection, it tries to bound that in a probabilistic fracture mechanics method.

MEMBER ROSEN: Using the kind of logic I just used, for example, there?

MR. MATTHEWS: Some of that is back up there.

MEMBER ROSEN: See, I would believe that as long as it was presented in a way that you took me through the steps and said here's why we think that. We think it's one in three if you are at this branch point. And here's why.

And I'd think about that with you for a while and come to a conclusion of my own. I could put a different number next to it if I thought it was different.

But at the end, you could tell me here's the likelihood of nozzle ejection. And I can say, yes, I think you're a little high, you know, maybe you're a little low.

But it would be a way of thinking about this in rational terms.

MR. MATTHEWS: And that's the point of the PFM work. And we do get into benchmarking network against the field data also with circ flaws and leakage.

MEMBER ROSEN: Well, what I'm trying to say is don't shy away from it even though I understand it wasn't its principal purpose.

MR. MATTHEWS: Yes, okay.

MEMBER WALLIS: When you talk about growth, I mean a flaw doesn't leak unless it opens up. So there's got to be -- growing is not just growing of a flaw. It's got to open up some. I don't see the opening up process in here.

MR. WHITE: Well, I mean if you have a crack that's communicating to the annulus, then you have some potential for some water molecules would be expected to go there.

MEMBER WALLIS: There's a different between a flaw growing in a material and something actually opening that flaw up so that it becomes a flow path. There's something -- it's not just growing of an infinitesimally thick flaw. That would be an opening up of that.

MEMBER RANSOM: Right.

MEMBER WALLIS: Or is it all just microscopic sort of flaw stage you're looking at?

MR. WHITE: Well, we're, I guess, conservatively assuming that you would have leakage occurring as soon as you have that leak path reaching the annulus.

MEMBER SHACK: That opening that you get is a function of the crack line. I mean that's something that you compute from the crack line. It must be calculated.

MEMBER WALLIS: It must be calculated. The crack opening must be part of this flaw growth analysis.

MR. WHITE: And it is.

MEMBER WALLIS: It is? Because that wasn't clear to me. So I wanted to make sure that is.

CHAIRMAN FORD: Could I make a suggestion just because of time here? Some of these questions might be answered by Pete Riccardella. Maybe, maybe not.

Could we try to bring this one to a close by --

MR. WHITE: I'll finish up.

CHAIRMAN FORD: -- say ten o'clock and then get into Pete's?

MR. WHITE: I can finish up right now actually.

MR. MATTHEWS: Last one.

MR. WHITE: So this is the last slide. Once we reach nozzle ejection, now we immediately have a loss of coolant accident.

And depending on whether or not there is something in the nozzle or not or for the particular plant, it would be classified either as a small break LOCA or medium break LOCA.

And then there's the potential concern for consequential damage because now we have a jet impinging on other nozzles. We have the housing that's been let loose.

And so there's potential for other -- damage to other components. And that's -- so there's a separate evaluation on that.

And those things flow upwards to the nuclear safety concern of core damage. Generally we don't have a large early release concern, separate from core damage, because these components don't -- these types of failures would not compromise the containment building function.

MEMBER WALLIS: Just to look at the big picture here, we look at Level 7 here, there are lots of paths to go from the bottom to the top without passing through RPV head leak. So the message I get from all this is you've got to be darn sure that you can detect those other parts, right?

At Level 7, there are lots of lines that go through Level 7 without passing through the box called RPV head leak.

MR. WHITE: Yes.

MEMBER WALLIS: So you've got to be darn sure that your inspection techniques can get these other paths.

MR. MATTHEWS: Right.

MEMBER WALLIS: That's the big message I get from this apart from all the other stuff.

MR. MATTHEWS: That's right.

Well, we've seen, and we carefully go through the plant experience, which strongly shows that you would expect leakage to occur before you'd have a break. But we're not relying on that.

And hence the non-visual ND inspections are an important part of the evaluations.

MEMBER BONACA: Why do you need to go at all above Failure Level 3? I mean it seems to me all you are focusing on is to prevent nozzle ejections.

MR. WHITE: Because we want to try to calculate the impact on the core damage frequency so we have an idea of what sort of impact this has on the nuclear safety question.

MEMBER KRESS: I would assume that those red boxes of LOCA and core damage and large early release have already been done in PRAs, the conditional.

Given you have a small break LOCA or medium break LOCA, you have the conditional core damage, the conditional containment failure. And those exist already.

MR. WHITE: Right. So we've got work --

MEMBER KRESS: You just plug those in somehow.

MR. WHITE: That's right. We've done work showing that those are bounding events for the RCS piping breaks. And they can be applied to the top head location.

And so what we're -- and Pete Riccardella will talk about how he calculates the initiating event frequency.

And then we can use the conditional core damage probabilities that have already been developed.

MR. MATTHEWS: And the purpose of the consequential damage block was to make sure that there was nothing going on with this particular failure location that would make the conditional core damage probability of the small break LOCA worse --

MEMBER KRESS: Worse than it would have been --

MR. MATTHEWS: -- for this location than for the pipe break.

MEMBER KRESS: -- worse than it would have been otherwise.

MR. MATTHEWS: And that's -- most of that work is done in your PRA space. And we just take that and plug it in.

MEMBER KRESS: Now tell me once again, I forgot the number what was the reactor pressure vessel head leakage rate that you said could lead to a Davis-Besse. I forgot what the number was.

VICE CHAIRMAN SIEBER: 21.

MR. WHITE: .15 gallons per minute.

MEMBER KRESS: .15? What the units on that?

MR. WHITE: Gallons per minute.

VICE CHAIRMAN SIEBER: Well, that's what they estimated Davis-Besse to be.

MR. WHITE: Right.

MEMBER KRESS: And that's based on the cooling required to keep the head --

MR. WHITE: Well, that was the Davis-Besse experience. And we've done calculations that show on the order of .1 gallons per minute is required to give you enough cooling to support a liquid pool or a turbulent rewetting on the top of that.

CHAIRMAN FORD: Okay. What I'd like to do is I'd like to finish your presentation at ten o'clock and then we'll take a break --

MR. MATTHEWS: You want to take the break early and then start on Pete's.

CHAIRMAN FORD: -- at ten o'clock so that Pete's not --

MR. WHITE: Interrupted.

CHAIRMAN FORD: -- interrupted. Okay, so maybe you come to a close here, Glen?

MR. WHITE: I think I'm finished.

MEMBER KRESS: There you go.

MEMBER WALLIS: Well, I do hope we get to something quantitative because all of this is very qualitative so far.

CHAIRMAN FORD: And what I'm suggesting is that -- or maybe you could think about this during the day, that once the staff have had their go at this, then we have a quantitative guidance this for both the boric acid and for the cracking.

MEMBER BONACA: But I think for this, I understand that logic can be qualitative. I mean like a license renewal, I mean you're looking for susceptibility.

And then you implement the problem. You don't measure exactly how much susceptible you have to be to implement the problem.

And so in this particular case, I don't need to know exactly the amount of fabrication or stress I have to deal with to decide to have an action which is yes, that may be a part which is credible.

So I think this is valuable to me as a decision.

CHAIRMAN FORD: I think we'd all agree that it's a very valuable approach. It's just a question of what is the uncertainty of outcome. And that only comes by looking at data.

It's good to go through an example.

MEMBER BONACA: Metallurgy, metallurgy, I mean --

CHAIRMAN FORD: At this point, I am going to recess. Come back at 10:15.

(Whereupon, the foregoing matter went off the record at 9:59 a.m. and went back on the record at 10:15 a.m.)

CHAIRMAN FORD: We're back in session.

And we're going to hear now from Pete Riccardella, Structural Integrity Associates.

Peter.

MR. RICCARDELLA: Thank you.

I sure hope I can live up to the advanced billing. I think on account of my name being mentioned about a dozen times so far this morning --

VICE CHAIRMAN SIEBER: Maybe we got the entire presentation.

MEMBER KRESS: That's the billy goat-Grinch effect.

MR. RICCARDELLA: I'm going to be discussing the probabilistic fracture mechanics project that we've been working on since September of 2001, the objectives of which are to develop a generic methodology to determine probabilities of nozzle leakage and failure, which by failure I mean ejection of a nozzle.

Then to apply that methodology to a sampling of USPWRs in support of the safety assessment work that Glen just described, and finally, use the analysis technique to define an MRP inspection plan that provides an acceptable level of quality and safety.

The report has been submitted. It's MRP 105, and it was submitted to the NRC, I believe, in about mid-April.

A quick overview, a preview of the summary and conclusions of the talk. As I mentioned, a PFM tool has been developed to predict probabilities of leakage and failure.

MEMBER WALLIS: I ask again the question I asked before. I mean, does the PFM tool predict the leakage rate given that you've got a through-wall crack?

MR. RICCARDELLA: We make a conservative assumption in that regard.

MEMBER WALLIS: Well, it seems to me important because we know that you need to get a certain leakage rate before anything interesting happens. So how does that leakage rate develop is important.

MR. RICCARDELLA: You know, if I could defer that question until I get a little further into the presentation.

MEMBER WALLIS: Okay, okay. You're going to address that later.

MR. RICCARDELLA: You'll see how we addressed it.

MEMBER WALLIS: Thank you.

MR. RICCARDELLA: Probably the most significant thing that I'd like to discuss today is the benchmarking and calibration of the tool with respect to the plant inspection data. That's probably the one thing that I -- most of the methodology I presented here last spring, but this is the new thing that we've done that I think is of great significance.

Then we used that benchmark tool to analyze a sample of operating plants, and we looked at three inspection scenarios. We looked at the inspections exactly in accordance with the current NRC order, and then we looked at two alternative inspection programs that the MRP is considering.

To give you a quick review of the conclusions, we concluded that these three inspection cases yielded essentially the same probabilities of leakage and failure; that these probabilities of leakage and failure are within generally accepted limits for those numbers.

Of course, as we know, there are sensitivities and uncertainties in the analysis, and we've looked at the sensitivity to the significant parameters in the analysis, but the specific parameters that we've chosen for the case studies are benchmarked, as I mentioned, and the other thing is that we're looking at this analysis in a comparative basis, and that reasonable changes in these parameters would affect the results for all three inspection plants.

So the comparison should remain valid for reasonable ranges of these analytical parameters.

We've also made deterministic crack growth predictions, and they demonstrate that the inspection intervals that we're coming up with on the PFM based analyses are conservative, and the MRP, as Larry mentioned, is in the process of producing a replacement for the prior MRP-75 document that will propose the MRP inspection plan, and the analyses approach that I'm presenting will be used to evaluate the proposed inspections.

MEMBER WALLIS: Can I ask you about crack growth? Are these cracks the kind of cracks the kind of cracks that go higgaley-piggaley all over the place or are they cracks that go sort of like a straight cut?

MR. RICCARDELLA: They tend to be intergranular branching type.

MEMBER WALLIS: So higgaley-piggaley. So what you actually mean by crack growth is some sort of average or something?

MR. RICCARDELLA: Well, it has been measured in experiments. I mean we're looking at --

MEMBER WALLIS: Well, how would I relate

that to a leakage path? It's very different to have a straight path and to have a higgaley-piggaley --

MR. RICCARDELLA: It's somewhat of a torque's path.

MEMBER WALLIS: Okay.

MR. RICCARDELLA: The elements of the analysis include a Monte Carlo based probabilistic fraction mechanics analysis. We've calculated applied stress intensity factors for circumferential cracks in various nozzle geometries, various plant designs.

A significant portion of the analysis is a Weibull analysis of plant inspection data we've set up to use as a predictive tool to predict the time to leakage or significant cracking. It isn't just leakage we're looking at, but it's any plant which has detected either cracking or leakage is included in the Weibull analysis.

A statistical characterization of laboratory crack growth rates is included. We have a tool for looking at the effective inspections which considers the inspection intervals, the type of inspection, and the probability of detection.

And I have a few slides where I'll show, as Larry mentioned, the probability of detection and how we've attempted to correlate that with inspection demonstration programs.

And then finally we've done -- okay. The most important are the last two bullets and the ones I'm going to spend the most time on today, the benchmarking and calibration of the method with respect to field inspection data and the case studies and results of some of these case studies.

MEMBER WALLIS: The question of stress intensity factors for the circumferential cracks. If you remember back to the pipe cracking days when we were doing some measurements of residual stress, there was tremendous variation of data even for the same classification of pipe and presumably because of different weld heat inputs and things like that.

Have there been any measured residual stress profiles for these types of geometry?

MR. RICCARDELLA: Not on these specific geometries that I'm aware of, but the methods that are being used have been demonstrated, you know, from the piping program that you talked about.

You know, we've taken measurements and then done analyses, and we're using the same analysis technique that was verified with experimentation back then.

CHAIRMAN FORD: But as far as I know,

there have been no analyses done, fundamental analyses done to explain the scatter or the variance of the observed residual stress profiles. So even though you're using the same methodology, will you be going into how you tackle the inevitable fact that there will be a range of residual stress profiles and, therefore, a range of applies stress intensity factors?

MR. RICCARDELLA: Well, I think that we'll be covered by the benchmarking because we're calibrating. We've benchmarked against observances of circ. cracks, and there have been, you know, a significant number of those.

CHAIRMAN FORD: So some of the database, observed database, you use for calibration of your methodology, at what point do you then go off and do it independently?

You can't use your benchmark data to then go and show that you've got the right answer. Obviously it will be the right answer. You've force fitted.

Do you understand what I'm saying?

MR. RICCARDELLA: Yeah. Well, we've set up the analysis as best we could on a theoretical basis. We've considered, you know, the basic elements of crack initiation, crack growth, and then -- but in any probabilistic analysis of this type, there are always variables that can be adjusted that are uncertain, and so then what we've done is we've taken that theoretical analysis technique and said, "Okay. Let's evaluate how it did against the real behavior."

And we actually made some adjustments in some of those parameters to make it match as best we could the data.

CHAIRMAN FORD: At what point do you stop making adjustments?

MR. RICCARDELLA: Well, --

CHAIRMAN FORD: Somewhere along the line, you can no longer fudge reality compared with calculation.

MR. RICCARDELLA: Well, I would say that we stopped. We made these adjustments based on inspection data through spring of 2003, and we have had two outage seasons of inspections since then, and there have been no surprises that, you know, we've used the model --

CHAIRMAN FORD: To readjust your --

MR. RICCARDELLA: Yes. We haven't had to go back and readjust yet.

CHAIRMAN FORD: Okay.

PARTICIPANTS: Yes.

MR. RICCARDELLA: Okay?

CHAIRMAN FORD: Okay.

VICE CHAIRMAN SIEBER: The call for fudging is minimized.

CHAIRMAN FORD: Well, or sharpening a pencil.

MR. RICCARDELLA: Okay. As I mentioned, it's a Monte Carlo PFM model, a time dependent Monte Carlo analysis scheme. So each iteration or each simulation in the analysis steps through a full 40 years of 60 years of plant operation and predicts the probability of leakage or nozzle versus time for a specific set of parameters.

We have a series of deterministic parameters and a series of random variables. The most significant of the random variables are noted in the second bullet here, and because you have a head with multiple nozzles, it's actually two nested Monte Carlo dooloops (phonetic). If we're analyzing a head that, say, has 50 or 60 nozzles, we step through time for each nozzle from zero to 40 years and predict crack initiation, crack propagation and then we do it for the next nozzle and then for the next nozzle.

MEMBER WALLIS: Well, what's different

about the nozzles?

MR. RICCARDELLA: Oh, there are different nozzle angles, therefore different stresses, and then there's also variabilities in the material properties.

MEMBER WALLIS: Temperature?

MR. RICCARDELLA: Temperature we've treated as a random variable. So if --

MEMBER WALLIS: Do you know enough about the material property variations between otherwise identical nozzle?

MR. RICCARDELLA: Yeah, we have data on crack growth rates based both on the mean of a heat of material. The testing that has been used, I think, was 26 different heats of material, a total of 156 specimens. So we have heat to heat variability as well as within heat variability based on the test data.

MEMBER WALLIS: So you have test data for every batch of nozzles.

MR. RICCARDELLA: Not for every batch, no, no.

MEMBER WALLIS: Not for every batch. So what do you do with the ones you don't have any test data for?

MR. RICCARDELLA: Well, we assume that

they're bounded by the 26 heat --

PARTICIPANT: They come out of that population.

MR. RICCARDELLA: They come out of that population.

MEMBER WALLIS: It's a pretty broad population.

MR. RICCARDELLA: Oh, yeah, it is. It's, you know, data from many different countries and many different laboratories, and the data has been reviewed --

MEMBER WALLIS: Well, I thought heat was one of the biggest variables here.

MR. RICCARDELLA: Pardon me?

MEMBER WALLIS: Heat is one of the biggest variables. You might just happen to get a batch of bad heats and --

CHAIRMAN FORD: But their argument is, I think, was it 26 heats?

MEMBER WALLIS: Somehow.

MEMBER ROSEN: But they do have a bad heat in there, don't you? One is very bad.

VICE CHAIRMAN SIEBER: That's right.

MEMBER ROSEN: Can you imagine that could be worse? I guess you could, but --

VICE CHAIRMAN SIEBER: Well, it would have leaked by now were it worse.

MEMBER ROSEN: You know, that data set was compiled and reviewed by a panel of international experts, and this is, you know, based on a report that was published by that panel of experts, a report published based on the opinions of that panel of experts.

A lot of data was actually removed because of uncertainties in the test conditions. There's a term you use for the data. It was --

PARTICIPANT: Scrubbed.

MEMBER ROSEN: Scrubbed.

CHAIRMAN FORD: Could I ask a question? Going back, you mentioned that one of the prime geometric variables is the angle of attack, and I know that a lot of work has been done about changes in stress. Is it, therefore, a relatively small jump to go to, for instance, a pressurizer nozzle? Are we not taking a quantum jump in unknowns here? Is that correct?

MR. RICCARDELLA: Well, you know, the physical geometry of the pressurizer, they're smaller diameter, thinner walled. The welds are different. The welds are totally within the cladding. There's no direct weld in the pressurizer nozzles. The J-groove weld is totally within the cladding. The cladding is Alloy 600 or Alloy 82. So there are some significant differences.

CHAIRMAN FORD: Well, the reason why I'm asking the question is, of course, we've now got this whole question about the pressurizer nozzles and the bottom head nozzles and what are the risks associated with these new ones, an we have all of the work on these vessel head penetrations.

Are you seeing another five years before we have a methodology for --

MR. RICCARDELLA: Certainly the methodology that we've developed is directly applicable to the pressurizers, but we would have to rerun some of the stress intensity factors and that sort of thing.

MEMBER SHACK: Pete, why is the head temperature a random variable? I would have thought that was one of the few things that we actually did know.

MR. RICCARDELLA: What it turns out, in most of the analyses we set, in the analyses that I'm basing the results on we set it as known. Okay? But it's in there as a random -- you know, we put it in there with a mean and a standard deviation of .001 in those analyses, but then I did look at the sensitivity of it. You know, I used the standard deviation of five and --

MEMBER SHACK: Okay. So you're really looking at an uncertainty in the head temperature.

MR. RICCARDELLA: Yeah, and specifically, you know, just some data from the field. A couple of plants have instrumented the nozzles to measure temperature, and the conclusions of those studies were that the mean head temperature is pretty consistent with what we've expected, but that nozzle-to-nozzle variation could be as much as plus or minus ten degrees.

MEMBER SHACK: Oh, which is quite significant.

VICE CHAIRMAN SIEBER: Because of the cooling.

MR. RICCARDELLA: Well, yeah, because you can get streaming. You can get flow streaming and differences, and so we looked at that.

So we looked at that, and the conclusion is that as long as your mean temperature prediction is pretty good, the effect on the probabilities of leakage and failure of that variability is not much because you have some nozzles are all faster and some nozzles are slower, but on the average they come out about right.

MEMBER ROSEN: Isn't the one that we're worried about the one that's faster?

MR. RICCARDELLA: Yeah, but we're predicting the probability of the one that's the fastest. I mean, that's why we step -- that's why we stepped through all of these nozzles as we do, and we do, you know, hundreds of thousands of head simulations to predict that one or two nozzles that leak the fastest.

MEMBER WALLIS: When you quote head temperatures, is that the temperature of the stainless steel cladding or is that the temperature on the average across the field or what?

MR. MATTHEWS: It's actually the coolant.

MR. RICCARDELLA: The fluid temperature in the vicinity.

MEMBER WALLIS: The fluid temperature?

MR. RICCARDELLA: Yeah.

MEMBER WALLIS: That's not at temperature at all.

MR. RICCARDELLA: No, but that's what we've correlated all of the data with, is the fluid temperature.

MEMBER WALLIS: But the temperature of the head is presumably not uniform.

MR. RICCARDELLA: That's true.

VICE CHAIRMAN SIEBER: It's not.

MR. RICCARDELLA: That's why we treat it as a random variable.

MR. MATTHEWS: He means uniform through the thickness of the --

MR. RICCARDELLA: Oh, no, oh, no. Yeah, that's right, but we've just chosen to calibrate or to -- "calibrate" is the wrong word -- to index all of our leakage and cracking data with respect to coolant temperature.

MEMBER WALLIS: If it's the fluid temperature, okay.

MEMBER ROSEN: And that's the degree of conservatism because the head is not as hot as the fluid.

MR. RICCARDELLA: No, I wouldn't argue that that's conservative because we're just taking -- you know, we're plotting the number of cracks which is something. But we're looking empirically at the number of leaks and the number of cracks versus something, and we've chosen to plot that versus coolant temperature.

If we plotted it versus head temperature we'd get a different curve.

VICE CHAIRMAN SIEBER: And you don't measure head temperature. You measure coolant temperature.

MR. RICCARDELLA: Yeah. Well, you --

VICE CHAIRMAN SIEBER: It's reflected in the pH temperature depending on the flow pattern.

MR. RICCARDELLA: Okay. So just stepping through, and again, the first few bullets on the summary, I'm just going to go through real quickly because I've presented this before, we've performed a series of stress intensity factor calculations for four different plant types listed as Plants A through D below. We have a BW plant, a CE plant, and a couple of different types of Westinghouse plants. We've done several nozzles in these heads looking at different nozzle angles ranging from the top head center nozzle down to the steepest nozzles in any of the plants, which the steepest tend to be the worst in terms of predictions of nozzle ejection.

We've looked at different nozzle yield strengths and the effect of material yield strength on the calculations, and we've assumed conservatively that a circumferential crack follows the path of maximum stress in the nozzle, and one conservatism in the analysis is that we've looked at the stress intensity factor over the entire propagation length from 30 degrees to 300 degrees around the nozzle, and we assume that if a crack exists based on our Weibull analysis of time to cracking, time to failure, we conservatively assume that we instantaneously have a 30 degree through wall circumferential crack. So we take no credit for the time that it would take for this meandering crack that you were talking about to either turn or produce enough leakage to generate a circ. crack, and then the circ. crack starts propagating in around the circumference. We assume instantaneously that if we predict a crack or a leak we've instantaneously got a 30 degree circumferential crack.

VICE CHAIRMAN SIEBER: Is there any reason why you skipped Westinghouse three loop plants?

MR. RICCARDELLA: Just to limit the number of analyses we did. There wasn't that much --

VICE CHAIRMAN SIEBER: I noticed in this chart the highly susceptible plants, a lot of them were three loop Westinghouse plants.

MR. RICCARDELLA: Actually most of them

were the B&W plants. Yeah, the older westinghouse plants, because the newer four loop plants tend to be cold head. That's why. It has to do with temperature. There was no reason. There wasn't a significant enough difference between the two loops and the four loops that it really warranted going into analysis of a three loop.

MEMBER WALLIS: So you've got this crack that is cut off, an as soon as it reaches the surface it suddenly grows to 30 degrees?

MR. RICCARDELLA: Yeah. That's the assumption we make. That's not what really happens. We -- no.

MEMBER WALLIS: And what does the back hand of the crack do? I mean, the crack is coming through material and it leaps to 30 on the surface. What does it do inside the material?

MR. RICCARDELLA: Well, no, it leaps to 30 --

MEMBER WALLIS: All the way around?

MR. RICCARDELLA: -- all the way around.

MEMBER WALLIS: So it suddenly goes from a thing like this or a wiggly-squiggly thing to a sudden thing which is open?

MR. RICCARDELLA: It turns and goes -- it branches and goes circumferentially.

MEMBER WALLIS: And it goes that all the way through.

MR. RICCARDELLA: All the way through the thickness.

MEMBER WALLIS: All the way through the thickness.

MR. RICCARDELLA: So it becomes a 30 degree through wall crack.

MEMBER WALLIS: All the way, yeah.

MR. RICCARDELLA: Above the well.

MEMBER WALLIS: It's through over the whole 30s. It's through.

MR. RICCARDELLA: Yes. And then we go into the crack propagation mode. Now, if you look at the four plants we picked, and this might address your question, this is a plot of the geometric characteristics of the J groove weld that are significant to residual stress. One of them is the size of the weld, the average cross-sectional area of the weld. Obviously, the larger the weld in general, the higher the residual stresses you will get.

And we also looked at the ratio of stresses, a cross-sectional area of the weld uphill to downhill. Some of the plants were designed where the uphill weld is much bigger than the downhill side of the weld, and in others the downhill was much bigger than the other.

This is a plot of those geometric characteristics for essentially all of the plants that are out there, and the red data points are the ones for the nozzles that we analyzed, the four characteristic plants and the nozzles that we analyzed.

So we've basically bounded all of the data as far as size of the nozzle, as far as uphill to downhill ratio. The only thing that we didn't bound was the smallest welds, which of course --

MEMBER WALLIS: It looks very strange. That C there is .3 or something. The other one is two. There's a huge range, and this ratio --

MR. RICCARDELLA: Yeah, sometimes the uphill weld is twice the size of the downhill, and sometimes it's vice versa. Yes, sir.

MEMBER WALLIS: Point, three? It's a third of that?

MR. RICCARDELLA: Yeah.

MEMBER WALLIS: Is there some rationale for this extraordinary variation?

MR. RICCARDELLA: No. That was, you know, original design concepts. In general, what you'll find is that all of the B&W designed or built heads ar on one side of this and all of the CE designed and built heads are on the other side of that ratio. It's just the way the fabricators chose to lay out their well prep and make the welds back 20, 25 years ago.

MEMBER WALLIS: It's only the ones that are on the hill. The zero degrees are all uniform presumably.

MR. RICCARDELLA: Yeah, the zero degrees are pretty uniform. Just some of them have bigger welds than others, yeah. And it affects -- you know, we make assumptions in our analysis about whether the crack is a downhill or an uphill side crack, and this would affect which is more critical, a downhill or an uphill side crack.

Okay. A key element of our model is our Weibull model of the plant data, and we started constructing this Weibull model two or three years ago based on just leakage data, but what we've done is we've now considered that we've done a lot of nonvisual exams, and so we've taken into account the plants where you've done a non-visual exam and you've found cracking, but not leakage. So it's not just a time to leakage model; it's a time to leakage or cracking.

And also we've taken out plants that have only performed visual exam. We've only included in this database plants that have done nonvisual NDE, and that's a population of 30 plants, 14 of which had leaks or significant cracking. Fifteen were inspected and found to be clean and they were treated as suspensions in the classic Weibull analysis.

One plant only performed partial NDE, and we included that in the analysis, but it was an early light suspension. So it had no effect on the analysis, and was mentioned, plants that performed only visual examinations and were clean were excluded from the analysis.

This is a summary of all of the inspection results through spring of 2003, and it's the same data as the big plot that Larry handed out, but I like to plot it in this form because it shows this parameter effective degradation years that we use, which is the equivalent number of years as if the plant operated at 600 degrees.

So on this scale here, I plot the actual head operating temperature, and then on this scale I plot the effective tears at the head operating temperature, okay, basically the total years of operation.

So if I plot the blue lines here, these are lines of constant EDY. A plant that has operated for five years at 600 degrees is, of course, five EDYs. If you take the five EDYs at 580 degrees takes about 12 years, and to get to five EDYs at 560 degrees takes 25 or 26 years or so.

So these are the lines of constant EDY and the data points are actual plant data. Every plant, all 69 plants have performed at least a visual examination, and the code for the data points is shown here. Plants with leaks are the read triangles. Plants that have done NDE and discovered cracks but no leaks are the red squares shown here, and then now we have got a number of plants that have performed the NDE and were clean. Those are shown by the green squares, and the balance of the plants, the blue data points are plants that have performed visual and were clean.

In general, if you perform a visual and find the leak, you automatically get into doing NDE. So, you know, some of those plants are included in the data. If they did visual and found a leak, that's included in the Weibull analysis.

So the 30 data points that we've used in the analysis are just the green and the red points, not the blue points.

MEMBER WALLIS: I presume with the blues that are above the 15 EDY is not going to do --

MR. RICCARDELLA: They already have. Yeah, they're plotted twice sometimes. Some of these data points are plotted. If they did a visual and then they did an NDE, it's shown twice.

So, for example, -- I don't have an example.

MEMBER WALLIS: Then why aren't the data points on top of each other?

MR. RICCARDELLA: Well, they might have done the following items.

MEMBER WALLIS: Different times.

MR. RICCARDELLA: They did a visual and then the following outage they did a -- like this might be one that did a visual, and then the following outage did an NDE.

MEMBER WALLIS: But the others couple haven't done that, I mean, because they're so far apart.

MR. RICCARDELLA: Well, some of them, for example, this one here, I happen to know the plant. They just replaced their head. They never did an NDE.

They replaced the head. There have been a number of plants that have been replaced.

MEMBER WALLIS: That would explain it, yes. Thank you.

MR. MATTHEWS: And this data in spring '03, my chart is through spring '04. So there's been another year's worth of volumetric and visual inspections sine this data used to set the model up.

MR. RICCARDELLA: Yeah, you have to stop sometimes and write a report and get it done, and so I drew the line at spring of '03, and that's the data that our model is based on.

I have looked at updating the model for the more recent results, and nothing would have changed the results.

This is a plot of the Weibull analysis. This is plotted, a cumulative probability of a leak, of at least one leak in the plant versus EDYs. The actual plant inspection data is shown by the blue data points in this chart. If you just fit a curve to that line, you would get a much steeper Weibull slope. You would get a slope of about four, four and a half.

The general consensus of the experts on this top is that for a phenomena like PWSEE, the maximum slope that you would expect to see is about three, and one of the things that occurred here is we referred to it in the past as an inspection transient. That is, in the early days we weren't doing a lot of inspections, and then we started doing more and more inspections, and by the time we did some of these inspections, for example, Millstone did an inspection and they found three cracks, not one, three leaking nozzles.

North Anna II, when we did that inspection, they found it was at 20 EDYs, but they found some 16 nozzles with significant cracks or leaks.

So what we did is we extrapolated the data back based on what was found, the number of cracked nozzles found at the time of inspection, to when we would predict they had their first cracking, had they been inspecting routinely all along the way.

MEMBER WALLIS: Well, the reason that these are all going off to the right monotonically is because you are plotting on the basis of EDY.

MR. RICCARDELLA: Yeah, basically time.

MEMBER WALLIS: So you're forcing the curve to be monotonic.

MR. RICCARDELLA: Oh, yeah. that's the standard Weibull approach of number of failures versus time.

MEMBER WALLIS: It makes it look better.

MR. RICCARDELLA: If I plotted this on linear paper, it would look like an S shaped curve. It's plotted on Weibull paper. That's why.

MEMBER WALLIS: This way you're forcing it to look nice.

MR. RICCARDELLA: Yeah. Well, no, I'm just forcing it to be a straight line so that I can do a curve fit. I've got a plot later on in the presentation that I'll show where it's plotted on linear paper. Okay?

But the point I'm trying to make here is that we didn't just fit the data. We have extrapolated the data back to when we would predict the time of first cracking, and then we fit a curve to that.

And, in fact, that Weibull curve, the best fit Weibull curve that we put through there has a mean or characteristic time to failure of about 15 EDY's. The time at which you'd predict 63 percent of the plants will have had at least one leak, and it extrapolates back, and even with our model, even -- we predict even in relatively early in time in EDYs, you have some finite chance; at about four EDYs, you still have about a two percent change that a plant would have a leak by our model.

Now, the actual data breaks off of that curve a little bit in the low probabilities or in the low EDYs, but the model that we're using in our PFM analysis is the straight line.

And then the other thing that we do is we put an uncertainty band around that. And so we take the best fit curve and then we look at probability or standard deviations above and below that best fit curve.

I show this because this mean theta and the standard deviation, the variability in theta is what we use, is one of the adjustments we make in our calibration of the model.

MEMBER KRESS: How did you arrive at the values for --

MR. RICCARDELLA: For the two dashed lines?

MEMBER KRESS: Are they based on the red data?

MR. RICCARDELLA: Yeah.

MEMBER KRESS: Okay.

MR. RICCARDELLA: They're just standard statistical analysis of variability around the best fit line, yeah.

So that's our crack initiation model really. It's an empirical model based on inspection data. I think I missed some stuff on the --

MEMBER WALLIS: I'm trying to figure this out. I mean, you're plotting accumulative based on -- you're already sorting by EDY. So this doesn't sort of prove that EDY is the dominant variable because you've already sorted by EDY. Then you're plotting cumulative based on EDY. So you're forcing the data to slide up and show this trend.

MR. RICCARDELLA: It's just a Weibull analysis of data, and I don't -- you know, it's --

MEMBER SHACK: It could have plotted randomly. If EDY had no effect on this, you know, you'd expect to see a shotgun.

MEMBER WALLIS: No, you wouldn't because it's already sorted by EDY. It's forcing it to go up to the right monotonically.

MR. RICCARDELLA: No, but all I'm plotting is the failure points.

MEMBER WALLIS: But this showed, the first curve, the one you just showed shows there's a randomness. There's the variation around there.

MR. RICCARDELLA: Yeah, but it also shows that all of the leaks were at 15 EDY.

MEMBER WALLIS: But when you do it by this cumulative, you're already sorting by EDY, and then they have to have an upwards trend to the right trend no matter what.

MR. RICCARDELLA: Yes, they do.

MR. MATTHEWS: If it wasn't correlated with EDY it wouldn't.

MEMBER WALLIS: Yes, it would.

MR. RICCARDELLA: I mean, that curve has about a 95 percent correlation coefficient, as I recall. I mean, I think we --

MEMBER WALLIS: I guess we can discuss this. I can discuss it with Bill Shack in private. it seems to me you're forcing the trend to be up and to the right because of the way you're plotting cumulative.

MR. MATTHEWS: Well, if it's a cumulative probability, that's --

MEMBER WALLIS: It has to do it. It has to do that. That's right. It has to do that.

MR. MATTHEWS: It's going to be an S shaped curve or something similar to that.

MEMBER WALLIS: And if you had a random variation with EDY, you'd still have that trend.

Yes, thank you.

MEMBER ROSEN: How did you extrapolate back? It said in this chart you knew the time of EDY at the detection of first leakage or cracking and you extrapolated that back to, well, when they had the first crack.

MR. RICCARDELLA: Based on the number of cracked or leaking nozzles that they discovered at the time. In other words, a plant that goes in and does an inspection and finds 11 leaking nozzles is worse than a plant that goes in and finds just one leaking nozzle.

MEMBER ROSEN: Right. So let's take the one at 11. How did you figure out what EDY is at its first?

MEMBER ROSEN: With a Weibull slope of three, using a Weibull distribution for the time to nozzle failure in that particular head with a slope of three, you can then extrapolate back in time to the number of EDYs you predicated had its first leak. Okay?

And then you reshuffle all of the data because, see, if you just look at the raw data, no stone was the worst. It found three leaks at just around 11 EDYs. Okay? Or three cracks.

MR. RICCARDELLA: Three cracks.

MEMBER ROSEN: I'm sorry. Three cracks at around 11 EDY. So it was the worst in the original data, but now if I take North Anna that had, I think, 16 leaks at 20 EDY, it actually extrapolates back to be worse than Millstone. It extrapolates back to essentially the same. They both extrapolate back to the first leak at about seven EDYs. Okay?

And interesting one is Davis-Besse. It had about I think was it three or four cracked nozzles? I forget the number Three cracked nozzles at around 19 EDYs. That extrapolates back to its first crack at around ten EDYs, and that's somewhat consistent with, you know, the expectation that all of that boric acid that built up over a long period of time, it built up because that plant was leaking for some period of time.

MR. MATTHEWS: I think you'd better get some results or we're going to run out of time.

MR. RICCARDELLA: Okay.

CHAIRMAN FORD: Yeah, I'm just trying to work out here the timing here. I'd like to finish around about 12, quarter past 12, and not go much beyond that because we've still got Alex Marion and yourself. So I hate to do this to you, Peter.

MR. RICCARDELLA: I'm going to cut to the chase.

CHAIRMAN FORD: Could you try and abbreviate?

MR. RICCARDELLA: Yeah, I will. I will.

Okay. Material crack growth rate statistics, I won't go into it at all other than to say that, you know, it was based on a qualified -- that was the word I was looking for -- qualified data set of 26 heats of material, 158 total data points. Statistical distributions were developed for heat-to-heat variation, as well as for variability of the crack growth rate within a heat, and this is sampled for BFM analysis.

And when we do that, we assume it to be correlated with the Weibull statistics for the time to leakage. That is, if we have a heater material that has a very high crack growth rate, then the expectation is that it probably was a bad actor from the standpoint of crack initiation.

And what we've done is we've correlated our selection of random variables, and we can put in a correlation factor into the model. This is an input to the code. If we input R equal to zero, it assumes that they're totally uncorrelated. We're picking two completely separate random numbers.

If we put in R equal to minus .8, we get some correlation, .9, minus .9, a strong correlation. One is that we assume they're totally correlated, and that's the second parameter that you'll see that we use in our benchmarking. We don't really know what the degree of correlation is, and that's a second parameter that we use.

The parameter is negative because a short time to leakage corresponds to a high crack growth rate. That's why we use a negative correlation rather than a positive correlation factor.

CHAIRMAN FORD: I'm not going to help my case for pushing the time, but it's an important one. You are aware. You're talking about crack growth rate data. You are aware of some relatively recent data showing that you can get a factor of 30 increase in the crack propagation rates --

MR. RICCARDELLA: Yeah.

CHAIRMAN FORD: -- in the zone right next to the --

MR. RICCARDELLA: I have a slide on that that I'll show later. I don't think that's significant to my analysis for a number of reasons.

CHAIRMAN FORD: Okay.

MR. RICCARDELLA: Okay. One of the reasons is that if you look at that particular set of data, the base metal data point was a factor of 20 below the MRP curve. So that the factor of 30 increase only took it to about one and a half above the MRP curve.

CHAIRMAN FORD: Well, that may be true, but the physical fact is you have extreme concentration adjacent to the affected zone, but if you do the crack propagation rate rather than the bulk material --

MR. RICCARDELLA: Understand, in my propagation model I'm assuming a through wall crack. This 30 degree crack I assumed is through the entire nozzle wall. So only a small portion of that crack is in the heat affected zone. The majority of that crack front is in the tube away from the weld.

CHAIRMAN FORD: My sole purpose for bringing it up is you know about it.

MR. RICCARDELLA: I am aware, yeah. I'm aware of that.

Okay. This goes to a question that people ask: how we simulate the effect of inspections. If we do a bare metal visual exam, we assume that you have a POD, probability of detection, of only .6.

Okay? A fairly conservative assumption, and that's on the first exam. On subsequent exams, we assume that if you've done inspection of a nozzle that's leaking and you missed it and you come back and do a repeat exam, that you have a smaller -- we put a reduction factor on that POD because there's something that may be very difficult to detect in that nozzle.

CHAIRMAN FORD: Are there any data of team performances to substantiate those POD values? Are they just engineering --

MR. RICCARDELLA: On the visual it was just a judgment that I think most people feel was pretty conservative.

MR. MATTHEWS: I think that just having watched hours and hours of videotape, I think .6 is very conservative.

MEMBER ROSEN: I've watched a lot of those videotapes, too, Larry. I think, you know, if you're using a crawler or something like that, a modern crawler, and you have a leak, there's almost no chance you're not going to see it if it has been leaking for a while.

So you know, I would argue that the .6 is conservative as well, but I would argue that you're going the wrong way with the second bullet, which is you say on the second inspection you only have a 12 percent chance of catching it if you missed it. You had a 60 percent chance of catching it the first time. The second time you look at that same nozzle, you only have a 12 percent chance. There's something wrong with that.

It seems to me that you have at least a 60 percent chance and maybe more because it has leaked more and it has got more boric acid on it.

MR. RICCARDELLA: But the assumption is that it's very difficult to see, that it's in a hidden area or --

MEMBER ROSEN: Yeah, but it's not any more difficult the second time than it was the first time. Technology has moved ahead and you've had more leakage and there's probably more boric acid on the head.

MR. RICCARDELLA: You're probably right. I understand that we had some -- part of this is to address the North Anna situation where we had some nozzles with circ. cracks that had no evidence of leakage whatsoever. So we wanted to get in some finite probability that you do a leakage exam and you have some serious cracking and you just don't discover it.

MEMBER ROSEN: Yeah, for those cracks that haven't come through the surface, that's right. For bare metal visual you're not going to see it either way, but for ones that have come through the surface, but there was only a little bit of boric acid, you know, like the South Texas case where they got like an aspirin size piece of boric acid, that's a pretty good catch.

I mean, aspirin isn't very big, especially when you're looking for it remotely, but the second time you go through it, if it has leaked an aspirin, it might have leaked a whole jar of aspirin the second time.

MR. MATTHEWS: I think the presumption why we put the .2 in there was for those cases that perhaps you just can't find it, can't see it because it's jammed up against the insulation or something like that.

MEMBER ROSEN: I understand your argument. I hope you understand mine.

MR. MATTHEWS: But I think it's very conservative.

MR. RICCARDELLA: Or also, you know, the possibility that you wrote it off because you thought it was coming from up above.

MEMBER SHACK: Or there's just no leak

path.

VICE CHAIRMAN SIEBER: Well, that's Beaver Valley's case. Three cracks, no leaks.

MEMBER WALLIS: That's just like Davis-Besse didn't detect a leak because, I mean, .6 is not a very high probability of detecting a leak.

MEMBER ROSEN: They didn't look what it was leaking.

MR. RICCARDELLA: No, they looked.

VICE CHAIRMAN SIEBER: I mean, look at the air locked door.

MR. RICCARDELLA: The NDE, we've picked a POD curve which is a function of crack depth from a prior EPRI report, but the curve that we use isn't directly relevant necessarily to these nozzles. Just a generic POD curve for NDE, but on this plot here, what I show is a comparison of that POD curve to vendor demonstrations and by both of the vendors that are doing the NDE on the plant right now. These are demonstrations at the EPI NDE center on blind mock-ups with cracks in them of different sizes, and what I show is that this is the range of crack sizes that were not detected in the vendor demonstrations.

This is the range of crack sizes that were detected. Our POD curve says that, for example, if the crack is a tenth of an inch deep in the tube that you have about a 50 percent chance, 50 or 60 percent chance of detection. Actually at that time, they detected some and they didn't detect others. Okay?

As we get up to large crack sizes everything greater than about .15 or .16 inches was detected, and at that crack size we're predicting about a 75 percent probability of detection, and then as we go up in size above that, we're predicting the POD, probability of detection to get larger, but we do saturate out at about 95 percent.

So our POD curve says that no matter how big the crack you've got at least a five percent chance that you're going to miss it, that the NDE is going to miss it.

So it's not, you know, a totally rigorous analysis of POD, but it's just a comparison of the POD curve that we're using to actual vendor demonstrations in the inspection.

Okay. So in our modeling then, for every nozzle we predict a time to cracking. At that point in time we predict a 30 degree crack. We grow it in accordance with the crack growth curves, the statistical variability of the crack growth curves, but then at any point in time, either in the initiation or the crack growth process, we can impose one of these inspections, and the inspection will look at that particular nozzle, and based on this POD determine whether the crack is detected or not.

And so that's the answer to the question that somebody asked earlier, is how do we reduce the probability of leakage through inspection. If we do an NDE, we discover a crack when it's only a tenth of an inch or two-tenths of an inch deep. Then we assume that that nozzle is repaired and that it no longer represents a possibility of leakage or cracking.

Okay. Now, when we get into the results, we've run a series of tests. We've run a series of base cases. We've run sensitivity studies to look at the effect of the various parameters on the analysis. So I'm not going to go into those at all.

I am going to go into the benchmarking analysis and the case studies that we've done with the benchmarked parameters.

This is the curve perhaps that you were asking for earlier. This is that same Weibull plot, but plotted on linear paper rather than probability or rather than Weibull paper and shows the effect of -- shows the same 14 data points that I described earlier and shows how our predictive model goes through those.

That's a pretty trivial case because all we did was we took a Weibull model. We set up a Monte Carlo routine to simulate that Weibull model, and then show that, in fact, we can do that reasonably, and it predicts the results.

But I show this because then as I got into further calibration, I wanted to make sure that I didn't screw up that Weibull model, and so the dashed curve is our actual calibrated curve.

The real essence of the benchmarking was to see how well we do at predicting circumferential crack data, and of the vessels out there that did volumetric exams, we found a total of 11 circumferentially cracked nozzles ranging in size from about 30 degrees, which is our initial assumption, up to the 265 degree circ. cracks that were detected at Oconee, and so this is the list of those plants --

MEMBER WALLIS: So Davis-Besse was the mildest crack of all?

MR. RICCARDELLA: Circ. crack, yeah. That's not the nozzle that led to the wastage.

MEMBER WALLIS: No, it can't be.

MR. RICCARDELLA: It's a different nozzle.

MEMBER WALLIS: It another nozzle. Okay.

MR. RICCARDELLA: It was a different

nozzle that had a long axial crack that led to the wastage. We're not looking at wastage here. We're looking at the possibility of ejecting a nozzle.

MEMBER SHACK: Pete, just a question.

MR. RICCARDELLA: Yes.

MEMBER SHACK: Did you ever compute the number of leaks? Now, you've computed a benchmark of time to first leak, but the actual number of leaking nozzles that you would have expected to find?

MR. RICCARDELLA: Yeah, that --

VICE CHAIRMAN SIEBER: At a given time.

MR. RICCARDELLA: In a given plant or in all plants?

MEMBER SHACK: Say in all plants, you know, for the EDYs that you've got. You know, how many leaking nozzles would you have expected to find?

You're going to compute how many circumferential cracks you've found, but did you actually compute the number of leaking nozzles you would have expected to find to find out if that matches?

MR. RICCARDELLA: You know, I computed it because the program computes both leaks and failures on a per nozzle basis, as well as on a per plant basis, per head basis. But I just haven't gone back and extrapolated that and seen how that compared. That's a --

MEMBER SHACK: But every leak becomes a circumferential crack.

MR. RICCARDELLA: Is assumed to be a circumferential crack.

MEMBER SHACK: Whereas in experience something like .2 of the leaks are circ. cracks.

MR. RICCARDELLA: That's right. That's right.

MEMBER SHACK: And, in fact, I think you'll see that in my calibration. I'll show you what. You can see that effect in my calibration plot.

So this is the circ. crack data that we're using to see how well the model does at predicting crack growth. So what I've done with that data is to break it, to put it into bins of 30 degree increments. So there were four nozzles that had cracks in the 30 to 60 degree range. There was one in the 60 to 90, down to the two that were in the 150 to 180.

Put that into a cumulative distribution of number of nozzles with cracks greater than 30 degrees is 11, and so on. So there's a cumulative distribution of number of circ. cracks of various sizes, and a convenient thing about this is virtually all of these cracks occurred at about 20 EDYs. They were like some were 18, some were 21, but the average was 20, and they were all very close to 20.

So that allowed me to take the time element out of the calibration, and so what I did in this benchmarking is to just look at what the program predicts at 20 EDYs versus what we saw at 20 EDYs. Okay?

And it turns out there were a total of 881 nozzles that were inspected at about 20 EDYs. Okay? So I can compute a frequency of circ. cracks of these various sizes, and these are the frequency calculations, just dividing the number of cracks by 881.

Interestingly, a trick I learned from the PRA guys that I was working with on another project was that we've had no ejections. Okay? And so you can estimate the probability of a nozzle ejection somewhere between zero and one. Okay? And you assume a uniform -- it's called an uninformed prior, I guess.

VICE CHAIRMAN SIEBER: It's pretty tricky.

MEMBER SHACK: A strange, noninformative prior.

MR. RICCARDELLA: A noninformative prior. So I used that as .5, and that would yield to a predictive probability of collapse of these 881 nozzles to about 5.6 times ten to the minus fourth. Okay?

Of course, this all assumes no inspections. This is, you know, without inspections.

So then I went through and ran my model. MRPERCRD is the software, the PFM software that we're using, and if I just picked the base case parameters that I started out with in the beginning, it turns out I under predict somewhat, particularly for the large cracks. I'm predicting a probability of a crack in the 150 to 180 degree range of about nine times ten to the minus fourth, when the actual observed frequency was 2.7 times ten to the minus third, almost more than a factor of two.

So then I said, "Well, what can I do?" Well, I have my correlation factor, and I list here what I changed as I went through these four cases, but I just increased -- I made modifications to the input parameters, the two input parameters being the Weibull theta, both the mean and the range of the Weibull theta, as well as the correlation factor between initiation and growth.

And you see the first thing I did was to push that correlation factor up to minus one. Assume that they are totally correlated, and that increased it somewhat. That got me to 1.36, but it didn't quite get me where I wanted to be.

Then I used a more conservative theta. I put the mean time to failure down a couple of years and increased the range on it, and that got me right -- that's this yellowed in box here, which is right about where I want to be predicting 2.25 E to the minus three, for a probability of 165 or a large circ. crack versus the 2.27 actual.

And, again, I'm also -- but note that when I do that, and I think this is the comment that somebody had earlier -- I'm way over predicting the number of 30 degree cracks. Okay? And I actually show that graphically on the next chart.

So here is my base case parameters. Here's the actual data plotted on the probability of a crack exceeding a certain size versus that size, and then here's my benchmark case, and of course, by benchmarking, what I'm really most concerned about is the large 165 degree cracks. So that's what I calibrated against.

But once I get that calibrated, you see I'm over predicting the number of 30 degree cracks by a large margin, and I think that results directly from my assumption that any time I get a significant crack or a leak, I have a 30 degree crack. I end up over predicting on the small end.

So anyway, I've settled in on a set of what I call benchmark parameters, and that's what I use to go forward and analyze the effect of --

MEMBER SHACK: But the other way to look at that, Pete, is that you have to way over predict the number of cracks that you have in order to get one that leaks two out there. So it says you're under predicting the rate at which the cracks are growing.

MR. RICCARDELLA: I guess. It says I'm not perfectly modeling this curve. I'm over predicting in some areas, under predicting in others, but I guess if I had a -- the other thing is remember we only have 11 data points that we calibrate against.

MEMBER SHACK: When you're comparing a population with tone sample, right. Things get a little tricky.

MR. RICCARDELLA: Okay. So using the benchmark parameters I performed a series of analyses of actual plants. The plants naturally subdivide themselves into four groups. There's plants that have replaced or are replacing. I basically say upcoming RFO, but 14 plants have already replaced.

Fifteen plants that have announced plans to replace, but they still have one RFP between now and when they replace. So there's still a decision that has to be made as to whether they need to inspect or not.

We've got 17 plants in the NRC moderate category with no replacement plants announced, and then we have 23 Westinghouse cold head plants. So those are four groups.

I picked four actual plants, case study plants, from Groups 2, 3, and 4. Plants 1, obviously, if they replaced they're no longer of interest at least until we have a 690 version of the model.

And then we analyzed each case from the three inspection scenarios. Inspection from the NRC order and then two example MRP inspection plans, and I summarize those here, but I have a more detailed slide on those inspection plans that I'll get into later.

The case study plants shown here, there are two CE plants, two Westinghouse plants. There were different head temperatures ranging from 595, 592, 580, 567, and they're in different categories. Most of them are in the moderate or in the case of the cold head plant, it transitions from low to moderate at a certain point.

And then I looked at these inspection scenarios. As I said, the first case we ran was inspections exactly in accordance with the NRC order. Then I looked at two example inspection schemes, a baseline NDE and periodic BMV. Both of the two example schemes assume that you do the baseline inspection and the periodic bare metal visual exams exactly in accordance with the order.

But the subsequent NDE schedule is different. We looked at basing the NDE schedule on delta EDYs, the number of EDYs you accumulate before the next inspection.

And we looked at if you do the inspection scope, as with the NRC order, where you do 100 percent of the nozzles, but you're not required to do any weld exams, we said that if you do that inspection which set the frequency at two EDYs, but if you're willing to inspect at least 50 percent of the J groove weld surface, then we try to set up an incentive to get people to do weld inspections. Because if your goal is really to avoid leakage, to minimize the probability of leakage, you really have to do some weld exams because you could do inspections exactly in accordance with the NRC order and not inspect any of the J groove welds. You could start up and you could have a leak fairly quickly if you had a crack in one of the welds.

So what we're trying to build into this order, into these examples here is sort of an incentive to do some weld inspections in the form of a reduced inspection frequency.

Okay. So here's a layout of the inspection schedule. In the report I only picked one of the case studies, case study No. 2, but in the report, in MRP 105, there's details on all four of these and detailed results on all four of these. But this is the way the inspections line up.

The bold is where you're doing inspections so pretty. NRC order for this particular plant, they were required to do an inspection in the fall of 2002. So all of these assume that you did that inspection in the fall of 2002, and then since this is in the moderate category, you would do another inspection in the fall of 2005 in accordance with the order and at that point it transitions to high, and so after that you'd be required to do an inspection every outage.

In accordance with the MRP plan, the two EDY interval gives you ever other outage and continues every other outage for those inspections. So that's the difference between the inspections per the order.

And then under Plan C, the three EDY would actually give you every third outage for this plan, but that assumes that you did 50 percent weld inspections back when you did this first exam. so it's really a hypothetical case. You couldn't -- you know, that plant already did the inspections. But if they had done 50 percent weld inspections, then in accordance with MRP Plan C, they wouldn't have to do another inspection, another volumetric until spring 2007.

They would still be doing bare metal visual every outage, as indicated by the bottom line. So let's look at the results of that.

This is the probability of leakage for that case. It shows the probability of leakage was built up. At the time of the first baseline inspection, the probability of leakage was about eight percent. Had they not done any inspection, and I think this was kind of a -- that curve keeps going up, and the probability of getting a leak keeps going up, but --

MEMBER WALLIS: But inspection without some action doesn't change the probability of leakage.

MR. RICCARDELLA: The assumption is that

when you inspect and you find something, you find a crack, you repair it, which is basically what everyone does.

MEMBER WALLIS: It's required.

MR. RICCARDELLA: Yes.

MEMBER WALLIS: There's a probability of leak on one?

MR. RICCARDELLA: There's a probability of one leak in a plant.

MEMBER WALLIS: Oh, in a whole plant.

MR. RICCARDELLA: In a plant of 69 nozzles or however many nozzles are in the plant, 90 nozzles, I think in this plant, in a year. It's the Weibull hazard rate, if you're familiar with Weibull analysis.

If we did an inspection though, now in the fall of 2002, we've eliminated a lot of those potential leakers because if they're going to leak here, they had some cracking earlier, and if you do the inspection, you've got a probability of detecting those cracks. That knocks the probability of leakage down.

In the case where we didn't do weld inspection, the two cases, the NRC case and the Plan B, it comes down to about a four percent probability of leakage, and then up a little bit, and then we do the second inspection in fall of '05 here and knocks it down even further.

The magenta curve is the Plan C curve where because we did weld inspections we knocked the probability of leakage down even further, and then it builds up, but you know, we do the inspection a little bit later. So we get to essentially the same point by doing the weld exams.

I'm losing my battery in my pointer here.

In both cases we keep the probability of leakage in around the four and a half percent range. We've been shooting for a target of about five percent. That's sort of the target that was set up for probability of leakage.

Thank you.

Anyway, and then a similar result occurs in the probability of failure. In this case, again, we had gotten up to a little over one -- you know, again, the goal for probability of MET (phonetic) section collapse, if you assume a conditional core damage probability of about ten to the minus third, and then you're shooting for about ten the minus third.

For this particular plant, we had actually exceeded that before we did the first inspection, but once we did the first inspection, we knocked that down, and it tends to stay down.

MEMBER WALLIS: Why doesn't it grow much more rapidly after 20 EFBYs? I mean, aren't they much more susceptible after 20 EFBYs?

MR. RICCARDELLA: But we're doing periodic inspections.

MEMBER WALLIS: Yeah, but that doesn't change the susceptibility. I would think that the growth rate would --

PARTICIPANT: Would grow back the same.

MEMBER WALLIS: -- at the end because those are more susceptible to the cracking.

MR. RICCARDELLA: Yeah, but what happens in these assumptions, we didn't build in that. You know, if you've got an 80 percent probability of finding the crack the first time, the first time you do an inspection, you come back and do a second inspection and you've got another 80 percent probability. The probability of that crack actually escaping, it's down to around four percent.

MEMBER WALLIS: So there's no sneak crack which then grows very rapidly after 20 --

MR. RICCARDELLA: No, but I put most of the stock in this second inspection after the first.

All of my conclusions are based on the second peak, and I'm not really taking any credit for these inspections that occur out further in life.

You know, the inspection knocks it down by about a factor of five, and then you start to grow back up.

MEMBER WALLIS: So the crack growth rate doesn't increase with time. No. So the EFPY doesn't increase the crack growth rate nor the FPY.

MR. RICCARDELLA: Well, if the crack gets bigger, the K goes up. Yeah, it does increase with time, but remember we're eliminating -- this isn't the crack growth rate. This is the probability of a crack growing.

MEMBER WALLIS: I know that. I know. But as the plant gets older, the suspectibility to cracked growth isn't any worse.

MR. RICCARDELLA: No.

MEMBER SHACK: It'll initiate cracks with a greater likelihood, but he keeps detecting them anyway.

MEMBER WALLIS: He keeps detecting.

MR. RICCARDELLA: This is a summary of the results for all of the case studies both in probability of rejection. NSC is net section collapse, which is nozzle ejection basically.

Leak. A probability of leak, you can see that essentially in all of these cases we've come up with sort of constant probability cases. The probability of net section collapse we're maintaining down in the ten to the minus four. You know, something well under, at least a factor of two under ten to the minus third; a probability of some leakage in general we're keeping down in the under five percent regime.

MEMBER SHACK: Are these based on your base case analyses or you yellow highlights analyses?

MR. RICCARDELLA: Yellow highlighted. All yellow highlighted.

MEMBER WALLIS: Even thought your probability of detection is never greater than 95 percent?

That's right.

MEMBER WALLIS: So the sneakers, there aren't any sneakers that get through a couple of inspections without being detected? Yeah, that's what's left over.

MR. RICCARDELLA: There are going to be some.

MEMBER SHACK: Down there there will be

some.

MEMBER WALLIS: So it all depends on being able to inspect without 95 percent effectiveness.

MR. RICCARDELLA: For a big crack, yeah, 95 percent for a big crack, but you know, some of the inspections we're doing, I mean, sometimes when you do the inspection the cracks are only a tenth of an inch, and you're only finding them with like a 30 or 40 percent probability.

MEMBER WALLIS: I'm surprised. Ninety-five percent probability and you've got how many of these controller drives? Sixty or something?

MR. RICCARDELLA: Yeah.

MEMBER WALLIS: And you've got 95 percent. There's a probability of one of them sneaking through, it would seem to me, to be pretty large.

MR. RICCARDELLA: Oh, yeah, but remember the probability of it being cracked isn't 100 percent

MEMBER WALLIS: Okay. I guess that's it.

MR. RICCARDELLA: Right? You know, even if you go back--

MEMBER SHACK: To now inspections.

MR. RICCARDELLA: I'm going the wrong direction. I mean, even if I did no inspections for the next few years, my probability is going to be, you know, you just extrapolate this curve up. You've just got a couple of years before that goes up.

MR. MATTHEWS: We'd better hurry.

MR. RICCARDELLA: So I can just go through the summary and conclusions again, but I don't need to read these again in the interest of time unless anyone has any questions.

MEMBER WALLIS: This looks awfully optimistic to me.

MR. RICCARDELLA: Well --

PARTICIPANT: Conservative to me.

MR. RICCARDELLA: -- there's an awful lot. You know, in many cases where we had uncertainty we took the conservative assumption, and I think the point is that, you know, we're comparing inspections here. So if there's optimisms, or you know, effects of assumptions have the same -- will have the same effect on the inspection of the current order as they will with what we will be proposing under the MRP inspection scenarios.

MEMBER ROSEN: So when do we hear about that, what you're going to be proposing?

MR. MATTHEWS: Next meeting.

MEMBER ROSEN: Next meeting?

MR. MATTHEWS: Yeah. We don't have it

proposed yet. We're just going through our review process right now. I think we're planning on submitting that this summer. I think that's what we said.

CHAIRMAN FORD: Well, what I'm going to be suggesting, assuming that it all works out right, is that by the fall it seems to me that you may well have another one of these meetings where we can go over some of the data associated with 110 and also this MRP, whatever this one is.

MR. RICCARDELLA: One, oh, five.

MR. MATTHEWS: One, oh, five and 117 would be out by then.

CHAIRMAN FORD: And we would have more time to look at the data before we go into the meeting, and we can discuss it on a much more factual basis, data basis.

Does that seem reasonable to you?

MR. MATTHEWS: Yeah, some time this fall we can come back and perhaps you would have had some time to review some of the reports that we've submitted.

CHAIRMAN FORD: Yeah. It's a lot to take in just by looking to --

MR. MATTHEWS: Oh, yeah, it is.

CHAIRMAN FORD: -- words.

MR. MATTHEWS: Yeah, it's a lot of work has gone on.

CHAIRMAN FORD: Oh, sure, I see that.

Peter, thank you very much, indeed. I appreciate it.

Larry?

MR. MATTHEWS: I'm going to try and cover my two presentations in 15 minutes.

CHAIRMAN FORD: Okay. Then let's go.

MR. MATTHEWS: We will go fast.

CHAIRMAN FORD: Yeah, okay. Jolly good, and then we can --

MR. MATTHEWS: Yeah, it requires a little cooperation.

(Laughter.)

CHAIRMAN FORD: Don't ask questions? Golly.

MR. MATTHEWS: Oh, no, no, no, I didn't say that.

I'm going to cover --

MEMBER ROSEN: You can be certain that there will be a lack of cooperation.

CHAIRMAN FORD: Thirty slides?

MR. MATTHEWS: I'm going to go fast. In

fact, in one of the presentation is repeating a lot of the conclusions already.

This presentation is on the Alloy 690 and the weld metals and what we know about it now. What we did was try and go out and evaluate all of the existing field and lab test data on those materials, demonstrate and quantify the margin of improvement of those materials over the Alloy 600, 82 and 182 based on the information that is available today. We provide a technical basis for the development of future inspection requirements for the replacement heads, and to identify gaps in that knowledge base where we might need to -- and strategies to fill those gaps.

MEMBER WALLIS: Is there another program going on due to stress corrosion cracking resistance? Is there another program going on to do weldability of these alloys or the relevant alloys, 52 and 152?

MR. RICCARDELLA: Weldability of them?

MEMBER WALLIS: Yeah.

MR. MATTHEWS: They're being welded day and night, right and left. People have learned an awful lot about welding with these metals, and --

MEMBER WALLIS: I forget which one it was but we've had already one example of --

MR. MATTHEWS: Yeah, it was the first one. They used the Oconee I, and it's not clear to me they --

MEMBER WALLIS: But then, again, if you talk to the welding engineers, they all kind of throw up their hands and say, "Golly, a terrible thing to weld," and we've had one incident already for a repair weld which didn't work out, and so the experience base is not that great. Good.

MR. MATTHEWS: There's a good number of heads. There's ten heads operating already, too. Not all of them have an inspection under their belts, but --

MEMBER WALLIS: Yeah, but it is a fact, isn't it, most welding engineers will say these are not the easiest alloys to weld.

MR. MATTHEWS: Well, I don't think any of the nickel based alloys are a piece of cake to weld with.

MEMBER WALLIS: As you increase the chrome content it gets harder.

MR. MATTHEWS: That makes it worse. I do believe so. I'm not a welding engineer. So I can't really speak to that. I do know that many of the new heads that are being manufactured are being very carefully scrutinized as to the welds and the condition of the welds before those heads are put into service.

MEMBER WALLIS: Now, I'm told that at some of the defense laboratories they do have welding techniques for which they are very good. Are we taking advantage of that information?

MEMBER SHACK: If we could.

MR. MATTHEWS: If I could, I guess. I'm not sure I know about --

MEMBER WALLIS: Well, my point is that there are techniques out there which are being used which increase the -- well, not increase the weldability -- improve on the integrity of those welds. I'm just asking the question.

Are we going to get used to that information?

MR. MATTHEWS: I would certainly hope that the manufacturers are making use of everything that's available to them to improve their capability to manufacture these. Whether they know something about specific defense department information, I do not know at this point.

MEMBER SHACK: Do any of the vendors -- you know, are they only doing essentially the code required, you know, dye penetrant checks? Has anybody tried X-raying these things? I mean, it's kind of difficult, but --

MR. MATTHEWS: Yeah, I don't think they're doing X-rays, but if you look at the code, there's no pre-service required on these things other than the PT, as you will.

MEMBER SHACK: The PT that you will allow, right.

MR. MATTHEWS: All of the plants, the MRP put out a recommendation for a much more thorough pre-service inspections, and I believe all of the plants that have the opportunity, some of them have been kind of crunched as far as their schedule., but they're going to into very thorough pre-service. Many of the plants are demanding a PT white surface, no coat acceptable indications. No indications is what they're demanding on the welds and getting it.

You know, it's not easy. Sometimes you've got to chase some stuff, but they're going after much better initial conditions. And I think almost everybody is doing that.

MEMBER SHACK: But are they doing baseline inspections so that when they do their net UT they really know that, you know, this squiggle was there from day one?

MR. MATTHEWS: Yes, they're doing that.

MEMBER SHACK: Did they do a baseline eddy curve on the J groove weld then, too?

MR. MATTHEWS: Many of them are, in addition to the PT White, they're doing baseline eddy currents.

MEMBER SHACK: Because that would certainly be pretty effective, I think.

MR. MATTHEWS: Conclusion. The existing lab test data provide an average improvement factor the way we've calculated in our MRP 111 of 26 relative to the Alloy 600 milannealed, and 13 relative to Alloy 600 thermally treated material, and we feel these are conservative numbers due to the absence of PWSCC and most of the Alloy 690 specimens within the test derivation.

Field service has been excellent, and based on this it has been concluded that it's very unlikely that you would be developing PWSCC in these materials during the plant lifetime.

We covered a number of lab test conditions in the data that was analyzed for this report, audit lab test methods, W advanced, reversed events. Most of the data admittedly is coming from steam generator tubing. I mean, that's where most of the tests have been performed to date, but not all of it.

There were a number of materials that were tested as well as Alloy 600 controls, about 40 heats of 690, a wide range in carbon content. Not all of them were steam generator tubes. There were some weld metals. There were some plat material, et cetera in the test sets that were analyzed.

There were some shall intergranular cracking and some of the 690 material was observe, but it was mostly --

MEMBER WALLIS: It would be nice if you didn't say vast majority, if you said some number.

MR. MATTHEWS: Where?

MEMBER WALLIS: I don't know what you mean by vast majority.

MR. MATTHEWS: It's almost all of them

MEMBER WALLIS: There's only one or two out of 100,000 that have any cracks?

MR. MATTHEWS: I don't have the report, and I haven't studied the numbers here, but I don't believe it's very many at all that had these cracks that were consistent with the microfissuring, but in the interest of openness we're reporting everything here that we found in these analyses.

They didn't see any PWSCC in any of the weld metals either to date.

We analyzed the data by two methods. If there was enough data, we did a Y base analysis and we calculated the Y based data, the characteristic time to failure for the Alloy 690 over the Alloy 600 time to failure

We did increase the beta on the 690. It's a conservatism measure there because that gives you a shorter time to failure.

Here's just an example. The curve on the left is the 600 milanneal material, and it's just a cumulative probability of the samples that were in the test. The curve in the middle is the thermally treated 600 material, and then there were no failures in the 690. So as far as tests went, we assumed the failure right after that and drew a curve with a theta of five and calculate the improvement factor for that.

MEMBER ROSEN: Is that just an error on your slide, the 680. Six hundred and eighty degrees should be 680 --

MR. MATTHEWS: I was afraid somebody would look at that. I don't know. I saw that last night.

MEMBER ROSEN: It must because you wouldn't want to change its apogee. You'd invalidate the whole business, wouldn't you?

MR. MATTHEWS: I think it's a typo, but I'd have to go back to the report to make sure.

MEMBER ROSEN: That's a huge difference in this one.

MR. MATTHEWS: Yeah, it sure is. It's an extreme conservatism if it is not a typo.

MEMBER WALLIS: There's no data showed there.

MR. MATTHEWS: That's because there were no failures. So they assumed a failure to the right there.

VICE CHAIRMAN SIEBER: See what a little crime can do.

MR. MATTHEWS: In some of the lab data, there were not enough samples to do a full-blown wide base. So what they've done there is just calculate how long you ran the test for the 690 over the time to first crack for the 600 in that particular set of tests.

And here's a plot of some of those data. You see from this that the longer you run the tests, the higher the improvement factor appears to be on this particular analysis. So averaging all of this data is a conservative way to come up with an improvement factor, and the reason it gets longer is because you're failing the stuff, and the longer you run it, the bigger the improvement factor gets.

The results. The first method came up with an improvement factor of 26 and a half relative to the milanneal and 13.3 relative to the thermally treated, and there's some variation, but that's what the average of all the data sets came out to be.

The second method they used where they were using a smaller number of samples in each set came up with an improvement factor of 27.1 averaging all of those data. So it's consistent with the first one.

Field experience. We've had excellent field experience with 690. There have been a number of plants now operating for a number of years with Alloy 690 tubing; haven't really had any failures I don't think that were certainly attributable to PWSCC.

Many other components in the plant containing 690 in this weld metals are also in service. Some of these weld metals for over ten years, and to date there haven't been any indications of corrosion degradation in any of those components.

And the conclusions, stating them again. I'm through with that. Okay? Any questions on 690?

CHAIRMAN FORD: These are all -- I agree with you, and we discussed this at the beginning -- these are all based on at least initiation or at least specimens.

MR. MATTHEWS: Exactly.

CHAIRMAN FORD: And you still haven't tackled the question of whether if you do not get cracking at a defective weld which would then propagate on into the 690.

MR. MATTHEWS: We have -- the MRP has testing underway in our future work to try and quantify a crack growth rate in Alloy 690 base metal and weld metals, I believe.

CHAIRMAN FORD: So this is not finished. There is some propagation?

MR. MATTHEWS: No, no, no. This was just a review of the available information at the time. We have data, and there's also more data coming in. I thought that was in there. I think the Japanese have some information, and there were some 690 samples included in a WOG program that we didn't have in this data set, but we're going to try and get our hands on all of that data to modify MRP-111.

But separate from that, we also have test programs underway to try and crack it, and once you crack it, try and quantify some form of crack growth and what the crack growth rate might be. Okay?

CHAIRMAN FORD: Thank you.

MEMBER ROSEN: That 608 degrees that you did the tests at, is that -- how was that chosen? Is that a cold head, representative cold head?

MR. MATTHEWS: No, 608 would be a very hot head. In fact, it would be hotter than the hottest head we have, but you know, that was just one set of data. You know, they were run at various temperatures and all. They pulled out that particular lab set of data, and I'm not sure why they chose 608 for that particular situation.

MEMBER ROSEN: But it's a relatively extreme temperature, given today's current configuration.

MR. MATTHEWS: It's hotter than the hottest head that we have in service. I think Davis-Besse was the hottest one.

MEMBER ROSEN: I just want to be sure we don't invalidate our own conclusions by --

MR. MATTHEWS: It may have well been chosen because of steam generator testing, which runs at T-hot, which is hotter in general than the head temperatures. So these were probably coming from steam generator tests, which were trying to be more characteristic of the steam generator conditions.

I'm guessing. I don't have the report, all of the databases memorized.

MEMBER ROSEN: So the heads won't crack, but the tubes might.

MR. MATTHEWS: I think I can skip most of these conclusions here because we've already stated many of them. This was the structure of the MRP 110, and it has all of these supporting documents.

Glen covered the FMEA. We also have in 110 a flaw in wastage tolerance calculations, figuring out how big flaws we could tolerate. That's included in 110.

The inspection experience, this is as of December 2003. All of the original heads have been inspected by bare metal visual, all of them and/or nonvisual NDE, and some of the plants even had a second nonvisual NDE this spring.

The inspection experience indicates that time at temperature is a key factor. It's not the only factor, as Dr. Ford has continuously pointed out and we've acknowledged. Some of the material and fabrication categories are experiencing significantly lower rates of degradation compared to the others, but we're pretty much basing our inspection program and everything on the worst cases here.

And that is what the NRC has done, and you know, it's a conservative way to go. Some of these plants, if they decide they don't want to replace their heads and they're in a category that's just experiencing much less degradation because of their manufacturing techniques, they, in fact, come in and try and get something a little less rigorous.

I just throw this one in, and this one is sorted to make it monotonically increasing. To the left it's sorted by the EDY. This is just the inspection data. This is the chart I passed out. I tried to make sure you understand this is just me and the telephone. There's not a lot of verified data on here.

But it does tend to show that all of the red, which is anything that has had a leak, is in the higher EDY. Over about 16 EDY no leaks have been detected below that.

Some cracks have been detected down as low as about nine and a half EDY, and I don't know if I can tell you what point that is.

PARTICIPANT: Cook 2.

MR. MATTHEWS: That was Cook 2. The early

Cook 2 inspection they had that ID flaw and to repair it and have run since then. They did some other cracks sine then, but basically this says to me that as a fundamental screening tool of deciding when you need to do what kinds of inspections, it's pretty well holding up. You know, the NRC order, I believe, it sten -- is it ten or 12 where you go down there?

VICE CHAIRMAN SIEBER: Twelve.

MR. MATTHEWS: Twelve you go to high susceptibility, and so far we've only had two plants that have detected even any cracking below that, and then below eight, you got or above eight you go to moderate.

Now, I must admit these are all visual inspections down here. The NRC order and our inspection program would push a plant to do a baseline.

MEMBER WALLIS: Are these cracks left, are they the ones which left them because they're going to replace the head or something?

MR. MATTHEWS: There were a couple of outages. There was one plant. I can't remember which one it was. I think it may have only been one plant that had a flaw. In fact, Cook 2 left the flaw in service for one cycle, and then there was another plant more recently that found some flaws that were measurable, shallow, and they were able to determine and disposition those that they could run another cycle without violating their wall thickness, et cetera, and then they were repaired later.

Nozzle ejection evaluations, I believe Pete covered those.

Head wastage. Included in MRP-110 is a probabilistic evaluation of the wastage and the probability that we might could get a rupture from a wastage situation.

CHAIRMAN FORD: Are you all using a probability of detection greater than 60 percent?

MR. MATTHEWS: I'm not sure, and Steve and Glen had to leave. His wife is in the hospital. I'm not sure. I'm not sure what Glen used in his model.

Do you know, Pete?

I think he did.

We did do a consequential damage assessment. That's a separate section in the report where we looked at what's above the head and what could happen if you ejected a nozzle. There's not a lot up there except control rods and some instrumentation. Cabling for the control rods is the main thing up there and most of that is failsafe.

We did look at what happens if you could prevent more than one rod from going in, and the most likely outcome is that it's just another LOCA in a very favorable position for a LOCA, and there's plenty of shutdown margin to handle the situation.

So we're not doing any increased in the conditional core damage probability because of the consequences up there.

Now, the staff did raise a question about the rivless (phonetic) system, and we need to go see. That's not immediate accident. That's downstream when the operators are trying to figure out what they need to do. We need to go take a look at that.

Replacement head materials I just covered.

MRP-110, primary conclusions. I covered it at the start.

We're going into the inspection plan. We have a plan under development. It will be patterned similar to what Pete was talking about. I won't say it will be exactly one of those scenarios, but we'll use tools to evaluate whatever we do come up with.

It will maintain that extremely low probability of core damage and low probability of pressure boundary leakage.

Our intent was to try and replace the

requirements of the NRC first revised order that came out last February. I understand NRC is working on a rulemaking. I understand the code is working on a code case. So it will be -- I can't guarantee what it will look like, but hopefully it will look something like ours.

The logistics of all of that we have got to work out so that we don't wind up with multiple conflicting requirements on the plant. To get it we need one set of requirements that are imposed. Staff needs a regulatory control over it. So we've got to figure out how to make all of that happen

This is an inspection plan. Basically the top through Oconee 2 there in the spring, 11 plants have replaced their heads. There are several more that will be replaced in short order. And there's quite a number of plants.

Most of the high susceptibility plants are marching toward replacing their heads. Inspecting every cycle is too expensive.

We do have a boric acid corrosion test program underway. I think we've touched on that. This is the schedule of mock-up testing. I thought it was in '05, but it goes on into '06.

And we will revised or we do plan to

revise the boric acid corrosion guide book to include the results of these corrosion test specific for the reactor head.

MEMBER WALLIS: What about the understanding of the test? Have the tests proceeded to the point where you have a very good verified model of boric acid corrosion?

MR. MATTHEWS: I think we already had, if we knew the exact conditions, we had good data for a number of conditions for boric acid in the boric acid corrosion guide book. Plants use this day in and day out for assessing leaks, et cetera, and the impact.

The head, we don't have those tests yet. Tasks 1 through 3 are evaluating separate effects, the galvanic effects and all of those kinds of -- and the flow accelerated corrosion effects that may be going on in the specific geometry of the reactor vessel head.

Those results from those tests will provide data to help benchmark Glen's wastage model, and they'll also be used to help guide Task 4, which is our mock-up testing. What parameters are important? How do we need to model those or mock those up in the mock-up testing.

CHAIRMAN FORD: The trouble is that, if I fully understand the line of thought, we've heard about the boric acid to corrosion work for two years now, both from the staff and from the industry, and yet we haven't yet seen a quantitative model that says corrosion rate with OSTL is a function of the numbers against the algorithm

And so you can't save, although that particular nozzle is more likely to show corrosion versus that particular nozzle, and that's what the concern is.

Until we have that, you're going to have to do volumetric exams forever, until you can make a rational case as to why and can explain why we have had so many observed observations of no boric acid corrosion. It's a relatively un --

MR. MATTHEWS: At this point we're attributing that to the crack length above the weld that can be leaking, and that's correlated to the Davis-Besse situation versus the other nozzles. That's where we are today.

Test 1, 2, and 3 that we have underway are going to try to help us quantify those initial stages of that corrosion cavity and how it progresses and, you know, if it bears out what we think is the case, that it's related to the flow rate and the size of the crack above the weld. Then, you know, it would support it.

CHAIRMAN FORD: And so that would have been input, therefore, into your inspection program as to whether you could ever get to that situation.

MR. MATTHEWS: Right, right, and we believe that these visual inspections that are required by the order and will be required by our inspection program are going to be sufficient to catch something in the early stages if we miss a crack and it does develop a leak. The visual inspections will be adequate to catch it in time to prevent any kind of situation like Davis-Besse.

Six, ninety I've mentioned. We're trying to get the data and the Jaffee data and the WOG test data into our revision of the MRP-111, and we also have ongoing tests with alloy 690 to demonstrate and quantify that improvement, not just in initiation, but if we can, in crack growth rate also.

And that's the end of my slide show. And let me see. I believe I can close.

CHAIRMAN FORD: Okay. We've got, I think, one more talk before lunch.

MR. MATTHEWS: And if I can find it, I believe this is it.

CHAIRMAN FORD: We're going to catchup on schedule.

MR. MATTHEWS: It's because I talk fast.

CHAIRMAN FORD: Thanks very much.

The next time we will really attack you. We'll have you on first.

MR. MATTHEWS: I'm going to retire.

CHAIRMAN FORD: No, you can't do that.

MEMBER ROSEN: Well, maybe he got away with it because he didn't show any data.

MR. MATTHEWS: Well, the data are in the reports.

MR. MARION: Good morning. My name is Alex Marion. I'm a senior Director of Engineering at NEI.

What I thought I would do with the few minutes that I have is --

CHAIRMAN FORD: Don;'t feel constrained. This is important.

MR. MARION: Okay. How much time do I have?

CHAIRMAN FORD: Well, I would suggest if you finish by quarter past 12.

MR. MARION: I should be able to do that. Thank you.

My objective this morning is to provide you an overview of the materials initiative that was undertaken by the nuclear industry, and there are two supporting documents that we have provided you, NEI 03-08, which is a guideline and I'll talk about that briefly, and also a strategic plan that was issued in March, and we provided them to you in advance.

What I'd like to do is very briefly go over background, discuss the self-assessment that we had conducted of all the industry programs, briefly touch upon the intent and substance of the materials initiative and define some of the oversight structure of committees we put in place, and then talk about future changes and the way the industry is going to be managing materials issues as we move forward.

These next couple of slides just represent some of the areas where we've had materials degradation issues and BWRs, PWRs, and then I had a list of some of the degradation experience and some of this you're already familiar with, and I'm not going to go through it in any detail.

But one thing I do want to indicate, that fuel performance is part of this initiative primarily because of the effects of water chemistry on fuel integrity, and you'll be hearing more about that in detail in the future.

The bottom line relative to these events that have occurred over the past three, four years is that they're very costly from an economic point of view, but more importantly they raise questions about plant safety and plant operational performance.

And let me give you a perspective on that. In May of 2002, the NEI executive committee raised a very fundamental question that is at the root of all of this, and that was given that the industry at that particular time was spending approximately $55 million in research, why is it that we're having these events?

And that's $55 million in programs that were being administered by EPRI, as well as materials activities being carried forward by the NSSS owners groups.

MEMBER ROSEN: Now, that's $55 million across the whole scope, not just materials, right?

MR. MARION: No, no. That's just in the materials area. The EPRI MRP program, the BWR VIP program, certain aspects of the fuel program, the steam generator management program, the NSSS Owners Group material subcommittees.

CHAIRMAN FORD: Really?

MR. MARION: Yeah.

CHAIRMAN FORD: In addition to the reactor operators, the GEs and the Westinghouses.

MR. MARION: Yeah.

MEMBER ROSEN: That's a surprising number. That was a good question.

MR. MARION: Well, that was the reason I asked the question. You know, why is it that we're in this apparent reactive mode? Why are we waiting for something to happen, given this investment that we have in place?

So the obvious question that came up is are we going to relive the past. What we do know and we do believe it's not a matter of waiting for something to fail. We know what will fail next. We know when it will fail, and the obvious question in response to these failures, we've identified new replacement materials. Are they as susceptible or are they legitimately going to perform better?

But the point of all this was to position the industry to be more proactive instead of reactive in dealing with materials performance issues.

So the executive committee charged NEI to establish an activity to conduct a self-assessment of all of the materials related issue programs, and this was conducted in 2002.

The overall goal was to evaluate these programs, identify what's working well and why, what's not working well and why not, where there are areas of overlap and duplication, and where there are areas where we should be doing something in terms of research and investigation, but we're not for some reason or another.

And a couple of interesting observations came out of the self-assessment, and let me just focus on two here.

There were approximately nine issue programs or separate groups that we're dealing with material performance in some aspect or another, and we found that each one of those groups was competing for the same resources, resources both in terms of funding and resources in terms of technical personnel being involved, as well as leadership personnel being involved in our groups to provide guidance and direction.

And given that, we recognize that there wasn't an overall integration or coordination among these groups to make sure that the right level of effort and resources are being applied to PWR related issues or some PWR related issue, and not sacrificing that at the expense of something else.

Okay. But to make sure on an industry wide basis that we are prioritizing activities and programs and making sure that we're applying the appropriate level of resources to those programs so that over an industry-wide basis we're doing the right thing for the right reason.

As you can imagine a number of these programs have developed guidance documents over the years, and we found that implementation of follow-up on that guidance was lacking,m and we also found out that there wasn't any verification of implementation of the guidance document.

And I talked about the resources to support the groups, and one thing that was recommended by the team that did the self-assessment is that we ought to take advantage of the NEI/NSIAC initiative process, and I'll talk about that briefly.

The self-assessment included these groups. I'm not going to go over them in detail. You have the material, but you can see it's completely comprehensive in terms of everything going on in the industry relative to material performance issues in one form or another.

Recommendations from the self-assessment was to create an executive level and technical oversight groups to establish a policy for the management of materials issues, to use the NEI initiative process to expand NPO's role, improve communications, and do a more consistent job on defining and establishing an effective regulatory interface.

The guideline document I referred to earlier is very straightforward. It establishes the two standing committees. There is an executive oversight committee that will deal with the policy level issues, and that committee is chaired by Chris Crane from Exelon Nuclear.

There's a technical advisory committee that is going to deal with some of the technical issues, not necessarily addressing and resolving those issues, but assuring that there is an issue program in the industry that has the resources and the charge, if you will, to deal with a particular technical issue.

And the guideline establishes policy. The most important aspect of the guideline is it defines roles, responsibilities, and expectations not only for these advisory committees I just talked about briefly, but also for the issue programs, as well as the individual utilities.

And if you've had an opportunity to read it, I'm sure you'll find that it's very effective in that regard, especially since I'm one of the people who wrote it.

The materials initiative is rather straightforward. I'm not going to read this to you, but it calls for the utilities to or the initiative to assure that the industry is going to continue to focus on safety and operational performance in dealing with materials issues in the future, and it calls for the utilities to endorse and support NEI 03-08, and it was effective January 2004.

Now, what we've done over this period of time since January is establish some criteria and the protocols for consistent implementation of this initiative, but the issue programs as well as with the individual utilities.

The purpose of the initiative is fundamental. I already touched on some of these aspects. I'm not going to read through them.

Actions required by the initiative. Now, this initiative is there have been 19 initiatives that have been developed in the nuclear industry since the formation of NUMRC back in 1988. NUMRC, Nuclear Utility Management Resources Council, as one of the predecessor organizations to NEI.

And the initiative process is straightforward. It's where the nuclear industry gets together, and the chief nuclear officers establish a policy position or a specific strategic or tactical course of action to address an issue that's of concern to the industry.

Now, the concern could range from something that is purely economic, an efficiency process improvement, to something that is a regulatory concern that the NRC may have, not to necessarily supplant the NRC's regulatory responsibility, but to demonstrate that the industry is going to take care of this, and if the NRC finds that the NRC was effective in that, then the NRC would be reasonably happy. If they're not, then the NRC will issue supplemental regulatory requirements or whatever the case may be, depending upon the nature of the issue.

And in all those cases, except for a couple of initiatives where we were short of 100 percent approval, our requirement is 80 percent approval of the chief nuclear officers, but I'm here to tell you that most of those initiatives that have been taken in the past, it's been 100 percent, and I'm pleased to tell you in this one on materials, it was 100 percent approval.

And the actions required on the part of the utilities consistent with this initiative was to commit leadership and technical personnel to the issue program, a commitment of funds for the materials issue programs, and commitment to implement the applicable guidance documents that are developed by these issue programs.

I might add as part of this there was an agreement that each utility will provide an additional $60,000 per reactor for a two-year period, the two year period being this year and 2005, to basically help us deal with emergent issues because as you can imagine, I'll take the MRP as an example. Let's say they have ten projects planned for fiscal year 2004, an let's say in January something happens at a plant that has generic applicability that was not factored into those ten projects.

So now MRP is in a position of dropping one of those ten which are already determined to be important so they could pick up this new one. That's the way these programs have been working in the past.

The intent of this emergent materials issue fund is to compensate for that kind of action, provide seed money to deal with these emergent issues as they come up.

MEMBER ROSEN: That's $60,000 per unit or per site or --

MR. MARION: Per reactor. It's a little over six million a year. Over the two years, it's 12 million, and that's on top of the 55 I mentioned earlier.

So we're positioned right now about $70 million.

VICE CHAIRMAN SIEBER: So does that go to EPRI or do you guys --

MR. MARION: Well, that emergent issues fund is being managed by NEI through these two advisory committees, the executive oversight and the technical group. EPRI is involved basically functioning as the banker.

We're received -- and don't ask me to elaborate on that. It just seemed to work out that way -- but we are in the process of soliciting project proposals, and the technical advisory group is meeting in mid-June to start evaluating proposals, and you'll be hearing more about that as we move forward.

The materials initiative was approved in May, and this is rather straightforward.

This is a statement of the policy commitment that captures some of the key elements of what we want to do with the industry. Clearly, establish industry materials management activities is forward looking and coordinated. We want to effectively respond to emergent issues that come up. We believe that there will be emerging issues coming up over the next couple of years.

Our primary focus is plant safety and operational risk significance.

This slide represents the groups that fall within the umbrella or the scope of the materials initiative. I'm not going to read them.

The owners group subcommittees. This slide represents the membership of the executive oversight group. Each of the utilities represented are the chief nuclear officers. You see the vendors are represented as well as NPO, EPRI and NEI.

The technical advisory group is chaired by Dave Maldon of Arizona Public Service, and I want to point out that what's interesting about this group is you look at the information and all of the current issue programs are represented on this group, and this is a demonstration of the integrated, coordinated approach.

We rely on these individuals listed on the slide to communicate to the respective program what they're doing and what needs to be done. For example, with regard to the MRP, we rely on Mike Robinson to work with Larry and his peers to make sure they have the right resources to do what needs to be done, et cetera.

They don't communicate back through mike, as an example.

MEMBER WALLIS: Why don't you have outsiders on this to keep everybody on their toes?

MR. MARION: Why do we have outsiders?

MEMBER WALLIS: I think you ought to have outsiders in this. You ought to have some people from academia, for example.

MR. MARION: Well, that's an interesting point. This is an internal industry activity. We are at this particular point -- I'll get into it a little bit later --developing the state of knowledge relative to materials performance, and as part of that activity, we are engaging outside experts to give us their insights so that we can establish this knowledge base.

But right now in terms of managing the materials issues, we feel that the industry needs to position itself in an effective manner. Now, maybe once that's established, we may want to broaden that a little bit, but right now it's strictly an internal --

MEMBER WALLIS: I'm just thinking there may be ways of thinking or myths or things like that which will permeate the industry, and you need some check on that from outside.

MR. MARION: That's a good point. We'll take that under consideration.

Roles and responsibilities, we want to make this very clear. The executive oversight group is directly responsible to the industry chief nuclear officers through NEI. They're going to provide oversight and road policy guidance.

The technical advisory group assures overall coordination, development of strategic plan and protocols for the issue programs. The issue programs that are identified on the other slides still have fundamental lead responsibility for doing the technical work.

I just want to make sure that sinks in because within the industry even now there's still some confusion as to what the roles and responsibilities are.

The document that I've provided you was the initial publication of the strategic plan. It refers to appendices and some discussion on work plans, and we are in the process now of compiling the issue program work plans, and we're also developing a degradation matrix and issue management table that I'll speak to briefly.

But this document is going to be revised in July, incorporating the basic technical state of knowledge, the materials degradation issues, but not only the issues themselves. The consequences of materials degradation in terms of system and component performance as it may affect plant or operational safety.

And so that revision will be ready in July, and I'm hoping at a meeting in August or September of this subcommittee and the main committee we could spend a reasonable amount of time, like a couple of hours -- and I suspect we'll need that at least -- to go through the technical substance of these documents we're referring to.

And Dr. Ford is involved in that effort as part of expert panel solicitation, and I'm sure he would agree that we would need a couple of hours.

These slides capture some of the content, and it's kind of difficult. You're not going to see that in a document that you already have, but you will see that in a document that we provide you in July time frame.

We're doing a tactical assessment of all the issues. We're looking at those over the near term and those over the longer term, and we're trying to identify for those issues what work is in progress, what work needs to be planned, what work is not done that should be done that we need to factor into the process.

The document already captures the activities that are planned for this year, in terms of near term tactical issues, and that represents the time frame of zero to three year development, planning, and execution.

The one principle we're conveying to these issue programs is you've got to identify deliverables. You have to identify a schedule. You have to plan to those deliverables, plan to that schedule, and if you can't, you have to articulate what it is you're going to be doing over the longer term.

But you're not going to be receiving funding to allow you to play with stuff over ten years or 15 years without some level of accountability. Okay?

And as you can imagine in the spills area that's a significant challenge put upon anyone, but the idea is to identify what progress you're going to be making and how that progress contributes to an overall objective or an overall answer to a particular question.

Fundamental Management 101. It's going to be interesting as I'm sure you can all appreciate.

The materials technical advisory group is working with the issue programs to make sure that some of these principles are being incorporated into their development planning and execution activities.

Again, as I mentioned, we're focusing on technical gaps. What is it that we're not doing that we should be doing to improve our knowledge base. Of those items, which ones pose risk to the industry and what is the risk ongoing that may exist prior to having the final answer? And how do we compensate for that risk? Would that be in some conservative inspection requirement? It may be, okay, until the final solution is established.

And I think it would reflect on some of the presentations that Larry is giving you from the MRP work. You can see that that philosophy is playing out, and I think that's a positive thing.

In terms of future activities, the

technical advisory group has a monthly phone call. Their next call is scheduled, I think, this week on Wednesday.

The executive group meets quarterly with NRC senior management. We've established a number of protocols relative to some of the findings that came out of the self-assessment.

As I mentioned before, we're positioning ourselves to review project proposals and start disbursing some of the money.

We are going to be working on performance metrics, and we'll have them established by the end of the year, not only for the two committees that we put in place for this initiative, but also for the issue programs as well because we want to make sure that this effort focuses on the conclusions and findings of the self-assessment, and effectively does position the industry to be more proactive and forward looking.

And as I mentioned before, we will be issuing the strategic plan in the July time frame, and we will look forward to briefing this committee on it in the future.

In terms of changing within the industry, I think you're going to see issue program work products that are going to be very specific relative to implementation requirements in terms of what the expectations are of the utilities on how they're supposed to use that work product.

Larry mentioned a couple of letters in his presentations. Each of those letters were also sent to the industry chief nuclear officers to apprise them of the recommendations coming out of MRP, as well as the expectations within the framework of this initiative.

The communications on new materials issues will improve as well as the interface with the NRC. As I mentioned before, I firmly believe that the industry is going to be proactive. As we complete this next phase of work towards the end of this year, I think it will be clear to everyone who reviews this next version of the strategic plan that the industry is, indeed, positioned to be proactive moving forward.

And we're doing our best to improve integration and coordination among the issue programs, and you'll see more and more positive results coming out of that in the future.

That completes my presentation, and thank you.

CHAIRMAN FORD: Thank you very much, indeed.

Questions?

MEMBER ROSEN: One quick question. Implementation requirements. Someone is going to check that the utilities actually do the things?

MR. MARION: Yes, yes. The Institute of Nuclear Power Operations has a review visit program that is focusing on three areas. They're looking at the guidance that's been issued by the Boiling Water Reactor Vessel Internals Program. They're looking at the Steam Generator Management Program. And the third area is they're looking at guidance that has been developed to assure the integrity of the primary system pressure boundary.

And as these new guidelines come out of the issue programs, INPO will pick them up and integrate them into the appropriate review visit program.

MEMBER ROSEN: And the INPO expertise for this, conducting these review visits, will come from?

MR. MARION: The industry. INPO makes up a review team comprised of six to eight individuals. INPO, and individual from INPO is involved, but the balance of the team is made up of representatives from the industry, individuals like Larry Matthews and like Robinson and others.

And our intent is not to supplant or replace or provide an alternative to NRC actions in this area, but as I said before, to clearly demonstrate that the industry is dealing with these issues, and then the NRC determines what they need to do in terms of future regulatory action.

We're hoping we get to a point where we establish some level of confidence in industry's performance in light of what happened at Davis-Besse.

CHAIRMAN FORD: Thanks.

Any other comments?

MEMBER WALLIS: Well, this sounds to me like a generic plan for any broad research program. You could change it and say this is a thermal hydraulics research program or whatever. Is there anything special about this materials area which led to a different strategy than you would have for other areas?

MR. MARION: I'm an electrical engineer by training. All of this is special to me.

MEMBER WALLIS: You see what I'm getting at. I mean, it looks like a very good, big program, and you're doing all of the right things. But is there something special about materials that led you to do something this way rather than that way?

MR. MARION: Well, I think what really comes to the point here is that we were spending a lot of money as an industry in a number of programs, and apparently for one reason or another, I wasn't effective. We were still having events at plants.

So the special nature of it was you're spending 50, $55 million. You're still having problems at plants. What's wrong with this picture? That's why we did the self-assessment.

Basically the concepts that we're applying here are fundamental management focusing on positioning, integration, and coordination, roles and responsibilities and expectations, not to say that they didn't exist in the issue programs. In some they did; in others they didn't do as well as some of the other ones.

But the idea is to capture that, put it together, and make it work.

MEMBER BONACA: And you know, I mean, the industry was very successful with the programs like BWR VIP, for example. That was an example where you had the situation in early '90s and late '80s where we thought that BWRs would be goners, and I mean, now it becomes a very structured plan, and I think successful plan, and I think I was pleased to see that BWR VIP is still an element of this.

MR. MARION: Oh, absolutely, and you raise an excellent point. I should have mentioned this in the presentation.

We're using the BWR VIP model for managing these issues. The idea of evaluation, evaluation of the mechanism, develop an inspection plan, evaluate the results, repair replacement activities, and it's a continuous process.

MEMBER ROSEN: Well, I have to say in response to Graham's comment what's different about this plan to me is that the fuel research is integrated into it. Fuel has always been treated as something apart from these kinds of issues.

In fact, here I think that's going to be very challenging.

VICE CHAIRMAN SIEBER: I think that just recognizes that everything is corroding all the time and really what you're trying to do is to get ahead of the surprises. You know, if I look at thermohydraulics or operations or fuel management, there aren't a lot of surprises there, but there seems to be a lot of surprises in the materials business, and every one of them not only costs a lot of money, eventually causing you to replace it, but it's affecting outage times and plant operability.

And so it's a very expensive proposition for the licensees to deal with, and the more they get ahead of it like they're trying to do right now, and the staff is trying to do basically the same kinds of things, the better off everybody is going to be.

CHAIRMAN FORD: Any other comments?

(No response.)

CHAIRMAN FORD: Alex, thank you very much indeed, and we look forward to seeing you back in the fall.

We'll go into recess until 1:15.

(Whereupon, at 12:17 p.m., the meeting was recessed for lunch, to reconvene at 1:15 p.m., the same day.)

CHAIRMAN FORD: I would like to come back into the session. This afternoon we have presentations from the staff and RES. Bill, I will pass it on to you to start off.

MR. KOO: I am Bill Koo with Materials and Chemical Engineering Branch of NRR. The purpose of my presentation is to provide you an update on the reactor vessel upper head inspections.

The staff had briefed this committee on this objective in April of last year, about two months after NRC had issued an order to mandate the reactor vessel upper head inspection of all PWRs.

A few months ago in February of this year, NRC had issued a first revised order. Therefore, my presentation today will focus on what are the changes in the first revised order and an update of the inspection results.

CHAIRMAN FORD: Bill, will we be hearing about the potential bulletin that the ACRS heard about a couple of months ago, to do with the pressurizer?

MR. KOO: I will not be -- Will Matthew address that?

MR. MITCHELL: This is Matthew Mitchell of NRR staff. No, I don't believe that was intended to be covered as one of the subjects for today's presentation.

CHAIRMAN FORD: Okay.

MR. KOO: I will start with a brief introduction of the background of this issue. Then I will discuss briefly regarding the process to implement all the inspection requirements into the regulation.

In addressing the reactor vessel upper head degradation and all the cracking issues, NRC had issued three bulletins, one in 2001 and two in 2002.

Because the industry did not identify an acceptable long term inspection program in their response to the bulletin, NRC issued an order in February of last year.

The subject order requires all the licensees of PWR to perform specific inspections of the reactor vessel upper head and associated penetration nozzles to ensure there is no corrosion degradation on the vessel head and no cracking in the associated nozzles.

A few months ago in February of this year, a first revised NRC order was issued. Why a revision of the order is needed? This is because, since the issuance of the original order, the staff had received a large number of relaxation requests to seek relief from some portions of the order.

Many of these relaxation requests are common issues. During the period of February through December of last year, the staff had received 24 relaxation requests, and some requested the flexibility in implementation, and some requested the relief from all the examination requirements due to physical obstructions, complex nozzle configuration and instrument limitations.

Therefore, a revision of the order is

needed in order to address to these issues, which were not considered in the original order.

Five items are updated in the first revised order. Those items are: Bare metal visual inspection requirements; penetration nozzle inspection coverage; combination of examination methods; flaw evaluation reference; and the replaced vessel head inspection requirement. Let me review each of the five items.

Let me first go to the fourth item on the slide. This item is an update of the flaw evaluation reference, because new guidance has been issued. This is basically an editorial change, because the order allows the reference to be revised and also requires the licensees to follow the latest guidance for flaw evaluation.

There is no change in the method of calculating the EDY. The EDY stands for effective degradation years. The EDY is used to evaluate the susceptibility of the reactor vessel head which is based on operating time and head temperature.

There is also no change in the ranking criteria in the inspection requirements for the high, moderate and the low susceptibility trends. However, the original order did not provide any guidance regarding the inspection requirements for the replaced vessel head.

Therefore, the first revised order established a new category called Replaced category for the inspection of replaced vessel heads. The inspection requirements for this category is similar to plants in low susceptibility category.

I would like to point out that for the replaced heads, there is no difference in the inspection schedule between the Alloy 600 head and the Alloy 690 head, because at this time we need more service experience and test data to justify any changes for the Alloy 690 heads.

This item concerns the bare metal visual inspection of the reactor vessel head. The original order required 100 percent coverage of the entire vessel head surface. For some plants, this requirement is difficult to meet, because a small portion of the vessel head surface was obscured by the support structure interferences.

Therefore, the First Revised Order reduced the vessel head surface coverage requirement from 100 percent to no less than 95 percent, provided the support structure causing the obstruction must be located at an elevation away from the outermost vessel head penetrations so they will not interfere with effective visual examination of the vessel head and associated penetration nodules.

CHAIRMAN FORD: Would you mind just going back to the previous slide, please, to 6?

MR. KOO: Slide Number 6, right.

CHAIRMAN FORD: I'm sorry, 5. On the high susceptibility plants, even with the Revised Order, we are saying that you must have bare metal visual plus -- and a nonvisual NDE.

MR. KOO: Right.

CHAIRMAN FORD: Now I understand that Millstone II is a high susceptibility plant, and yet it was asking for a relief on the inspection on the original Order because of insulation, and that was undecided. As to how that would be dispositioned, it was undecided at our last meeting. How was that resolved?

MR. KOO: I think Jay can --

MR. COLLINS: Jay Collins with Materials and Chemical Engineering Branch. Millstone II did remove their insulation and did perform that bare metal visual inspection.

CHAIRMAN FORD: Because they were asking for relief, I understand.

MR. COLLINS: They were asking for relief, but they determined that it was necessary to perform that. They looked at a number of different alternative methods of NDE to assure that integrity, but at that time they did not have enough to justify it. So they went with a full bare metal visual inspection.

CHAIRMAN FORD: Thank you very much. Okay, Bill.

MR. KOO: Okay. This item concerns the inspection coverage of the penetration nozzles. The original Order required inspection to cover from two inches above the j-groove weld to the bottom of the nozzle. Due to physical interferences and test probe limitations, many plants cannot meet the Order requirements of inspecting all the way to the bottom of the nozzle.

Therefore, the First Revised Order revised this requirement of inspecting to the bottom of the nozzle, and allows the examination to be performed from two inches above the j-groove weld to two inches below the j-groove weld or to the bottom of the nozzle, if less than two inches, or from two inches above the j-groove weld to one inch below the j-groove weld plus all the area below the j-groove weld that have an operating stress of 20 ksi in tension and greater.

This would require a plant-specific stress analysis to determine any additional area beyond the one-inch zone to be included for inspection. The operating stresses considered in the stress analysis consist of normal operating stresses and the welding related stresses.

This revision reduces the area of the inspection coverage below the j-groove weld. The reduction of inspection coverage is supported by a review of a number of stress analysis reports showing that a region of two inches long below the j-groove weld will cover all the high stress area with operating stresses of 20 ksi in tension and higher.

This is also based on a consideration that the likelihood of crack initiation in the low stress area is low.

CHAIRMAN FORD: Where did the 20 ksi come from? It presumably relates to some data.

MR. KOO: This is considered low in comparison with the u-strength of the materials, the nozzle materials. Normally, the u-strength of the nozzle material is in the range of 37 ksi to 65 ksi. So 20 ksi is about 54 percent of the low end of the u-strength.

CHAIRMAN FORD: So there is data, I am assuming, to show that there is no cracking below 20 ksi under all conditions, operating conditions? I'm just interested to know why 20. Was it picked out of the air? Was it an engineering judgment?

MR. KOO: It is basically an engineering judgment, considering the service inspection experience.

CHAIRMAN FORD: Okay.

MR. KOO: This is a schematic drawing of a nozzle cross-section to show the required inspection areas. The dark area is the area requiring inspection. It consists of two inches above the j-groove weld to two inches below the j-groove weld or one inch below the j-groove weld with a stress analysis.

This item concerns the examination methods that can be used for an Order inspection. The wording in the original Order is very rigid, as it requires either volumetric or surface examinations to be performed. In other words, only one method can be used for the nozzle inspection. However, this is not the intent of the Order.

Therefore, in the First Revised Order it permits a combination of volumetric and a surface examination to be performed for nozzle inspection. This gives the licensees the flexibility to choose the most appropriate inspection methods to achieve the required inspection coverage, such as while the upper portion of a nozzle was inspected by volumetric method and some lower portion of the nozzle could be inspected by a surface method. There is no restriction to what method can be used for nozzle inspection.

CHAIRMAN FORD: I can understand why you are wanting to maybe relax some of the conditions, but was there any analysis done of the associated risk of relaxing of these requirements?

MR. KOO: In this particular relaxation, there is no real change in the inspection results between volumetric versus surface examinations, because for surface examination you have to inspect both sides of the nozzle. So it would cover the whole volume.

CHAIRMAN FORD: Okay. When you say a combination of volumetric and surface, there is no prescription as to what that combination should be?

MR. KOO: The next slide will show you the examination area.

CHAIRMAN FORD: Okay. Oh, okay, this is rather similar to that which we saw before?

MR. KOO: Right. The green one is the ultrasonic inspection area, and the red one is the surface examination.

CHAIRMAN FORD: So we don't have color on our things here. So --

MEMBER SHACK: Now can you do the wetted surface inspection in lieu of the ultrasonic?

MR. KOO: Portions of it. For example, for the bottom of the nozzle sometimes there is a funnel attached to the nozzle or thermal sleeves or sometimes there is a blind zone. By using a blade probe, you know, then you can apply UT or eddy current on that particular surface to make up the area or the volume.

CHAIRMAN FORD: I don't know if you were here this morning, but we have heard some questions of the industry regarding probabilities of detection for these various techniques and for a particular component area being inspected.

When you were coming up with these criteria, did you take into account your own analyses or probabilities of detection and the consequences of those probabilities?

MR. KOO: I don't think we have considered that, but in terms of the inspection requirements and the frequencies for the high susceptibility plants, the licensee has to perform bare metal visual plus the NDE every outage.

CHAIRMAN FORD: If you remember, back in the spring of last year we had a very extended discussion -- that is, with the staff and with industry -- on the whole question of inspection detection capabilities, probabilities of detection; and we got a very maybe confused answer. At least, it was confusing to me.

Let me put the question this way. Was it your understanding of the probabilities of detection and the detection capabilities -- What do you think you could detect in terms of depth of size by, for instance, ultrasonics?

MR. KOO: My view is, since this is a very complex geometry, I don't think we can say you can detect every flaw in all situations. There is always some kind of a POD. You may miss one or two. Since for the high susceptibility plants you also have to perform bare metal inspection on the surface, so in the event you miss one or two cracks, and also it has to go all the way through the weld and through the nozzle, then we can't always find the leak.

CHAIRMAN FORD: Let me tell you what is worrying me. I am hearing from various people with and without the industry that -- One comment I keep hearing is it is impossible to inspect volumetrically with any degree of accuracy these complex geometries. I mean, that's a worst case statement, but I have heard that.

We saw data this morning saying that you could detect with a certain degree of probability a defect in these structures in the j-weld of the order of 0.1 of an inch. 0.16, I think it was. So I am hearing two ends of the spectrum, and I am interested to know what the staff's perception of that capability is and how that relates to the safety of these components.

MR. KOO: I believe the UT of the j-groove weld is very difficult to perform.

CHAIRMAN FORD: That is in line with what I've been hearing, but so what? There is a "so what?" to it. It worries me. You say it's very difficult, and yet what I would like to know is, well, what is my danger? Where am I at risk, because we don't have the technology to inspect reliably?

MR. HISER: Bill, maybe I can add. This

is Allen Hiser, Assistant Branch Chief, Materials Engineering Branch of Research.

The industry does have performance demonstration type of requirements for these inspections. So it is not shooting blind with the UT or the eddy current. So they do have requirements that they have to go to EPRI and demonstrate their capabilities to find flaws.

So from that perspective it's -- I think the inspections, as Bill mentioned, are not perfect, but I think they have a performance requirement there. Maybe Pete has some additional information.

MR. RICCARDELLA: This is Pete Riccardella. I just wanted to make a correction. The data that I presented this morning on the 0.16 being detectable with a certain probability is for UT at the tube. It is not intended to cover the j-groove weld. We don't UT the j-groove weld.

The j-groove weld as shown here, you do a surface inspection by either eddy current or penetrant type tests. So maybe there is some disconnect in what the two experts that you have been listening to have been talking about.

The UT of the tube, I think, is a very doable thing with a fairly high degree of reliability, and in terms of what the consequences are, we have concluded that cracking in the j-groove weld might lead to a leak but, in and of itself, cannot realistically lead to a nozzle ejection without the crack first propagating into the tube, and that once it propagates into the tube, then again it is detectable by the ultrasonics.

CHAIRMAN FORD: I keep putting you through this exam, and it's not meant to be an exam. Do you go through the assessment of risk? When you say it's very difficult to inspect, and Pete Riccardella just told us -- corrected me on some of the facts, do you independently go through the risk assessment or do you just take the industry's methodology? Do you accept the industry's methodology, their conclusions?

MR. KOO: You are talking about the risk?

CHAIRMAN FORD: The whole question of probability detection, the inspection capabilities, and the risks associated with those -- do you go through an independent evaluation?

MR. COLLINS: So far, I believe more of our analysis for the inspection plan has been through a deterministic approach rather than a probabilistic at this point, or looking at specific areas of risk. We are looking at what is necessary to detect flaws within the high stress region, what should be the inspection zone, and how we can back up things like the bare metal visual which has had a problem with detecting leaks through the volumetric, making sure that that inspection zone coverage is sufficient, and that the time periods are adequate.

MR. HISER: I think, Dr. Ford, I almost read into what you said like there are multiple risk submittals that staff has received, but there isn't. There's one that Pete Riccardella described that, I think, the staff received maybe a month and a half, two months ago. So the staff is in the midst of reviewing that at this point.

CHAIRMAN FORD: Maybe I'm not explaining my problem simply. I recognize that there are accepted UT inspection procedures. You got teams on it, EPRI, NDE, Center going through an evaluation as to what their capabilities are for detecting. So that's the fact. That is the technological fact.

The fact that they don't have a probability of detection of 100 percent of all the areas of that component, subassembly, means that there are, therefore, this risk. Pete Riccardella has gone through the consequence of that by their analyses.

My question is: Do you do separate

independent analysis to assure yourselves, to assure ourselves, that in fact we are safe? That's my question. Does the NRC do an independent analysis or the risk?

MR. HISER: The NRC has not performed an independent risk analysis, no. We have not.

MR. COLLINS: But our inspection by defense in depth allows us two verified ways of verifying a leak path determination. We have a bare metal visual which identifies if any leakage has reached to the head. As well, through volumetric we require a leak path determination and through a surface examination by examining all surfaces, including the j-groove weld surface, provide that defense in depth to the best of our inspection capabilities at this point.

Quite honestly, as far as what additional inspections we could perform to ensure that was necessary --

CHAIRMAN FORD: No, no, I recognize there is a limit to what the technology can do right this instant. That's a fact. That's a real fact. All I'm just asking is, given those limitations, have we done independent analyses of the risk. That's all.

MR KOO: To answer your question is no, we haven't done any.

Okay. This table is a summary of all the plants that were found to have cracked or leaking penetrations. So far there are a total of 15 plants that were found to have cracked or leaking penetrations. Of the 15 plants, ten plants were found to have leaking penetrations, and five plants were found to have only cracked penetrations.

All plants are high susceptibility plants with the exceptions of Millstone Unit 2. Millstone Unit 2 was a moderate susceptibility plant when the cracks were found, with an 11.6 EDY. 11.6 EDY is very close to 12 EDY, which will qualify the plant to be a high susceptibility plant.

So far, the inspection results appear to support the susceptibility ranking criteria in the Order, since almost all leaking or crack penetrations were found in the high susceptibility plants.

This slide shows some statistics of the inspection data. So far, about 140 vessel head penetrations were found to be cracked,with a total of about 393 cracks in the nozzles or associated welds. Twenty of those cracks were circumferential cracks at or above the j-groove weld, and 55 of those cracked nozzles were leaking.

I would like to point out that several high EDY heads were not inspected prior to replacement. Therefore, the total number of crack penetrations could be higher, if those plants were fully inspected.

I also would like to point out that an NDE inspection has not been performed on the low susceptibility plants.

This slide summarizes the vessel head replacement activities. A total of 11 plants have replaced the upper head. Ten heads have Alloy 690 nozzles, and the one has Alloy 600 nozzle. That is at a Davis-Besse plant. In addition, 22 plants have announced plans to replace their vessel upper heads.

Two instances of high crack growth rate at the upper head nozzles were reported. One instance is at Millstone Unit 2. A few nozzles were reported to have a crack growth rate of over 50 percent throughwall in one cycle. The cracks were located below the j-groove weld.

The second instance is at Arkansas Nuclear 1 Unit --

CHAIRMAN FORD: Just to make sure I understand, Millstone 2, 50 percent throughwall?

MR. KOO: Yes.

CHAIRMAN FORD: This was not a circumferential crack? It was an axial crack?

MR. KOO: Yes, axial crack.

CHAIRMAN FORD: Okay.

MR. KOO: The second instance is at Arkansas Nuclear One, Unit 1. The growth of an axial crack was reported to have a crack growth rate over 1.3 inch per year. The crack was located at the outside diameter of the nozzle in the weld region.

These two instances of apparent high crack growth rate at the nozzles need to be further evaluated to determine if it is bounded by the crack growth equation used in the relaxation request.

CHAIRMAN FORD: I can't do the conversions easily, but is 1.3 inches per year -- What percentile is it of the database? It must be way, way out of sight. That being the case --

MR. HISER: Six hundred percentile.

CHAIRMAN FORD: Well, amusement aside from that, is there an explanation as to why the cracking at that rate?

MR. KOO: At this time, we don't really have any solid basis for this crack growth rate.

MR. COLLINS: But this crack is identified in the j-groove -- from the j-groove weld region of the nozzle in the heat affected zone in which we have indications and data supporting significantly higher crack growth rates.

We do an analysis for the j-groove weld area, and while we require the inspection of the nozzle material underneath the j-groove, it's because we don't give credit for this j-groove wear area for a crack to grow axially through that area for crack growth analysis to determine a susceptible inspection zone beneath the j-groove weld.

CHAIRMAN FORD: Hold on. Let's just follow this for justa wee minute. We heard this morning that the industry were taking into account very high crack growth rates had been seen associated with cracking right next to the weld fusion line, and you are telling me this is --

MR. COLLINS: This is one of those cases.

MR. KOO: Yes, there's an interface between the weld and the nozzle.

CHAIRMAN FORD: And I also heard this morning that at 1.3 inches per year you could have a situation where you could -- within a refuel cycle, you could have a substantial amount of crack into the annulus.

MR. COLLINS: The way this crack would

grow would be through -- This is through the weld material. So it would have to go through the triple point where the nozzle meets the weld meets the butter. So we have this accelerated crack growth up to that point.

Once it reaches that point, it would either have to go into the nozzle material to continue to grow larger or into the head material or the butter material, which would have a greater resistance to crack growth in those areas. So at this point --

CHAIRMAN FORD: Well, I apologize, but this is going beyond inspection aspects, but it is interest. We've now got a whole lot of information from the BWRs showing that you can get extensive cracking right adjacent to the weld fusion line at higher rates than normal.

We've got one for the -- data in the laboratory showing a factor of 30 higher crack growth rates for Alloy 600 adjacent to the weld fusion line, and now we've got one in the field.

So why shouldn't I assume that we are going to see many more of these, and that the "so what?" of it is that it may not be a safety issue from the cracking point of view, assuming it remains axial, but it would be of consequence as far as wastage is concerned.

MR. COLLINS: But we feel that our inspection program for high susceptibility plants would protect against wastage of the head for even this particular crack growth. It's just a data point which we are identifying it, and as well we are also looking into the data to make sure that we have a full grasp of the situation at ANO. This happened just this particular outage.

MEMBER SHACK: How hot is the ANO head?

MR. COLLINS: ANO is one of the highest EDY heads, and I believe they are scheduled for replacement very soon.

MR. HISER: Around 600.

MR. COLLINS: Oh, I'm sorry.

MR. HISER: Take comfort form a couple of things. If you are a moderate susceptibility, high susceptibility plan, you are doing an inspection of every nozzle every outage.

CHAIRMAN FORD: Right.

MR. HISER: If you are a moderate susceptibility and you have a crack that runs through that interface, it allows a leak. You are either doing a bare metal visual, which should detect evidence of the leakage, or you are doing a UT or surface exam, which will detect the cracking.

Also, if you are doing a UT, you are doing an annulus -- an interface fit zone check to see if you have any leakage. So you would identify the leak that way.

I think one point Jay was trying to make, for the relaxation requests due to limits on the nozzles, the interface zone is not included in that analysis. We do not allow the cracks to grow up into the nozzle elevation adjacent to the weld. So that area is cut off.

So the higher crack growth rate is excluded from those analyses. It is a concern that there is higher crack growth rates, but we think at this point that the inspections will capture any rapid cracking that is not anticipated.

As you mentioned, these are axial cracks. They do not pose a significant safety issue within the one cycle that they could propagate.

CHAIRMAN FORD: Yes, but it could affect the wastage.

MR. HISER: Absolutely. But I think, you know, there should be an expectation that, when you start a cycle, that you do not have a leak and, if you are doing a leak, you probably do not have a crack there. So you have a certain period of time before you begin to leak during a cycle that you could leak. So that would tend to restrict any degradation that could occur.

CHAIRMAN FORD: Thanks very much. Sorry I got you off track.

MR. KOO: The last slide will discuss the staff's long term goal to codify the special requirements to ensure structural integrity of the vessel upper head and associated penetration nozzles.

The staff considers that the implementation of upper head inspection requirements through the Order is a temporary or short term measure. For long term inspection requirements, it should be incorporated into the regulations.

There are two methods we can use to implement the inspection requirements into the regulations. One method is to endorse the new ASME code requirements, if the new code inspection requirements are acceptable.

The industry is currently working on such an inspection plan. However, it is difficult to predict how long it would take to complete this process.

The other method is to proceed with the

rulemaking process to incorporate all the inspection requirements into the regulations. This process would take about two years to complete.

The staff has already initiated a rulemaking plan to incorporate all the inspection requirements into the regulations. If the proposed plan is approved by the Commission as scheduled, the staff expects the subject rulemaking plan will complete by June 2006, about two years later.

CHAIRMAN FORD: The ASME code requirements -- modifications to them -- generally take some time. So is it reasonable to suppose that you will probably go with the second -- proceed with the rulemaking rather than rely on waiting for an ASME code provision?

MR. KOO: Well, we can go two methods in parallel.

CHAIRMAN FORD: Why does it take two years to alter the rule?

MR. COLLINS: That is the current process of issuing the ruling. We will be before you again a couple of times to show you where we are as far as that rulemaking plan.

MR. HISER: That's just the proposed rule and then final rule process. That's just the time frames that are involved.

CHAIRMAN FORD: Just because of going out for public comment and all that?

MR. HISER: Public comments and --

MR. KOO: Public comments. There is a period open for public comments, yes.

MR. HISER: Scheduling meetings with groups like ACRS.

CHAIRMAN FORD: Maybe I shouldn't say this, but it worries me that we are all saying, hey, that's just what it takes within this bureaucratic organization.

MR. HISER: Well, but that's why we have vehicles like Orders, so that if we have to implement something immediately, that can occur.

MR. COLLINS: And the rulemaking plan allows us to take in stakeholder input, as well as the industry input. It gives them time in that timetable and the framework so that we can proceed ahead and gather in all information available.

CHAIRMAN FORD: Okay.

MR. KOO: This completes my presentation.

CHAIRMAN FORD: Thank you very much, Bill.

MR. KOO: Following me, Meena will discuss the BWRVIP issues.

CHAIRMAN FORD: Oh, good.

DR. SIEBER: Do you have to recuse yourself?

CHAIRMAN FORD: My colleagues tell me that I have to recuse myself from this. No?

DR. SIEBER: Recuse, not accuse.

CHAIRMAN FORD: Allen, would you like to introduce the research people, please?

MR. HISER: Yes. As you will see, we have numerous activities in support of NRR on vessel head penetration and head wastage. Dr. Bill Cullen will provide of many of those activities and will go into some depth on a couple of them.

Then we have Dr. Gery Wilkowski will talk about some of the leak rate work that we are doing. I think a little bit on vessel head penetrations, maybe a little bit more on some pipe leakage, I believe. But, Gery will go over that, along with some residual stress calculations.

Then, Dr. Bill Shack will talk about some probabilistic calculations that he has been doing as well. The first will be Dr. Bill Cullen.

DR. CULLEN: Thank you Allen. I'm going to talk about -- is there any indication we should wait a couple more minutes for some folks to -- it's slim pickings here.

CHAIRMAN FORD: Well, no the others will--

DR. CULLEN: All right. I'm going to talk about several items, as Allen indicated. Among the major things that I will be talking about is a summary of the work on corrosion and boric acid solutions that has been essentially completed by our contractors at Argonne National Laboratory.

And I will go over some of the, particularly the wastage, corrosion rate information that we have obtained from that particular program. I'll also talk a little bit about the decontamination NDE and destructive exams of nozzles that have been removed from the discarded north end, the two head.

I also will mention the fact that we will be looking later this year at a couple of the nozzles that have been recovered from the discarded Davis-Besse head as well.

I will talk very briefly about some of the work that we are headed for on the testing of crack growth rates in alloy 690 and 152. And I will just kind of almost in passing mention a few of these other items down here at the bottom.

Okay. The forecast program at Argonne consists of four tests. The first one is to evaluation crack growth rates in materials that have been removed from nozzles out of the discarded Davis-Besse head.

We are not going to report any data from this program today. We have completed a couple of tests on the CRDM material, and on the attachment weld material.

We are involved in some replicate testing on those materials at the present time. This work is a little bit difficult to accomplish. The specimen size of necessity is fairly small.

The weld, in particular, was rather poor quality. Argonne found it difficult to get a specimen out of there that didn't already have cracks in the thing from the lack of fusion, some of the porosity issues that were in that particular weld.

However, I'd like to say that, with respect to the CRDM material, the metalography that we did on it, the yield strength testing that we did on it, or stress testing, suggested that it would be a relatively good quality alloy from the standpoint of crack growth rates.

In other words, the yield strength was nominal, or about in the middle of the range. The metalography looked pretty good, and adequate amount, shall we say, of carbide precipitation on the grain boundaries.

Nonetheless, the preliminary results -- and I really want to stress that these are preliminary, and repeat again that we are doing some replicate testing -- did indicate a fairly high, in terms of the percentile, against the existing database, or within the existing database, a rather high crack growth rate.

I think, just stand by. We will get some more testing done, more data on that, certainly by the end of this calendar year. I heard you mention this morning about a meeting in the Fall.

Perhaps we will have another chance to be a little more specific about these particular results. You will hear later this afternoon about the computational model from the probabilistic assessment of the initiation and time to leakage of a CRDM.

And Dr. Shack will be talking about that right after Gery and I get done. I do want to spend some time talking about the wastage rates in particular, and a little bit on the electric/chemical potential testing that was done for some of the materials that are typical of structural materials in a reactor head.

So, in the corrosion tests that were done at Argonne, these are kind of the goals and some of the details of the testing that we did, we were particularly interested in corrosion of the low alloy steel and of 308 Stainless representative of clad in both flowing and quiescent solutions of varying concentrations over a rather substantial temperature range.

You heard Glen White this morning say that probably the solution in the Davis-Besse cavity was at or near about 100 degrees centigrade. We will show some data corresponding to that.

We also did some testing in more nominal PWR coolant chemistries, both de-aerated and aerated. But it's the de-aerated versions that we are interested in here.

There was a question -- was it from you? -- this morning about the wastage rates. Or was it from Graham? But, wastage rates in pressure vessels where the clad had been exposed.

Participant: I had that question.

DR. CULLEN: Okay. We've got a little bit of data to showy about that. And, lastly, and I will spend a little bit of time on this, Argonne has made some determinations of corrosion rates in what amounts to molten boric acid.

That's not an aqueous solution, that's basically boric acid. Although, it was with or without a little bit of humidity. But we will see some slides on that.

CHAIRMAN FORD: The end result is going to be an empirical relationship of corrosion rate for A533B as a function of temperature --

DR. CULLEN: Right.

CHAIRMAN FORD: -- boric acid.

DR. CULLEN: Solution concentrations.

MR. GAULKER: Solution concentrations and flow rate. Is that right?

DR. CULLEN: Well, I would take out the flow rate a little bit. We did slowly stir. You will see in a minute. Well, by slowly stirring is 50BM, roughly.

That's not intended, let me just pre-input what you might say, not intended to be an erosion sort of measurement.

CHAIRMAN FORD: Right.

DR. CULLEN: It's just intended to be stirred.

CHAIRMAN FORD: Stirred, I understand.

DR. CULLEN: Erosion was not a part of our program.

CHAIRMAN FORD: So, where in the experimental matrix or calculation matrix are you going to come up with a correlation between flow rate temperature and --

DR. CULLEN: Okay, again, take --

CHAIRMAN FORD: -- and leak rate? That sort of thing.

DR. CULLEN: Oh, leak rate. Okay.

CHAIRMAN FORD: Temperature and --

DR. CULLEN: To me you have pulled in three things from fairly wide open spaces.

CHAIRMAN FORD: Because leak rates is what you measure?

DR. CULLEN: Leak rate is what you measure, right. We, in this particular program, our objective was kind of the first thing that you said, to get a matrix of corrosion rates and ECP measurements as a function of solution concentration, temperature, and the materials that we were interested in, alloy steel, stainless steel, clad, alloy 690.

Flow rate was never a part of our particular program. I can't remember whether Glen specifically mentioned this morning that flow rate is a part of the industry program.

But, I do know that it is, okay? It's not my position to speak about the industry program. But I do know that they are fussing with that in some detail.

We did not. The other part of your question, Peter, was on leak rate.

CHAIRMAN FORD: Yes.

DR. CULLEN: And, again, our program, this part of our program, was not intended to correlate or bring into the mix as a variable the leak rate and the corrosion amounts or wastage resulting from such and such a leak rate at such and such a temperature.

You know, we had kind of a more closely defined matrix that was just, as I indicated, solution concentration and temperature for several materials. Okay?

Again, let me -- I'm not being apologetic about that particularly. We defined a program and we went for it. I do know that the industry, in their mark-ups in the 2005 and six time frame, they are going to be looking at experiments which will more directly address your question.

CHAIRMAN FORD: But, the thing that's going to come out of those is corrosion rate is a potential temperature and boric acid concentration.

DR. CULLEN: Correct.

CHAIRMAN FORD: And, my thought is, how does that help me in trying to resolve my problem of wastage in one assembly and nothing in the adjacent assembly.

Both have got liquid in the endings. It doesn't help with that problem, is that right?

DR. CULLEN: I think that's fair to say. And, again, I don't want to be apologetic that we didn't hone in on that particular question or get to that particular answer.

CHAIRMAN FORD: So, how does knowing what that -- is from these experiments, how does that help me in managing my boric acid --

DR. CULLEN: The only thing that I can think of saying there is that we need, you know, some thermo hydraulics people drug into this thing to talk about what happens to the solution under certain leak rate conditions.

If people with other types of expertise can tell us what that solution would arrive at, or would condense to, then we can probably say something about corrosion rates in the various structural materials that may be exposed to it.

MEMBER KRESS: You have a dynamic

situation. So you will have to do this all in the function of time, because the leak rate is probably changing at time, and you got circulation patterns, if you've got wastage, and a different temperature distribution's in that wastage area.

So, it would be a dynamic situation. You're right, there's no use trying to do all of that in one set of experiments. This attacks one part of it.

DR. CULLEN: It does attack one part of it.

MEMBER KRESS: In the other part you might be able to do mostly by analysis.

DR. CULLEN: And, basically we were attacking the equilibrium conditions.

MEMBER KRESS: Yes.

DR. CULLEN: Now, you will see some plots of corrosion or wastage versus time. You could do some derivatives and things. But, again, that was not the objective that we were seeking in this.

MEMBER KRESS: Did you take your boric acid solutions all at the saturation level?

DR. CULLEN: Yes, we did.

MEMBER KRESS: Okay, so you did cover the full range.

MEMBER SHACK: And, just Peter, I mean, you do get the information that you need to know whether a bare metal visual examine every outage will give you a reasonable assurance that you're not going to chew away enough of the head, you know, assuming that all nozzles leaking will chew holes.

You can at least bound the upper rate. You know, maybe you can't yet predict what fraction of it will chew it up, but you assured yourself that you're not going to lose the head, which is what we really --

DR. CULLEN: If you're talking a saturated solution at the optimum temperature, that bound is pretty short.

CHAIRMAN FORD: You will say hey, one inches to two inches per year is a bad situation. But is that a kinetic limitation to that volume? Why can't you have ten inches per year?

I mean, if it does, we wanted two inches per year at one time, I would have said whoa. But now you're saying -- I'm countering, say, why can't it be ten inches per year?

Is there a kinetic limitation, diffusion limited or whatever it might be? That's the real question that we should be asking ourselves. Are we or not?

You're going to go through algorithms and say, whoa, I'm going to have a -- potential temperature such a combination to get to one inch per year -- thing's a wee bit. Could you get ten inches per year?

DR. CULLEN: One part of me is halfway agreeing with you. But the other part of me is saying we need to be realistic about this too. We could, in the laboratory, conjure up some very aggressive experiments that would give us a very high kinetic rate of wastage.

But, is that experiment relevant to something that might happen? I don't know.

CHAIRMAN FORD: Ten inches per year is easily at your machining rate.

DR. CULLEN: That's about right.

CHAIRMAN FORD: Well, it is possible, to get ten inches per year.

DR. CULLEN: Yes, I wouldn't deny that. But, you know, again, are you going to get anything like that sort of like that sort of conditions, those sorts of conditions, over long periods of time?

MEMBER WALLIS: Well, you're going to answer that question, aren't you?

DR. CULLEN: I'm not going to try to quite answer that question. But, we'll get some data that talks about some of the kinetics of this, yes.

MEMBER WALLIS: Doesn't oxygen matter here?

DR. CULLEN: It certainly does.

MEMBER WALLIS: Where does that come from?

DR. CULLEN: About three or four slides down the road.

MEMBER WALLIS: Oh, okay.

DR. CULLEN: Let's talk a little bit, just a schematic of how many of these experiments were conducted. You will see in another what this stack of specimens looks like.

But, we had various materials supported on a rotating rod, driven by an electric motor at the top. This mixture down here could be either molten boric acid in some experiments, or an aqueous solution at various concentrations in other types of experiments.

In the experiment where we had molten boric acid down in here, this funnel was used to drip water into this boric acid solution so that we would very slightly hydrate the boric acid.

And you will see in a minute or two that some of the most interesting results that we got in this test program are from this particular experiment. So, again, just to repeat, so we are on the same page here, we did experiments in both aqueous solutions of boric acid at various concentrations, ranging from PWR coolant typical to fully saturated at specific temperatures.

And we also did kind of a second set of experiments, if you will, in straight boric acid with and without slight water additions.

CHAIRMAN FORD: You were -- it so that it's not diffusion limited? It's kinetics limited in some way? Are you --

DR. CULLEN: I would say maybe kind of half and half on that particular answer. What we wanted to do was make sure that the solution was stirred so that we didn't get --

CHAIRMAN FORD: The amount of stirring makes the difference to the rate of reaction?

DR. CULLEN: I would tend to say yes. I'm not sure I remember an experiment. We didn't do anything dead stated. I didn't think we did the dead static.

We didn't just stick it in there and walk away. Every time we did this experiment we were rotating at least a little bit slowly.

MEMBER WALLIS: But, if you're going to develop a theory, you have to put in the diffusion and the turbo mixing and all that stuff, don't you. You have to get it under control?

MEMBER KRESS: Unless you are fairly sure those not controlled.

MEMBER WALLIS: Unless you're sure there's --

MEMBER KRESS: I would suspect that's not the control.

MEMBER WALLIS: Well, I don't know.

DR. CULLEN: In a way we are trying to, by adding the variable of rotation, we are trying to simplify the explanation of the experiment so that we kind of took out this business of diffusion and boundary layers, and stagnation, and all that.

We tried to get an experiment where we get down to a set of variables --

VICE CHAIRMAN SIEBER: 50 RPM is pretty fast.

DR. CULLEN: Well, once a second or a little less. It's not exactly surf type stuff.

MEMBER KRESS: My guess is the chemical kinetics is controlling this.

MEMBER WALLIS: A guess is no good. I want an answer.

MEMBER KRESS: My guess is based on years of research --

DR. CULLEN: Here's a look at the tests that our guy put together for this program, many of which were A533B. We put some stainless steel welds in there.

From time to time we put some alloy 600 in there. You'll see some of the experimental data a specific time intervals, like after 24 hours, after 100 hours, after such and such and such and such.

Specimens were put in, taken out, replaced as this experiment went on. So, as an example, in this stack of specimens, this stack of specimens may have been immersed for like 24 hours.

At that point, some were removed and replace with brand new specimens, and the corrosion was measured on the specimen taken out at 24 hours. The other specimens continued on.

So, you kind of had a mixture. We'll see an example of how that works later on. But, a typical sample size is a half an inch in diameter, about a half inch long.

So, not particularly big, but enough to

get accurate weight loss measurements on these things. This is a picture of a typical stack of specimens. I say typical, it probably was typical.

The alloy 600 and stainless steels are down here at the end. Still, after a maximum of 411, perhaps 311 hours -- 300 or 400 hours -- in a saturated boric acid solution at essentially the boiling point.

These specimens still look pretty good down here, nice and clean and shiny. These are A533B specimens here in the middle. Some have 311 hours. Others have 411 hours.

But you can see there's been substantial material loss from these specimens. This is the aluminum support, the stirring rod up at this end. Everything was separated so there was no galvanic difference among these various materials.

So, they are all electrically isolated. And we were able to measure, essentially, a pure corrosion rates.

MEMBER KRESS: Can you operate this thing under pressure at fairly high temperatures?

DR. CULLEN: This particular experimental setup, no. Other tests were done at temperature and pressure. And we will see a little bit of that data going on.

But, this stack, this technique was used at basically --

CHAIRMAN FORD: You've got some beautiful sketch. Obviously the alloy steel corroding. But you've got some steps though.

DR. CULLEN: We do. Well, this is two different materials. I'm not really sure what that business is right there.

CHAIRMAN FORD: Yes.

DR. CULLEN: I would tend to think it's, you know, I didn't ask specifically, whether it's two specimens that have come together, or whether there's an O-ring there at one point.

Yes, I don't know. I just don't know that. But I can clearly see that there's a break right there. Since there's an O-ring here and an O-ring here, we know that that's a unique specimen in between.

MEMBER SHACK: If he did 8533, he may not have separated the m with O-rings. If he had no coupling, then he could just put the rings together. And my guess is that's what you're looking at.

CHAIRMAN FORD: That's true.

PARTICIPANT: What is the 8533? Is that

the 600 alloy material?

DR. CULLEN: That's the low alloy steel for a typical pressure vessel head of that area. Pressure vessel head nowadays are being made more of 508 than they are of A533B.

But, that was the plate material that 35 years ago was generally formed and perhaps one or two or up to six welds in a given head, depending on the fabricator and how it was all put together. A533B is the plate material from this era.

CHAIRMAN FORD: So, this is an eight of an inch before, or something like that? You are getting a --

DR. CULLEN: No, I think the batteries are just losing contact every now and then. And I've got spare batteries as well.

VICE CHAIRMAN SIEBER: Give him a hammer.

DR. CULLEN: Is that a little distracting.

CHAIRMAN FORD: This is about a quarter of an inch a month, you said?

DR. CULLEN: I've got another laser here. I've got two lasers here. I can fire away with two hands. Okay. So this was a typical stack of specimens. Now, here's a little bit of data that we got out this thing.

I'm going to point out again that the colors or the cross hatching tell you how long these materials have been exposed to the environment. Now, one of the main points of all this, is to demonstrate that corrosion rates, at the very beginning of exposure, are a bit higher than they are later on.

Note that this is a log, log, flop on the axis here. So, be aware of that. During the first 24 hours, the corrosion rate here would be a little less than 90 millimeters per year, going down to about 75 or so if you look over the first 100 hours.

And if you look under just the last -- help me out here -- 76 hours, the corrosion rate is down to about 50 millimeters per year. So, the corrosion rate drops off by not quite a factor of two, as you go on in time.

So, for a very fresh surface of materials that are exposed to these environments for the very first time, right away, corrosion rates are a bit higher over the short-term than they are if you integrate over the longer term. That's fairly important, so this.

VICE CHAIRMAN SIEBER: Why does that happen?

DR. CULLEN: We didn't look

mechanistically at all of this. But, you know, my kind of assumption is that these things build up a film or a layer of the corrode -- of the oxide that is formed.

And that kind of prevents or at least slows down access of the solution to the virgin metal underneath.

VICE CHAIRMAN SIEBER: I would think that would occur for a period of time and then remain relatively constant after that.

DR. CULLEN: At this rate, yes. Well, what's a period of time here? You know, is the first 100 hours the --

VICE CHAIRMAN SIEBER: Right, it may not.

DR. CULLEN: Will it stabilize after the first 100 hours, the first 1,000 hours, whatever? But, it turns out, as far as we can tell, that these rates out here -- I'll show you a little bit more.

There's another slide coming up that kind of plots these versus time. And you can see that by 100 or 200 or 300 hours we have pretty well flattened out.

MEMBER WALLIS: Where do the products of corrosion go?

DR. CULLEN: Well, that's an interesting

question. We did a couple of experiments. And you will see a data-point coming up here. But, for the most part, the solutions were not renewed.

So the corrosion products just went into the beaker, went into the container. Since these are still relatively short tests, lasing at most a few hundred hours, the solutions build up in these corrosion products.

MEMBER WALLIS: That doesn't affect the --

DR. CULLEN: Oh, I guess it does. And that's one of the data points that we will see. It defiantly does. The build-up of corrosion products in the solution does slow things down.

MEMBER WALLIS: Right.

DR. CULLEN: So, you know, was that what was happening in, for instance, Davis Besse? Well, maybe so, maybe not. I mean, we probably feel -- I feel like that cavity was kind of getting flushed out with some regularity.

Certainly an awful lot of water was coming in. And there's certainly evidence, from looking at the head, that it was flowing out and over, shall we say.

You know, but clearly we would have to have corrosion products in that pool at the top of the

Davis-Besse head. Okay. So that's what this data is. The low alloy steel is over here on the left-hand side.

Please not that the stainless steel weld or cladding type materials over here had very low corrosion rates of the order of a millimeter, one point something millimeters per year corrosion rates in the stainless steel cladding.

So that helps us to understand why the cladding at Davis-Besse did not seem to be degraded, in terms of wastage. Now, we do know about the existence of some cracks that were in that clad.

But, as far as the wastage goes, if you look at some of the slides, the micrographs that were generated by BWXT as a part of their work on that, you can see some evidence of pitting here and there in that clad.

But, all in all the quality of the clad looks pretty good.

VICE CHAIRMAN SIEBER: All the clad cracks could have been there from the beginning, or they could have been caused by plastic deformation.

DR. CULLEN: We're all hypothesizing about that. But, the morphology of the cracks does really smack the intergranular stress corrosion cracking.

VICE CHAIRMAN SIEBER: Yes, okay.

DR. CULLEN: And, you know, my thinking, remember that was clad, it was intimately bonded to the low alloy steel back in 1970 something or another. It's hard for me to imagine that a network of cracks as extensive as what we ultimately found in the exposed clad could have been there from day zero. That's another day, another topic.

MEMBER KRESS: When I see corrosion rates in low liters per year, I immediately bring to mind a similar flat surface.

DR. CULLEN: Yourself.

MEMBER KRESS: You have circular, cylindrical tubes.

DR. CULLEN: Yes.

MEMBER KRESS: Is there correction needed for the change in surface?

DR. CULLEN: These corrosion rates are calculated from weight loss. And there was weigh -- in that type of test.

MEMBER KRESS: But, is there correction needed to the surface area change? Or is this too small a change its surface area to make much difference.

DR. CULLEN: Well, it's a loss of about

half of the volume. So it's not too small. It's a question I don't --

MEMBER KRESS: I don't have a problem with millimeters per year. But --

DR. CULLEN: I understand what you're saying. Certainly, for the zero to 24 hour range, we're probably good to go.

MEMBER KRESS: Yes.

DR. CULLEN: Just a linear, radial corrosion rate. As to the question, I'll have to get an answer for you. I'm not sure exactly how the contractor did that. Good question.

PARTICIPANT: Dr. Kress, are you getting to maybe they should have used the cylindrical view of this?

MEMBER KRESS: Of course these were cylindrical specimens, yes.

PARTICIPANT: Rather than the linear-type prints?

MEMBER KRESS: Yes.

MEMBER WALLIS: But the area changes, your point is --

DR. CULLEN: Correct. Yes, the surface area is constantly decreasing as this test proceeds, and particularly, you know, the decrease is spastic at the beginning, not at the end.

MEMBER WALLIS: Right.

PARTICIPANT: You're dealing with a concentration profile, just like you would in a heat transfer problem or anything else.

MEMBER WALLIS: Are they calculating mass and dividing by the original area?

DR. CULLEN: That's my understanding, that's correct.

MEMBER WALLIS: So it's not really a true millimeters per year, is it?

DR. CULLEN: No. Not give the question that was asked earlier here. It's a good point. We'll get this straightened out. The report is under review. I just received the report last week.

And that clearly becomes one of the questions that I should ask. Thank you. Okay. This slide will begin to answer several of the little issues that we have chatted about here in the last couple, three minutes.

For one thing, this is the corrosion rate kind of as a function of exposure time, or corrosion rate versus time, if you like. And you can see that, as you get out in around 300 hours or so, this has fairly well flattened out.

So, that kind of answers on question that ultimately you kind of get to an equilibrium, or stable situation where the corrosion rate, the oxide formation, and all have kind of balanced each other out.

As I mentioned earlier on, the corrosion rates towards the beginning are the fastest. At the end of 24 they are at a maximum for saturated and aerated solutions.

They are close to about 100 millimeters per year. In the saturated but de-aerated solution, for some reason --

MEMBER WALLIS: Four inches a year?

DR. CULLEN: That is correct.

MEMBER WALLIS: Thank you.

DR. CULLEN: That is correct. At the very beginning, for a relatively short period of time. And I think that's a kind of an important caveat on that to it.

Those sorts of rates don't go on forever. You know, it's kind of flies in the face of intuition about this one data point there. And I don't understand why it's up there.

You know, it's far more satisfying to me that the excursion rates in the de-aerated solution came out at the end of the day to be less than of what they were in the aerated. Is that where you were headed, Peter?

CHAIRMAN FORD: No.

DR. CULLEN: Okay.

CHAIRMAN FORD: I was going to say, is diffusion controlled four inches per year, I thought.

DR. CULLEN: You would certainly think so, yes. Okay. Somebody else asked whether we were doing tests as a function of solution concentration, and we did half-saturated solutions and found these corrosion rates down here.

It turns out, in a plot that I don't have, the corrosion rate is essentially linearly dependent on solution concentration for a given, you know, like aerated solutions at a specific test temperature.

So, corrosion rates is pretty much a linear function of boric acid concentration here.

CHAIRMAN FORD: Tell us why you didn't turn the cylinder.

DR. CULLEN: Did not do that test, as near as we can determine.

CHAIRMAN FORD: Just to see if it made a difference?

DR. CULLEN: No. Again, trying to

eliminate diffusion is --

CHAIRMAN FORD: No but --

DR. CULLEN: I hear you.

CHAIRMAN FORD: -- when you compare, then you've got some sort of measure there. But if you just do one test, there's no comparison whether or not diffusion makes a difference.

DR. CULLEN: Well, we did lots of tests on a different axis of variables. We just didn't do any tests at that --

CHAIRMAN FORD: All at the same speed, though.

DR. CULLEN: Yes, that's true. We didn't do multiple tests with stirring rates or --

MEMBER ROSEN: And if Dana Powers was here he would say the tests aren't worth anything unless you did them twice at least, each one.

DR. CULLEN: Unless we did them twice, at least?

MEMBER ROSEN: Yes, you'd have to be able to replicate.

DR. CULLEN: Yes. I'm not sure about the degree of replication within this program. There was some of it. I'm just not sure exactly where it was.

CHAIRMAN FORD: And this is all magically

97.5?

DR. CULLEN: Just sub-boiling. Yes, that is correct.

CHAIRMAN FORD: This is boiling of the solution, or boiling of water?

DR. CULLEN: Well, boiling of water, obviously, at 100 degrees centigrade.

MEMBER WALLIS: Water boils at a lower temperature.

DR. CULLEN: Yes. In Chicago you have to account for the altitude difference.

CHAIRMAN FORD: And this is simply because that's the boiling point of the water.

MEMBER WALLIS: And the impurity in the water.

DR. CULLEN: That's probably more important.

MEMBER WALLIS: What was the likely temperature on the head?

DR. CULLEN: Well, again, from our industry partners, they have computed, and I kind of believe it, that it was very close to 100 degrees centigrade.

MEMBER WALLIS: Oh, okay.

DR. CULLEN: So, this test temperature

should be very close.

MEMBER WALLIS: Because there was so much excess water?

DR. CULLEN: That is correct.

PARTICIPANT: Well, I mean, it turns out that you don't get the high corrosion rate until you get --

DR. CULLEN: That's also true, yes.

MEMBER WALLIS: But he hasn't tested that yet.

CHAIRMAN FORD: Well, that's what I'm looking for, is corrosion rates as a function of temperature.

PARTICIPANT: It's certainly not erroneous in the equation.

DR. CULLEN: I didn't bring along that specific type of plot. But, we do have some corrosion rates at higher temperatures. We just didn't plot them all together like you've suggested right here.

Okay. This is -- I mentioned this earlier, that corrosion rates, as a function of concentration, show a virtually linear dependence for a given set of conditions in this case, again 100 degrees centigrade.

And a question was raised this morning

about the corrosion rate in, quote, normal PWR coolant environment. I didn't have a chance over the noon hour to look this up.

But this corrosion rate -- my recollection is 0.7 mils per year, or something of the order of 30 microns per year, if that's -- you're thinking -- in normal PWR coolant.

So, part of the answer to your question of what would happen if you just had an exposed portion of clad, how rapidly would the low alloy steel corrode?

Well, this is a part of your answer. It turns out that the industry provides to the NRC occasionally a calculation along this line. And the calculation actually consists of a kind of an amalgam, if you will, of three different corrosion rates characterizing this sort of corrosion rate for normal PWR operation, at power.

But you have to allow for the fact that during a small fraction of the year, presumably, the plant would be an outage status. Perhaps the lid would be off, the head would be off, and the water would be aerated.

And there's a much higher corrosion rate that is associated with that. More of the order of twenty --

VICE CHAIRMAN SIEBER: And the boron is way up.

DR. CULLEN: And the boron is way up too. Then there's a third corrosion rate that's kind of what's called a moderate corrosion rate of about seven or eight mils a year.

It is also used to refute tens of hours in this equation that is intended to be representative of the total corrosion rate that might be expected on the finding of a holiday in the clad.

We get this sort of thing when a licensee finds a holiday in the clad and, you know, the NRC or the regulation side typically says well what's going to be the corrosion rate of that exposed low alloy steel.

And the plant would then go to their books, find out what their typical uptime is, their typical outage time, put all that into the formula, and they'll report back well, we expect a corrosion rate of 1.8 mils per year for the next 20 years or something.

VICE CHAIRMAN SIEBER: That's a good number too.

DR. CULLEN: And, you know.

PARTICIPANT: Does this take into consideration an aging analysis under re-licensing?

DR. CULLEN: Probably not for me to say, but I would tend to think it could be.

PARTICIPANT: Dr. Shack is shaking his head yes, I think. Is that true, that under the aging analysis that --

MEMBER SHACK: Well, it's very -- you know, most plants don't operate with gaps in their watts bar -- has got the biggest ones I think.

DR. CULLEN: Well, we've got one just as of a couple three weeks ago.

PARTICIPANT: Are they detectible by some of these ultrasound methods?

VICE CHAIRMAN SIEBER: You visually see it.

PARTICIPANT: But these are inside the vessel, aren't they?

DR. CULLEN: As an example, we had --

VICE CHAIRMAN SIEBER: You empty the vessel every once in a while and look down there.

DR. CULLEN: We had a --

PARTICIPANT: How about microscopic?

VICE CHAIRMAN SIEBER: No these are -- well, the ones I'm familiar with are one to two inches in diameter. They're big.

PARTICIPANT: They're just -- the weld is separated more or less?

VICE CHAIRMAN SIEBER: The clad is missing.

PARTICIPANT: It's missing?

DR. CULLEN: We had a plant come in about a month ago. And they had done a very nice, very thorough bottom-mounted instrumentation inspection from the inside.

VICE CHAIRMAN SIEBER: Yes.

DR. CULLEN: You know, slipped their probes over the BMIs that were there. And, in the process of doing this BMI inspection on their 60 or 70 some odd tubes, they happened to notice a holiday right on the lower head.

It was about one and a half inches long by five eighths inch wide. So, you know, a couple of postage stamps put together. And they brought that to our attention.

They did exactly the kind of disposition with that little formula that I just mentioned, and came up with we expect to have 1.8 mils per year material loss from here on out.

VICE CHAIRMAN SIEBER: The original

dispositions of these, particularly they were found pre-service, would be for the 40 year license life. And, you know, the rates are so small compared to the thickness of the vessel, that going from 40 to 60 years doesn't add hardly any. What, 20 mils?

DR. CULLEN: Seven or eight mils a year of 60 years is --

PARTICIPANT: Still, a fair amount of materials goes somewhere.

DR. CULLEN: It goes somewhere. But, again, these holidays are typically quite small.

MEMBER ROSEN: What worries me about that is that's been non-conductive solutions. If somehow that vessel of water becomes conductive, now you've got this huge cathode all of stainless steel, and a tiny little anode.

And it could just start to bore a hole in the vessel.

DR. CULLEN: Well, there may be cathode anode, but the galvanic difference between those two, even in a reasonably -- boric acid, I man, PWR coolant is not non-conductive.

I mean, it's mildly conductive, I would say. And, even if you were to contaminate it, you know, for some reasonable amount of time, you know --

what's reasonable?

Certainly the licensee is going to catch this after a few hours, I would hope.

VICE CHAIRMAN SIEBER: You would hope.

DR. CULLEN: Yes. And then they are going to start some sort of a clean-up operation.

MEMBER ROSEN: So, you're saying that it won't last long if it happens. We know it happens.

DR. CULLEN: Yes, right.

MEMBER ROSEN: But we --

DR. CULLEN: We've had intrusions.

MEMBER ROSEN: It won't last long if it happens, and the electrochemical difference between stainless steel and grade A533 --

DR. CULLEN: Whatever.

MEMBER ROSEN: Is not so large.

DR. CULLEN: Is not large. There is some. It's just not large. And, again, that's very dependant on exactly the solution that you're talking about.

MEMBER ROSEN: Think about the area now. The area difference is enormous.

DR. CULLEN: Is enormous, there's no doubt about that. But distance is also important. I mean, the clad that's in the upper head is probably not going to have a whole lot of galvanic affect on a little holiday that's at the bottom.

MEMBER ROSEN: No.

DR. CULLEN: So, you may have acres and acres of clad, but it's only that clad right near by that's really going to be the cathode that you're talking about.

VICE CHAIRMAN SIEBER: The ions have to have to go through the solution.

DR. CULLEN: That's right, which is moderately conductive. You don't want to say zero, but it's darn small. Okay. I think this is about the last slide on conventional coolant.

And, again, this is just a -- this speaks to Dr. Ransom's question, I think, from a little while ago about -- no, I'm sorry, it was Dr. Rosen's question about whether solutions were -- I can't remember.

One of you guys over here asked whether solutions were cleaned, removed -- whether the oxide products were removed from the test solution or not. Here's the yes and no sorts of answers to that.

Here we have specimens that were rinsed, specimens that were not rinsed. I should back up a little bit. This is not exactly the answer to your question.

That was on, I think two slides ago. There we go. I started to answer this question right here about this data point down here. This is the corrosion rate in a half-saturated solution.

So, compare triangle here to triangle here. But this is solution that had been used in a previous test.

MEMBER WALLIS: So, it's been in for 300 hours or --

DR. CULLEN: Or some amount of time. I'm not sure what amount of time. But, at any rate, this solution was crapped up, to put it gently, with corrosion products, and was re-used in a brand new corrosion test with that corrosion rate resulting roughly about half of the corrosion rate that you would realize in a brand new, perfectly clean solution.

MEMBER WALLIS: It's interesting, though, that corrosion rate seems to be consistent with a long term, you know, which would say, okay it does have some affect on the declining rate.

DR. CULLEN: That's a good point. Agreed. I'm not sure that that's the answer to that. But, you know, you've noticed the coincidence, if you will, between the corrosion rates here and this corrosion rate here.

Okay. This is what happens in specimens that are cleaned versus not cleaned. This does not have anything to do with the flow rate question that we've talking about.

But, this is simply what happens in you remove the loose corrosion products from the outside of one of these weight loss specimens, as compared to not removing those products.

So, when you do remove those products, you get a higher corrosion rate, because we are talking about loss of volume as a function of time. So, if you remove those products, you get a relatively higher corrosion rate.

But with the rinsing, PWR environment without rinsing, you get a bit lower corrosion rate. The same tests were done, not in a boric acid solution, PWR simulated coolant, but in ultra-high purity water, without rinsing and with rinsing.

So, without rinsing you actually had a weight gain, which appears to be a negative corrosion rate. But, after you rinsed it, removed a small amount of corrosion products that did form.

You've got a little bit of a weight loss, which shows up as a positive corrosion rate.

VICE CHAIRMAN SIEBER: I've got to write that down, weight gain?

DR. CULLEN: Weight gain.

PARTICIPANT: What is the UHP?

DR. CULLEN: Ultra high purity water. So, no --

DR. CULLEN: No boric acid. Think very low conductivity, very high reactivity water. But it was oxygenated. Is this oxygenated or aerated? I think it's aerated.

VICE CHAIRMAN SIEBER: Aerated.

DR. CULLEN: Aerated water, yes. Oxygenated is a bit of a misnomer here. This was aerated water here.

MEMBER ROSEN: The oxygen in with nitrogen?

DR. CULLEN: My guess is that they probably used -- either the water was just simply exposed to the air without a lid on the container, or --

MEMBER ROSEN: Bubbled through it.

DR. CULLEN: Or bubbled through it. One or the other.

MEMBER ROSEN: That's what I said, you

spurged it in with the nitrogen.

DR. CULLEN: Well, I'm not exactly sure what they did with it.

MEMBER ROSEN: That's called aeration.

DR. CULLEN: Okay. Let's talk a little bit. I've got four slides in a row here about testing that was done in molten boric acid solution, both with and without water additions.

Here we have a slide talking about the weight loss in these particular specimens. This shows the total weight. This is not zero down here, this is a three gram.

So, these specimens started out as something a little bit over six grams in weight. The as-received is this first column here. We are talking about A533B now, in a series of tests without any water additions, except the purple or magenta bar that's down here at the end.

So, let's go through this first, these first four. This is A533B, molten boric acid solution after various times, as received, after 24 hours, at 300 degrees centigrade.

That's the second bar, virtually no change. 26 hours at 260 degrees you see little bit lower temperature, still no change in weight. After

24 hours at an even lower temperature, 150 degrees C.

So, the point is that in molten boric acid solution, regardless of the temperature, there's virtually no weight change, no corrosion rate. However, if you add -- I'm going to use the word small amounts.

We'll talk a little bit more about what I mean by that. Small amounts of water dripped into this boric acid solution, the amount of weight loss is virtually a factor of two, or half of the weight disappeared from roughly about six down to a little bit over three.

So, in 45 hours, half of the sample disappeared in molten boric acid with just small amount of water added. The second replicate experiment, same sort of thing.

But alloy 600, no matter what the situation, wetted or not wetted, regardless of temperature, no change in weight. So, the alloy 600 did not corrode in this particular molten boric acid solution.

But the A533B corroded very seriously. But only if you had small amounts of water added. This is what happens when you melt boric acid. Simple experiment. Think of just put boric acid into a beaker, put it on a hotplate, turn it up, measure the temperature.

This is not empirical or laboratory data. This is all calculated from formulas that are available in literature. But, as the temperature increases, you start out with boric acid, BOH_3, or H_3BO, however you want to say that, down here at the bottom, at about 169, 170 degrees centigrade.

So, right up in around here, this boric acid, H_3BO_3, loses some water, and becomes HBO_2. If you continue to raise the temperature, at about 300 degrees centigrade, the HBO_2 loses another water and becomes B_2O_3, which is a solid.

So the water keeps going off as a gas or a vapor. And that's the way boric acid changes chemical forms as the temperature is increased.

PARTICIPANT: Which is the popcorn?

DR. CULLEN: Which is the popcorn? That's the stuff down in here, down in the lower range. Okay? I think. I'm going to put I think on the end of that.

What's been noticed on reactor heads is quote unquote popcorn. But, how long has that been there. I'm kind of looking over to the industry guys here now.

VICE CHAIRMAN SIEBER: It's dry.

DR. CULLEN: It's dry. It's definitely dry. I'm kind of going to -- this is my story, I'm sticking to it. I think it's probably the H_3BO_3 form, you know, when we see it as quote unquote popcorn.

MEMBER WALLIS: If it were HBO_2 would mean that it got hotter than you've been saying, perhaps.

DR. CULLEN: Well, remember, this is 170 degrees centigrade. You know, typical head temperature is 300 and changes. So --

MEMBER WALLIS: Well, that's on the inside.

DR. CULLEN: That's on the inside. We learned this morning that's coolant temperature. So, what you have up on the head, a lot of ventilation under that --

MEMBER WALLIS: The first popcorn might well be that temperature. But, later on, when it gets wetter, you see a change.

DR. CULLEN: I'm going to mention this, exactly, in a couple of slides. I'm going to stick my neck out and speculate, as if I haven't been doing that enough already.

PARTICIPANT: Is that BOH_3, isn't that a hydroxide?

DR. CULLEN: Well, written this way I guess it sort of -- I guess it looks like hydroxide. But it's -- think H_3BO_3. That's boric acid.

PARTICIPANT: Acid, right.

DR. CULLEN: Written this way it looks a little strange.

MEMBER ROSEN: No, that's a solid when you put it on that hotplate in your experiment?

DR. CULLEN: That is correct. It's Epsom salt. You go to the drugstore and buy Epsom salt.

MEMBER ROSEN: Or Epsom crystals?

DR. CULLEN: You got it. That's the experiment. Here's what actually happens when you dot that experiment, when you do a weight loss experiment. Again, just as you said, put boric acid in a beaker, start turning up the heat, and watch what happens.

This test was done by removing the beaker at selected intervals and weighing what was left after specific periods of time when the oxygen went off -- I'm sorry, the water vapor went off.

So, what they did is put it on a hotplate, keep measuring the temperature, and taking note of when the significant weight changes occurred. So, you start out with 100 grams of this stuff, which is boric acid -- the stuff you buy at a drug store if you like

-- and heat this up to 280 degrees centigrade in normal room air.

After just a couple of hours, the weight has dropped down to 60 something grams. And, it's at this particular time that all this stuff became the HBO_2 phase, and continued to hold it under this temperature for -- continued about a time of about five hours or so.

The weight has dropped down to 56 or seven or eight grams. And, it's at that point that you have now changed entirely to the B_2O_3 phase. But it's important to note that this test was done at that temperature.

You would not necessarily get all these phases at a lower temperature, for instance.

VICE CHAIRMAN SIEBER: Well, all you're doing is just driving off the water.

DR. CULLEN: Just driving off the water. That's all this experiment is.

MEMBER ROSEN: It turns to a glass-like transparent boric oxide?

DR. CULLEN: That is exactly correct.

MEMBER ROSEN: And then, if you just let it cool of naturally, what happens to that boric oxide?

DR. CULLEN: If you just leave it in the beaker, it's going to stay that way.

MEMBER ROSEN: As boric oxide?

DR. CULLEN: As boric oxide, yes. But if you pour water on top of it --

MEMBER ROSEN: No, I'm not talking about pouring water on it. I'm just saying, let it cool in normal humid air, say like this room.

DR. CULLEN: It just is going to stay there.

MEMBER ROSEN: It stays as boric oxide. It doesn't go back to boric acid?

DR. CULLEN: Now I think, I'm not sure about this, but over a long period of time, given the humidity that's in room air, some of it is going to start to revert.

MEMBER ROSEN: It will hydrate, yes. On the surface.

DR. CULLEN: Yes. It' hydroscopic to some extent.

MEMBER ROSEN: So, this happened on the head of Davis-Besse's vessel.

DR. CULLEN: Here we go, the point I was going to try and get to.

MEMBER ROSEN: You get boric oxide, which then, during refuelings they take the head off and put it someplace in a humid environment. The boric oxide maybe hydrates a little.

DR. CULLEN: Careful now. Now you are -- but you are at nominally at room temperature. Kinetics are pretty slow.

MEMBER ROSEN: It stays boric oxide, transparent glass solid?

DR. CULLEN: Stays that way, maybe --

MEMBER ROSEN: So if anybody happens to get a probe in there to look at it, remember it's hard to see, sees something transparent, i.e. he doesn't see anything.

DR. CULLEN: Oh, I wouldn't say that. I think my experience from a couple of decades of laboratory work with PWR environments is that this stuff kind of retains sort of a brown milky color to it.

Transparent is, by my experience, a bit of a stretch.

MEMBER ROSEN: It's on your slide. I'm just reading it.

DR. CULLEN: Well, this is a controlled experiment, and I had glass beakers -- well probably not glass.

MEMBER SHACK: Low impurities, you know, no corrosion products, no, you know, this is just a clean experiment.

VICE CHAIRMAN SIEBER: Compared to a PWR.

DR. CULLEN: Just anecdotally, I ran tests in autoclaves in simulated PWR coolant. And, inevitably, you've got leaks. And we would occasionally notice on the head of an autoclave, that we would have this glassy smooth but slightly colored, in terms of brownish streaks in it or slightly milky appearance, to the boric acid that would melt up there.

And that may be the addition of corrosion products or something that came from the lid of the autoclave. In a controlled experiment like this, where you have clean environment, yes, it's going to be clear.

In the real world, probably not. I think I know where you might have been headed, is, you know, could a visually inspection just totally miss this because he'd be like looking through a plate glass window.

I don't think so. Okay. One more slide on this. And this shows the corrosion rates that would be expected from this particular sort of a combination.

Two temperatures -- now, these temperatures were achieved by an experiment that is kind of a combination of the beaker full of boric acid that you saw earlier, and about ten slides back where I showed that funnel and the rotating mixture.

This experiment was actually done with that funnel and the rotating stack of specimens, okay? But it was done with pure boric acid in the container. And water was slowly dripped in the funnel at two rates, one rate a little higher than the other.

The higher drip rate resulted in the lower temperature, 150 degrees centigrade. But these are two not calculated drip rates. They just started a drip rate.

This is the temperature that the test went to. They ran the test, measured the corrosion rates. So, with a specific drip rate, they gave you 150 degrees centigrade, and the temperature in this molten boric acid solution that has not been slightly hydrated.

You had some very high corrosion rates, 120, 130 millimeters per year. And, for one specimen, something approaching 150 mil, six inches per year, getting close to it, five and a half or so.

MEMBER WALLIS: All the way through the head in one year.

DR. CULLEN: For pure molten boric acid, slightly hydrated that would be the.

MEMBER WALLIS: So we're going to inspect every five years or something?

VICE CHAIRMAN SIEBER: Well, you don't have molten boric.

MEMBER ROSEN: Well, we don't know quite what we're going to get. Do we?

DR. CULLEN: It's a point worth thinking about. Again, this is an experiment. You've got to make the connection that there's a possibility of getting this molten boric acid slightly hydrated in a local situation, and contained there 8,760 hours a year for years, in order to get this sort of a situation set up.

If you can figure out the scenario through which that would happen, then I would say it's time to start worrying about these sorts of corrosion rates. But, first thing, I can't get there from here.

MR. HISER: Is the temperature another problem? In order to get the temperature you need water to cool.

DR. CULLEN: You've got to drip -- But Al, we've got steam coming out from these same leaks. You could to that. Conceivably you could do that. I mean, there are scenarios.

There are drip rates out of -- the reason I put these slides up here from -- this was an incident that was presented to us by Sequoyah last year. They came down, they were chasing another issue after eight months of running.

They had done a head inspection, bare metal visual of the head. No problems. They came up, they ran for eight months. They had to come down for another issue.

They noticed after that eight months, in just an inspection, that this reactor vessel level indicator valve had not been properly connected during that preceding outage.

And it had blown boric acid solution down onto the insulation. And it was noted that some boric acid products had snuck down through a gap in the insulation, landed on the head, and caused a little area of corrosion.

Now, most of what you see here is staining from the products that accrued there. But, although you can't see it, this kind of gray area, I'm trying to run around the periphery of it here.

That is, if I remember the numbers right, it's a grove in the head that was about five inches long, about five eighths wide, and a quarter deep. Those numbers are from memory.

But that captures the essence of it, a small grove. They used the term thumb-sized in the report to us, okay? So, after eight months, they found a thumb sized grove in a head that had been previously okay.

They didn't speculate on how this happened mechanistically. At the time the incident was presented to us, none of us could really understand it either.

But I'm going to speculate now -- believe me, this is just speculation, that maybe these Argonne experiments might be showing us how this degree of corrosion could have occurred in a matter of eight months.

We do know that this valve leaked continuously. And it was just basically a gentle spray of water that was down on this particular are of the insulation.

So, I kind of ask a rhetorical question without giving you an answer. Could that have wetted the boric acid just enough so that boric acid that had snuck down in between through the gap in the insulation was just wetted somewhat similar to what we have in this Argonne experiment, and caused this corrosion.

I don't know. But, to me, it now becomes at least plausible that we could think about that kind of model.

VICE CHAIRMAN SIEBER: That presupposes that most of the water leaves at the point of the leak and boron goes down on the head with just a small fraction of that water.

DR. CULLEN: And then the boron melts.

VICE CHAIRMAN SIEBER: Right.

DR. CULLEN: And is maybe wetted by, you know --

VICE CHAIRMAN SIEBER: Is it hot enough for it melt?

DR. CULLEN: Well, you know, it's head temperature underneath the insulation. So, we've heard this morning that the exterior of the head is probably not something.

But, you know, it's fairly well insulated. Sequoyah had insulation that was rather firmly attached to the lid, very small gap. I would tend to think that the exterior of the head was pretty hot temperature.

But, you know, boric acid on it, maybe gently wetted by this spray that was emanating from this leaky valve all the time. It's speculation, but, you know, it's sort of old water.

VICE CHAIRMAN SIEBER: Okay -- water so to speak.

DR. CULLEN: So to speak. Okay.

CHAIRMAN FORD: Excuse me.

DR. CULLEN: Yes.

CHAIRMAN FORD: I wondered if you could possibly get through your whole presentation in the next quarter of an hour?

DR. CULLEN: Yes, I think so. Because we are almost done with this boric acid stuff.

CHAIRMAN FORD: Okay.

DR. CULLEN: And I can kind of skip pretty rapidly through the North Anna stuff. Okay. All right. This is just, again, to answer another question.

Did we have corrosion rates as a function of temperature for certain situations? Yes, we do. This is several boric acid solutions. These were little capsules that were created, filled with boric acid solution and exposed for various times at various temperatures.

And, again, corrosion rates were computed from the weight loss. So, you know, here we have open and closed symbols simulating either relatively high pressure.

The solution was saturated at room temperature. Or, ambient pressure saturated at the specific test temperature for the closed symbols, which, of course, don't extend much above 150-160 degrees centigrade.

This is the molten boric acid solution that we are looking at up here. Okay.

CHAIRMAN FORD: So, the way I'm looking at that, is it's just telling me that the only way you're going to get corrosion rates that we're interested in, one to three inches per year, is if somehow you can get the temperature of the head down to about 150 degrees centigrade.

DR. CULLEN: In an aqueous solution of boric acid that's true.

CHAIRMAN FORD: And the other question --

DR. CULLEN: The other side of the coin is that in molten boric acid, if you can figure out how to get that on top of the head, and hold it there for a while, you will get those corrosion rates and even higher.

As long as it's slightly hydrated in a continuous way.

MEMBER KRESS: Or if you can get the concentration up close to saturation.

DR. CULLEN: At a specific temperature. Well, you know, we know that it peaks out right at about 100. You need the aeration there too. So, there's this plausibility thing.

How would you get, say at 250 degrees centigrade, a fully aerated saturated solution? I don't see how. But, at 100 degrees centigrade, we can get fully saturated and aerated. That gives us a very high corrosion rate.

MEMBER KRESS: Do they put oxygen in the PWR water to control the hydrogen?

DR. CULLEN: I hope not.

MEMBER KRESS: It's in BWRs.

DR. CULLEN: No, the other way around. Put hydrogen in.

MEMBER KRESS: Put hydrogen in.

DR. CULLEN: The answer to your question is no.

MEMBER SHACK: Very few people put oxygen in.

MEMBER KRESS: No, you wouldn't.

MEMBER SHACK: That's not a good thing to do and you only do it once.

PARTICIPANT: What do you know about the volatility of boric acid? You know, how much of it would flash off with the steam?

DR. CULLEN: Personally, I don't. We heard this morning from Glen that ten percent will go off. That's something I've never looked into.

MEMBER KRESS: They gave us a curve of equilibrium vapor pressures of boric acid as a function of temperature of the water. And we have the curve and you can convert that.

DR. CULLEN: Right.

MEMBER KRESS: If you make some assumptions of how much the steam is saturated as it leaves the water. I did that. And that's how I arrived at the fact that at low pressure atmosphere boiling you drive the boric acid off.

PARTICIPANT: So it must be, say, 2PSI at --

MEMBER KRESS: I've forgotten.

PARTICIPANT: At 100 degrees centigrade or so.

MEMBER KRESS: Yes, I've forgotten what

that number is. We have a curve of it that was provided to us. I didn't bring it with me.

PARTICIPANT: You hear about the popcorn on the head. And that must be boric acid that remains after you flash off the steam, you know, from an impingement there.

So, it doesn't continue to apparently, what do you call it when it sublime away into the atmosphere.

DR. CULLEN: Well, --

PARTICIPANT: I mean, normally, like ice or anything else will sublime, you know. Even though it's a solid, it will gradually vaporize and leave.

DR. CULLEN: Well, remember now, this boric acid is going to change forms as long as the temperature remains high. And the vaporization rate of $B(OH_3)$ could be a whole lot different from B_2O_3.

PARTICIPANT: But you're saying like the boric oxide is a stable solid.

DR. CULLEN: My guess is that has a very, very low vapor pressure.

VICE CHAIRMAN SIEBER: But you're always adding to it.

DR. CULLEN: Well, when you are or aren't, I think it's going to be -- if you get a B_2O_3 up there it's going to hang around for a long time and never change. Just a guess.

PARTICIPANT: On one hand, if you don't get that kind of situation, and any time you get a little water on it it rehydrates and becomes very corrosive.

DR. CULLEN: It could be corrosive for a short period of time, yes.

MEMBER ROSEN: If you had that one there and you wanted to get it off, you'd probably have to try and scrape it off with something.

DR. CULLEN: Certainly anecdotally we have heard about that happening a few times.

MEMBER ROSEN: So I know.

PARTICIPANT: We actually seen a video of it.

MEMBER ROSEN: Yes. I mean, it wouldn't come off easily because it would be like a rock-like solid, a lava-like solid even.

DR. CULLEN: Where have you heard those words before.

MEMBER ROSEN: Well, I'm just grasping for the words.

DR. CULLEN: I bet. Okay. In the process of going through those slides, I have gone through these conclusions a few times, at any rate, just very briefly summarizing.

The corrosion rates are significant only from low alloy steel. You saw some data that the alloy 600 and 308 stainlesses were quite corrosion resistant.

It's insignificant as long as the boric acid is totally dry, or if it melts and does not get hydrated. But, you have significant corrosion in various aqueous solutions.

You also have significant corrosion in molten boric acid. We said all those things in a few different ways.

MEMBER ROSEN: So, can we put this on a table sort of explicitly?

DR. CULLEN: Yes.

MEMBER ROSEN: What happened at Davis-Besse -- and I'm going to postulate a scenario, and you can tell me if it's wrong based on this research -- is we got a leak deposited liquid on the surface which --

DR. CULLEN: Deposit of boric acid which melted in the form of --

MEMBER ROSEN: -- which vaporized.

DR. CULLEN: Okay.

MEMBER ROSEN: Leaving behind some boric acid, maybe not all of it. Some of it went away, but some of it stayed.

DR. CULLEN: Yes.

MEMBER ROSEN: That boric acid concentration continued to increase until it solidified as boric oxide.

DR. CULLEN: Yes, perhaps.

MEMBER ROSEN: And then, maybe when the plant was cooled off a couple times a year, a couple years, and the head was put over on the side, that boric oxide might have hydrated a little during refueling outage.

But, basically, you had boric oxide, this hard, lava-like substance was probably boric oxide back then.

DR. CULLEN: Well, it's a theory. I wouldn't even say maybe to answer that. I certainly wouldn't say yes, I agree. I'm reluctant to even say maybe.

We don't know what the temperature was up underneath that insulation. Remember Davis-Besse had the reflective insulation that was supported. Now, the center of the head was only a couple of inches from the top of the head.

But, there's like a gale blowing through there all the time. And could the boric acid have gotten to it? Could deposits of boric acid have gotten to these kinds of melting point temperatures?

I don't know, kind of tough. We did talk specifically about this a little less than a week ago with some of colleagues. And whether or not around Davis-Besse you could have accumulated a little boric acid right near the nozzle that melted, kind of ate its way down into the annulus.

I'm trying to come up with ways of expanding that annulus, you know, theories of how that annulus might have expanded. And that would be one.

MEMBER ROSEN: There's not a gale blowing down in that annulus?

DR. CULLEN: Not into the annulus, no. But there's a gale blowing across the top.

MEMBER ROSEN: Across the top maybe.

DR. CULLEN: Under the support skirts.

MEMBER ROSEN: 99 percent of the time, unless the fan tripped. Unless we were wrong and they might have lost power.

DR. CULLEN: You've got ample cooling up there a lot.

MEMBER ROSEN: But you don't -- what I'm

going to try to say is that maybe you don't have 100 percent of that time, you don't have that gale blowing.

Maybe you have some quiescent periods for some reason.

DR. CULLEN: Perhaps.

MEMBER ROSEN: I mean, you have al the ingredients. You just don't have a particular scenario.

DR. CULLEN: Maybe. Okay, I'd like to talk a little bit and very quickly now about a collaborative program between the NRC and the industry to examine some of the nozzles from the discarded North Anna two heads.

Very quickly, you've heard in a previous industry presentation to the ACRS that seven nozzles were removed in June of last year, shipped up to the Pacific Northwest lab, where under an NRC contract we had them decontaminated.

And then four industry NDE teams came in and have now completed exams on four of these seven nozzles. So, in the end of the day we successfully decontaminated just four of them.

It got to be a long-running and kind of an expensive procedure after a while. And now, destructive exams will follow on at least two, probably three, of these four.

The NDE exam was in the laboratory. So, it was under a situation where there was more time available. There was more ready access to the nozzles then when they were typically on a head.

This is a view of nozzles that were removed. You can see a fairly significant section of the low alloy steel from the head was removed. This is the upper or external part of the nozzle that you can see here.

This is a different nozzle, number 31 here, number 59 here. This is the inside of the surface that's normally wetted by the coolant. So this would be all clad.

And this is the stub-end of the nozzle that you're looking at there. This is as they were -- or just after they were flame cut out of the head. They were shipped from EnviroCare is the facility in Utah where the head had been brought.

We developed a decontamination procedure with a lot of assistance from the NRP people, decontaminated them, and used various procedures to remove as much of the contamination as was readily possible without any attack or any chance of contaminating the cracks that were in there.

The whole objective -- there are two major objectives of this decontamination procedure, the whole process. One was to get these nozzles decontaminated enough so the NDE teams could come in and spend some serious time looking at these things without incurring the kind of dose that they would normally incur if these vessels were in the full head.

The second was to preserve the cracks that are in these nozzles for subsequent destructive examination on down the road, which is going to take place in 2004 and 2005 under a rather well designed program, at leas in my opinion.

So, we decontaminated these things, moved them to the NDE test stands where these industry teams came in. Just another view of the surface, the wetted surface.

This is clad or the underside of the surface, if you will. After the decontamination, after the cleaning procedure, showing that, you know, there's some oxide on here.

It's not bare, shiny metal. But, we were able to remove all of the loose contamination that was here, being transported out of the decontamination chamber headed over towards the CRDM test stands.

And this is just a view of one of the technicians painting on, fixing, some of the contamination that was on the exterior flame-cut surfaces of these things.

This is where we're headed with these nozzles, number 54, one of the four that we cleaned up, was, about three weeks ago, shipped up to Westinghouse Pittsburgh where they are going to do a destructive examination on it, with completion later on this year.

On the NRC funded side we are going to take a second nozzle. It's going to be number 54. I have assurance it's going to be number 54, which is the -- I'm sorry, number 59 will be the one we're going to look at.

It's the companion to number 54, companion in the sense that it has the same inventory of flaw indications on it, including some OD circumferential flaws on both number 54, which is at Westinghouse, and number 59, the one that we're going to be looking at.

But, as important to me, we are going to do a very thorough NDE examination of at least nozzle number 59 and with a destructive exam of that same nozzle to be completed next year.

Our focus in the NRC funded program is

going to be on weld defects. Because, looking forward, we have heard clearly this morning that alloy 690 is not expected to show anything like the crack growth rates or initiation times of alloy 600.

Basically, ally 690 is assumed to be pretty immune from possibilities of crack initiation and crack growth rates. If there's going to be a problem in the replacement heads, in my opinion, it's going to be the welds, in the alloy 152 welds.

So, I'm going to take advantage of having these nozzles to allow our Pacific Northwest to work on the techniques, work on improving the various techniques that they have for looking at the attachment weld, thinking that as we go forward down the line that's where we really aught to be focusing our attention and where the industry, after a while, should be focusing their attention for the examination of the replacement head.

So, our focus is going to be on weld defects. Also, please note that we're going to look at a couple of Davis-Besse nozzles. Number 46 had some anomalous NDE indications.

We are going to try to further dispose those. And nozzle number two had a corrosion cavity much smaller than the corrosion cavity around number three.

But, we're also going to take a look at that. Larry Matthews showed you an updated version of the EDY versus ranking model this morning. Bear in mind that this one I've just used it as a place holder.

But, the last opportunity that I had to get updated information was more than a couple of years ago. So, this part is a couple of years old. Don't use it as something contemporary.

All of these plants have moved out, however slightly down here for the lowest EDY plants, and more out here for the higher EDYs. And some of these data points have now been replaced by a replacement head on those particular units.

The point I'm trying to get to is that the EDY formula is based only temperature at the present time. There are no other factors in it like stress, like microstructure, like whatever.

MEMBER KRESS: What are you summating over then?

DR. CULLEN: The summation is to allow for different operating periods at different temperatures.

MEMBER KRESS: I see, different temperatures, okay.

DR. CULLEN: So the summation is only going to go over one to two or one to three, or maybe one to four in a couple of case. But, it's a very small summation.

MEMBER ROSEN: There are some plants that lowered their --

DR. CULLEN: Correct.

MEMBER ROSEN: -- head temperature by going through higher bypass flows after some time of operation.

DR. CULLEN: And then raised it again, and then lowered it again. So it does range up to about a four. I'm sure a four, it might even be five in a couple of cases.

But, that's what the summation's for. Okay, the point of talking a little bit about the susceptibility model is to point out that we've had some incidences recently of head leakage which don't seam to satisfy the susceptibility model.

In other words, a low EDI plant with head leakage. We had one recently in Japan. Other people have wondered, well how does the South Texas lower head fit into this equation.

You all know about that one. We talked about it earlier this morning, because the lower head is at a fairly low temperature. Well, I want to point out that the susceptibility model, as it's now constructed, is based only on temperature.

Items like stress, like materials properties are not in the model. And, I can't say for certain, but I definitely feel that some of these other factors at the end of the day could we worked in to the susceptibility model to give us an even more improved EDY calculation.

MEMBER ROSEN: But you're never going to get everything. Like, you'd have to include something like weld defects, lack of fusion.

DR. CULLEN: Well, that's -- I think another strong point is that the susceptibility model is based only base metal leakage through the CRDM, through the alloy 600.

We use the activation energy for alloy 600 in there, not the activation energy for welds. This is a single material thing. It's got nothing to do with welds.

It turns out that, if the leakage had occurred in a particular plant through a weld, and -- oh, shoot.

VICE CHAIRMAN SIEBER: There you go. Are you done. Your time is up.

DR. CULLEN: Now I've got to go through this whole cotton-picking thing. If leakage had occurred through a weld, then that would have shown up on the susceptibility plot as a leakage point.

But we don't know where that leakage occurred. I mean, these defects are bored out and repaired before we can really get a true disposition on exactly what the crack path was.

MEMBER WALLIS: In most cases.

DR. CULLEN: In most cases.

VICE CHAIRMAN SIEBER: Since you've already established a rigorous schedule for examination and a lot of people are replacing the head, is it really worth the effort to try to -- this to take into account, these other things?

DR. CULLEN: Yes.

VICE CHAIRMAN SIEBER: It is? Why?

DR. CULLEN: For the new heads. I mean, we'll have different formulas, different activation energies.

VICE CHAIRMAN SIEBER: For the new heads?

DR. CULLEN: For the new heads. And --

VICE CHAIRMAN SIEBER: The ones that aren't supposed to crack.

DR. CULLEN: And maybe, to address Dr.

Rosen's question or implicit question, is maybe for the new heads it will be based on welds, and not on alloy 690. I mean that's speculation.

VICE CHAIRMAN SIEBER: But, until you get some deterioration in the new heads, which will be many years from now, probably, you won't have a basis to decide what the algorithm is, and what the important factors are. Do you know what I mean?

DR. CULLEN: Yes, I do know what you mean.

VICE CHAIRMAN SIEBER: Okay. So, wonder if a lot of work in this area to provide you with --

MEMBER ROSEN: You're arguing that it's too proactive?

VICE CHAIRMAN SIEBER: Well, that's one way to put it. But, in my prior life, I tried not to spend money that I didn't need to spend to solve the problem.

But, it's very interesting, regardless of what it costs.

DR. CULLEN: It is interesting. All right. A strong --

PARTICIPANT: What is J being summed over in that last equation?

DR. CULLEN: Different periods of time at a specific head temperature. We heard just a few minutes ago that some plants may have started out at a head temperature of 315 degrees centigrade and later on went down the 307.

So, the time period of 316 would be N is one.

PARTICIPANT: Whatever periods of time they were at that temperature over the life of the head?

DR. CULLEN: No, but the life -- over the --

PARTICIPANT: Over the operating time.

DR. CULLEN: Over however many years.

PARTICIPANT: Right.

DR. CULLEN: Just add them all up. So J equals one could have been the first five years. J equals two could have been the next ten years. You have to allow for the fact that you could have different temperatures.

And, since this thing is dependant only on temperature, you have to sum up over the -- and, literally, some licensees have had a head temperature and then lowered it and raised it and so on.

Okay. Kind of the point I wanted to stress more than anything else was about stress. Stress is not in this model right now. And we all know from different welding procedures and so on that stress can be very important in driving these cracks along, this degradation along.

VICE CHAIRMAN SIEBER: But every nozzle has a different stress depending on where it is and how it was installed.

DR. CULLEN: Perhaps true. Okay, should I just stop.

CHAIRMAN FORD: I think so.

DR. CULLEN: Yes.

CHAIRMAN FORD: Sorry, Bill. It's fascinating stuff, but --

VICE CHAIRMAN SIEBER: He was just getting warmed up.

CHAIRMAN FORD: Could you --

DR. CULLEN: When I did this a couple minutes ago I just should have left it, right?

CHAIRMAN FORD: Oh, don't be rude, Bill.

DR. CULLEN: No. Let's see if we can get Gery up and running here.

CHAIRMAN FORD: Okay. Thank you very much, Bill. We appreciate it.

VICE CHAIRMAN SIEBER: Well, he's got us all warmed up.

DR. CULLEN: Gery, did you want the

labeler, or do you know want to sit down and talk?

CHAIRMAN FORD: We are not planning on having a break between now and the time we finish.

MR. WILDOWSKI: Okay. I'm going to talk to you about two different topics today. One is a little bit of work on a leak rate analysis that has been done in several programs. Tanny Santos, the Barrier Integrity Program, the alloy 600 cracking program. Wally, Norris, which is really the CRDM J welds.

And the large break piping program of Rob Trogoning, which is more piping stuff. Also some residual stress work is going on in two of these programs, the CRDM cracking program, as well as in the piping area.

So, in the leak rate stuff -- see, I knew I was after Bill, so I've got the conclusions right up here. He didn't get through all these slides, I know that's going to happen.

So, anyway, conclusions, leak rate evaluations work. We did some work on looking at PWSCC cracking. And it changes the crack morphology significantly, such that the leakage would give you a lot longer crack with that tortuous flow path than the type of crack morphology that was used in the original

LBB submittals for many of the plant piping cases.

So, having gone back to some of the original submittals, we see that it's difficult to satisfy leak before break now. I've got more words on all of this stuff.

CHAIRMAN FORD: Yes, that's because the liquid can't get out through this tortuous path?

MR. WILDOWSKI: Right, so you have to have a lot longer crack for the same amount of leakage. In the Barrier Integrity Program we show that for piping there's a large range in crack sizes for a given leak rate, depending upon what the stress level is the plant piping system, the type of cracking mechanism that you have, whether it's a fatigue crack, a corrosion fatigue crack, or a PWSCC crack.

Also, the tech-spec limit leakage, the touching capability, we showed that that was not sufficient to detect insipient failure of a partial penetration nozzle, like a CRDM nozzle.

MEMBER ROSEN: You mean the one GPM unidentified leak rate is -- you would need to be considerably lower --

MR. WILDOWSKI: Right.

MEMBER ROSEN: -- before you could pick up a leak that would lead to insipient failure.

MR. WILDOWSKI: Insipient failure. That's right. And that's why we're doing the bare metal visual inspection, because leakage rate is so low that we can't really detect it by the systems used typically for the tech-spec leakage.

Also in the Barrier Integrity Program we made a suggestion. This is a draft report. We recommended that an acoustic emission used for plants for leakage detection for crack growth monitoring.

It's a technology that's been in the code for ten years. It can be used for leakage detection, as well crack detection. We have so many different types of crack orientations, of morphologies, and locations with this head penetrations, that it seems like a weibull technique that aught to be used a little bit more.

In the residual stress evaluation work I'm going to show you some of the ongoing and past CRDM J weld residual stress analysis that we have. But I don't have enough time to go through some of the piping stuff. Maybe on another date.

MEMBER ROSEN: Could you explain acoustic emission? Is that listening to the sound made just by the crack itself when it forms?

MR. WILDOWSKI: Right, exactly.

MEMBER ROSEN: Like seismic monitoring.

CHAIRMAN FORD: I thought it was a whistling.

PARTICIPANT: Is it for leaks more than cracks.

MR. WILDOWSKI: Leaks it works very well. But it's also in there for crack detection, for listening to the crack growth rate.

CHAIRMAN FORD: I didn't know that acoustic emission worked for crack propagation.

VICE CHAIRMAN SIEBER: It design.

MEMBER WALLIS: This isn't a crackling sound, though. It's sort of a little crack every so -- every hour or every year.

MR. WILDOWSKI: Yes, you'll get very low signals. I'm not a real big expert in that area. But, the little bit of work that I've done, for instance.

You know, I tend to hear a greater amplitude of sound from cracks in welds than I do in base metals in some of the tests that I've done.

VICE CHAIRMAN SIEBER: But they usually use a computer to analyze the --

MR. WILDOWSKI: There are all sorts of computer enhancements.

VICE CHAIRMAN SIEBER: Then you have to figure out where it is by having --

MEMBER ROSEN: So, what does a crack sound like?

VICE CHAIRMAN SIEBER: Pardon?

MEMBER ROSEN: What does a crack cracking sound like?

VICE CHAIRMAN SIEBER: Bing.

PARTICIPANT: Have you ever been out on the ice in the winter when it cracks? That's what it sounds like.

VICE CHAIRMAN SIEBER: You'll have to ask some computer some place. Because they are the ones that listen to that.

CHAIRMAN FORD: Okay.

MR. WILDOWSKI: All right. Residual stresses, as we'll see, on CRDM nozzles, is affected by many parameters. The height of the J weld is an important parameter.

The yield strength of the tube is an important parameter. Nozzle angle is important. Weld sequencing, we're starting to see that's an important aspect as well for a circumferential through-wall crack.

The case solutions vary with all of these same parameters, but also can vary considerably through the thickness of the material. We've also made, recently, several suggestions for enhancements to an ASME code case for axial cracks and CRDM nozzles.

So, the leak rate evaluations. We have a leak rate code called SQUIRT that has been recently updated as part of the Large Break-LOCA program. It now handles single phase all liquid flows, single phase steam flow, two phase flow.

We included a model that accounts for the effects of crack opening displacement on the roughness, the number of turns, as you can imagine, a tighter crack.

You're going to have a lot more turns with a tight crack, but not as much roughness. You take that same crack and open it apart, everyone of those little turns now becomes a larger roughness factor.

So, we have some methodology that we developed from computational fluid mechanics to try to make some improvements there. And that becomes very important when you get into the very tight cracks.

We've done a lot more comparisons with experimental results and leak rate codes. I'll show you a little bit of that in a window or two, a frame or two.

Some of the applications, we looked at reassessing leak before break for pipes susceptible to PWSCC. I'll show you a little bit of that. Leak rates through CRDM nozzles.

We've looked at assessments of leaks with degraded different components for the Barrier Integrity Program. And also implemented this code into a new probabilistic code for piping fracture evaluations.

This figure shows experimental results from a lot of older tests. And some of these tests here, the Collier test, were IGSCC cracks and BWR piping with various levels of crack opening displacement.

And what you tend to see is that any leaks that are greater than about .32 GPM, that the variability is about a scatter of plus or minus the factor of two.

But, when you get to the tighter cracks, then we're getting into scatters of plus ten, minus five. And we think that's probably coming in because of this COD dependence on the crack morphology as one of the parameters coming in there.

In the assessment of leak before breaks

were pipes susceptible to PWSCC, the initial LBB submittals frequently used in air fatigue crack, that is, you had a very low roughness.

You had no turns on the flow path, and the flow path was considered to be exactly equal to the pipe thickness. That crack is going straight on through.

With BWSCC the crack morphologies were measured from several cracks removed from service. And we see that when we did that the calculated leakage crack size increased by a factor of 1.8, assuming the cracks are growing parallel to the dentritic grains, as opposed to through the buttered region.

If you were going through the buttered region, the crack goes back and forth and back and forth even more torturous. And this number, in stead of being 1.8, might be more like 2.6.

So that becomes very hard then to satisfy a leak before break with those much longer cracks form this torturous flow path. One thing I don't have in my hand outs is we just recently finished some JR curve fracture toughness measurements in canal 82 and 182 welds.

The good news is the fracture toughness is very high. And, when we did this analysis, we used the critical crack size that was in the original leak before break submittal.

So, it's probably worthwhile for us to revisit that now that we have that data. At the recent MRP meeting we had back in April the MRP said that they're not planning to address leak before break at this time. I see a hand up there in the background.

MR. RILEY: This is Jim Riley, NEI. That last statement isn't true. I mean, at the time that statement was made. So I don't mean to say that what you heard wasn't true.

But MRP is evaluating what we're going to do about leak before break. I don't have any answers to give you right now. But we are looking at it.

MR. WILDOWSKI: Okay. Thanks.

MR. RILEY: I didn't mean to say, you know, you heard.

MR. WILDOWSKI: Okay, for leakage through CRDM nozzles, we did some analysis there. We analyzed the worse case of an incipient failure of a CRDM nozzle with a circumferential crack.

What's that worst case mean? That is that there essentially was no pressure drop through the crack itself. The crack was opened so large already that the only pressure drop you have is through the annular region.

And when you do that then we see that the pressure loss that we got through the annular region was quite significant. And we were only getting leakage rates of less the .2GPM, so that the tech-spec 1GM leakage detection limit wouldn't necessarily catch a CRDM nozzle crack that was about really to fail. Other leak rate applications are --

MEMBER ROSEN: Go back for a minute, didn't we hear this morning or earlier that the Davis-Besse crack was estimated to be a .15 GPM?

MR. WILDOWSKI: Yes.

MEMBER ROSEN: So that's consistent with this.

MR. WILDOWSKI: But they didn't have any circumferential -- oh, yes, I guess it's --

MEMBER ROSEN: I'm saying --

MR. WILDOWSKI: Well, they eroded the wall on the way out.

MEMBER ROSEN: But they didn't exceed the tech-spec?

MR. WILDOWSKI: That's right, that didn't even exceed the tech-spec. That is correct. That part is consistent.

PARTICIPANT: What is the last sentence, normal leakage detection systems? The do not work or --

MR. WILDOWSKI: Well, they wouldn't work as an inspection tool for a CRDM nozzle crack in that you wouldn't be able to prevent failure.

PARTICIPANT: Well they're only designed to --

VICE CHAIRMAN SIEBER: The leak is too small.

MR. WILDOWSKI: Yes, the leak is just too small for it.

PARTICIPANT: Right.

MEMBER KRESS: You just need to put the word will in front not, or would not.

PARTICIPANT: They will work or will not?

MR. WILDOWSKI: Will not.

PARTICIPANT: Will not, okay.

MR. WILDOWSKI: All right, since I've got 25 minutes, what can is skip here? Barrier Integrity Program, this is some calculations that we did for circumferential cracks in pipes.

And what I've got in this plot here is leak rate versus crack length. And if we just concentrate at 1 GPM leak rate, then you se a whole bunch of different curves here.

And the smaller crack sizes here correspond to operating at a stress level of about 100 percent of the surface level A Stress limit of the ASME code, as far as its normal operating conditions.

The large cracks here correspond to piping systems that would be operating at 25 percent of level A stress limits. So, you can see it's quite variable in that you could have a crack from two inches to 18 inches with a 1 GPM crack, in this particular sized pipe.

This is, you know, like a collate pipe or a smaller -- pipe.

MEMBER ROSEN: That's very counterintuitive, isn't it? You could have a crack that's 18 inches long, and it leaks the same as a one inch crack?

MR. WILDOWSKI: It depends on the stress level that you have here.

MEMBER ROSEN: Stress?

MR. WILDOWSKI: Yes.

MEMBER ROSEN: It has to do with the tightness of the crack?

MR. WILDOWSKI: Yes, how much the crack

opens under load. Okay. So, these cracks here that are the one inch type of cracks or two inch to four inch type of cracks, they are operating at 100 percent of the surface level A stress limits.

So, the crack is -- you know, there's a fair amount of load to open the crack up. So the opening areas, effectively about the same as a crack that's under a lot less load, but is a longer crack.

MEMBER ROSEN: That makes sense.

MR. WILDOWSKI: And then you have to take account for all of the friction factor losses through the crack and everything. So, given that our results show that there's a large range in crack sizes for a given leak rate, and the tech-spec limit is not sufficient for partial penetration nozzle leak detection, one of the recommendations was to try to apply acoustic emission in the future for plant operations.

It's already in the ASME code for both leakage detection and for crack detection, and has been demonstrated in a NRC program in the past in a plant.

And, you know, personally I think it would be ideal for cracking locations that are difficult to inspect, like upper and bottom heads, penetrations, pressurizer, heater sleeve nozzles.

And it also covers a myriad of other things in that cast stainless steel, if we ever had a cracking mechanism in cast stainless steel. You can't use ultrasonics in cast stainless steel, but you might be able to use acoustic emission to detect if anything is happening in it.

MEMBER KRESS: You have to put those acoustic pick up right on the track location --

MR. WILDOWSKI: No, it's pretty good. You can be pretty far away.

MEMBER KRESS: So you don't have to have one for every nozzle?

VICE CHAIRMAN SIEBER: No.

MR. WILDOWSKI: No. I think -- to the guys that do more about this, they thought that maybe four transducers were needed for a head.

MEMBER KRESS: Oh, okay.

MR. WILDOWSKI: For the whole head.

MEMBER KRESS: So you just put them on the head itself.

MEMBER ROSEN: What do you do, triangulate?

MR. WILDOWSKI: Yes.

MEMBER ROSEN: To find the crack? Because otherwise it doesn't help you to know -- hear ping.

MEMBER KRESS: Do you know what the sound speed in the steel is?

MR. WILDOWSKI: They look at the signal over a certain frequency range, for one thing. And secondly, when they use a technique called a wave guide technique that they attach to the head, what happens is cracks tend to give -- separate into two different types of signals.

You get the longitudinal wave as well as the share wave coming off, which arrive at different time periods. So, if you hear something at one time period and you hear something else again at exactly the critical time period corresponding to the difference in the wave velocities, it is your wave.

Yes, for the speed of sound in your guide wave, then you can say that's a crack. That's not random noise.

VICE CHAIRMAN SIEBER: Do you know where it is?

MR. WILDOWSKI: By triangulating then you know. You just reach one of those sensors.

VICE CHAIRMAN SIEBER: But the original application was for doing hydrostatic tests without having to wait the time.

MR. WILDOWSKI: Yes.

VICE CHAIRMAN SIEBER: And you could listen for leaks and so forth. And you could put about 20 or 30 transducers on a reactor coolant system that would monitor valves and things like that.

This is a more sophisticated application where you can't do this during normal operation. You do it during a hydrostatic test or something like that where you can control to some extent the transducers and what you're picking up and how much noise there is in the systems.

MR. WILDOWSKI: Well, I think they've done a lot of work on it for time period.

VICE CHAIRMAN SIEBER: A lot of filtering, a lot of time analysis, a lot of tranquilization. And with the speed of computers now, it's pretty accurate.

MR. WILDOWSKI: You probably could to it a heck of a lot better than ten years ago.

VICE CHAIRMAN SIEBER: Oh, yes.

MR. WILDOWSKI: So I think it's something that's worth revisiting again. I need to march on guys.

MEMBER WALLIS: Does it make a difference what kind of insulation you have on this thing during transmitions.

VICE CHAIRMAN SIEBER: And you have to --

MR. WILDOWSKI: Because your wave guide is going right into direct contact with the head itself.

VICE CHAIRMAN SIEBER: Yes, you have to surface prep where you put the pickup.

MR. WILDOWSKI: And it's pretty cheap to put those wave guides and the transducers well ahead of time. If you are buying a new head, it's probably a really good investment to make at the time.,

VICE CHAIRMAN SIEBER: If you're buying new head you probably don't need it.

MR. WILDOWSKI: One would hope.

VICE CHAIRMAN SIEBER: Okay.

MEMBER ROSEN: Well, you don't need QA for anything that isn't going to fail.

VICE CHAIRMAN SIEBER: That's true.

MEMBER ROSEN: You just have to know ahead of time which it is.

MR. WILDOWSKI: Okay. I'm shifting gears on you to weld residual stresses, and cutting off the discussion. We are going to go into -- I said I didn't I have time to talk about piping.

I may not have time to talk about all the CRDM nozzle tracking work either. But, we had a phase one program that ran from January of 2002 to January of 2003.

In that program we did weld simulation analysis for a center hole and steepest side-hill nozzle cases. We did calculations for a circumferential through-wall cracks.

We developed a Visual-Fortran probabilistic code that we benchmarked against Bill Shack's spreadsheet code. He will talk about his stuff more.

We did some stuff for Davis-Besse. Everybody seems to have done that. Our ongoing programs started July of last year and goes on to January 2006.

We are looking primarily at circumferential cracking and CRDM nozzles for probabilistic time to failure for leakage. But we are examining different types of weld residual stress conditions.

We've looked at the ASME code case for axial cracks in CRDM tubes, some more Davis-Besse work, that always gets in there. The overall modeling strategy involved the following types of steps.

We always model the whole head and part of the vessel in our model. So it's a very large model. Way down here someplace this is the center hole example.

There's a little teeny J weld down there in the element model. Each on of these elements has -- I'm sorry, weld deeds that we use in our simulation -- has 13 to 20 elements in it.

So, we have a very detailed model compared to all of the other models that have been used. In making up the weld model we also put on cladding and we simulate the heat treatment for stress relieving of the cladding.

We installed the tube in a reactor pressure vessel head by shrinkage fit. We simulate the welding of the J grove. We simulate the hydrostatic testing that's involved.

Because the welding simulation with this many elements is very time consuming to do what we then do is we use a stress mapping technique where we create many meshes with different types of cracks, whether I put a circumferential crack right up here above the triple point of the weld.

And I can create many finite element meshes with different crack sizes and then just map these stresses that we have from the weld simulation onto that solution.

So that allows us to transfer the full

stress tenser, the strain tenser, the plastic strains as well, displacements, boundary conditions, to this pin-crack mesh.

We then take that up to the service pressure and temperature. We unzip the crack and solve for the case solutions, curvet those case solutions for the probabilistic code.

MEMBER KRESS: Is that a sub-bullet under the first bullet? Is that a boundary condition stress you apply?

MR. WILDOWSKI: Boundary condition -- I'm sorry, for what part?

MEMBER KRESS: I presume what you do is have a stress applied to the periphery of the tube.

MR. WILDOWSKI: Yes, there's gap elements that go in there that allow for the interference fit.

CHAIRMAN FORD: So, from what I was hearing earlier today, this weld stress analysis presumably is for a very specific welding condition, weld, heat -- speed, etcetera, etcetera.

And the physical constraints. And the finite element model that you're using to come up with stress versus distance and three dimensions was calibrated against data from piping, is that correct?

MR. WILDOWSKI: Basically, true.

CHAIRMAN FORD: And there has been little or no recalibration of that residual stress profiles against prototypical large weld assemblies. The assumptions in the modular are --

MR. WILDOWSKI: There's been some work done on looking at, for instance, I'll call them global displacements remote from the weld, like how much the tube ovalizes, you know, at the bottom.

But you have to have those conditions right. But that doesn't mean that what you have up in the weld is necessarily right either. And I'm going to show you some different weld sequencing results that give you different weld residual stresses.

Because, you have to understand that if somebody just come in and says, here I've got this J weld from Davis-Besse plant. I'm going to go ahead and do some strain gauged drip panning, and I'm going to say, wait, how did you make the weld.

When you see these results you'll understand that it's important to understand how the weld is made in order to say whether the experimental results are reflected in the analysis correctly.

CHAIRMAN FORD: And you're confident -- sorry.

VICE CHAIRMAN SIEBER: Do you know how the welds were made in every case?

MR. WILDOWSKI: I've got a couple examples that I'll show you. But --

VICE CHAIRMAN SIEBER: I mean, it would seem to me that that is important in order to do this. And it's not clear to me that you know how the weld was built up in the first place for every weld that's out there.

MR. WILDOWSKI: Probably don't know that for every single one. And that's part of the probabilistic nature of this whole exercise.

VICE CHAIRMAN SIEBER: Go ahead.

CHAIRMAN FORD: But you feel happy enough that you don't feel the need to do some measured residual test profiles on a typical --

MR. WILDOWSKI: You know, that's been done so much on simpler geometries than this geometry, that I don't think that it models themselves to being wrong.

I think it's the variability of things like the weld sequencing, the strengths of the materials, those sorts of parameters are coming in to play.

CHAIRMAN FORD: Okay.

MR. WILDOWSKI: The surface stresses, I

will talk about that a little bit, time permitting.

CHAIRMAN FORD: Okay.

MR. WILDOWSKI: So, here's a couple examples. I don't have a whole lot of time, so I just picked out a couple. Here's a center hole nozzle.

This is what the equivalent plastic strain looks like in the plot here for the low strength tube. Here you see the cracks, so we've got a circumferential crack in the tube coming around, kind of a spider web at the crack tip.

So, if you understand that, here's the J weld for the center-hole nozzle. So you're looking inside the tube. This is kind of a cutout, it's not bulging out, it's going in.

An interesting thing you see here, look at that crack face. It's kind of sliding radially inward, isn't it? It's got a little opening, but, you know, it's not only opening mode, it's sliding mode.

MEMBER WALLIS: Doesn't that weld affect the tube? I mean, you've got a very fine mesh in the weld material.

MR. WILDOWSKI: Yes.

MEMBER WALLIS: It suddenly goes to a very course --

MR. WILDOWSKI: Well, the course here --

right at the crack tip it gets very fine. You just don't see the crack.

MEMBER WALLIS: Isn't the tube itself affected by the weld?

MR. WILDOWSKI: I'm sorry?

MEMBER WALLIS: Isn't the tube material affected by the heating from the weld, so you've got changes in the tube itself near the weld?

MR. WILDOWSKI: Yes, it's affected by heating.

MEMBER WALLIS: You didn't model that?

MR. WILDOWSKI: Well, in the actual model -- remember, I have two models -- the mesh where I do the weld residual stress modeling, actually it's a finer mesh over here.

MEMBER WALLIS: It is finer there.

MR. WILDOWSKI: Yes, it is finer here. But when I created the mesh with a crack in it, then I create the mesh and map the stresses onto this new model that it's coarser here, but I'm really interested in having finer elements over here where the stress intensity is calculated.

PARTICIPANT: Are these -- symmetric, so you're modeling all the way around?

MR. WILDOWSKI: Yes. When we create the

center hole model, we do it a 2D. It's actually symmetric. We revolve that solution to 360 degrees or 180 degrees and then put in the crack.

And so this is a half crack that you see here. The interesting thing is when you go to the high strength tube, look at the difference you see in the pattern that occurs there in the plastic strain and the resulting stress field that you will have as well.

Also, notice that this crack here -- we put our cracks in perpendicular to the tube. In this case the crack was -- it wants to tilt also. Okay.

So you're going to have a lot higher crack driving force on the OD surface of the tube than on the ID surface of the tube. This is a calculation of some values of maximum K values that we had through the thickness, versus the average values.

The average values were used in our K solutions that we then gave to Bill Shack, and that Pete also uses in his model. This shows our results the center hole sensitivity case.

The low yield strength material here is the blues material. And the higher strength material is the green diamonds. And what you see is that the difference between the maximum and the average for the low strength material was only about 3 KSI square root inch in the stress intensity.

But, for the high strength materials, it was about 20 KSI square root inch difference in the driving force, quite a significant difference. So that means that crack in the high strength material really is probably going to propagate more around the outside circumference and lag on the ID side of the crack.

We did a nozzle sensitivity study that we're just finishing up. We looked at grove angles of 15, 22, 45 degrees. We looked at weld heights. 20 millimeters was about the lowest or minimum weld height that you would have and pass the ASME design code.

Whereas, these other weld heights were getting larger and larder, obviously. We wanted to see what happens with the hoop stresses on the ID surface as well as looking at the longitudinal stress just above that triple point there where you're going to form a circumferential crack.

A lot of information here. But I think the important aspects are to see -- first of all, this is the ID hoop stress that occurred here. Here we have weld angles are 15 degrees on this side.

Let me see if I can dot s left handed. This pointer is a little bit brighter. 15 degrees, 22 degrees, 45 degrees. And then the weld height is changing with the color code.

There does tend to be not a very large significant affect. But, the larger the angle gave slightly lower hoop stresses if you have time to look at it closer.

The 20 KSI stress level that was talked about earlier, that's at a distance of about 2.6 to 2 inches from the root of the weld. So from the bottom of the weld the two inches covers that distance.

If we look at the axial stresses through the thickness of the tube, two millimeters above the weld, here you can see the effect of angle and weld height again.

And the really important thing is that the height of the weld really controls the axial stresses very strongly here. You see only the blue symbols, the smaller weld height, are tinsel values above the weld where you're going to get a circumferential crack.

CHAIRMAN FORD: It was mentioned earlier on that -- and I forget who mentioned it -- the weld height is a function of the model of the reactor as to when it was fabricator. Is that true?

MR. WILDOWSKI: That did change, I think. You know, MRP has a lot of nice data on that and looked at very nicely.

CHAIRMAN FORD: So, is there a correlation between axial cracking -- circumferential cracking and the weld height or the model number or somewhere along those lines.

MR. WILDOWSKI: That would be nice information to look at. I know there's a database. We talked about it at the last MRP meeting, about trying to get access to a database as to how many cracks, and where the cracks are in the nozzles.

I don't have all that information to be able to say that.

CHAIRMAN FORD: Because, Bill mentioned the Ohio reactor which is cracking up to very few degradation, yes? Did that correlate with --

MR. WILDOWSKI: Personally, I haven't look at that to see whether that makes sense.

CHAIRMAN FORD: Okay.

MR. HISER: It's not clear where all it is cracking at the moment, whether it's in the weld or in the --

MR. WILDOWSKI: Oh, okay.

MR. WILDOWSKI: One of the explanations that happens -- I'm going to skip this slide. I don't have enough time. Sidehill nozzles, okay? This one we have to create the weld in a very three dimensional manner.

So it's a lot more complicated geometry. Again, we have the whole head going down to part of the vessel included in the model. So, it's an extremely big model. This shows, I think we had it balanced up hill and down hill.

Weld areas were equal, which was one of the conditions that existed for some of the plants. And this was the minimum weld height. This was a very steep angle, 53 degrees in this particular case, so way out on the outer edge.

VICE CHAIRMAN SIEBER: But that's not where the --

MR. WILDOWSKI: Well, it's where you get the higher stresses and the higher K values.

VICE CHAIRMAN SIEBER: Right.

MR. WILDOWSKI: Here are some results. We are going to do some comparisons. But this, unfortunately, the older, not the recent, MRP results.

Pete's got a lot of stuff that he just issued recently. I haven't had a chance to show that or compare it yet to our results. But I thought it was interesting to look at one thing.

The blue line is kind of our results with the low yield strength sidehill nozzle. And what's happening because of that mode-1, mode-2, mode-3 sliding combination is that the K value has almost stayed constant for a long time in that center region once you add up all the contributions from those different driving force components.

CHAIRMAN FORD: So, when you look at these complex shapes and you're saw confident about your residual stress analysis, can you apply it to repair welds?

MR. WILDOWSKI: Oh, yes. It's been used for repair welds many times.

CHAIRMAN FORD: So why don't we, since repair weld cracking is so often potentially correlated with whether it was repair welded or not, and in fact North Anna.

There seemed to be a correlation between repair welding.

MR. WILDOWSKI: Yes. In the piping work we've been looking a lot more at repair welds. We haven't done anything in the J welds for repair aspects.

CHAIRMAN FORD: So, can we come up with specifications for repair welds as to how they should be done in terms of heat input, welding speed, and things of this nature?

MR. WILDOWSKI: There's a number of suggestions I would make for girth welds if you're doing a girth weld. I'm not sure about a CRDM nozzle yet.

CHAIRMAN FORD: Okay.

MR. WILDOWSKI: I'll have to think about that. But I know things that I wouldn't do for a girth weld on repair welds.

CHAIRMAN FORD: Okay.

MR. WILDOWSKI: This I wanted to show you some comparison of some weld sequencing work that we've done for the sidehill nozzle. How much time have we got?

CHAIRMAN FORD: Could you possibly finish by 22, for sure?

MR. WILDOWSKI: Yes.

CHAIRMAN FORD: Because Bill, at least ten minutes.

MR. WILDOWSKI: Now, one thing I didn't tell you is the sidehill nozzle work that I just showed you, really what we did was we used a weld sequencing that was -- we tried to follow what we were told by one welding engineer at an older PWR manufacturer.

And what they did was they would weld a 90 arcs on the side quadrants first. And then go to the downhill side and do 90 arc segment, and then on the uphill side do the 90 degree arc segment.

So, they would do these 90 degrees, 90 degrees, 90 degrees. And that's what we including our model, was that 90 degree weld sequencing process.

PARTICIPANT: Are these manually welded?

MR. WILDOWSKI: Yes. Now, recently we did something to create the weld beads completely around the circumference all at one time. I call it like a flash weld.

And this is similar to what Dominion Engineering efforts have done for EPRI and the MRP program, that is for a whole weld bead, or a series of weld bead you just assume that whole weld just occurs all at one time, instantaneously, with the appropriate cooling.

MEMBER WALLIS: How do you do that?

MR. WILDOWSKI: What?

MEMBER WALLIS: How do you make that weld?

MR. WILDOWSKI: Physically you can't.

Numerically it's a lot more efficient to do it that way, of course.

VICE CHAIRMAN SIEBER: use molten boric acid to --

MR. WILDOWSKI: Now, the other way that I think that some of the newer head are probably going to be made -- and I've seen something like this -- is they use a weld sequencing that starts on the downhill side, and then they make a series of short arcs.

They might alternate from one side to the next side until they finish on the uphill side of the nozzle. So you can see the weld patterns are quite a bit different.

Here you are making 90 degree segments. In this one you are going all the way around all at one time when you're doing the numerical simulation.

And here you've got a series of steps that you're making, you know, one side starting from the downhill, finishing up on the uphill side.

So, here's the work that we just finished looking at, axial stresses, making the weld beads all at one time, similar to the Dominion EPRI analysis procedure.

And what you see is this is after welding, and at room temperature, not at service conditions too. The maximum stress value is here. If you look at your handout, that is the same scale as the next picture.

And you see that well, you got really high stresses here on the uphill side. You've got some here on the downhill side. And, you know, they taper off in between.

When you do the 90 degree arc segment type of analysis instead, oh, you start to get higher stresses at about 90 degrees away from the uphill and the downhill side.

So you shifted the stress distribution significantly between this welding procedure and that welding procedure.

MEMBER WALLIS: This is beautiful stuff. Is it related in any way to anything in reality about what's observed in a head?

MR. WILDOWSKI: Well, I think what you'd like to do, again, is going back to Peter's question. If somebody just gives me a random J weld nozzle and says I'm going to do strain gauges trepanni, and they have no records about how the guy has made the weld, you can get all sorts of answers.

If you really want to see whether this stuff is working right, you make the J welds exactly by one procedure or the next procedure to try to verify it.

You just can't do it randomly. Okay. You know, that's just pointing out some of the high stress spots being different. Actually, this is a little bit higher down here than over on this side too, on the downhill.

It's kind of interesting that you're seeing things shifting around.

MEMBER WALLIS: But is there any evidence of cracking that occurs because of these stress distribution and --

MR. WILDOWSKI: I don't have enough information to tell me where all the cracks really are located. And that's in that database that Avery has, that maybe we'll get some time.

PARTICIPANT: Out of this analysis, do you get some result as far as the deformation that results in the tube?

MR. WILDOWSKI: Oh yes, sure. You can look at the things that people have looked at. What's the ovalization at the bottom here that has been measured many times.

PARTICIPANT: Don't they do some straightening, as a matter of fact, after the thing is finished?

MR. WILDOWSKI: I have heard that that's happened.

PARTICIPANT: Which also puts stresses.

MR. WILDOWSKI: Yes, which would also do something.

MEMBER SHACK: You didn't push these on. Did you see what difference it made in the actual case solutions, which is, you know, where the rubber meets the road, really?

MR. WILDOWSKI: That's right. We'll be doing that next.

MEMBER WALLIS: It's where the -- meets the weld, not the road.

MR. WILDOWSKI: Can I go back one? I want to go back one, there. With this case here, which is going to be our new heads, I think that's interesting.

And maybe a lot of the older heads are like this too. I think what's going to happen is if you did this weld sequencing type of analysis, you're going to find higher stresses at the start point, and higher stresses at that stop point then if you had done the weld all at one time.

So you're going to get another distribution yet. Let's see if I can get through these. Okay, this shows -- I'm going to skip these couple things.

I don't have time. Axial cracks and CRDM nozzles, we did some stuff eighth the code case, made some suggestions to the section 11 committee. This is my last slide.

I'll just tell you where we're going a little bit on this stuff. I have 10 minutes, I can drag it out. Two minutes. Okay. So, one of our tasks here is fission and versal computational procedures.

We'll working with Dave Parks at MIT for a tetrahedral type of element post-processor for calculating J. These meshes are so terribly complicated to make the 3D meshes when we want to put in many weld beads or many elements in the weld beads.

And, if we want to put cracks at angle through the thickness, because the cracks going to grow in the mode one direction. It's not going to really grow with all these mode-2, mode-3 contributions.

We have some additional analysis we are doing through-wall cracks and the -- we're going to stat examining the effects of manufacturing stressing on the tubes by procuring some tubes from France, as well as examining tubes from North Anna and Davis-Besse.

Remember, our model, when we calculate the hoop stresses in the pipe system, or in the CRDM nozzle, we are assuming that tube is stress free when we get it and we start our modeling.

It's not. We have some results in the literature that says, hey, the hoop stresses were, in at least one case, 18 KSI from just the manufacturing process.

We'll look at the effects of the compressive stresses from surface honing or other surface grinding techniques. And we want to see how these surface stresses change under the operating conditions.

We have some work for, again, the steepest sidehill nozzle. But now we're going to want to use the high yield strength material and have the crack angled through the thickness.

For the center hole nozzle we are doing a fundamental evaluation of what happens with this angled crack through the thickness if the crack really wants to grow in only the mode-1 direction.

Are we calculating K properly? We have some intermediate nozzle angle analysis that will go on once we've finished the steepest sidehill and the axial crack work.

We have upgraded our probabilistic computer to Visual C++ code. And we are doing some work in a taskforce, some coordination, you know, coming to meetings like this.

But also an important thing is, we have some agreements with the guys over at Dominion Engineering to exchange our residual stress results and try to find out when we are getting the same answers, and when we're not getting the same answers.

And when we're not, why aren't we getting the same answers.

CHAIRMAN FORD: Now, what's the difference between the underlined and the non-underlined?

MR. WILDOWSKI: The non-underlined are non-active. So that's not active, that's not active. This is ongoing work in all the underlined activities.

You noticed that. I'm really proud of you.

CHAIRMAN FORD: Well, thank you very much.

MR. WILDOWSKI: I have an answer for that.

CHAIRMAN FORD: Any comments? Well, what does all this feed into. This looks like very detailed stuff.

MR. WILDOWSKI: Yes.

CHAIRMAN FORD: And then earlier we heard some very general stuff today. And I want to know how this fits into some scheme for making decisions.

MR. WILDOWSKI: I can talk, but of course Bill has got a probabilistic model that he's been working on with Steve Long.

CHAIRMAN FORD: That sounds like an academic, therefore very virtuous piece of work. How does it fit into the decision making processes of the Agency?

MEMBER WALLIS: Well, the probabilistic fracture mechanics model becomes something that is used for the significance determination process, among other things.

MR. WILDOWSKI: The axial crack work that I cited here, but didn't have time to go into, that went directly into making some suggestions to the ASME code case.

I'm trying to think of all the other stuff, where it comes into checking. Part of our work is the checks and the balances on some of the things that our friends in industry are doing.

You know, that's a big part of looking at this Dominion Engineering modeling. In their modeling they have to do a lot of cases. They do lots and lots and lost of cases.

And these guys are really busy cranking out numbers. But they simplify their model a lot. So, what we're doing is we're doing some fundamental checks to make sure that the simplifications and their model are appropriate.

MEMBER WALLIS: Okay, now you're a contractor for NRC?

MR. WILDOWSKI: Correct. And EPRI, I heard this morning, is spending maybe ten times as much money as the NRC. Are they doing ten times as much work?

Well, you've done a tremendous amount here. Is industry at the same level of complexity?

MR. WILDOWSKI: In different ways, I'm sure. I mean, they've done so many cases. I've seen a lot of it. You know, I'd like to see more of the details. Pete's here, he's getting ready to talk.

MR. RICCARDELLA: This is Pete Riccardella. You know, I presented an overview presentation this morning that has a lot of underlying details that are very similar to this.

I just didn't have time to get into them. But they are in that MRP 105 report.

CHAIRMAN FORD: So this is the way I would have answered your question if I was them. It would have been to say that essentially his work feeds into Bill's work.

And Bill's work double checks what Pete's doing. They've done a similar thing. So, just being in the position of being an informed regulator.

That's the way I would have answered it. I don't know if that would be the correct answer.

MR. WILDOWSKI: And I think that was your question originally that you asked, is what type of probabilistic or checks on the probabilities are you doing?

Well, we're doing it at a deterministic point at different steps in the modeling. For instance, looking at the weld residual stresses is one of the terministic steps that's very important in order to get the K solutions right for knowing that he probabilistic model works correctly.

CHAIRMAN FORD: So, should we move on to the NRC's Pete Riccardella?

MEMBER ROSEN: You know, if you did anything else besides playing with computers, you might find some other interests in life.

MEMBER SHACK: What other interests could there possibly be? Let me just go right to it since everybody knows that I'm Bill Shack from Argonne National Lab and we are talking about CRDM probabilistic models.

Actually, let me go back. Oh, it's not going to work too well without that, is it? Okay, there we go. Talking about a probabilistic fracture mechanics model that's somewhat complimentary to Pete's.

And I should mention that we start from the same place, that I'm using the same database on leakage and things that Pete used to develop hi model.

We approach it in somewhat similar ways. We develop essentially a liable empirical model to describe initiation. We use an estimates of crack driving force for crack growth.

And we're both using the MRP 55 distributions for crack growth rates developed to predict nozzle failure. We do some things differently.

I would claim that the way Pete analyzes the field data, what he's obtaining is a wiable parameter that describes the average plant behavior.

I've tried to develop weibull distributions that it described the full range of behaviors that you could expect in the field or, another way of putting it, the uncertainty you have if you try to apply the results based on a population to a single individual plant.

So, when I compare with Pete's results, I will be comparing the average of my results to his results. And we'll find out that they're reasonably close, so that my averages look like his predictions.

But I get a wide range of results that accomplish what I believe is a true variation in behavior between plants. And, again, the calculations really started with the calculation of the probability of the failure of a nozzle.

You then build up a head by looking at a collection of these nozzles. And these nozzles are different because you have center versus sidehill, which give you different K distributions, as Gery talked about.

You can also have something like one to seven heats in a head of material. And, again, some of those heats may be good materials, some of those heats may be bad materials.

Again, not only just heats, but variability in the nozzle because some nozzles will have repair wells and some will not. So, there's a fair amount of variability in there that has to really be taken account, I think, in the variation of the calculation of failure.

But I've tried to do that. And, again, we'll see how you can still make decisions out of that.

MEMBER KRESS: Do you treat those as random variables?

MEMBER SHACK: I treat them as random variables. My heat information is based on the B&W plants where I know how many heats there are in each plant.

What I found there is that there's one to seven heats in the plant. It's approximately log-normal distributed. So I assume that the number of heats in any plant is picked from that log-normal distribution.

The number of nozzles from any heat is also log-normally distributed. I picked that number from the distribution to generate my populations of plants.

To get myself back to reality, of course, I can't compute just probabilities of failure since, luckily, we haven't had any failures. But what I can do is just what Pete does.

You know, we benchmark against the number of leaks that we see, and the number of large cracks that we see. And, hopefully the models do predict realistic versions for those.

And, as I said, the distributions can be interpreted as essentially the range of behavior we might see in the whole fleet or, if you want to pick one plant, the kind of uncertainty you might have in making a decision, if this is the only information you have.

And I'll sort of talk about additional information you might bring to bear to reduce those uncertainties.

MEMBER KRESS: When your model sees a leak, is that just a through-wall crack?

MEMBER SHACK: Minus a 30 degree through wall crack, just like Pete's. And, again, the reason that we do that is that there's a whole complexity of things that are going on. That is, when you generate the crack, you have the possibility of multiple initiations on that circumferential crack.

What we've really sort of assumed is that complex process is difficult to model in detail, but by the time you've got the 30 degree through-wall crack, the process really is driven by the growth of that large through-wall crack.

And that's something we can compute. And we think by assuming instantaneously jumping to that, we're making a conservative estimate of this rather more complicated process that really gets you there in the first place.

One of the other things I'd like bring out, and again, Pete is taking no credit for it in setting up his inspection program, but there are differences from fabricator and suppliers that you can see in the data.

And so, the data that's being used in my first cut, and in Pete's model, is in fact conservative for the actual population of plants that's really out there in the real world at this point.

And, again, we first start with a description of an initiation model. And, again, it's described in terms of the weibull statistics. I fit the data to the -- essentially the field data.

And I've sort of already described this. The stress intensity factors come from Gary Wilkowsi's solutions. Again, we have solutions for center nozzles, for sidehill nozzles.

We have them for high yield stresses and for low yield stresses. And so, there's a variety of K solutions that we have to consider. The crack growth rate comes from the MRP 55.

These are Gery's solutions. Again, this is a center nozzle. This is the low yield, high yield sidehill nozzle, low yield high yield -- again, the K values are dominated by these welding residual stresses until the cracks get very large and the pressure stress takes over.

So, it really is the welding residual stress we have to work at. He's got a strong dependence on yield stress here. You know, in an ideal world we'd also have, you know, solutions for a high heat input, high weld speed, more variability.

But, at least we've taken into account the variability. And one of the major variables that you have with residual stresses, and that's the actual yield strength of the material.

That is a high yield strength material can in fact sustain higher residual stresses than a low yield stress material. I use a random variable to sample the K solutions.

And I've made the simplifying assumption that I got a high yield stress solution and a low yield stress solution. I interpolate between them to take account of intermediate yield stresses.

And, again, you don't have to really think of this a yield stress parameter. Think of it as a parameter that takes into account yield stress, weld speed.

I've got high stresses, low stresses, and I'm sort of distributing over that whole range of possible stresses that have to be considered. What is a little different is the way I go about determining the weibull factors for initiation.

Pete showed you his approach. What I try to do is I try to do this on a nozzle basis. He takes his collection of plants. He then extrapolates back to that first failure for the nozzle.

I try to consider all the nozzles for all the plants at once in one big distribution. I postulate that all these nozzles are drawn from some distribution of -- the weibull factors are drawn from some population.

That gives me a likelihood function. This likelihood function just tells me what the probability of actually getting what I really saw out there in the real world if I was picking these nozzles from this -- that I'm assuming.

And I'm going to then maximize this

likelihood of the real world actually occurring, to find that my estimation of that population distribution function from which I'm drawing these nozzles.

MEMBER KRESS: Do you use variation of calculus to get that maximization?

MEMBER SHACK: No, I do a group force --

MEMBER KRESS: Do you take the derivative and --

MEMBER SHACK: No, I don't take the derivative. I just keep calculating maximums and searching around until I find the peak.

MEMBER KRESS: Well, that'll do it. If you've got a good computer.

MEMBER SHACK: This computer -- we do it brute force, you know. It's amazing. And what do I get? Well, here's my distribution. As I've mentioned, I really do this thing on a per-nozzle basis.

But, to compare back to Pete's stuff, and to get something that sort of jives a little better with experience, I've sort of given you pseudo head weibull things here.

And the head weibulls is -- I have 69 nozzles of the identical properties. Or I have center hill nozzles and I have sidehill nozzles. But all the nozzles have the same probability.

I would get then a range of behavior that say -- doesn't surprise me, I can predict that I'll have leakage down here at five to six years. You know, that's my median time to fit leakage for that worst plant.

But I'm going to have plants that, you know, will operate for 60 years without leakage. And this isn't taking into account temperature. This is just saying of all plants operated at 600F, you have enough variability in fabrication procedures, cold work, residual stresses due to welding, material structure, that you can get that kind of variability just from those variables along.

If I include temperature now, what really happens is that if I'm a cold-head plant, I shift this whole thing to the right by a factor of about four.

So I can have susceptible plants operating at 600F. I can have susceptible plants operating at 580. Obviously, you know, for the same degree of susceptibility, the plant at 580 is going to last a lot longer than the plant at 600.

But, from Peter's point of view, I've taken account of the distribution includes all that variability. I get a factor here of something like 30.

Does that seem physically reasonable? Well, we know that the crack growth rates from careful laboratory rates differ by a fact of 20 to 30.

So, it doesn't surprise me at all that the initiation variables vary by a factor of 30. That strikes me as a quite reasonable kind of value. At least the sanity check kind of thing.

MEMBER WALLIS: Aren't you interested now in the tails. I mean the first ones that failed are the ones you're interested in.

MEMBER SHACK: Yes, we'll come back to that.

MEMBER WALLIS: Why is there a kink? There are kinky things, like at 30 years there's a kind in there.

MEMBER SHACK: Well, because these are log-triangular distributions that I've not turned into real time distributions. So, it's a kinky kind of solution.

I've shown here Pete's solution which, again, you know, I claim his distribution is really the average value of this distribution, plus some uncertainty bounds on that average value.

So, his distribution is much narrower than mine. But, again, we're talking about different things. If I then, okay, you know, this is my distribution.

What I really want to find out is to say look at the probability that I'm going to get leakage from head as a function of essentially effective degradation years.

And, because I have a distribution here, I will get a distribution of times to leakage. I can look at any percentile of this that I want. What I do find is that, essentially, if I take my average for my probabilistic distribution, it's pretty close to what Pete computes from his because, again, he's sort of looking at an average and it compares fairly well with my average.

Now, again, if I was just picking plants at random, that's the whole population out there, do I know something more about particular plants? Well, I might know that a plant is operated for a certain number of years without a failure.

And, if it's operated for 20 years without a failure, it sort of stands likely that it really doesn't have nozzles from that tail that's way down at the short end.

And so, well, you know, the real way that you do that is to do a Baysean update. So I take that overall broad distribution I have for all the plants and all experience.

And I'll say, well, if I have a plant that's operated for five years with failures it tells me something. Well, it turns out it doesn't me tell me very much.

So, if I look at my generic distribution and I say I've operated for five years without failure, you know.

MEMBER KRESS: You didn't expect very much.

MEMBER SHACK: I didn't expect -- you know, it doesn't tell me very much. You know, I don't see much there. Now, this is of course years at 600F.

An interesting example is something like South Texas, which has operated for 20 years without a failure. But because it operates at such a low head temperature, it's got about five EDY years, even though it's got many more effective full power years of operation.

But, I haven't learned anything about the distribution of the population at South Texas, you know. Whether it's the generic distribution, or it's better, I couldn't tell. It just hasn't got enough miles on the vehicle to tell me where I'm at.

If I get out to, say, ten years -- again, EDY years, that is years at 600F without failure -- okay, I know something about this plant, it's really much better than my generic distribution, than picking something at random.

And if I've managed to operate for 20 years without a failure, I really know something about this plant. It's much better than, again, picking some plant from my random distribution.

VICE CHAIRMAN SIEBER: Or you're due.

MEMBER SHACK: Or you're due. Well, you always have a probability that things are going to go wrong. But, you know, the question is which way do you bet?

Now, what I do on an individual plant basis I can also do on a broader basis. And it turns out that there's three useful bins to look at here.

One is the sort of generic estimate that I've made over the whole population. Then I look at populations for plants where you have B&W fabricated the head, and you have B&W material.

And that's this ring. And, again, this doesn't take into account anything about temperature.

This just says there's something about the fabrication procedures and the material that's different about that plant than there is from the C fabricated plants that have Huntington and Sandvik nozzles, which are my red curve over here.

And there's an intermediate sort of thing, a B&W fabricated vessel with Huntington nozzles. And so you have these three populations.

MEMBER WALLIS: Why does this -- 30 years?

MEMBER SHACK: Because that's where my nozzle failure times are starting at 30 years. Remember if the nozzle fails at 30 years and I've got 60 for them, the vessel fails at, you know, 69 to the one third power.

The weibull scale factor goes down by a factor of N to the one third.

MEMBER WALLIS: And, the 00

MEMBER ROSEN: It's 53 percent.

MEMBER SHACK: Right, that's the median time to failure.

MEMBER ROSEN: Those are the fabricated CE and B&W for the vessels in this --

MEMBER SHACK: I haven't shown eh Rotterdam, those are point off the curves.

MEMBER ROSEN: All right.

MEMBER SHACK: There is --

VICE CHAIRMAN SIEBER: A different animal.

MEMBER SHACK: A different animal. Again, the ones of interest here, the ones that are still in operation by and large are the CE with the Huntington heads and the B&W with the Huntington heads.

The B&W with the BWN heads are basically out of the population pretty much.

MEMBER ROSEN: What is Huntington?

MEMBER SHACK: A materials supplier.

MEMBER ROSEN: And they made the heads?

MEMBER SHACK: No, they made the nozzle tube material. It's the fabricator of the head and the nozzle tub supplier.

MEMBER ROSEN: Okay. Now I'm going to start doing my Monte Carlo analysis. In a Monte Carlo analysis I pick a random variable. I have a couple of random variables I'm going to look at here, or a couple of distributions I have to worry about.

I have to worry about the distribution of stress intensity factors, the distribution of my weibull initiation parameter, and the distribution of my crack growth rates.

And so I'm going to sample all those in my Monte Carle analysis. But, as Pete observed, these aren't really independent. You sort of expect the material that's very susceptible to initiation to have a high crack growth rate.

A material that has high residual stresses you might also suspect to have essentially a low initiation value. So, again, I think physically it's reasonable to believe that these variables are not independent, they are correlated.

The details of the correlation however, are a little difficult to determine. You know, we know that they are inversely correlated. You know, if I have essentially a short time to initiation, I expect to have a high crack growth rate, or a high residual stress.

I sort of described my degrees of correlation by these so-called windows that you can see here. I'm sort of saying, well, I could have a wide window.

That is, my distribution's going to be centered. If I have something from the 25th percentile on the weibull initiation, then I'm going to be centered around the 75th distribution over here.

But I could have a broad distribution about that. I could have a narrow distribution about that. I could also take a very conservative assumption, that is I'm going to take this bracketed one that says I can't have anything lower, but I can have everything higher.

So that's my sort of conservative distribution. It's a sensitivity study for me, since I know they're correlated, but I can't really describe the correlation very well.

I'm going to do a sensitivity analysis. The other way I differ from what Pete does is that I only do the Monte Carlo on the parameter to determine K and to get the Weibull factor.

I don't sample from the A distribution. I do this all at once with a correlation integral so that instead of doing a whole batch of Monte Carlo calculations trying to determine a low probability of failure, I can evaluate one correlation integral and get a probability of failure.

So, every time I sample from a K distribution and a Weibull distribution, I compute a probability of failure. And so I get a distribution of probabilities.

And, I can then take that distribution of probabilities for a nozzle. I can go into another Monte Carlo simulation where I pick -- to get eh failure of a head I decide whether I have somewhere between one to seven heats of material in that head.

MEMBER WALLIS: What do you mean by failure of the head?

MEMBER SHACK: That means I get a nozzle that ejects.

MEMBER WALLIS: The whole --

MEMBER SHACK: The failure of a nozzle is essentially the crack grows all the way around and it pops. And a head is a head on which I have one such nozzle.

Obviously, the probability of failure for a head is -- you know, since I have 69 chances to win the lottery or to lose the lottery -- you know, it turns out, of course, that when I first did these calculations I did them with a single heat of material, because that's very simple.

You know, I can add those up. But, with this broad distribution that I have it makes a difference if you have more heat, which, all you need is one shot to get one of those bad ones.

And so having more heats essentially is a problem here. It moves my tails around when I do that. And, again, I can do the -- this is, again, a head which receives no inspection.

So my probability of failure is just going up. And an inspection hopefully will knock it down just the way Pete does. What's important here, again, is I get this wide distribution, but my average is actually pretty close.

I'm comparing here with Pete's Bates case. So my average will turn out to be close to Pete's base case. Although, again, you get a range of materials.

His highlighted yellow will be up close to my 95th percentile. So, you know, he's really bounding things pretty well. Again, for a distribution that we're arguing is actually conservative, because most of those heads have been replaced.

Now, you can sort of see that just lowering the temperature, again, that really moves this whole thing to right, and so, at a given time, my probability, you know, was the average, is now the 95th percentile by lowering this thing ten degrees. So, again, I get a big benefit.

MEMBER WALLIS: But we got this this morning. Is this temperature really uniform? Aren't the bypass flows ant things and the mixing underneath that head?

It's probably not your subject, but isn't there some --

MEMBER SHACK: Just considering --

MEMBER WALLIS: IS there temperature distribution on these heads? So they could be 600 in one part and 590 in another?

MEMBER SHACK: I don't think so.

MR. RICCARDELLA: I looked at head temperature as a random variable. And I looked at it with essentially zero variability. And I looked at it with a five degree --

MEMBER WALLIS: But that's in your model. But in reality, isn't there a flow distribution in there?

MEMBER SHACK: Yes.

MR. RICCARDELLA: Well, that's why I put it in as a random -- been treated.

MEMBER WALLIS: How about the flood mechanics on -- isn't there -- does anybody know what the temperature variation is in that?

PARTICIPANT: It's been measured in a couple of plants.

VICE CHAIRMAN SIEBER: But not with a lot of precision. It's like ten degrees.

MEMBER WALLIS: Really.

MEMBER KRESS: I think ten degrees is a lot of difference on those.

MEMBER ROSEN: I would say that my

intuition is that it's not that high. I mean, we've got this massive head --

MEMBER KRESS: That's exactly right.

MEMBER SHACK: There's fluid temperatures, and then, of course the metal conduction kind of smoothes that out. I don't know what the answer is.

VICE CHAIRMAN SIEBER: But there's variable air flows too. Where the insulation's closer, there isn't a lot of air flow. Down below there is.

MEMBER WALLIS: I would think about the fluid flow. Aren't there bypass flows that come up there?

VICE CHAIRMAN SIEBER: There are some.

MEMBER SHACK: It's going to be streaming.

VICE CHAIRMAN SIEBER: That depends on the plant too.

MR. RICCARDELLA: But if you're looking at the collection of 69 nozzles, and some of them are a little hotter, and some of them are a little colder, on average, the probabilities come out to be about the same.

We ran that as a sensitivity study. And it didn't change the results significantly.

MEMBER SHACK: You know, I have the

feeling that when I look at the range of distribution I get from the weibull, you know, the possibility I could have there, it sort of swamps out all these others.

And that's what's going to drive the major uncertainty in this result.

VICE CHAIRMAN SIEBER: I think that one fair assumption you could make is that the distribution is relatively the same from one plant to another.

In other words, the average head temperature tells you more about what the hot nozzle is doing than some differences in flow distributions and temperature distributions inside.

MEMBER WALLIS: It's all random. It's probably cooler on the outside than the middle or something. And then there are different stresses on the outside than the middle. So, there's a correlation.

VICE CHAIRMAN SIEBER: That's true.

MEMBER SHACK: Again, the benchmarking, you know, we've done this now, let's try to get back and see if we can predict something out there in the real world to give us some confidence in what we're doing.

One thing I've done here, I've looked at my three populations that I consider, my B&W-B&W, B&W Huntington, CE-Huntington. In my population of plants I have -- again, when I say nozzle leaks, I'm also like Pete, I also include significant cracks in that nozzle leak.

And so, you've observed 56, 10 and four. The model predicts 45, 13, 18 with standard deviations. And, again, the argument is, well, okay, you know, it's statistically consistent.

The only one that's a little funny is the CE Huntington. And that's sort of because when you did the Baysean update for the CE Huntington I used this prior that included everything, including these bad plants.

And what I'm saying is I don't have enough experience yet, you know, out there in the real world, to drag me all the way down. But I've gotten some improvements.

So, I'm not surprised that I'm sort of on the ragged edge here. I need every bit of standard deviation I can get to get the observed within my expected for the CE Huntington.

But that's okay. With more inspection experience, and I continue to do the Baysean updating, I would move the distribution. I'd probably get closer and closer.

But this is all the credit I'm willing to give them at this point with this much experience, because I don't want to do the Baysean update just on that limited data alone.

I want to include all the ranges of possibilities that I have in my whole population.

VICE CHAIRMAN SIEBER: Can you tell me how many heads are in each of those three categories?

MEMBER SHACK: Right off the top of my head I can't.

VICE CHAIRMAN SIEBER: Is there a lot of heads with B&W nozzles that would account?

MEMBER SHACK: No more.

VICE CHAIRMAN SIEBER: There used to be.

MEMBER SHACK: There used to be. Okay. Now, we've taken care -- but, again, the leaks, you know, all that says is I've done the sums right, that I took the field data, I fit things to it, and then I did Baysean updates on it.

Well, you know, hopefully I better get that stuff skewed out of here.

MEMBER WALLIS: What about the heats? It seems to me you're not going to get around the selection of heats. Heats are made in batches. Isn't it likely that if made at a certain time that more of them went to a head that were just done at a certain time, that's all?

MEMBER SHACK: Well, when I do the sampling, you know, I have a chance of having one heat in the head -- I could only make --

MEMBER WALLIS: It's random.

MEMBER SHACK: For me I can only draw from populations. I can't narrow my predictions down very much. This is as far as I can go on a generic basis, is to look at these three populations.

If I had a particular plant that I knew had operated for 10 EDY without a failure, then I could update and tell you about that plant. But, in general, this is as localized as I can get.

And I think we want sort of generic things here. In setting up the inspection plan, we're not going to have an inspection plan per plant.

We're going to have an inspection plant that looks at the whole fleet. Again, the circumferential cracking, what I've looked at are these cumulative distributions because that's all I can predict from my model.

I can't predict anything else. I only

sort of -- I've thrown out all information on anything except whether I have a crack that's bigger than either failure or one of these other things.

And so, again, this is my observed size versus angle. These are my predicted size versus angle. And this is my standard deviation. And I should mention that these calculations are done with my broad correlation window.

And to me that's my base case because physically that's the way I picture things looking. I would say that the stress intensity and the crack growth rate are correlated with the initiation variable.

But it's a broad correlation because there's all sorts of things that affect initiation. The only part of the thing that affects the initiation and relates to the crack growth rate is the material structure.

The crack growth rate is sort of independent over whether I have surface -- work, whether I have high residual stresses. That's really a material property.

And the same think, I expect a correlation between the initiation and the stresses. But the stresses are only one thing that affect the initiation.

And so, because of all those other variables, I would expect my correlation window, I would expect a broad correlation window to be the most reasonable one.

And it becomes my base case. Well, over here are my sensitivity studies, where I look at the different correlation windows. And, again, I'm measuring these against the predicted big cracks greater than 165.

And, again, I sort of squeaked through with my broad window. I get 1.1 with enough standard deviations to get me out to my observed two.

But, you know, if I do the narrow window, it's sort of interesting. My mean value is the same, but my standard deviation goes way up. So, statistically, I can drag my observation better in.

This is my I lean the thumb on the thing just to bias it a little bit in the conservative direction. So, I'm willing to let you have higher crack growth rates, but I don't let them go down as much.

And that gets me up again to something that is close to the reality. And this is my most conservative correlation window. You can't go any lower.

But you can go all the way to the top. And, again, I get --

VICE CHAIRMAN SIEBER: Front stop, but no back stop.

MEMBER SHACK: Front stop. And, again, the way to interpret this is, you know, there are variabilities. I could have had anywhere from zero cracks to four cracks.

It was the luck of the draw that I had two. You know, that's just the way it turned out. Which of these values should you use? That's a good question.

My own personal opinion is that I would use this correlation window, because to me it's the physically most reasonable. I'm statistically consistent with my observations.

You could also argue that you should pick one of these two windows because you don't know the correlation. And when you're ignorant you pick a conservative value.

But, again, contractors propose, and the staff disposes. So, whatever they chose to use they will use. One version of this model has been supplied to Steve Long to use as essentially a tool to use for his significance determinations procedure when he has a crack to dispense with.

But that's the summary, just what you've seen already. Again, to come back, I think that my average values will turn out to be very close to Pete's especially when he uses his highlighted yellow version, which ups the things.

Again, so, instead of bounding just my average value, he'll impact and bound even a larger portion of the population. And, as I've argued, we're both dealing here with a population that's conservative now, compared to the real world population of plants that we're dealing with.

So, although we have large uncertainties, we can make useful decisions about inspection programs based on, I think, the information we do have.

MR. RICCARDELLA: Just a quick comment. If you go back to the previous table, when you get into what we're really doing, is we're making decisions about different inspection programs and looking at the effect of them.

We could use any one of those windows. And, if you come to the same decision on each one of them, then you know it's a good decision. And I think that's the way these types of probabilistic analysis should really be used.

MEMBER WALLIS: Or you can take the sort of extreme thing and be careful, take the worst --

MR. RICCARDELLA: But I'm saying, you take the worst and you look at three different inspection scenarios. Then you take the average and look at three different inspection scenarios.

Then you can take the best and look at three different inspection scenarios. And if you come to the same decision regarding those inspection scenarios, then I think it's a valid use of the probabilistic analysis.

MEMBER SHACK: I guess my -- I'm a little reluctant here, if I'm using a conservative model already for the initiation I sort of hate to head them again with another conservative model for the crack growth.

But again, as Pete says, if it doesn't make a difference in the decision you come to, that's fine. You know, you've got that comfort that you've got a conservative model.

CHAIRMAN FORD: Any other comments?

PARTICIPANT: The only comment I have, and maybe I don't understand this completely, but there seems to be an implication here that if you operate for 20 years on a plant that somehow that changes the probability in the future of having a failure.

And, to me, if you have a probabilistic system its' somewhat like saying well I flip a coin and I got nine heads. Now that changes the probability of getting a --

MEMBER SHACK: No. What it says is I can have any -- or, you know, something that characterizes susceptibility of my head. If I have to pick that at random it could be anywhere from this to this.

By operating for 20 years it says no it can't be this, it has to come from some narrower --

MEMBER ROSEN: Try this one on. If you throw the dice 20 times and you get the distribution you normally would expect when you throw dice, instead of getting snake eyes every time.

The first time you would say I probably have a pretty good set of dice. The second time you would probably say these dice are loaded if you get 20 snake eyes.

And that's really what this is saying, that if you run 20 years you probably don't have a loaded set of dice. You probably don't have the bad heats. I think that's what it's saying.

MEMBER KRESS: My loaded dice gives me

sevens and elevens on snake eyes.

MEMBER ROSEN: It depends.

MEMBER SHACK: It doesn't give you servitude here. You still have a probability of having a pretty bad hunk of material here. It's just that the probability is a whole lot lower than it is if you just say I don't know anything about this plant, it's just some random plant out there.

I could have picked it from anywhere. You have learned something from operating for 20 years with no failure.

VICE CHAIRMAN SIEBER: Or you could be due.

MEMBER SHACK: But it doesn't change the actual probability that that plant's going to have a leak. All it does is tell you the way to --

MEMBER WALLIS: Probability is your state of knowledge.

MEMBER SHACK: Yes.

MEMBER WALLIS: There is no such thing as a probability. It's a part form your state of knowledge. There isn't some absolute measure of probability.

CHAIRMAN FORD: What I would like to do is just to go around if we're finished with Bill for the time being. Unfortunate choice of words. I'd like to go around the table and just for a quarter of an hour, talks and just get impressions of the day's --

VICE CHAIRMAN SIEBER: Well, I think a lot of work has been done. And I'm very impressed and I learned a lot today. And I don't see anything that's been done that's inconsistent with good practices and where the staff aught to be headed.

And so I guess my comment is congratulations on the work done so far, and I hope that it's useful in the future.

CHAIRMAN FORD: I guess you're -- Tom?

VICE CHAIRMAN SIEBER: You can say about everybody else's stuff.

MEMBER KRESS: My impression was that the industry has taken this very seriously in putting resources and time into it, and have what looks to me like a really good comprehensive program to deal with this issue.

I only have one real problem with what I heard. And that has to do with linking the Davis-Besse event in your failure modes and effects with the leak rate.

I really think that's problematic. And I think it needs to be revisited and re-looked at. I also had some problems with the wastage being related to atmospheric pressure boiling water.

I think we need some better understanding of how boric acid concentrates if you're boiling away at low pressure versus high pressure. I think that needs to be looked at a little more.

So far the alloy 690 looks pretty good to me. You guys were scaring me for a while. But, from what I can see, it looks pretty good.

VICE CHAIRMAN SIEBER: It looks as good as 600 did 30 years ago.

MEMBER KRESS: Is that it? I thought some of the EMC findings were very interesting, particularly having to do with are leak before break criteria and the variation in crack size versus leak rate.

I thought those were very interesting findings. I think we need to make use of them in some way. Shack of course was excellent, so that's all I'll say about it.

My overall impression is of a very good piece of working going on here. And I congratulate the people doing it.

MEMBER ROSEN: I've got to go, so could I?

CHAIRMAN FORD: Yes, of course.

MEMBER ROSEN: I just would say that Shack's stuff was average. Which is to say -- his stuff is excellent after all the -- but this was just excellent like all the other stuff.

So I would call it average. The industry has taken it seriously. I agree with that point. It's pretty obvious given the amount of money they are putting in to it.

I agree with a lot of the other points that were made. One thing that's encouraging is that there hasn't been the problem I envisions a year or so ago of one day we do an inspection and it blows this temperature thing completely out of the water.

That hasn't happened yet. So EDY calculation -- I would call it for all the other complexities that we know are there. It's still holding up.

MEMBER SHACK: Well, I would argue that we have considered a lot of other complexities here. And they are in the model. And the inspection particular implicitly includes them, if not explicitly.

VICE CHAIRMAN SIEBER: I agree.

MEMBER ROSEN: But all we do is count to see how many EDY we've got and put the plants into that category. And what I'm worried about one day is we'll get -- what I have been worried about, but continues not to have happened.

The inspections continue to hold up. All you have to do is know the EDY. And you will know whether or not you've got a problem. So that's a good thing.

VICE CHAIRMAN SIEBER: I think actually what you're saying should be thought of in terms of what Bill said when I said here's four different manufactures of heads.

And we knew there was some bad heats out there and some other factors. And Bill Wisely said well they're gone now. They're out of the database.

And maybe that's what we're seeing, that when you eliminate these bad actors from the pool of vessel heads that are out there, it becomes more time and temperature related than it would have right in the beginning.

And that sort of supports your arguments that you've been making as to how to interpret your results.

CHAIRMAN FORD: Graham?

MEMBER WALLIS: Well, this crack stuff is very impressive. It's always miraculous to me how you can take stuff which has all kinds of tremendous uncertainties and then, by doing this great statistics and -- it comes down to something which looks as if you were really able to predict things.

And it's very impressive that way. I think the crack stuff is probably the most impressive stuff. Where I feel I really didn't get time to go into the relationship between the cracks and the leaks.

It was covered a little bit, but in a very summary way. And I'm not quite sure what the bridge is when you assume the thing suddenly goes to 30 degrees, how much does it open up?

Can you predict the leak rate realistically from -- in this sort of knowledge about cracks. And in the leaking there's flashing going on and so on.

And if flashing goes on, you get concentration of the acid. And I didn't see all that. I don't know how you go from this microscopic crack to something which is a significant leak, which them some how manages to pool acid in the right concentration.

A lot of stuff seems to be missing there, that we didn't hear about today. I think in the -- I heard today that you can get 6 inches per year dissolving rate.

What's the realistic thing? What is realistic about the kinds of boric acid concentrations, the kinetics, the interplay between the kinetics and the flood mechanics and the temperature fields and all that?

I think those areas need more investigation. Also, with all of these things, I hear these wonderful engineering scientific presentations.

And then I wonder what the industry's going to do with it. They are going to do something with it at a much more elementary level. How are they going to make decision. I haven't really seen that.

MEMBER KRESS: They are going to determine in the inspection procedure in intervals, probably.

MEMBER WALLIS: They are going to have to make some judgment decisions, I think, along the way too. Probably they are going to have to say well, we don't really know this well enough, so we're going to be a little more conservative than might be predicted if you believe all these.

VICE CHAIRMAN SIEBER: I think the inspection schedule may end up being the same regardless of how fast the corrosion rates occurs. You know, you can --

MEMBER WALLIS: Well, every outage would

be a thing --

VICE CHAIRMAN SIEBER: That's where you are right now in susceptible.

MEMBER WALLIS: Maybe you can't back off from that.

VICE CHAIRMAN SIEBER: Yes.

PARTICIPANT: I thought it was a lot of very impressive work and I can see its application to the regulatory space as well as future plant designs.

And, in a way, I'd like to hear a lot more about this kind of thing from the Westinghouse and GE's, in terms of being assured that they are on top of these kinds of problems and can fold this kind of consideration into future plants.

It's pretty interesting too to see that through this Baysean updating you can take plant data, I guess, and predict more about the plant's ability to operate in the future, which would lend more credibility to the license extension process.

MEMBER KRESS: You reduce the uncertainty about distribution and it's behavior in the future.

PARTICIPANT: Right. By learning more about what I guess the real distribution is of these factors within the plant. You sometimes wonder if these kinds of issues couldn't be avoided if the manufacturers or designers would take more of this kind of thing into consideration in future plant designs.

MS. WESTON: Yes I think that the work is quite impressive. However I still have a concern about the length of time that it takes to complete some of this work and apply it to the situations and issues that we have here at NRC

CHAIRMAN FORD: From my point of view, I really do like the idea of the MRPs, FMEA -- whether that will be used by the NRC is to be decided.

But it will be something along those lines, I suspect. And therefore I'm a wee bit frustrated like we all were I think, by the fact that we didn't see a lot of data an analysis detail.

And it's understandable given the time that we had today. And so, what I would like to see is some time in the Fall, once the staff have looked at this in detail and have come to a conclusion about it that we would have maybe another day and half meeting to go over this in some detail.

Prior to that I hop you will get the MRP report so that we can read it beforehand and get some of the details. On the cracking issue I'm still concerned about this factor of 30 that you've heard me talk about, which may be -- way in the plant, certainly in the laboratory, and how that is -- 30 increase crack propagation rate for certain crack orientation, and how that melds into some of these probabilistic approaches.

I'm still concerned like we all are about this boric acid wastage. We tend to be pushing it away now. It's not really a problem. We only need just one more, six inches or one to five inches per year and it might miss the inspection schedule.

And then where do we stand? I'd like to really have a prediction of what are the conditions in the actual head, in terms of geometry of the head and leakage rate, and relate that to --

I'm still concerned about inspection techniques. I feel better than I did a year ago. But, even so, I'm still concerned that I'm not getting positive group answers from the staff in terms of their understanding of probabilities of detection and the consequent risks associated with that.

I agree with Tom that he's convinced that alloy 690 is good. I think it is. But, being a devil's advocate, if we had a crack originating form something else, would it propagate into the 690.

It's the situation we've had with allow

steels before now. And the funny thing is that this repair -- we keep on hearing some of the -- coming on with repairs, of using half shells, etcetera.

All we've seen is rather sketch engineering drawings, which are shown almost under the table, so to speak. I'd love to look at some of those engineering aspects of repairs associating with half shells, perhaps more detail.

But I'm really pushing for in the fall or the early winter a one and a half day meeting with lots more data than we were able to see in this very brief meeting today. But I'd like to thank everybody.

VICE CHAIRMAN SIEBER: Before you close, I guess there was one slide that I had some concern about that was Gery Wilkowski's slide number five. It talks about leak before break, saying that when you recalculate leak before break, which is applied, you know, throughout the plant you no longer get the safety factors in all case.

And so, to me, leak before break was used some time ago to remove pipe strength from PWRs and so forth. The question is, are we finding ourselves approaching an unrealized condition or is the margin disappearing or what have.

MEMBER KRESS: Yes, that's why I thought

that was a very interesting finding also.

VICE CHAIRMAN SIEBER: So, I'd like to hear more about that some time in the not too distant future. Because, to me that was the --

MEMBER KRESS: It sort of raised the red flag.

VICE CHAIRMAN SIEBER: That was the startling moment of the day for me. So, it's slide five in the EMC set of slides.

CHAIRMAN FORD: Okay. At that point I'd really like to thank everybody, and especially giving up part of the memorial day for those of us who have come up from out of town. I hereby adjourn this meeting.

(Whereupon, 5:30 p.m. at the above-entitled conference was concluded.)