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ANN RILEY & ASSOCIATES, LTD.
1025 Connecticut Ave., NW, Suite 1014
Washington, D.C. 20036
(202) 842-0034

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

OCTOBER 12, 2000

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UNITED STATES

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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AD HOC SUBCOMMITTEE ON DIFFERING

PROFESSIONAL OPINION ISSUES

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Thursday, October 12, 2000

U.S. NRC

11545 Rockville Pike, Room T2-B3

Rockville, Maryland

The above-entitled meeting commenced, pursuant to notice, at 8:30 a.m.

PARTICIPANTS:

Dana Powers, Chairman, ACRS

Mario Bonaca, ACRS Member

John (Jack) Sieber, ACRS Member

Thomas Kress, ACRS Member

Ivan Catton, Consultant

James Higgins, Consultant

Ronald Ballinger, Consultant

Jack Strosnider, Division of Engineering, NRR

Jack Hayes, Probabilistic Safety and Assessment Branch, NRR

PROCEEDINGS

[8:30 a.m.]

DR. POWERS: The meeting will now come to order. This is the third day of the meeting of the Ad Hoc ACRS Subcommittee on Differing Professional Opinion Issues.

The purpose of this meeting is this subcommittee will review the technical issues contained in the differing professional opinion on steam generator tube integrity.

The subcommittee will be hearing from the NRC staff today.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Mr. Sam Duraiswamy is the designated Federal official for the meeting. Ms. Undine Shoop, a staff member who is assisting the panel, is also present.

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

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

Do any members of the panel have any opening statements they would like to make before we get started on today's proceedings?

[No response.]

DR. POWERS: Seeing none, I will turn the floor over to Mr. Jack Strosnider, Director, Division of Engineering, NRR. Welcome, Jack.

MR. STROSNIDER: Thank you. Thank you, Dr. Powers, and I want to thank the subcommittee here and consultants for your time and effort that you're putting into reviewing the steam generator issues. I wanted to started off with that acknowledgment.

As Dr. Powers indicated, I'm Director of the Division of Engineering in the Office of Nuclear Reactor Regulation. Historically, the Division of Engineering has had the lead responsibility for steam generator integrity issues, specifically related to inspection and maintenance sort of activities.

But I think as everyone is aware, steam generator issues are really a much more multi-disciplined effort and if you look at my second slide, which is an abbreviated version of the agenda, as you can see, in the next few days, we're going to be talking about a lot of different technical issues, ranging from iodine spiking and metallurgy and NDE to probabilistic risk assessment, thermal hydraulics, and, ultimately, integrated decision-making.

So you'll be hearing from staff both from the Office of Research and from NRR and from a wide variety of different branches and disciplines.

If you look at the more detailed agenda that you have, the names that are listed there are the principal speakers. I would just point out that they may be relying on other staff members to help supplement some of their discussions, and I would remind the staff that when they're asked to do that, to use the microphone and identify themselves.

We've tried to arrange these issues in some sort of logical order. Basically, these various technical issues are all issues that feed into a more integrated assessment and culminating tomorrow afternoon, we're going to talk about the integrated decision-making process, which is what I described in Reg Guide 1.174 on how to do risk-informed license amendment reviews.

And we're going to talk about two specific examples, one which was mentioned yesterday. That was the Farley review, in which the NRC found the risk-informed amendment request acceptable, extended the operating cycle.

We're also going to talk about review earlier this year on Arkansas Unit 2 regarding a risk-informed amendment, which the staff followed this process and we found that amendment unacceptable. The plant subsequently shut down to perform steam generator inspections.

The presentations that we're going to make will cover the DPO issues. However, they go beyond that. In response to the agenda that we were provided, we are going to talk about additional issues.

I think that's an appropriate thing to do, because I think it will help to provide some additional context for the issues and really bring you up-to-date on the whole area of steam generator regulation.

With regard to NDE and cracking phenomena, I think there's going to be a little bit of rearrangement in the way we present some of that. Based on the discussions yesterday with regard to Generic Letter 95-05, I think it's very important that we spend a little more time on that than we might have originally planned.

Frankly, I don't think the committee was left with the clear understanding of exactly what's in that letter or the basis for it or our experience with its implementation.

So when Ken Karwoski talks about these issues, he's, first, I think, going to talk about the regulatory framework in general, but then I think he's going to talk to Generic Letter 95-05. We want to take that as sort of a unit before we move into other NDE and cracking phenomena issues, because I think we need to be careful. There is a potential to mix different issues.

And Generic Letter 95-05 is a very specific -- deals with a very specific mode of degradation and there are some very specific requirements, and we want to make sure that's clear for everyone.

The final thing I would mention with regard to the agenda, I've put some hours in here which I hope roughly correspond to what's in the more detailed agenda you have.

The main reason I did that is just to point out that I think it's important that we do try to stick to the time schedule. Obviously, the staff is here, it's your meeting, and we're going to respond to what you want us to respond to.

But as I indicated earlier, all these things, we want to try to show how they fit together and add up to this overall process. So I would just encourage that we do watch the clock.

I would also point out that there has been an extensive amount of background information provided. My understanding is you've got about a foot-and-a-half of paper, literally.

DR. POWERS: It's about twice that.

MR. BALLINGER: Eighty-nine pounds.

MR. STROSNIDER: Eighty-nine pounds? Okay. But it goes to a point. There is an extensive amount of documentation on these issues. However, I'm somewhat sympathetic in that I suspect you might suffer from some information overload there. So as we go through these issues, if there are specific questions that come up, I would offer that we could help point to the right reference and maybe the right location of that reference that you could study later to help address some of your concerns.

DR. POWERS: Understand the weight is not usually an indicator of content.

MR. STROSNIDER: Okay. If I could move on to the next viewgraph, Dr. Hopenfeld presented, yesterday morning, a very detailed timeline that he went through. This is my abbreviated version.

I was hoping to make perhaps a few bigger picture observations. If I start with the lower line on here, it talks about the DPO activities and you're familiar with when the DPV and the DPO were submitted.

I did indicate in here one ACRS meeting that was held in October of '97 on the DPO consideration document. I went back and I'm not sure I've got an accurate count, but I think I'd just mention, for the record, perhaps, that there have been 11 ACRS meetings from 1994 to the year 2000 on steam generator issues.

Seven of those dealt explicitly with the DPO issues and it's several of those Dr. Hopenfeld did make presentations.

Having said that, I recognize, in Dr. Powers' memorandum, I think it was September 11th, to the EDO, that the intent is to take a fresh look at these issues, notwithstanding that there have been prior meetings. And I think there is merit to that, also, because things have evolved. We have information today that we'll be presenting that wasn't available in prior meetings. So I think that's a worthwhile thing to do.

But I did want to point out for the record that these issues have been discussed publicly in the past.

If we could look then at the upper timeline for a minute, I wanted to focus for just a second on what went on here with steam generator rulemaking, Generic Letter NEI-9706, and there's -- I guess the best way I can say it is that there is some frustration for everybody perhaps in how long it takes to see things happen.

But I did want to point out there are some processes that the staff follows in these sort of activities. The processes were established to provide appropriate checks and balances, opportunity for public participation, opportunities for presentations to ACRS, CRGR, those sort of activities.

So it does take time. I continue to read and hear about the failed steam generator rulemaking. I just wanted to comment on that, because I guess I can understand the perspective that, yes, the staff said they were going to embark on generating a new rule, and that in the end, we didn't do that.

But what I wanted to point out is that part of the rulemaking process is to go through a regulatory impact analysis to determine whether the rule is justified, and there are several different ways it can be justified, but nonetheless, we went through that.

It involved a lot of work. It involved some groundbreaking risk assessment to support the evaluation, which subsequently was published in NUREG-1570. It took time to do that.

In the end, when we look at the criteria, it didn't support the idea of a rulemaking. I don't look at that, personally, as a failure. I look at that as we followed the process, we looked at the results, and that was the outcome. We also recognized when we finished that, though, that there were some improvements that needed to be made within the existing regulations and regulatory framework, and, of course, that led us to the generic letter.

There was some suggestion yesterday that abandoning the rule had something to do with the fact that the industry might not like a steam generator rule. That was not why the rule didn't happen. It was because of the reg impact analysis.

When we started looking at the generic letter, and we did have a lot of interaction with the industry, and I think the staff had some influence on them. I'd like to think they saw the technical and safety merits of some of our issues, and they developed this industry initiative 9706.

This is a good thing. I think we need to credit the industry for taking that initiative, and I'll just give you a few examples on that.

The existing steam generator tech specs basically say plug at 40 percent, except for where some alternate repair criteria would ever have been added, but for most plants, it's shut down, do the inspection, plug at 40 percent and you can restart. There are no explicit requirements to do a condition monitoring of the steam generator, to understand exactly what the condition was at the end of the cycle, or to forecast or do this operational assessment.

By 9706, all licensees, all PWR licensees have committed to do those and they started performing those. So that's a good thing. Those two things alone make this a very important initiative.

MR. HIGGINS: Jack, you said all licensees have committed to 9706. Is that a formal commitment or informal?

MR. STROSNIDER: We have a written commitment and I don't remember if it's on each docket or if it's through

NEI. I think it may be through NEI. But we have a commitment that all the PWR licensees will follow the guidelines in 9706.

I would also point out that doing that did not relieve licensees from meeting any of the existing regulatory requirements. In fact, we sent a letter to NEI, which was distributed to the licensees to make that clear to them.

So this has been a good initiative and if you follow this timeline, it didn't put on here direction-setting initiative 13, the Commission guidance to interact with the industry to look for voluntary initiatives, and it made sense, when we started looking at what was happening in 9706 and in the generic letter, that we should work on that approach, and that's what has been happening.

I would indicate that -- and I'll talk a little bit more about this in the summary tomorrow, but we have put this effort on hold following the Indian Point 2 steam generator tube rupture, so that we can factor lessons learned from that event into our review of this steam generator licensing change package, which is 9706 and more. This actually involves some new and improved tech specs.

But we haven't gotten to approving that yet and we're going to factor in lessons learned from the most recent tube failure event.

So that's what we'll talk about, how things have evolved. The other thing I wanted to point out here is a lot of the discussions that we had yesterday and that we'll have in the next two days deal with the risk-informed approach to addressing steam generator tube integrity issues.

I put a couple things on here. If you look at NUREG-1477, there was some risk analysis in there. It didn't deal supplementary with the area of severe accidents; that is, core damage events leading to tube rupture, except in a qualitative way.

I would remind people that back in the early '90s, when we first reviewed some of these proposals by the licensee for voltage-based approaches, that we were still doing a very much deterministic licensing basis sort of review. When we got to this point, we tried to factor in some of the risk perspectives.

As you move off in time, as I indicated, one of the major things that happened with the steam generator rulemaking was the development of NUREG-1570 and the work that was in there.

It was very important work, because it dealt, again, with some sequences that weren't specifically addressed before.

Finally, I put a milestone on here of the issuance of Reg Guide 1.174. That's the first time that the staff actually had Commission-approved and formal guidance on how to apply risk-informed regulation in reviewing license amendments.

So you have to keep in perspective that this has been an evolving process. As such, some of the information that you're going to hear has evolved with time. And, again, I'd mention I think it's worth going back and reviewing today's state-of-the-art as opposed to some of what we can talk about historically.

So those are a couple of the major points I wanted to make with regard to the timeline. But one other thing I wanted to come back to, when we talk about this process and the time it's taking to work toward this improved framework, we're often questioned, well, what about safety.

I would point out that during that timeframe, and I counted them up last night, the staff issued seven generic communications on steam generators, ranging from subjects like the importance of optimizing the inspection methods you're using to how you deal with circumferential cracking, to dealing with U-bend cracking, externals, degradation of steam generator secondary side components.

So we have been dealing with the issues as they come up, interacting with the industry through generic communications and through our other processes for dealing with the licensees.

Finally, I'd like to talk just briefly on, I guess, maybe I suggestion on what I think you need to be looking for in terms of resolution of some of the DPO issues. You're going to hear, in some of the areas we talked about, some specific technical answers, if I can characterize it that way, and one example might be the first presentation we had this morning on iodine spiking.

You're going to see where work was done, parametric studies were performed, and we concluded that certain assumptions are valid for performing these analyses.

But there are other issues where you're going to hear that it's more of a process resolution and I'm not talking about the same -- well, in some cases, it might be the process I talked about earlier. Some of the recent issues with regard to vibration and dynamic loads during blow-downs.

We put in the GSI process and it's working through and you'll hear something about that.

But there's another aspect of this. Some of the issues that come up, particularly from a risk-informed perspective, when we start talking about probability or frequency of bypass events and that sort of thing, what we concluded from some of the work was that various alternate repair criteria for steam generator tubes or ultimate repair methods can, in fact, influence those frequencies.

Now, we don't know, the NRC staff doesn't know what the next alternate repair criteria is that the industry is going to send in to us for review.

So we can't, ahead of time, come up with here's a specific solution. But what we have said is that we will review those things considering the risk-informed aspects. In fact, with regard to 9706, as an example, the industry guideline document, and the tech specs that have been developed, the industry very much would have liked to have had freedom to define their own alternate repair criteria, to define their own alternate repair methods.

We have, in working on those tech specs, indicated that, no, whenever you come up with a new alternate repair criteria or a new repair method, you need to come to the NRC staff for approval, and the reason we did that is so that we can look at it from a risk perspective and determine what the impact will be with regard to some of these risk-informed aspects of the issue that we've been studying.

Unfortunately, we don't have guidelines out there at this point where the industry could pick it up and do it themselves. That's what I mean by a process resolution.

We committed to look at some of these things as part of our reviews. I think when you hear the discussions on Farley and Arkansas, you'll see that we're doing that.

There may be discussions about assumptions that are made, how we do those analyses, and I don't think that that should be a surprise, given that, again, these are some of the first ones that we've done and those discussions are good.

But the point I want to make is that where we said we were going to include these things in our evaluations, that we did that, and that's the way some of these issues have to be addressed.

So that's basically the opening comments I wanted to make. I'd just ask if you've got any questions for me.

If not, again, we appreciate your time, and I will turn it over to Jack Hayes. I think he's our first speaker on iodine spiking.

MR. HAYES: Good morning. I'm Jack Hayes, and I'm with the Probabilistic Safety and Assessment Branch, and I will be discussing that aspect of the differing professional opinion which deals with iodine spiking this morning.

This morning, I'm going to be discussing the DPO author's concern. I'm going to be discussing the staff's assessment of that concern, but I think it's really important to understand that it was not the DPO's concern that had us address iodine and spiking.

That was part of an overall reassessment we were doing with respect to accident analysis. So to understand how we arrived at our assessment of the DPO concern, I think it's important to understand the staff's reassessment of iodine spiking as a whole and the conclusions which we drew with respect to iodine spiking.

Now, the DPO author's premise is the following. If you have a reduction in the tech spec value of primary coolant activity level of dose equivalent iodine-131, which is typically one microcurie per gram, to low activity levels, that that may result in a spiking factor which is greater than 500.

Now, if you had that situation where you reduced activity levels and you have a spiking factor greater than 500, then the premise is that the consequences you'll have if you have a main steam line break accident, that Part 100 dose limits will be exceeded. That's the premise.

DR. KRESS: A question.

MR. HAYES: Yes, sir.

DR. KRESS: Have these tech spec changes been approved?

MR. HAYES: Yes. It's tech spec values lower than one microcurie have been approved.

DR. KRESS: What levels have they been taken down to.

MR. HAYES: I think down to probably the lowest has been about ten-to-the-minus-two, about one-times-ten-to-the-minus-two, that ballpark.

DR. KRESS: Okay. Thank you.

MR. HAYES: Now, in order to understand this further, there's some background areas we believe you need to discuss. One is iodine spiking, what is it. We think you need to understand that how do you incorporate iodine spiking in the calculations of releases associated for a main steam line break, and then how does this figure in with the voltage-based criteria that licensees implement.

Now, what is iodine spiking? Well, it's an increase in release rate from fuel to primary coolant resulting from a transient. Most of us like to deal with mathematical expressions. In essence, it's a release rate from post-trip over the release rate at steady-state or release, if you will, pre-trip. That's a way to define it.

Now, the question is how does it occur. Well, in order for iodine spiking to occur, you have to have a fuel defect. If you have a fuel defect, you will get water into the gap associated with the fuel pellet.

When that water enters, because of the large delta T between the fuel pellets and primary coolant, some of that water is going to vaporize. Now, at the beginning of a transient, the fuel pellet temperature decreases. This causes the steam which is in that fuel rod gap to condense.

Once it condenses, it causes an imbalance between the reactor coolant and between the fuel. You get additional water which enters. Now, because the fuel pellet is still at a much higher temperature than the reactor coolant, it causes some of the entering water to vaporize and sets up a local delta P, such that as you reduce temperature in the fuel, you're going to get water which will enter back into reactor coolant.

That's what the spike is. Now, as the fuel temperature decreases, eventually you will shut off the spike.

Spikes occur, power transients, startup, shutdown, typical occurrences. These are included in the analysis which we perform for steam generator tube rupture analysis and main steam line break accidents.

DR. CATTON: This kind of says that the post-trip release is somewhat independent of the steady-state release rate, because you're really sort of clearing out the iodine from the fuel itself.

MR. HAYES: Yes.

DR. CATTON: It's kind of separate.

MR. HAYES: Yes.

DR. CATTON: That would be why the ratio would increase when you reduced.

MR. HAYES: That's correct.

DR. CATTON: Do you account for this?

MR. HAYES: Yes, we do, and we'll be going right through it right now. It's a good lead into how we do these calculations.

When we do a main steam line break accident or a steam generator tube accident, we usually presume that the calculations are done at tech spec values. For example, normal operating primary to secondary leak rate, this value is typically one gallon per minute.

The primary coolant activity level is also a tech spec value. This is typically one microcurie per gram of dose equivalent iodine-131. And then the spiking factor which we assume in these calculations is a factor of 500. Now, for this particular scenario, the dose acceptance criteria is 30 rem thyroid for the exclusionary boundary, EAB, the low population zone, LPZ, and for the control room.

Now, one thing I would like to point out is that when we do these calculations, typically we don't approach this value of 30. In other words, in essence, most of the PWRs, they're probably anywhere from a factor of three to ten below these values. That's where you typically are in terms of the evaluations in normal situations such as this.

Now, in order to perform these calculations, it's necessary to determine the equilibrium release rate associated with the fuel. For example, it might be that four microcuries per gram of dose equivalent iodine-131, that the release of iodine-131 is five curies per hour.

When you factor that into a main steam line break or steam generator tube rupture accident analysis, since the spiking factor is a factor of 500 larger, this goes into the release rate.

So in other words, when you start this accident, you have your primary coolant activity level and then you start releasing from the fuel into primary coolant. In this case, it would be 2,500 curies per hour of iodine-131.

DR. KRESS: A question. To get that five curies per hour out of the .4, which is what's measured, you have to favor in the cleanup system.

MR. HAYES: Yes.

DR. KRESS: And the decay constant for iodine.

MR. HAYES: Right. The terms, if you will, you have a release into the fuel, but you have a removal, and the removal consists of decay, let-down flow, and also primary to secondary leakage.

So all three of those factors are utilized to arrive at the five curies per hour.

DR. KRESS: The major one being the cleanup system.

MR. HAYES: Yes. And depending upon the isotope, also, decay. Primary to secondary leakage --

DR. KRESS: You deal with more than just I-131.

MR. HAYES: Yes.

DR. KRESS: You use the other isotopes.

MR. HAYES: Yes. All five isotopes of iodine are utilized.

DR. KRESS: All five isotopes.

DR. POWERS: A couple of questions. On the previous slide, you indicated most calculations were done with the tech spec limit, which you cited as one microcurie per gram.

MR. HAYES: Yes.

DR. POWERS: And now you've switched to .4 microcuries per gram. Was there a reason for that?

MR. HAYES: The only reason I did that is because in doing the slides and the preparations, one of the examples I had was that .4. Let me give you what an actual number is. I've looked up an actual number. I'm just doing an amendment associated with Watts-Barr. And this number, one microcurie is, for Watts-Barr, corresponds to like 13.8 curies per hour release rate and the 500 times that value is like around 6,900 curies per hour.

So that was just chosen. I probably should have put an example of one, but that's what the number is. Also, if you like numbers, the typical primary coolant activity level at one microcurie for Watts-Barr, that's 161 curies of iodine-131 starting out. So that gives you some numbers.

DR. KRESS: Is there a wide variation in the tech specs?

MR. HAYES: No.

DR. KRESS: It's generally around one.

MR. HAYES: Yes. In essence, for all plants which do not have the alternate repair criteria, the value is, in essence, one.

DR. POWERS: There is a peculiar habit of iodine, it usually gets called hideout. Did you attempt to account for hideout?

MR. HAYES: No. We don't have any special function that includes hideout.

DR. POWERS: What does DE stand for?

MR. HAYES: Dose equivalent. I'm sorry.

MR. SIEBER: Could you tell us again what the basis for the 500 spiking factor is and is it the same for every plant under every condition, with any amount or no fuel leaks?

MR. HAYES: Right. Okay. The value of 500 is the same for all plants which was utilized. I cannot tell you the basis, other than to say it's in the standard review plan. I can't tell you how it was arrived at back probably in the '70s.

I presume that someone did an enveloping estimate, but I don't know what the basis for it is.

MR. SIEBER: Would that not be the most important factor to consider in trying to figure out what the dose equivalent at the exclusionary boundary and the control room would be? It would seem to me to be the most important thing.

MR. HAYES: No, it isn't. The most important thing is your primary coolant activity level and also the primary to secondary leak rate, because -- let me go back to a slide.

MR. SIEBER: The primary coolant activity level, the spike actually is where the dose comes from and if it's 500 times greater than what you would ordinarily have as a dose commitment from primary coolant, that's a substantial increase.

DR. POWERS: We're talking about a release rate and not -- he's not multiplying his primary coolant concentration by 500. He's multiplying his release rate by 500.

MR. HAYES: The other thing I think is important, this is the definition of a spiking factor. We have two terms. We have one as the numerator and one as the denominator. For example, if the denominator is extremely small, this spiking factor will be very large.

So the absolute value of the spiking factor isn't as important as is really the primary coolant activity that you have and the primary to secondary leak rate.

In further discussions, when we go into the parametric analysis that we did, we hope that we will be able to show you that that is indeed the case.

MR. BALLINGER: A question. You say that you don't really consider hideout, as it were.

MR. HAYES: Yes.

MR. BALLINGER: But do you look at the sort of steady-state, if you want to call it steady-state, iodine concentration in the system as a function of time versus how the plants operate, to try to get an idea of whether you're using an average which has a lot of uncertainty in it or not?

I mean, how do you arrive at the .4, for example?

MR. HAYES: The value of one is a tech spec value and plants do not operate even close to that. The reality of the situation is this. When plants start to get at around ten-to-the-minus-two, they get real, real antsy and they start to take action.

And, see, the presumption that we have with this particular analysis is that you are already at one microcurie.

MR. BALLINGER: But the proposal is to reduce that in some cases.

MR. HAYES: Yes.

MR. BALLINGER: So a value that's supplied to you, that comes in at some number less than one.

MR. HAYES: Right.

MR. BALLINGER: But how do you evaluate whether or not that's a number which you believe?

MR. HAYES: You mean whether they're going to get to that number?

MR. BALLINGER: No. Whether the -- oh, even the lower one is a tech spec number?

MR. HAYES: Yes.

MR. BALLINGER: Okay.

DR. KRESS: The assumption seems to be that if you have a number in the tech specs that is an allowed number, that there is a potential then for when the accident occurs, the steam generator tube rupture, that you may be at that tech spec number, since it's allowed.

MR. HAYES: That's an assumption made in the calculation.

DR. KRESS: So when you do the design basis accident calculation, that's why you use the one or whatever the tech spec number is and not the actual, because the actual is not really of interest.

MR. HAYES: That's correct. That's very good.

Voltage-based criteria. As Jack Strosnider --

DR. CATTON: Excuse me. I kind of got lost in a little bit of this. You use the number one when you do your calculation.

DR. KRESS: Or whatever is in the tech spec.

MR. HAYES: Or whatever the tech spec value is.

DR. CATTON: Right. So if somebody comes in and says, gee, I want you to relax a little bit, I'm going to reduce my tech spec value to .1 or .5, in reality, nothing has changed because they're already operating at a much lower number.

DR. KRESS: Something would have changed, because if they --

DR. CATTON: If they operated at one, it would change, but if they operated at .1, then nothing really changes, except the fact that they've reduced the --

DR. KRESS: There is a virtual change, and that is that if they did approach this new number, they would have to shut down and do something.

DR. CATTON: We heard that they don't.

DR. KRESS: I know, but that's -- but it's a virtual change. They would have to if they did.

DR. POWERS: A virtual change is no change at all.

DR. KRESS: That is a change.

MR. HAYES: I understand some of your quandary. Let me share some experience with respect to generation of the steam generator rule.

The value of one is not a value that is frequently met. People maintain their concentrations much lower than that. So, if you will, it's not in a usual operating area.

In the discussions with some of the utilities, as part of the steam generator rule, they had mentioned to the staff, they said, hey, we think your evaluations are too conservative, and what the staff -- we went back and reassessed that.

Well, what we found is when you reassessed it, hey, one of the things you licensees and we can do is let's change the tech spec value, and the operators said, no, we don't want to change the values. We want that margin.

So what you actually find out is plants don't want to change that value of one unless they have to and what happens is with the ARC amendments, in essence, they're forced to change it, but they don't want to do that. They want to keep that value of one.

As another example, there's another tech spec value which is a maximum instantaneous value, that we're not talking about today, which is a value of 60. That value, no one has come close to it.

We've had three values above 15 in the whole operating time period. The highest value is 18. They don't want to change the value of 60. The value of 18 was in

1972, in Ginna.

DR. KRESS: In essence, then, what I read into that statement is that by changing the tech spec value, you have eroded the margins. You have to have. The question that comes to mind when you make that statement is how much margin do you need in design basis space.

MR. HAYES: You've eroded the operating margin. Dose margin, you're still at the same number.

DR. KRESS: Well, you've eroded the margin to how well you've protected against receiving a particular dose at the site boundary or the control room, which is what you're interested in, how well you control the potential for having that dose.

So you've definitely eroded that margin when you lower the tech spec value, there's no doubt, in my mind, because in essence, you allow more leakage to meet the values.

DR. CATTON: You're swapping real safety for virtual safety.

DR. KRESS: Yes, exactly.

DR. POWERS: You can do that on virtual changes, you can do that on margins, too. He has enormous capacity for --

DR. KRESS: So this all gets embroiled in how much margin do you need in design basis space and why did you have the margin you had in the first place and those types of questions.

DR. BONACA: It seems the issue is with the leakage rate, right?

MR. HAYES: That's correct. That's correct.

DR. BONACA: The accident analysis is used on GPM and that's why the number is in tech specs, but some plants operate with less than that.

MR. HAYES: That's right.

DR. BONACA: And if they have problems, in fact, meeting those limits, especially control room issues, leads you at times to the need to tighten up that leakage rate, which means you have to perform your analysis with a lower leakage rate.

But I'm trying to understand how that affects the margin issue and the tradeoff.

MR. HAYES: And I think you all are doing very good, well, leading right into the succeeding slides, because in this voltage-based criteria, that's the tradeoffs we're getting into.

As Jack Strosnider mentioned, he said that the tech specs, as they're written, at 40 percent through wall, you have to start plugging tubes.

What the voltage-based criteria does, it allows tubes that you would normally be plugging to remain in service, but there's a tradeoff with that. That is, it's postulated that if you have a high pressure transient, such as a main steam line break accident, you're going to cause, because of the high differential pressure across those tubes, you're going to cause those tubes to open up and they're going to leak at a certain level.

For example, let's say that you have a main steam line break associated with a given steam generator and let's say in that steam generator, you have 100 tubes which have this voltage-based criteria.

If it is postulated that each of those tubes which has that criteria would leak at half a gallon per minute when exposed to that pressure, then for those 100 tubes, you would have a 50-gallon per minute leak.

Now, that's a new source. We hadn't had that before. The source we had was normal operating primary to secondary leakage, but now we've got what we refer to as the accident-induced leakage, and that value is like, for example, in the case I used, 50 GPM. So that has to be added into your source.

DR. KRESS: If I view this from the perspective of Reg Guide 1.174, this is sort of a change to the licensing basis that increases risk a little, it's a question of how much. The Reg Guide 1.174 calls for maintaining the defense-in-depth philosophy and it also calls for preserving margins.

I'm not sure I quoted it correctly with those and that's why I'm looking behind me. It seems to me like this increase in risk, it's bound to reduce your defense-in-depth a little bit. I don't know how much. I don't know how that, quote, preserves the defense-in-depth philosophy and it also erodes the margins, so I don't see how it preserves the margins, which are sort of part of the whole integrated analysis.

Maybe someone back here could speak to those questions.

MR. HOLAHAN: This is Gary Holahan, from the staff. I think in Reg Guide 1.174, the principals are laid out to keep any risk changes small and to preserve defense-in-depth philosophy and to preserve sufficient margin, but the discussion of defense-in-depth and margin wasn't meant and doesn't mean that we're not prepared to make any changes.

So there's a judgment involved and there's some guidelines involved as to how much margin ought to be preserved and what does it mean to be preserving defense-in-depth.

I think what you really need to do is to look not only at this design basis case, but look at the implications of any of these changes from a severe accident risk point of view.

I think Jack isn't going to cover all of it, but when Steve Long speaks later, you'll see the whole picture of how we consider these issues and I think by the time we get to tomorrow's examples that Jack mentioned this morning, the Farley and the Arkansas amendments, you'll see, in fact, that there is an explicit discussion by the licensee and by the staff on safety margins and defense-in-depth and risk implications of these sort of changes.

DR. KRESS: Thank you.

MR. STROSNIDER: I'd like to provide one other perspective, too, which is with regard to the type of degradation that's being dealt with here, which is outside -- stress corrosion cracking at tube support plates, again, a very specific form of degradation.

There was some discussion yesterday and there will be some additional discussion today about the difficulty in using eddy current methods to size racks and that sort of thing.

Before the voltage-based criteria went in, what licensees were doing was they were attempting to size these indications and they were leaving them in service. So when you look at the delta between what the practice was and what's coming here, you need to ask yourself the question which of these is really providing a more reliable approach.

Now, I will acknowledge there is another approach, which would be plug every tube that has an indication, which is what they typically do with stress corrosion cracking.

But if sufficient margins can be demonstrated by the industry, then that's not necessarily the way you have to go. So you need to look at this in terms of the perspective, you know, do you have greater uncertainty trying to size these cracks in terms of their actual physical dimensions, which is what people were doing and there was large uncertainty in that, and, to a certain extent, dealing with accident-induced leakage was, to some extent, probably just acknowledging reality and specifically dealing with it.

So that's a different perspective. Steve, have you got something you wanted to add?

MR. LONG: This is Steve Long, with the staff. Just a couple of things to make sure we're clear on. When Jack does this calculation in design basis type analysis, he's assuming that leak rates that are measured as if they're in the free span are going to occur.

So he essentially has data from tubes that they tried to figure out what the leakage would be if the area that's normally captured by the tube support plates is exposed in the free span and then given the delta P change from the steam line break.

This is not something you just throw into a risk assessment in that manner. So you need to think of this somewhat differently than the way you would do a risk-informed application. These are not risk-informed applications under Reg Guide 1.174 and the leakage that he's using assumes that every flaw that's under a support plate is instead in the free span.

So it's sort of a virtual leakage calculation.

MR. STROSNIDER: A conservative one.

MR. BALLINGER: But the vendors, their correlation assumes no restraint by the support plate anyway.

MR. LONG: That's correct. But you would have to have what fraction would be --

MR. HAYES: The question becomes, well, what is the impact of this voltage-based criteria. Well, the goal is to minimize the number of plugged tubes.

DR. POWERS: Whose goal is that?

MR. HAYES: That's the licensees' goal. That's the licensees' goal. And because what happens, if you continually plug tubes, it's obvious that you have to de-rate your unit, and that's what they don't want.

Now, as we mentioned at the start, we had two criteria. You had the one gallon per minute primary to secondary leak rate and you had the one microcurie per gram of dose equivalent iodine-131. Those are an equilibrium now.

If, all of a sudden, you've raised that primary to secondary leak rate, then you have to lower the allowable primary coolant activity level if you're going to have this larger leak rate and still maintain your doses.

So that's what they do and that's what they're trying to achieve.

DR. POWERS: Is there a hazard, safety hazard associated with plugging tubes?

MR. HAYES: If you plug tubes, you remove the capability of the steam generator to remove heat from the core and you reach a point where you have too many tubes plugged, you have to de-rate your unit. Now, if the question is deeper than that, I'm going to have to refer to someone may be from the Division of Engineering to answer that question.

MR. HOLAHAN: Let me try. It seems to me that when tubes get plugged, the plant meets the same deterministic safety criteria in terms of mechanical requirements, in terms of thermal hydraulic analysis, in cases, they have to redo the LOCA analysis. In effect, they meet all the same requirements.

I'm not aware of any risk assessments that indicate that taking tubes out of service by plugging them introduce any risk changes.

DR. POWERS: No accident initiators associated with plugging tubes?

DR. BONACA: The only sensitivity I think I could think of would be if you had very uneven plugging in different steam generators. That, you would have to -- those kinds of issues are considered, have to be considered.

DR. POWERS: So if I plug all the tubes on one quadrant of the steam generator, I get some sort of problem.

MR. SIEBER: You'd get an offset in the core, because --

DR. BONACA: You would get --

MR. BALLINGER: Between one steam generator in one loop and another steam generator in another loop where you have a very odd -- a very large difference in number of tubes plugged, then you alter that.

MR. HOLAHAN: Those are the kinds of issues that are dealt with in the analysis.

DR. CATTON: So, in essence, what you're doing is you're just making sure the iodine that's sitting around ready to get out is the same.

MR. HAYES: Right.

DR. CATTON: You reduce the level in the primary system. You allow a little bit more to be dumped in. The more that's dumped in relates to the leakers. The leakers is tied to voltage.

DR. HOPENFELD: Right.

DR. CATTON: It sounds like a rather simplistic calculation. Do you put any uncertainty on the steps in this in order to --

DR. HOPENFELD: No. It's done in a deterministic manner. There's no uncertainties put on it. Now, realize that the criteria here is 30 rem thyroid, which is ten percent of Part 100 limits.

DR. POWERS: That's the final.

MR. HAYES: That's the final, yes.

DR. POWERS: You're playing around with some game in the middle somewhere.

MR. HAYES: The only uncertainty to that, I think, is utilized with respect to leakage associated with the tubes.

DR. POWERS: There is no consideration in the analysis that the events that lead or are associated with either a steam generator tube rupture or main steam line break could, in fact, cause a rupture to the cladding on some --

MR. HAYES: That's correct. There is no consideration for that.

DR. CATTON: When you do that --

MR. HOLAHAN: Excuse me. I know why. It's because there is a requirement that for those events, the fuel continues to meet specified acceptable fuel design limits, which are intended to not induce additional fuel failures.

Of course, the fuel failures that preexisted are covered in the analysis and the spiking. But you would expect no additional fuel failures because fuel is analyzed under those thermal hydraulic conditions to meet its design requirements.

MR. BALLINGER: Is there any data on spiking for actual delta P's which would exist during the main steam line break?

MR. HAYES: No.

MR. BALLINGER: As opposed to operational delta P's, which is all the stuff that I've been reading.

MR. HAYES: No, there is not.

MR. BALLINGER: So what is your judgment with respect to the spiking factor when you make the jump between what's been measured and what actually might occur?

MR. HAYES: We're going to discuss that and if we don't answer your question when we get to that point, please, bring it again.

DR. CATTON: So under this new approach, how do you calculate the spiking factor? Is it this 500 times the new lower primary system value?

MR. HAYES: That's correct. That's correct.

DR. POWERS: I guess I don't understand that.

DR. CATTON: So it goes way down.

MR. HAYES: That's correct.

DR. CATTON: And what happens to what leaked into the primary system? I don't quite follow -- I'm having a little bit of a problem with iodine conservation.

MR. HAYES: The iodine is not at the one anymore. It is at some lower value.

DR. CATTON: Reduce the level.

MR. HAYES: Reduce the level.

DR. CATTON: Now I dump some in from the fuel because I've got leakers.

MR. HAYES: That's correct.

DR. CATTON: I now multiply that new value by 500.

MR. HAYES: That's correct.

DR. CATTON: Or do I multiply the 500 times the steady-state value before it leaked?

MR. HAYES: Before it leaked, because, for example, I think the question that you're getting to is saying, hey, can I have this massive amount of iodine into the gap that's just ready in there to break loose and it hasn't, because it hasn't been exposed to this differential pressure or this transient, whatever it is.

And the question is, if you had that activity available for release, it would already be showing up into the primary coolant, if it's through the defects.

DR. CATTON: What it really gets down to is you're reducing -- if it's 500 times a lower number, where does the other go?

DR. BONACA: If I understand it, it reflects the conditions of the core. If you have a very clean core where you have no leakage, because assume, for example, you have a brand new core coming in, where every defect has been replaced with a new fuel rod, so there are no defects.

DR. CATTON: So you wouldn't have much iodine.

DR. BONACA: You would have not much iodine. Then if you have the accident, unless you postulate it, the accident causes the cracks in the cladding, which we don't believe that is the case.

DR. KRESS: It commits you to better fuel, basically.

DR. POWERS: I don't think so. It doesn't seem to me it does that at all; that there are multiple ways that I can get my primary coolant concentration down. One of them is I can buy a better cleanup system.

DR. KRESS: Yes, but he said you account for that.

DR. POWERS: He accounts for that.

DR. KRESS: You go to the rate and you account for that.

DR. POWERS: I'm accounting -- I want to get my steady-state concentration, normal operation.

DR. KRESS: You increase the cleanup rate. That doesn't help you any. It doesn't help you any in here at all if you do it that way, because --

DR. POWERS: That's right, that's my point is it doesn't help you at all.

DR. KRESS: No, but they account for that, he said.

MR. HAYES: We have factored that removal rate into the production rate.

DR. KRESS: Yes. They work on production.

DR. POWERS: I understand this all. My point is that it does not commit you to better fuel. There's another way around the barn here. So you don't end up multiplying --

DR. KRESS: You're basically right, but you still have to meet the dose limits.

MR. BALLINGER: But by having better fuel and by having, for a given cleanup system, a lower iodine concentration, you have a lower release rate, as well.

MR. HAYES: That's correct. And I think it's important to realize that you cannot commit to these lower levels, like ten-to-the-minus-two or so, because as I mentioned, they're not comfortable at those levels, unless you have good fuel.

Because what you see typical plants operating at now is like ten-to-the-minus-three, ten-to-the-minus-four. That's typically what they're operating at. And if they start to approach ten-to-the-minus-two, they get antsy.

MR. BALLINGER: It's about ten-to-the-minus-four, roughly, per failed fuel element. So it takes about one failed fuel element for them to exceed their -- to make them get antsy.

MR. SIEBER: One rod.

MR. BALLINGER: One rod, yes.

DR. BONACA: I think, historically, we have to look at -- I mean, plants used to run with several defects years ago. Today, I mean, since then, there has been the goal of INPO of zero defects, and I think the utilities have been very committed to it and it's very unlikely that you find plants --

I mean, today, if you find a situation like a Seabrook, where they had eight fuel failures, they had measure inspection and evaluation of why you get those kind of issues, and repair it. So I'm saying that reflects, in part, the way that the fuel is being treated today.

MR. HAYES: I think if you look at the spiking data, you can even see that. You can see that the levels at which the spikes have occurred and the values of the spike have really changed with time. You just don't see them occurring.

DR. KRESS: Could you address the 30 rems as opposed to the 300? Was that just chosen for margin or was there another reason?

MR. HAYES: You know, this is my own interpretation. There is no basis of fact. My interpretation is there is a calculation which is done at the 60 and that has the full Part 100 value. As I mentioned earlier, no one has ever come close to the 60.

I believe that the reason that you have the value of 30 is to account for uncertainty both with the spiking factor and the fact that people have been at one and higher than one.

So I think that was probably what was done, because at that time, in terms of accident analysis, a lot of thought was given to the more that you have the potential for a release, the lower you put the limit. For example, that's why a fuel handling accident has a lower one.

MR. HOLAHAN: I think there's also another factor, because the approach of using some fraction of Part 100 is used in some other cases, as well, and I think Part 100, the guideline is set up for really a maximum hypothetical accident, which is considered to be extremely unlikely.

And recognizing that some events, like tube ruptures are much more likely --

DR. KRESS: They're much more likely to happen, so you factor that in.

MR. HOLAHAN: You'd factor that in.

DR. KRESS: Makes sense.

MR. HAYES: It's important to understand that the staff's reassessment of iodine spiking was just one part of a total reassessment in terms of the way we did main steam line break accidents and steam generator tube ruptures as part of the rule, steam generator rule initiative, because we had a situation where industry is saying we're too conservative.

We have the DPO, which says we're not conservative enough. So we had the great opportunity to make no one happy. So what we wanted to do, you know, we were really truly interested in the 1996-1998 timeframe, really reassessing how we do the accident evaluation.

So one part of this was with respect to iodine spiking. Now, the industry was proposing some iodine spiking models. One was in a report by Postma, which was an empirical model. Another was a first principle model by Lewis and Iglesias, which the staff reviewed, but the staff determined was insufficiently mature, didn't have adequate V&V, and did not predict a priori what the spike would be.

So then we went to look at a couple articles by Adams, articles he did with Sattison and Atwood, and the data that Adams generated was really also included in both the Postma and Lewis and Iglesias reports.

Now, the Adams and Sattison article looked at 58 events. The spiking factors ranged approximately two to slightly over 900. Issue activity levels ranged from roughly ten-to-the-minus-three to about one microcurie per gram. The spiking factors in three cases were greater than 500. Two of those were in the range of about 900. The activity levels associated with those spikes were roughly two-times-ten-to-the-minus-two or less, and the maximum activity in any case was 3.5 microcuries per gram.

DR. POWERS: I'm going to have to ask you what you mean by maximum activity.

MR. HAYES: That the activity, at the end of the spike, the maximum activity which was obtained in reactor coolant, measured in reactor coolant, was 3.5.

These were Adams' and Sattison's conclusions. They concluded that, first of all, large spiking factors tend to be associated with small coolant activity levels and small iodine release rates and that to assume that you have a spiking factor of 500 in association with a dose equivalent iodine value of one microcurie per gram is overly conservative.

They recommended to expand the database to determine and reduce uncertainties.

DR. POWERS: It seems to me the codicil that the spiking factor of 500 in association with one microcurie per gram is very important. That's a non-negligible statement there. That doesn't seem to be in my viewgraph.

MR. HAYES: The viewgraph, I think it stopped -- this --

DR. POWERS: That's the one I wouldn't leave out.

MR. HAYES: Right. Well, in proofreading -- you're correct. In proofreading, that thought was missing and that was a very important thought.

DR. KRESS: That doesn't address the question of how conservative, if at all, the 500 is at some other --

MR. HAYES: That's correct. At this stage, it does not address that.

DR. KRESS: Okay.

MR. HAYES: And Adams --

DR. KRESS: We always thought it was conservative at one microcurie per gram. I don't think that was ever a question.

MR. HAYES: And you're correct. The issue which Dr. Hopenfeld has raised is a new issue and wasn't really considered at the time that Adams was doing this work, per se.

DR. KRESS: I see. Okay. That helps.

MR. HAYES: Because I believe, and Dr. Hopenfeld can correct this, but I believe that this is work that Adams was doing for you in the Office of Research at the time.

DR. HOPENFELD: He was doing this for me and I just thought there was something missing there. Yes. He was doing this thing for me and we went through all the data that was the conclusion of, and I'm glad you caught that one microcurie, because that was really the key thing.

But we didn't carry that beyond that point, for one reason or another, and that's what -- but I thought, at the time, they should have been carried, but we didn't. So we stopped right here.

MR. HAYES: The next report by Adams was --

DR. HOPENFELD: Excuse me just one minute, if I may. You left the impression that I'm trying to be more conservative, that I'm saying that the utilities are not conservative enough. That's not really my point. I don't know, they may be still very conservative.

My point is that what you are doing is not justified, just arbitrarily taking that 500 and leaving it there while you're doing something else. That's my point, my main point. There's no technical justification to it.

I don't know whether you're conservative or not.

MR. HAYES: The next report by Adams and Atwood looked at spiking and the information associated with LERs. What they decided to do is they decided that they would bound each spiking event. They decided that the -- they postulated that the maximum dose equivalent iodine-131, which is typically measured between two and six hours after the event, could be no greater than a factor of three higher than the measured value.

So they decided that they would bound those values. Let me show you --

DR. KRESS: Where did the factor of three come from? Because that certainly is not decay.

MR. HAYES: I believe that the value, that they came up with a value of three was probably associated with the pressure differential associated with the vent.

So they said, hey, we're going to presume that there is a linear relationship between the pressure associated with that particular --

DR. POWERS: Didn't they raise questions about exactly when the sampling was done in the solution and there may have been some decay?

MR. HAYES: There were three graphs that they presented, one of which did not include -- well, excuse me -- included the factors of three, but also corrected presumed -- I think that it was six hours and moved it back to two hours.

MR. BALLINGER: I don't think it says anywhere in what I've read that that factor of three is related to an increase in delta P. That's why I asked the earlier question.

DR. POWERS: I think it's a sample in their analysis, a question that they had about the data that were available to them. They came up and said, well, it can't be any worse than a factor of three.

DR. KRESS: It's uncertainty in the way you determine iodine-131 in a sample, I think.

DR. POWERS: That's my impression.

DR. KRESS: Plus, you might add a little decay if it's seven hours.

MR. HAYES: This is a curve of the Adams and Atwood data, which is without the factor of three. You can see that the highest spike that they had was approximately a value of 4,000 and it occurred at around ten-to-the-minus-three microcuries per gram.

You can look and you can see this is a spiking factor one and you see that there are data points which show a spiking factor of less than one.

If you look at the data from about ten-to-the-minus-two, you can see that most of the points are a value below, roughly, I'd say, 700.

DR. KRESS: The points below the line imply that when you undergo this event, that the rate of release of iodine from the pins decrease over what it was normally.

MR. HAYES: And you're going to ask me why that's the case, and I don't know have an explanation, whether that's just the data.

DR. KRESS: It could be data uncertainty, you're right.

DR. POWERS: When I look at this plot, it seems to me my first reaction is, gee, I believe I could run a straight line through these data on a log-log plot, and when I think about doing that, coming up and then using that straight line to make estimates on the spiking factor as a function of the RCS initial activity, I put error bounds around it for what the error in the estimate would be.

Because I run out of data at the one microcurie per gram, I would think that those error bounds would turn up pretty dramatically.

Have you made such a plot?

MR. HAYES: We did. What we did is when we got done with the industry data, Adams and Atwood, we thought, well, we think that probably the spiking factor is a function of reactor coolant activity level and we can have -- we'll have our contractor analyze this data and hopefully we'll come up with a plot of spiking factor as a function of primary coolant activity level.

And we couldn't get a good correlation and we really thought that we were going to be able to do that.

When we went into it, we thought we would be able to do it.

DR. CATTON: But you could easily get a bounding curve for this.

MR. HAYES: And we did and the bounding value that we got, the 95th percentile value we got was 335.

DR. CATTON: That's a single number.

MR. HAYES: That's a single number.

DR. CATTON: 335 times --

MR. HAYES: Times whatever the release rate would be at equilibrium.

DR. CATTON: How is that bounding? I can see one here that's more than 1,000.

MR. HAYES: We didn't take the highest value. We took a 95th percentile value.

DR. CATTON: So that means you weighted a lot of these negative ones down here.

MR. HAYES: That's correct. That's correct.

DR. POWERS: When I look at these data and I stand far enough back from the plot, I say, gee, it looks to me like there are two populations of data here. There is a population that comes along and includes these low ones and there's a population above.
Did you look at the data set to see if there was any indication there were two populations?

MR. HAYES: We asked Adams about that to see if he had an explanation and he didn't have an explanation.

MR. HIGGINS: That's a different question. What Dr. Powers asked is if you did it.

MR. HAYES: No, we did not.

MR. HIGGINS: Is there at least some consistency at a given plant? This is across many plants. Do you have multiple data points at a given plant?

MR. HAYES: We have multiple data points, but I don't think we would have a sufficient amount to really say that this would be reflective of a plant.

See, I think one of the problems you would have with such an evaluation is because of fuel changes. A lot of the spiking data that we have is pre-1980 data, in the '70s, because that's when you had a lot of these events. And it's really not reflective of the conditions that you have now.

It will give you data, but it's not really reflective of what we have today.

MR. BALLINGER: Has it been plotted as a function of the delta P?

MR. HAYES: No, it has not.

DR. CATTON: The points that are below that line are really kind of perplexing. I don't know how you can use them if you can't explain them. Maybe it's an error in your data processing. Maybe it's an error in your readings.

So how can you use it when you attempt to calculate some kind of a factor?

MR. HOLAHAN: I would suggest it's not really reflective of what we have today. If you think about what it means to go to the left on that chart, you're dealing with very small numbers and you're dealing with the ratio of small numbers, and I would expect the uncertainties to get larger.

There are two things going on. I think Dr. Powers suggested that there is less data as you go to the higher numbers, clearly, but the numbers are harder to calculate as you go to the left, so the uncertainties get larger.

DR. CATTON: I understand that, but normally when you look at a plot like this and you're trying to figure out what's going on and you want to come down on the safe side, if the bottom data points which pull the number down can't be explained, it seems to me they should be eliminated until they can and not be a part of the process that leads to the number.

MR. HIGGINS: Or call them, call them spiking factor of one.

DR. CATTON: You can weight them. I mean, there's all kinds of techniques for dealing with these things.

MR. HOLAHAN: But recognize that this is a log plot and they don't change the answers by all that much.

DR. CATTON: I see that, I see that.

MR. HOLAHAN: I think you should have the same --

DR. CATTON: I have a straight line that beautifully --

MR. HOLAHAN: You should have the same question about the value of 4,000, which has a large uncertainty on it.

DR. CATTON: It depends what you're trying to do. You would ask the same question about the points that sit way up by themselves, except that they're more important.

MR. BALLINGER: But there's very few instances where you would just arbitrarily eliminate data points, but one of them would be if it's simply non-physical, it can't happen. Now, is that the case here with the ones that are less than one? Is that a non-physical situation? That is to say, it just can't happen.

MR. HAYES: I think Gary Holahan's point is a good point. Remember that the spiking factor has a numerator and a denominator and the values associated here are very low.

MR. HOLAHAN: It may be non-physical, but it's giving you some insights as to the measurement of uncertainties.

DR. POWERS: If we were to put error bars on these data points, about how big would they be?

MR. HAYES: Since I didn't take the measurements, I really couldn't answer that. I'm sorry.

DR. BONACA: It could be, in effect, related to the type of transient that is taking place. For example, in a steam line break, you have a depressurization in a system, where probably, in the short term, you have almost equalization, you could have, between system pressure and internal pressure of the fuel rod.

MR. HAYES: It's important to understand that each of the -- none of these events involve a main steam line break.

DR. BONACA: But you have a scram. All right.

DR. KRESS: These are all team generator tube ruptures?

MR. HAYES: Well, there are a couple of steam generator tube ruptures, but most of them are transients. They do not involve a steam generator tube rupture. Fortunately, we have not had that many steam generators, or we might be dealing with a different issue.

MR. BALLINGER: Which points on here deal with the steam generator tube ruptures?

MR. HAYES: I would have to go back to the original Adams and Atwood. I couldn't tell you at this point.

DR. POWERS: It would be a substantial chore, even if you went back to it, sorting through his table to figure out which points are which.

MR. BALLINGER: But valuable.

MR. HAYES: I think you have the Adams and Atwood and Adams and Sattison articles from Nucleonics Week, I think, or some --

DR. POWERS: Nuclear Technology.

MR. HAYES: Nuclear Technology. And I think he does list in those documents which ones are steam generator tube rupture events.

DR. POWERS: Then when you find them and you go in and try to find the point on the plot, it's a chore.

MR. HAYES: This is the same data multiplied by a factor of three and that value, you can see, takes you around 10,000.

Okay. The conclusions by Adams and Atwood, you know, in many respects, similar to the conclusions of his previous article. In other words, a spiking factor could be reduced substantially, he believes, by a factor of 15 and still be conservative.

Again, it's important to point out that we're talking at the one microcurie per gram area. That's what he was addressing. I don't want to give you a misleading impression. That's what he was focusing on.

And the large spikes occur not because the post-trip release is large -- in other words, the numerator is large -- but, rather, because the steady-state release is low -- in other words, the denominator is small.

And that the spiking data that he had was really representative of a steam generator tube rupture rather than a main steam line break.

When we did our reassessment of the spiking factor, we reviewed the data without the factor of three and, as I mentioned previously, we thought we could assess it and come up with a relation as a function of reactor activity level.

DR. POWERS: Let me understand a little better. Adams had a reason for the reactor of three. He did it not out of any capriciousness. He did it because he --

MR. HAYES: He wanted the bounds.

DR. POWERS: But he had a rationale for choosing three. I mean, it wasn't a number that he plucked out of the air, I don't think. I think he made arguments about sampling and stuff like that, as Dr. Kress pointed out.

Have you rejected all those arguments?

MR. HAYES: No, we didn't reject -- in reality, it probably doesn't make a difference, because we -- his argument was at the value of one, the value of 500 was conservative.

DR. CATTON: Well, it is, but from that chart you put up there, it looked to me like it's only maybe a factor of five. Put that chart back up, the first one you had up there.

I think that's one that's on the right-hand side, isn't it?

MR. HAYES: Right.

DR. CATTON: How can you get a factor of 15? Do you take the mean through all those points? Is that what you're doing? The 95 percent level, just looking at the graph, looks to me like it's about 90. Maybe I'm not eyeballing that quite right.

MR. HAYES: You're asking me to justify his conclusion and I'm not going to --

DR. CATTON: That's fine. But you're asking me to accept 335 based on him saying that it was 15. I'm just turning that around. I think it's fair. Don't you?

MR. HAYES: That's fair.

MR. HOLAHAN: Well, wait a minute. I don't want to be too fair. I think the 335 is an analysis of the data, independent of whether someone else concluded that 15 could be drawn out of that data, and we've shown you the data.

DR. CATTON: Well, I'm still bothered by the fact that you can't explain the data points, yet you want to treat them all as equal, and it seems to me that if you can't explain the low ones, which weight the number, then you ought not weight the final result by those points.

DR. POWERS: On the other hand, if I look at the data and I said, gee, I was going to take a 95 percentile and I threw away everything that I call on my second category, 335 isn't going to be far away from what the one microcurie per gram.

I mean, it's not going to be far away and I'm not going to be -- if I'm in a bounding sense, for that one point, I'm not going to feel bad. In fact, I'd probably feel guilty about picking a number that high.

DR. KRESS: If you did multiply the number by three, however, you would multiply that 300-and-something by three, basically.

DR. CATTON: But, Dana, they're going to run out at .001. Now, we haven't gotten to a number for .001 yet. Well, it's 335 times .001 as the spiking factor. Where are you going to fall on this graph when you do that?

MR. HOLAHAN: The .001 never shows up in a regulatory analysis, because there are not and there will never be any tech specs as low as .001.

We're only talking about the portion, that curve between one and a few tenths.

DR. CATTON: I take .1 and multiply it by 335, what do I get? 33.5. I'm down to the middle of all those data points. That doesn't look very conservative to me.

DR. BONACA: To the right.

MR. HAYES: At .1, you're right here.

DR. CATTON: But if I multiply .1 by 335, I'm going to be just a little bit above that ten over there. That's a log scale.

DR. POWERS: Why would you want to do that multiplication? Well, they're going to use the tech spec of .1. They still won't be multiplying the spiking factor.

DR. KRESS: They won't be multiplying this number. They're multiplying some other number.

DR. CATTON: What number do you multiply 335 times?

MR. HAYES: You multiply by the release rate associated with this particular activity level in the coolant. In other words, for example, we don't presume that the activity level is now 33.5 because we multiply 335 by this value. No.

What it is is release rate and you build up in the activity in the primary coolant.

DR. POWERS: He's going to take a removal rate and a release rate and find a steady -- choose that release rate such that it matches the steady-state value of .1.

DR. KRESS: That's what they're going to do.

DR. POWERS: And he's going to multiply that release rate by a number.

Now, if the steady-state value were one microcurie per gram, we know it's 335 he's going to multiply it by.

MR. HAYES: For a steam generator tube rupture.

DR. POWERS: For a steam generator tube rupture.

MR. HAYES: Not for a main steam line break.

DR. POWERS: Exactly.

MR. HAYES: I think it's important, with respect to the point that you raise, is that since we did not take the data, it's inappropriate, I think, for us to throw any of these points out, because we have -- what is the basis?

I mean, you can go argue from either stage.

DR. CATTON: It depends what you're doing. If what I'm looking for is just a mean curve through the data of some kind, fine, but this is the safety business. If you don't understand it, how can you use it to bring the number down.

MR. HAYES: But it's a safety business, but at the same token, we have to be somewhat realistic.

DR. CATTON: There's nothing wrong with being realistic if you understand it. If you don't understand it, it seems to me it's inappropriate.

MR. HAYES: But it is also inappropriate to throw it out because you don't understand these points, as you don't understand these points.

DR. CATTON: Depends whose side you're on. If you're putting a barrier between me and the plant, I don't want you reducing anything that you don't understand.

DR. POWERS: That's one of the reasons we wanted to explore the phenomenology a little bit is that the lower points do seem to be physical, whereas the upper points, high though they may be, at least are not inconsistent with the argument on why there is a spiking factor all together.

MR. BALLINGER: But the physics, the description of the phenomena that you gave right off the bat matches what you would see, but the low points, the ones that are less than one, don't match it at all.

MR. HAYES: I don't disagree.

MR. HOLAHAN: I think if you were to put error bounds on the measurements, real error bounds on the measurements, they would, by definition, on the lower end, be large enough to be above one, and then you ought to put those error bounds on all the data and you'll see, as you move to the left, the error bounds get very, very large.

DR. POWERS: If that's true, the error bounds would cross multiple decades. I think if you were doing a regression of the data, that you would take that into account.

Those data points, they are high and low, would count very little, which is fine because as you said, the only part of the curve that you're really interested in is one, maybe .05, at most.

MR. HOLAHAN: Right. Personally, I see the data in different ways. In my mind, the data between .1 and one is reflective of what we might possibly use in the regulatory process.

By the way, all of those data points are above one. The rest of the data, in my mind, is sort of a demonstration that, in fact, iodine spiking is a real phenomenon, that it is seen, and you have a lot more data points.

But I don't think it tells you a lot about the range and the statistical analysis on the right-hand curve.

DR. POWERS: I think that's the point that's going to be -- we're going to discuss a lot of things here, but I think that's the most distressing thing; that let's confine ourselves one to .1, because that's all we're going to use, and say -- I'm looking at a log-log plot here and there is scatter on a log-log plot.

That suggests to me that we do not have the really operative physical variable being plotted. Much of these data, similar to a steam generator tube rupture, are not steam generator tube ruptures. There's some other accident.

There is something affecting those data over a decade in value other than just the activity in the coolant. In fact, we think the activity in the coolant can't possibly have any relationship to the mechanism giving us a spiking factor, second order, maybe.

How then can we be confident that a bounding that he does even at 335 really represents a bound on what happens in the steam generator tube rupture, if there is some other variable that is really controlling that spiking effect.

Otherwise, you've got a Poisson problem here. These happen to be the 25 data points that you've actually measured, which may accidentally not hit the particular value of the controlling variable that gives you high value.

DR. CATTON: I think if you cut the data off where you suggest and you were to redo it, you'd get a bigger number than 335.

DR. POWERS: You may be right.

MR. HAYES: Everything high seems to be at about .05 on down.

DR. CATTON: Any time you have two decades of spread, you really ought to go back and replot it, and if you can't go back and replot it, you put a curve over the top of it. You can't explain it and to do anything else, I think, is an in-road on the margin of the plant, because you don't know what point is there for what reason.

MR. BALLINGER: I think the idea -- the contention that you may not have the right variable is probably the one that's closer to the mark, that you just don't have a handle on the phenomena.

MR. HAYES: There is no question that the iodine spiking has not been pinned down specifically.

MR. HIGGINS: Isn't it really a plant-specific thing? A fuel, actually a fuel and a core load specific thing. It seemed like the amount of iodine that leaks out is very much related to the type of defect you have in the fuel and the specific type of fuel.

So what you're seeing here is a demonstration over many cores, over many years at many different plants.

MR. HAYES: Exactly. And I think I made reference to the fact that most of this data comes in pre-1980.

DR. CATTON: It sort of presents an option, doesn't it? You either accept it or you go back and do it again and do it right.

DR. POWERS: I think there's another alternative here that I'd like to understand a little better. You mentioned the Iglesias-Lewis report and I think there's also an EPRI empirical report and in both of those reports, they look at some notable events and they plot predicted curves versus the iodine concentration, I believe, and they're remarkable, actually.

I mean, the closeness with which they get just really amazes me. And there's a substantial time variation in what they're comparing against. Can you tell me why those empirical and mechanistic models were just cast out in favor of this data?

MR. HAYES: They weren't cast out. What we did is we had a contractor, INEL, evaluate those models and they thought that the -- after looking at the empirical model, they thought that probably the model to really look into and expend their resources on was the Lewis and Iglesias.

And what happened was they evaluated the Lewis and Iglesias model and what they did is, for example, if the event was at Prairie Island, they tried to take that model and utilize the predictiveness to determine what the spike would be for Prairie Island.

And when they did that work, you couldn't take it and predict it for Prairie Island, San Onofre, Watts-Barr or whatever. They couldn't come up with it.

So I think the way I related to it is similar to when you're in a lab and you're trying to come up with a relationship. You do it after you have the data versus before.

So a priori, they couldn't come up with a predictive tool using the Lewis-Iglesias. Adams, when we discussed this with him, he says he recommended, if we could modify the Lewis and Iglesias model, that that would be an approach to go and, therefore, maybe we could come up with a predictive tool, and that was one of the things we raised with industry, but industry did not choose to pursue it.

DR. CATTON: If you did fit -- if you fit a curve over just the two top data points, I bet you'll get a factor that is much less than the 300 when you're operating greater than .1. You're going to get about 120.

MR. HOLAHAN: Log scale, I think you get about 200.

DR. CATTON: Just by bounding the data. It's a nice logarithmic curve.

MR. HOLAHAN: But let me suggest that this is one piece of a design basis dose calculation which has lots of other conservatisms in it, like 95th percentile meteorology and lots of stuff.

And if we're not careful, we'll end up making every step in every calculation so conservative that the answers are meaningless and we're in danger of convincing ourselves to do things which risk-informed regulation, in my mind, is trying to get us away from, which is to put lots of attention on things that aren't necessarily important.

So when you say we could be more conservative by drawing the lines differently, that can be true, but that's not always the best regulatory approach.

DR. CATTON: You could include uncertainty in whatever fit you put on this, too.

DR. POWERS: Fortunately, our concern right now is really going contention by contention and not designing a regulatory process.

MR. HOLAHAN: I've tried to preserve a concern that we ought to have an overall safety perspective and the regulatory approach in mind when we deal with each of these issues.

MR. HAYES: Okay. What did we conclude with respect to iodine spiking? Well, we concluded that it was not a function of the reactor coolant activity level. We did envelope the value, as we mentioned, and we came up with a value of 335 and, again, that is what we considered to be representative of a steam generator tube rupture and not representative of a main steam line break. I think that's important to understand.

What are our conclusions with respect to the spiking associated with the main steam line break? Well, we concluded that you cannot extrapolate the spiking data from the data we have to a main steam line break, but we can do some sort of an assessment. We believe that there is a linear relationship between the differential pressure rate and the resultant iodine activity or release rate.

Now, because of the pressure, differential pressure associated with the main steam line break being maybe a factor of two or three greater than for a steam generator tube rupture, if you consider that to be two or three times the factor for a steam generator tube rupture, you also can say, well, maybe the expression is a quadriture. So instead of a factor of two or three, the value is four to nine.

I think we feel confident and we've had some discussions with Adams that it's reasonable to assume that the expression is no higher than a quadriture.

DR. POWERS: I guess I would understand that better if I understood the first line. You said there was a linear relationship between the delta P and the resultant iodine activity level or release rate.

Why do you have that confidence?

MR. HAYES: I think because, you know, it is the delta P that causes the spike, it forces the fuel out of the gap into the reactor coolant.

DR. POWERS: That delta P arises because of the vaporization of water on the hot fuel.

MR. HAYES: The delta P originates from the change in the fuel temperature and pressure and the primary coolant temperature and pressure. Then when you get to the main steam line break, the presumption is that you have it with a loss of off-site power. So you're going from the primary side to the secondary side and you have a direct release associated with the main steam line break.

You don't have the cover associated with it like you do with a steam generator tube rupture.

DR. KRESS: The delta P rate you're talking about here is the rate of change of pressure on the primary system in the vicinity of the core. I don't know what delta P you're talking about here.

DR. BONACA: I had the same question.

DR. KRESS: It's a del P, so it's a rate of change with time.

MR. HAYES: It's a change with -- it's not a rate. It's a delta P, period.

DR. KRESS: But what delta P is it?

DR. BONACA: But is it a delta P between the primary system pressure and the pressure inside the fuel rod, or is it the pressure --

MR. HAYES: It's the pressure between the primary side to the secondary side.

DR. BONACA: Explain to me what effects that has on the fuel cracks opening and accepting more water inside and vaporizing and then exiting from the fuel rod. I don't understand how the delta P between primary and secondary side is affecting that. I'm trying to understand it.

MR. HAYES: For the main steam line break, the faulted steam generator, that which experiences the break, is considered to be at atmosphere.

The leak which we have is then from the primary side to the secondary side. We get a rather rapid depressurization on the secondary side. So you set up between the primary and secondary a higher differential pressure.

Then the primary pressure goes down faster than it would for a steam generator tube rupture. So the pressure between the fuel and the primary side is a larger -- is a quicker and a larger delta P.

DR. BONACA: Between the fuel and the primary side.

MR. HAYES: Between the fuel and the primary side.

DR. BONACA: Okay. I understand.

DR. HOPENFELD: Part of the study came out of the Adams study that we were going to look -- take one step further, and that is, if you have to look at that data in relation to the depressurization, what happened during this transient and if you look at some of the data or some of the reports on that subject, it shows that you can very, very low release rate; in other words, you have very, very few defects.

Then you find in those cases, all the activity comes out at the end of the transient, where you really have to depressurize is very, very high, and that suggests that when you have a steam line break, you have much more release because of the high depressurization.

In other words, there is a relation between the number of defects you have and how fast you depressurize the system. But nobody really has taken the time to look into that beyond that point.

I can provide you the conclusion of a very lengthy report that was generated at Westinghouse.

DR. KRESS: I'm still confused by that first line. Tell me again what the delta P is. Is it between the primary and the secondary?

MR. HAYES: Okay. It starts out between the primary and the secondary and the secondary -- okay. With the main steam line break.

DR. KRESS: Yes, and it has a fixed value at normal operation.

MR. HAYES: Normal operating. Then with the steam line break, you go to atmosphere.

DR. KRESS: That's right. So that --

MR. HAYES: That reduces it down --

DR. KRESS: That reduces it. It increases the delta P.

MR. HAYES: It increases the delta P, and then you have --

DR. KRESS: Are you talking about the rate of that increase?

MR. HAYES: It increases both the rate and the net.

DR. KRESS: The net and the rate.

MR. HAYES: Right. Because you're going -- like, you're comparing it to a steam generator tube rupture.

DR. KRESS: What in the world should that have to do with the rate at which you extract iodine from the fuel?

DR. BONACA: The only way I would see it would be that the primary system pressure is also dropping fast.

DR. KRESS: That doesn't say that, though.

DR. BONACA: That's right. So that's why I was confused. I mean, I believe that -- I believe that the difference in pressure between internal pressure of the rod and the primary system, it's varying rapidly.

DR. KRESS: Then I could see it would have a marked effect on it, but this difference between primary and secondary.

MR. HAYES: But why is the primary varying? The reason the primary is varying is because you have a change on the secondary side, also.

DR. BONACA: Sure, but --

MR. HAYES: So you can't separate one from the other.

DR. BONACA: I understand that, but that's a driving force by which you are having a changing relationship between primary system pressure and internal pressure in the fuel rod, and that's the mechanism by which you would expect to have release of iodine activity to the higher level.

DR. KRESS: Let's say, for example, that you had no leaking steam generator tubes at all and you had this break in the secondary side, you get a marked change in this delta P and the rate may even vary with time, does nothing at all to the primary system because it's just sitting there.

There's no leak coming out. It's all driven by the secondary change and should have no effect on the iodine spiking at all or the iodine levels. That's why I don't understand the statement.

DR. BONACA: The only thing you have is scram and that will have some changes in system pressure.

DR. KRESS: If you scram, yes, but that's --

DR. BONACA: That doesn't have the same effect. So the driving force, seems to me, it would be on the effect that we're looking for would be the difference between primary system pressure, driven by --

DR. KRESS: Driven by that, yes.

DR. BONACA: -- the steam generator break. I understand that. And the internal pressure of the fuel rod.

MR. HAYES: But it will change your cool-down rate, though. You say it will not an effect, but it will have, because it will change your cool-down rate, because you're using the intact steam generators to cool down the primary side.

DR. KRESS: It might have some effect, you're right. That's pretty --

DR. BONACA: What is the average internal pressure of the fuel rod?

DR. POWERS: During operation?

MR. BALLINGER: What's the internal pressure?

DR. BONACA: Yes.

DR. POWERS: An intact one?

DR. BONACA: The fuel rod in a -- well, I'm talking about an intact one.

DR. POWERS: It would run about 100 atmosphere.

DR. BONACA: That's right.

DR. KRESS: So you look at that delta P that you normally get with the steam generator tube rupture and then you look at it, once you get to the main steam line break, and that's where you get this factor of two to three.

MR. HAYES: Right.

DR. KRESS: If you square that, you get four to nine.

MR. HAYES: That's correct. So what we concluded with respect to the main steam line break, and, again, this was in conjunction with the steam generator rule and addressing the issue of conservatisms in our evaluation, was that for a main steam line break, we thought that there was probably an uncertainty factor of ten.

Adams indicated, for a steam generator tube rupture, there is a conservatism, he believed, was a factor of 15.

DR. CATTON: This is the 15 I don't like to use.

MR. HAYES: This is the 15 you don't like to use, right.

DR. POWERS: But what I don't understand is that you went through and you looked at the data and you said, okay, I'm not buying this factor of three bounding and you looked at it and you came up and you said 335 versus 500, which is not a factor of 15.

How come now, all of a sudden, you grab a hold of Adams' factor, which nobody understands?

MR. HAYES: Because we incorporated more than just that particular factor.

DR. POWERS: I kind of wish you would tell us what those things were, because otherwise, this third statement down here is just going to cause me to --

DR. KRESS: To go ballistic.

DR. POWERS: Yes, ballistic, because that is definitely not the way you handle combined uncertainty.

MR. HAYES: The imaginary axis.

DR. POWERS: I mean, this is not even -- would not be acceptable in anything that I can think of, where you take two things that you don't understand at all and say they offset each other.

MR. HAYES: We concluded that the doses associated with a main steam line break are typically on the order of one to three rem, the ABLPZ, control room.

There is also a parametric analysis which we have done to demonstrate what the spiking factors would have to be in order to get a dose which would exceed Part 100. The criteria -- remember, the criteria associated with this is for a dose of 30 rem, not 300, but for 30.

Even if you took the factor of 335, which is a value, what, of 1.5, roughly, 1.6, 7, and take this factor of ten, in our opinion, you are still within the uncertainty that we had and still within the margin.

DR. POWERS: I can buy arguments that go that direction. I just can't buy this slide. I find no technical justification for the third sentence. That's the problem. A bounding operation that came in and said, look, the sensitivity to my dose calculation or the spiking factor is smallish. I have to get a very big value that seems to strain quadrulity to approach the limit, so I'm going to leave it at 500. I'd say, okay, fair enough, that's a decent argument, and go on.

Something that's just orthogonal to the treatment of uncertainties, this gets me excited. You're more relaxed than I am.

DR. KRESS: I just don't show it.

DR. CATTON: This last statement about should remain 500, that sounds like you completely -- you have either written off or ignore or don't believe that the main steam line break can have some impact on the internals of the steam generator, because it's certainly -- at least from what we heard yesterday, it's most likely going to cause more leakers.

Shouldn't that be factored in somehow?

MR. HAYES: It's already included when we postulate that those tubes which have voltage-based criteria.

DR. CATTON: But that's before. If that's what you're doing, what you're saying is that the main steam line break does not cause any disturbance inside the steam generator. The tubes are going to stay exactly the same.

MR. HAYES: No. What we have presumed is that we have presumed that those tubes which have voltage-based criteria, those cracks are going to open up and they're going to leak at a defined rate.

In other words, every tube to which that criteria has been applied is assumed to leak.

DR. CATTON: But that leakage correlation was derived from tested tubes that have not been subjected to the main steam line break. Am I missing something?

MR. STROSNIDER: This is Jack Strosnider. I'm not sure that Jack Hayes heard the discussion yesterday on dynamic effects and some of the Surry and other experience. We're going to talk about that later today.

DR. CATTON: But it impacts this.

MR. STROSNIDER: Well, it may or may not. I think apparently you've reached a conclusion that those events are, in fact, going to cause additional damage to the tubes and we heard the discussion yesterday.

We've put this into the GSI process and we're going to hear about how that's being looked at. If, in fact, we conclude that those dynamic effects are going to have that sort of effect on the tubes, then we'll have to deal with it, but I don't think we've seen that concrete evidence to this point.

There's certainly, in my mind, some things that ought to be followed up on with regard to was there a post-event inspection of the steam generators, what did they actually find, the tubes that were indicated as having degradation in yesterday's presentation, it wasn't totally clear that that was a result of the event.

So I think it's an issue that needs to be looked at and when we determine whether it really has an impact on the tubes or not, then we need to come back and deal with it. But at this point, you're right, the model does deal with pressure-induced leakage, and we'll talk about that later today, too.

DR. CATTON: Just if you had another line down there that said that this is an assumption. See, you're basing this on the assumption that the main steam line break does nothing to the internals of the steam generator and at this point, that's your assumption. I can read it, I understand it, we can go on.

MR. STROSNIDER: Right. And we've got some new information here that needs to be assessed and we're going to do that.

DR. CATTON: That's right. But that isn't what he was telling me.

MR. STROSNIDER: And like I said, coming back to my some of my introduction this morning, we need to go through and hear all the issues and look at how they all fit together, and that's a fair comment.

DR. CATTON: That's fair enough. That's fair enough, but I would liked to have seen another line on that viewgraph.

MR. HOLAHAN: Remember, it's more than assumption. It's a requirement. Plants are licensed and that licensing basis includes looking at main steam line breaks and tube ruptures, but it doesn't include main steam line breaks damaging tubes.

And if we find some basis for thinking that that's true, we'll have to deal with it, but the licensees will have to deal with it because that will be inconsistent with the current requirements.

MR. HAYES: Now, getting to the assessment of the DPO concern, we did a parametric analysis and we presumed -- we used, as the base case, a three-loop Westinghouse plant and we did the analysis consistent with a standard review plan 15.1.5, which is the main steam line break.

And we presumed tech spec value one microcurie per gram. The primary to secondary leak rate was 150 gallons. This should be corrected on your slide to per steam generator. And this value is, instead of 1290, should be 11 -- I think it's 1140.

I didn't conclude the two steam generators, just the one. It really doesn't have an effect -- it was included in the analysis, but not in the slide.

Spiking factor was 500 and what we did is we calculated the releases for zero to two hour and zero to eight hour time period.

Some other critical assumptions that we had, we assumed that it was eight hours for the faulted steam generator to be isolated. All primary and secondary leakage was assumed to be released directly to the environment. We did not presume, for example, for the intact steam generators, any partition factor.

It really doesn't have a whole big effect on it, an assumption, it's probably in the third decimal place or third significant number.

Spiking was assumed to occur for the duration of the accident and --

DR. KRESS: What does that mean? Does that mean that you kept the rate that you calculated constant over the whole eight hours?

MR. HAYES: Yes, for the whole eight hours. You think that's conservative?

DR. KRESS: I think that is.

MR. HAYES: And here is another big assumption. We assume that the releases associated with zero to two hours and zero to eight hours equated to a 30 rem thyroid dose. Now, as I've mentioned before, the typical values we see are somewhere between probably one to five or six rem.

Then we did a parametric analysis. We assumed three different primary to secondary leak rates, ten, 35 and 100 GPM. We assumed five different primary coolant activity levels. I think your slide has an error, a typo. This should be ten-to-the-minus-two. I think in your slide it has ten-to-the-minus-three. But these are the numbers we've presumed.

DR. POWERS: Gary tells us that .005 is not and will never -- ever going to occur.

MR. HOLAHAN: That's right, and the reason is not because I'm against low primary coolant activity. It's because I'm against allowing leakage rates higher than 100-GPM, which is what the implication is. And the point is the design basis calculations may come out with a reasonable dose, but the severe accident implications I think we would find unacceptable.

I think we will cover those sort of issues later.

MR. HAYES: I may need to go put in some clarifying information with respect to that. I think we need to probably check what the value was associated with Byron and Braidwood, because I think Byron and Braidwood had a number which was either at 100 or slightly greater and the value may have been down below .05.

But that's something we need to check on, because they have subsequently -- I think that was for one cycle and they subsequently replaced their steam generators. That would have been Byron Units 1 and 2 -- excuse me -- Byron and Braidwood Units 1. Unit 2 did not change steam generators.

MR. BALLINGER: These leak rates are prior to the event or during the event?

MR. HAYES: This is during the event.

MR. BALLINGER: During the event.

MR. HAYES: During the event. What you can consider this to be is your accident-induced leakage.

DR. KRESS: You're calculating the spiking factor you would have to get to get a 30 rem dose here.

MR. HAYES: Well, it was presumed that the release that we calculated for zero and two hours gave you a 30 rem dose. It did not. It did not, but we presumed that.

DR. KRESS: Okay. And you back-calculate those to find out what the spiking factor would --

MR. HAYES: Right, exactly.

DR. KRESS: -- would give you that.

MR. HAYES: Right. Here is what we did, for example. Let me walk you through a couple cases. We presumed RCS activity was .5. We took a ten GPM leak rate. In order to come up with the same release rate, we would have to have gotten a spiking factor of 86.3.

So for example, if you wanted to operate with the ten GPM leak rate, based upon our criteria, you would have to go down to a spiking factor of 500 before it would be acceptable to the staff.

So you would go down to the -- the reactor coolant activity level would have to be reduced from .5 to .1.

DR. KRESS: I understand what you're doing now.

MR. HAYES: That's what we were doing. Okay. Now, the premise is at low rates of reactor coolant activity level, that the values would exceed Part 100 doses, that the spiking factor would be greater than at 5,000, and you would have to exceed Part 100 doses at those levels.

If you go down to ten-to-the-minus-two, you can see that even at a release rate of 100 gallons per minute, the spiking factor is at least 500 or greater. If you went down to ten GPM, you're at 5,000. That's for a dose of -- that's presuming that your dose was 30 rem.

So to exceed Part 100, it would be ten times this value. You'd have to have a spiking factor ten times this value. It would be 5,000, 6,000, 51,000, that would have to be the number.

DR. POWERS: In order to do these calculations, you had to calculate what the steady-state release rates were.

MR. HAYES: That's correct.

DR. POWERS: Do you know what those numbers were?

MR. HAYES: I would have -- I could provide them to you. I'd have to look at what they were.

DR. POWERS: I think I'd be interested in seeing an example calculation.

DR. KRESS: In order to do that, you would have had to assume something about that the size of the primary system and the capacity of the cleanup system, did you use some sort of generic numbers for those?

MR. HAYES: I did a specific plant example. I took --

DR. KRESS: This was a specific plant.

MR. HAYES: Yes. What I did, if you go back to the base case, I took a particular plant, a three-loop Westinghouse, took the numbers I had for that and then I adjusted it to these situations. So it was an actual example.

MR. BALLINGER: What happens if you try 1,000 gallons a minute?

MR. HAYES: I think the point -- first of all --

MR. HOLAHAN: Your division director goes off-scale.

MR. HAYES: You have a LOCA, you don't have sufficient makeup. You start to get above now. I think some of the people from the staff can answer that, but I think if you get much above 100 GPM, you have a problem with makeup.

MR. BALLINGER: Well, previous tube rupture events have resulted in three, 400, 500 gallons a minute.

MR. HOLAHAN: Yes, but that's not what we're talking about here. We're not talking about tube ruptures.

We're talking about acceptable post-accident leakage.

MR. HAYES: Because this is going directly to the environment. You have no water level above your release point.

I think this goes to your argument in considering safety and Gary said that, hey, if we start to go to 100 GPM, he starts to get going off-scale.

I think realistically, what do we have to be concerned about from an accident standpoint? Isn't our weak link the steam generator? So that when you start getting into these areas, you have a concern.

Most of the people we're dealing with are in the ten to 35 GPM range. That's where the numbers are at. Now, we did have one case, yes, where Byron and I think Braidwood, or maybe just one of them, was in that ballpark for either a portion of a cycle or one cycle before they replaced their steam generators.

MR. HIGGINS: And those numbers in the second column are the ones you're going to talk to us later, about how those post-accident numbers are derived for the main steam line break.

MR. STROSNIDER: You're referring to the leakage values.

MR. HIGGINS: The second column there.

MR. STROSNIDER: Yes, and we'll explain how that's derived as part of the generic letter process.

DR. BONACA: The DPO makes the contention that the ultimate repair criteria results in a change to the design basis event that will cause to have a steam line break with leakage rate exceeding this number, right?

MR. BALLINGER: That was the point I was going to get at.

DR. BONACA: Right.

DR. KRESS: You said later on you were going to discuss that point.

MR. STROSNIDER: Right. As part of our description of Generic Letter 95-05, we'll explain how those leakage rates are calculated, if you will, design basis leakage rate.

MR. HAYES: Again, reiterating, at .01, we're talking about a spiking factor of 5,000 to 51,000 in order to exceed Part 100 doses and for lower reactor coolant activity levels, you can see the number gets even larger.

Again, this is the Adams data that we spent a lot of time looking at and discussing and the maximum value is a value of 10,0000.

And, again, we presume that the releases associated with those two time periods were at the 30 rem limit, which they're not. They're another factor at least three to ten lower.

The conclusions from the table; obviously, for some combinations of leak rate and primary coolant activity levels, it would require a spiking factor of less than 500. However, for ARC amendments, this would necessitate reducing the primary coolant activity levels and that's what they do.

The spiking factor, because we use 500 in our calculations, the actual spiking factor would have to be at least 5,000 for Part 100 limits to be exceeded, and that's based upon the 500 times the 300 rem Part 100 limit divided by the 30, which is our use.

And then for primary coolant activity levels of .01 or ten-to-the-minus-two microcurie per gram, the spiking factor would be 5,000 to 51,000.

DR. POWERS: Let me ask a question on this. Suppose in my criterion, I did not take the Part 100 limits, but I took GDC-19.

MR. HAYES: Which is the third.

DR. POWERS: Control room habitability effect.

MR. HAYES: You want the -- the answer to your question is this. It's what it already is right here, because that's what it's been based upon, 30 rem thyroid, same as GDC-19. So at the ten-to-the-minus-two, we'd be at the 500.

DR. POWERS: But if I look at the Adams data or ten-to-the-minus-two, I can find numbers that exceed 500.

MR. HAYES: Yes, you can. Yes, you can. But you don't -- you see them exceeding -- this is at three times. You don't see them -- let's see. Well, these are the ones over 1,000 right here. That's with the multiplier of three. There's not a whole lot of points.

And, again, look at where we're at for 1,000. Even at .05, we're at ten GPM. We're at 35 point for ten-to-the-minus-two.

MR. HIGGINS: So was the intent of this to justify the 500 spiking factor for main steam line break? Is that the intent of this example?

MR. HAYES: No. The intent of the example was to address the DPO and to say that, hey, at these particular low reactor coolant activity levels, the source of iodine that you have in coolant, both in terms of the initial coolant and then the release rate, is not significant enough to put you over Part 100 limits, and that even if you did, for example -- let's say our factor of 500 is wrong.

I think you can see from the numbers here, at these various leak rates, you would have to have a significant spiking factor in order to exceed just the 30 rem.

DR. POWERS: I think we have a significant spiking factor. Suppose that I go to the Adams data. Suppose that I, say, factor of three that he multiplies things at, but suppose that I do buy the linear hypothesis presented earlier on the delta P.

I don't even ask for the quadriture process. I just use the linear. Then I'm back onto this slide here.

MR. HAYES: Yes.

DR. POWERS: But I'm interested in complying with GDC-19, which has just as much effect on me as Part 100. I have to take that into account just as much.

DR. KRESS: And I see no reason not to accept the factor of three. So if you use that and the other factor of three --

DR. POWERS: Then you're in serious trouble.

MR. HAYES: I don't think you are in serious problem. Look at this again. This is at the GDC-19 value. This is at 30. You have presumed that these releases are at the 30 rem, and they're not. These releases are not at the 30 rem. If you take a base case and some of you that come from plants, you take the base case for these plants for main steam line break, the value is probably no more than one to two, three rem.

For example, we just did an ARC amendment, we're doing an ARC amendment for Watts-Barr and it's a ten GPM leak. This is instead of one. And the doses, the maximum dose is two and that's at the LPZ. Control room is one.

Now, for a regular accident, the main steam line break isn't your limiting primary to secondary accident. You're steam generator tube rupture is.

So this number probably, if you actually went to one, it would probably be even lower. But we have presumed that. If you want to take -- okay. We're going to say we have an uncertainty of ten. So what, you say the spiking factor has to be 5,000 then. Spiking factor of 5,000. Okay.

At ten-to-the-minus-two, you're still at ten GPM, and look what happens when you go here.

DR. POWERS: Look, I'm not interested in looking at it, because Gary has assured me I'll never go there.

DR. KRESS: I'm only interested up here at the .1, the point level. And even at ten GPM --

DR. POWERS: I think you're in a world of hurt at ten GPM. Now, if we factor in this additional thing that, in reality, the doses for this particular event at this particular plant at the control room is, as you say, three to five rem thyroid.

MR. HAYES: Probably the maximum, yes.

DR. POWERS: It's not obvious to me we're out of the woods either. That doesn't get you quite there either.

DR. KRESS: That I presume is taking account for transport.

DR. POWERS: I think the reality is that when they do this exact calculation for a particular plant as part of the FSAR --

DR. KRESS: The atmospheric transport.

DR. POWERS: Nobody comes back and says I'm at 30 rem, they always come back and say I'm five or six and less, .5.

MR. HAYES: Sometimes, in reality, with respect to the ARC amendments, the limiting is the control room and sometimes those values are high.

One of the reasons why we stuck to releases versus calculating doses is because we threw out the atmospheric dispersion and threw out, if you will, the control room removal mechanisms. We thought that was a less biased type of approach.

DR. KRESS: That's a good thing to do if it gets you out of the woods, because you're all right, but we're not so sure that gets you out of the woods yet, because we're not sure the spiking factor might not be 5,000, for example.

MR. HAYES: At this particular point in time, the staff has accepted the spiking factor of 500. So, for example, if we were at this level, at five-times-ten-to-the-minus-two, and found at 35 we were not at the spiking factor, we would have to go somewhere between .05 and .01 and at 35 GPM, you'd be going from 283 to 1490.

So you're probably talking about .4, in that vicinity. That would have to be our acceptance criteria and that would be for 30 rem.

DR. POWERS: I think it may be safe to say we understand what was done.

MR. HAYES: Okay. Our conclusions with respect to the DPO concern, yes, we agree that spiking factors greater than 500 can occur, but they are low dose equivalent iodine-131 activity levels.

We don't believe in any case that the spiking factor would be less -- would be greater than 5,000 and based upon the parametric analysis we did, we believe that if you were at an activity level of less than ten-to-the-minus-two microcurie per gram, that a spiking factor would have to be at least between 500 to 5,000 for the base case releases.

If you look at the amount to exceed Part 100, it would have to be ten times that or 5,000 to 50,000. At primary coolant activity rates of ten-to-the-minus-two, the primary coolant content is small and the equilibrium release rate is small.

And as we mentioned, we don't believe the spiking factors are greater than 5,000.

Are there any other questions? That concludes our presentation.

DR. POWERS: Seeing no questions, I think I will declare a recess until five after the hour.

DR. KRESS: I have just one question.

DR. POWERS: I think I'm going to recess. When he gets like this, I get very nervous.

DR. KRESS: If I want to know what the spiking factor is under a main steam line break accident, where I've got leaky steam generator tubes, I have no idea what it is, from what I heard. I have no idea, because we do not have any data at all related to that subject.

I don't know whether it's 500 or 5,000 or five. That's not a question. It's just a comment.

DR. POWERS: We'll take it at that and we can puzzle it over the recess.

[Recess.]

DR. POWERS: Since you're not Joe Muscara, I assume you must be Ken Karwoski. He doesn't look like Joe Muscara. The floor is yours, sir.

MR. KARWOSKI: Thank you. Good morning. My name is Ken Karwoski. Today, with the assistance of Joe Muscara, I'd like to discuss our three issues with respect to steam generator tube integrity.

The order in the package is a little different, as Jack Strosnider indicated. The first issue I would like to discuss is the regulatory framework and operating experience to date. The second issue I would like to discuss is the technical basis for Generic Letter 95-05, the voltage-based repair criteria, including a discussion of the leak and burst correlations.

And then the third issue I would like to discuss are the capabilities and limitations of NDE with respect to detection and sizing of flaws.

The guidance with respect to steam generator tube integrity is located in various places. The general design criteria, 10 CFR 50, Appendix A has general requirements with respect to the integrity of the reactor coolant pressure boundary. Appendix B deals with quality assurance requirements. Part 100, which you've heard about this morning from Jack Hayes, and dose limits.

Regulatory Guide 1.121 contains guidance with respect to the loadings that the tubes should be able to withstand. Regulatory Guide 1.83 discusses in-service inspection guidance.

The standard review plan addresses various things, such as in-service inspection; also, the design of the steam generators and water chemistry, to some extent. The ASME code has various repair criteria and the technical specifications.

The plant technical specifications, as you heard this morning, were developed about 25 years ago, when the prevalent forms of degradation were general wall thinning. The degradation that we're observing today was not anticipated or tech specs typically specify a depth-based tube repair criteria based on that general wall thinning type of phenomenon.

The requirements for the inspection, repair, and for normal operating primary to secondary leakage are contained within the technical specifications.

The typical technical specifications in plants today, plants that have not implemented an alternate repair criteria, are listed on this slide. The first thing in the technical specifications is the sampling program.

The basic sampling program involves a three percent initial sampling of the steam generator tubes. That sample is expanded based on the categorization of C-1, C-2 and C-3.

Basically, what those categories are, it says if I have so many tubes or a certain percentage of tubes that are either degraded or defective, I need to inspect more tubes. A C-3 classification can result in 100 percent inspection. Most of the plants with extensive degradation would end up a C-3 classification.

The sampling program in the technical specifications also require a reexamination of all previously degraded tubes. With respect to the frequency of inspection, the technical specifications simply say that once every 12 to 24 calendar months, you should do an inspection. That can be lengthened to 40 months based on the categorization of C-1, C-2 or C-3, and it can be shortened to 20 months.

It also requires inspections after certain events, such as tube leaks in excess of the normal operating limit, seismic occurrence, LOCA, and the steam line break.

With respect to the extent of the inspection, it basically says that you are required to inspect the hot leg of the tube around the U-bend to the top support plate on the cold leg side.

The technique for inspection is not specified, and I already mentioned that the repair criteria is typically 40 percent of the through-wall and it's applicable to all forms of degradation.

So that's what you will find in most technical specifications today for plants that haven't implemented an alternate repair criteria.

The 40 percent depth-based limit was based on guidance in Regulatory Guide 1.121. There are several structural criteria that the tube is required to meet. Typically, the most limiting is that the tube should be able to withstand a pressure differential of three times the normal operating pressure or 1.4 times an accident differential pressure, and, typically, the most limiting is the steam line break.

In addition, Regulatory Guide 1.121 indicates that the normal operating primary to secondary leakage limit should be based on a limiting crack length, that length that would be limiting in terms of the structural criteria of three delta P or 1.4 times steam line break.

Down here, basically what I have is a simple derivation of the 40 percent plugging criteria. Basically, assuming a general wall thinning, you need 40 percent of the tube wall in order to withstand the three delta P or 1.4 times steam line break, and if you include an allowance of ten percent for growth and ten percent for NDE uncertainty, you would arrive at 40 percent repair criteria.

DR. POWERS: Let me understand this. The 40 percent is the result of considering NDE error and uncertainty.

MR. KARWOSKI: Regulatory Guide 1.121 indicates that both NDE uncertainty and crack growth need to be accounted for in the plugging limit.

DR. POWERS: Right.

MR. KARWOSKI: Given that there is a -- the repair limit is 40 percent, there is roughly a 20 percent margin for both. Whether or not it was explicitly called out, the ten percent, each one.

DR. POWERS: But the bottom line is you're saying with 20 percent of the wall, you can meet the three times normal operating or 1.4 times maximum allowable.

MR. KARWOSKI: With 40 percent of the wall. With 40 percent of the wall, you would be able to withstand roughly three times --

DR. POWERS: I guess what I'm asking is if I had a tube --

MR. KARWOSKI: It's 60 percent.

DR. POWERS: If I had a tube that I absolutely knew had 20 percent of the wall there, an NBS standard tube, if you will, had 20 percent of the wall left, would I be able to meet -- now, I think we've had tests that said you do with 20 percent.

MR. KARWOSKI: I don't believe so. It's roughly 40.

MR. STROSNIDER: I'd suggest it depends on the type of degradation. For the analysis Ken's talking about, where the tube was assumed to be uniformly thinned, you would still meet ASME code, if you were uniformly thinned and the amount of material missing was 60 percent.

So if you had 40 percent remaining, you'd still meet the code allowables.

When you start looking at cracks and other types of defects, you may be able to withstand something deeper and still meet the code factors of safety.

DR. POWERS: Okay. I think I understand. Thank you.

MR. STROSNIDER: It depends on the length of the flaw.

MR. KARWOSKI: So what are some of the issues? Over the last few days, you've probably identified a lot of issues with the current regulatory framework. The major goal of the steam generator tube inspections is to ensure the structural and leakage integrity for the operating interval between inspections.

Structural integrity per Reg Guide 1.121 and the ASME code, and leakage integrity per Part 100 and GDC-19, as was pointed out this morning.

As you know, the technical specifications do not reflect either the current degradation modes or the inspection technology that we have today. The repair criteria of 40 percent tends to be conservative for cracks.

The inspection sample size, the expansion criteria and frequency do not explicitly take into consideration the severity of the degradation.

It is based on that classification of C-1, C-2 and C-3, which more is a function of the number of tubes that either exceed the repair limit or are degraded.

And as we know, the leakage limits don't prevent tube burst.

As a result of these shortcomings, the NRC and the industry have been taking action over the last several years, for quite a long time. I've listed here various efforts that have been underway. Some of the industry efforts are that they have improved their examination guidelines. I think the original version came out sometime in the early to mid 1980s.

They have subsequently revised those several times based on lessons learned and based on the changing forms of degradation and the technology.

The industry has a steam generator management program which actively participates with the NRC on various steam generator issues.

In addition, the industry, as Jack Strosnider pointed out this morning, in NEI-97-06, they've adopted a condition monitoring and operational assessment philosophy. I'll discuss those a little later, but basically what that involves is condition monitoring. It is a backwards look to make sure that you operated safely during a cycle. Operational assessment is a forward look to make sure that you can safely operate during the period of time before your next inspection.

With respect to some of the NRC efforts, the NRC has issued, over the last ten years, several generic letters, Generic Letter 9503 on circumferential cracking of steam generator tubes, Generic Letter 9705 on steam generator tube inspection techniques, and Generic Letter 9706 on the degradation of steam generator internals.

We've also issued numerous information notices on various topics, including sleeves, plugs, U-bend degradation and other inspection related issues.

In addition, the NRC has an extensive research program with respect to steam generator inspection and repair criteria.

As I previously mentioned, NEI has their guidelines, NEI-97-06. The staff has also developed draft regulatory guide DG-1074, both of which address tube integrity. Those are now being rolled up into an industry initiative to address some of the issues with respect to steam generators.

With respect to what we've observed to date, this picture just shows some of the -- shows a lot of the degradation mechanisms affecting steam generator tubes.

Just to go over some of the more prevalent ones, in the tube sheet region, which is depicted in these pictures, you have a variety of degradation mechanisms, including the buildup of sludge on top of the tube sheet. You have axial outside diameter stress corrosion cracking, pitting and wastage can occur in the sludge pile.

At the expansion transition, the region of the tube where it goes from expanded to unexpanded, you have both circumferential and axial primary water stress corrosion cracking and outside diameter stress corrosion cracking.

At the tube support plate elevations, you can have fretting, wear and corrosion thinning, the dominant degradation mechanism back in the '70s. You can have axial oriented outside diameter stress corrosion cracking and intergranular attack.

At dented intersections, we've observed axial primary water stress corrosion cracking. We have also observed circumferential outside diameter stress corrosion cracking and primary water stress corrosion cracking.

We've also observed fatigue at the upper most tube support plates and also in the wedge reason of B&W plants, although that picture won't show that. They have once-through steam generators rather than U-tube steam generators.

We've also observed free span cracking, free span outside diameter stress corrosion cracking, and we've also observed cracks in the U-bend.

DR. POWERS: In the U-bends, do you have -- I don't know how to describe it well -- a bend is made and it's too much and so they bend it back, so you get kind of reverse bends on things.

MR. KARWOSKI: Sometimes that occurs, but --

DR. POWERS: And is that a site of --

MR. KARWOSKI: That has been a site of corrosion. So what are some of the factors affecting tube degradation? I think Dr. Hopenfeld yesterday touched on many of these. Tube material, including the heat treatment. The degradation mechanisms that I just had up there are primarily observed in alloy-600 mill annealed steam generator tubing. That's basically most of your older plants, with their original steam generators.

There has been relatively little degradation in alloy-600 thermally treated steam generators, which are the later vintage of steam generators and some of the initial replacement steam generators.

The tube material of choice these days for the replacement steam generators are alloy-690 thermally treated.

DR. POWERS: Now, I understand that in Europe, they use something else, 800 alloy maybe.

MR. KARWOSKI: In Germany, they have used alloy-800.

DR. POWERS: And is there anything substantially superior or inferior to 800 relative to 690 and 600?

MR. KARWOSKI: I don't think I can --

MR. BALLINGER: The 800 works a little bit better if you're in phosphate chemistry and the like. It doesn't waste, doesn't get wastage like 600 did, does. So replacement steam generators in Europe are all going to be pretty much 690. I don't think there's any 800 that's going to be used for replacement generators.

MR. KARWOSKI: Other factors affecting tube degradation, grain size, carbide distribution, the fabrication of the tubes and stresses. For example, the expansion joints at the -- where the tube goes from the expanded to unexpanded region, there's been various means for expanding those.

Initially, utilities would role expand those. They tried to lessen the stresses at the transition. They then went to an explosive transition and currently most people now do a full depth hydraulic expansion.

DR. POWERS: Let me ask a question about carbide distribution. I see in the many documents we've been provided three types of carbide distribution, called, imaginatively, one, two and three.

I think I understand one. I don't understand the distinctions between two and three.

MR. KARWOSKI: I'm not sure of the reports you're referring to. Other people may be more qualified to address that later, too.

DR. POWERS: Okay.

MR. KARWOSKI: Tube support plate design and material effects, tube degradation, operating temperature and stresses, the water chemistry, operating time and the presence or absence or crevices.

There have been a number of tube ruptures. I've listed ten tube ruptures here. Some people call an event or a leak at Fort Calhoun a rupture, I did not include that.

The definition of rupture I used here is leakage in excess of the normal makeup capacity of the plant.

Just going over each one of these ruptures, as you can see, there's ten listed. Of these ten, eight have occurred in the U.S., two in foreign PWRs.

The first rupture occurred in 1975 at Point Beach. The rupture was primarily attributed to wastage. The tube wasn't pulled for destructive examination, but they think stress corrosion cracking may have also played a role.

The next steam generator occurred at Surry-2 in 1976. That was axial primary water stress corrosion cracking up in the U-bend of the steam generator.

The Surry rupture was attributed primarily to denting at the uppermost -- at the tube support plates forcing the tight radius U-bend tubes closer together and parting a stress up in the apex of the U-bend.

The Doel rupture occurred in 1979. It was also axial primary water stress corrosion cracking in the U-bend. However, in that case, they attributed the rupture to the bending process and the fact that there was ovalization of the tube that wasn't in accordance with specifications.

In 1979, there was a rupture at Prairie Island due to a foreign object. In Ginna, in '82, another foreign object. There was some discussion on the magnitude of the leak rates from a rupture. This rupture was on the order of 760 gallons per minute.

In 1987, there was a rupture at North Anna which was attributed to fatigue at the upper most tube support plate. Some of the factors affecting that was denting and improper or inadequate ABB support up in the U-bend region.

In McGuire, in '89, there was a free span rupture was a result of axial outside diameter stress corrosion cracking. That was on the cold leg side. The crack was associated with a manufacturing scratch.

In 1991, there was a rupture at Mihama, which was very similar to the event at North Anna-1.

In 1993, at Palo Verde-2, there was a rupture as a result of free-span cracking. This was on the hot leg side. The utility attributed that, in part, to a dryout region, which they refer to as an arc, which is present in the outer periphery of the bundle up in the top. Then Indian Point-2 in February of this year, which was a result of primary water stress corrosion cracking in the U-bend.

In addition to ruptures, there have been a number of leaks that have resulted in forced outages. Basically, what I show here is the number of forced outages as a function of year.

As you can see, back in the '70s and early '80s, there is a number of forced outages for a variety of reasons. Here in the '90s, there have been -- if you look at this literally, you could say that there has been a decreasing trend in the number of forced outages as a result of leakage.

Some of the shutdowns that are on that graph were initiated because the plant exceeded the primary to secondary leakage limit in the technical specification. In the standard technical specification, that limit is typically around 500 gallons per day through any one steam generator.

Some of these plants shut down voluntarily before the leakage exceeded those limits. In addition, I wanted to point out that some plants have operated with leakage over the course of a cycle and then shut down at the standard refueling outage.

Some of the causes of shutdowns as a result of leakage in the 1990s include sleeves, primarily the B&W kinetically expanded sleeves. That was the result -- that was the cause of the Trojan leak back in the '92 timeframe.

MR. HIGGINS: The typical tech spec limit that you mentioned of 500 gallons per day, the new NEI document that Jack had mentioned that the plants had all committed to has got a number of 150 GPD. Does that mean that the plants are now all observing that versus the 500?

MR. STROSNIDER: The plants have implemented administrative limits reflecting the 97-06 guidelines.

MR. HIGGINS: Thank you.

MR. KARWOSKI: So that the B&W kinetically expanded sleeves resulted in a number of leakers back in the early '90s. The last steam generator that has these sleeves installed is being replaced now at ANO-2.

There's been leaker outages in the '90s as a result of plug leakage, loose parts, fatigue primarily in the B&W once-through steam generators, in the lane wedge region or in the area bordering the lane wedge region, and leakage has been observed as a result or forced shutdowns have resulted as a result of leakage due to stress corrosion cracking at expansion transition, tube supports and free spans.

A number of plants have replaced their steam generators. I believe there's 25 plants that either have replaced or are replacing. Replacements started in the 1980s. Cook finished in July of 2000. ANO-2 and Indian Point-2 are currently replacing right now.

With respect to the tube materials, I'll just point out that these early replacements were all alloy-600 thermally treated. When you got to Cook-2, most of the ones after this are alloy-690 thermally treated, with some exceptions.

Palisades used steam generators available at another plant, so I believe these are 600 mill annealed. Salem also used previously available steam generators at a cancelled plant, so these are 600 thermally treated. And the Indian Point-2 steam generators, I believe, are 600 thermally treated. The rest are 690.

A number of plants also have indicated that they plan on replacing steam generators. Some of these replacements are a result of tube degradation or as a -- or in combination with license renewal, they believe that they will need new steam generators in order to operate 60 years.

To date, only steam generators from Westinghouse and CE have been replaced. Some of the replacements in the next ten years will probably be in B&W units, as well.

As a result of all the tube degradation, a number of utilities have proposed various alternate repair criteria. One of the first alternate repair criterias was for degradation within the tube sheet region.

When you have a tube fully expanded against the tube sheet, you only need a certain length of engagement in order to ensure that the tube does not pull out during accidents. The remainder of the tube can be degraded without any significant effect on the structural or leakage integrity, and that's what these repair criteria are basically for, degradation in the tube sheet area.

Of more interest are the alternate tube repair criteria that has been implemented at the tube support plate elevation with respect to predominantly axially oriented outside diameter stress corrosion cracking.

I've listed the plants that currently have this implemented. There have been other plants, but they have either subsequently replaced their steam generators or ceased operation.

Beaver Valley, Comanche Peak, Diablo, Farley, Kuwanee, Prairie Island, Sequoyah and South Texas currently have repair criteria and, I believe, as Jack Hayes indicated, there's others that are being reviewed now.

There's also been an alternate tube repair criteria for axially oriented primary water stress corrosion cracking at or near dented tube support plates. That's been approved on an interim basis at Sequoyah.

That concludes the first part of the presentation with respect to the regulatory framework and operating experience. I'd be happy to answer any questions on that.

The next part of the presentation deals with Generic Letter 95-05. This will tend to be lengthy. I'm not sure if you -- do you want to start this at this point?

DR. POWERS: Why don't we go ahead and start it.

MR. KARWOSKI: Okay.

DR. POWERS: And I presume that there will be some point in there that it's logical to take a break. Or is it continuous?

MR. KARWOSKI: I think this one might be continuous.

DR. POWERS: Okay. I run into problems starting early and starting late is okay, starting early is a problem. So I think what we will do is just interrupt you at 12:00.

MR. KARWOSKI: Okay. Generic Letter 95-05 addresses one form of degradation, axially -- predominantly axially oriented outside diameter stress corrosion cracking at the tube support plate elevations.

There's two fundamental goals of the repair criteria in this generic letter, to ensure adequate structural and leakage integrity.

The evaluation of structural and leakage integrity require periodic inspections. It requires correlating those inspection parameters with the tube structural and leakage integrity and evaluation of the tubes accepted for continued service.

And I apologize, this is where I've jumped ahead in the presentation. It's page 10-27. And I also apologize, as a result of some of the presentations yesterday, there are some additional slides that I have prepared to address some specific comments.

DR. POWERS: Thank you.

MR. STROSNIDER: Ken, I think we might also mention, I think we provided -- there was discussion yesterday of some of the proprietary data and I think we provided copies of that information for the panel, or we will.

MR. KARWOSKI: Yes. This is the proprietary information containing the data in the database.

MR. STROSNIDER: And I would just point that because of it's proprietary nature, we won't be presenting it on the screen, but the members of the panel will have it so they can look at it, and, of course, need to treat it as proprietary.

MR. KARWOSKI: Okay. So just so that everybody understands what degradation mechanism we're talking about, Generic Letter 95-05 primarily addresses axially oriented outside diameter stress corrosion cracking at the tube support plate elevations.

It does not permit -- or it does not apply to circumferential cracks, primary water stress corrosion cracks or cracks that go outside the tube support plate, and it does not apply to general wastage or thinning.

As I mentioned, in order to ensure the structural and leakage integrity of the tubes, you need to do inspections. The generic letter specifies specific inspections that must be performed. Take this into context of the current regulatory framework, which says three percent initial inspection and expand based on the results.

GL-95-05 requires the licensees to perform 100 percent bobbin coil inspection; basically, all the way around to the cold leg, to the point where they've observed degradation, and a 20 percent sample at the next tube support plate elevation.

I say that because that's what is in the generic letter, practically speaking, everyone does 100 percent tube end to tube end.

The bobbin coil allows for a rapid screening of the tubes for defects. The extent of the degradation is measured in terms of the voltage response for the defects at the tube support plates.

There are detailed procedures to ensure that the voltage response of the degradations being measured in the field is comparable to those in the structural and leakage integrity databases.

So basically it tells the analyst what size probe to use, what frequency mix to use to size the degradation. It instructs them to record the maximum voltage response at that location, as the voltage.

So there are detailed procedures with respect to the data analysis.

MR. HIGGINS: Can you say what that database is?

MR. KARWOSKI: I'll get into the database in a few slides. In addition to the bobbin coil examinations, rotating pancake coil examinations are performed. This permits a better characterization of the defects to ensure that degradation is confined within the tube support plate and is predominantly axial.

Make sure that you're not applying this to circumferential degradation or other forms of degradation; that you can get some idea of the morphology as a result of the rotating pancake coil.

DR. POWERS: You tend to speak of cracks as axial or circumferential. Is there a case where things are at an angle?

MR. KARWOSKI: Absolutely.

DR. POWERS: And how do you make a distinction between axial and circumferential when you're at an angle?

MR. KARWOSKI: There is some analyst judgment involved. With respect to that specific issue, if you look at the database, clearly, from the metalography, you can tell -- you will see that the degradation at the support plates occurs in networks or is a cellular type of corrosion.

So there are oblique angles. There may be short segments that are circumferential in extent. With respect to the eddy current data evaluation, though, those typically, what you will see is you will see a pattern.

Usually those short circumferential extents will not be discerned in the NDE examination.

MR. SIEBER: So the NDE doesn't tell us about circumferential cracks.

MR. KARWOSKI: It cannot readily detect the short segments. It will find, and that will be this afternoon, it will find distinct circumferential cracks or large circumferential components. It can do that, and I will present some data to show that.

MR. STROSNIDER: Ken, and you may get to this later, it might also be a time to interject that there is a tube pull requirement associated with it. Are you going to talk about that?

MR. KARWOSKI: Yes.

MR. STROSNIDER: Okay. But I would just interject that plants are required to periodically pull tubes to verify that the degradation mechanism is consistent with what's in the database and what they've seen in the past, but Ken will talk about that.

MR. KARWOSKI: These rotating pancake coil examinations are performed at intersections with degradation exceeding specific voltage limits and I will discuss the limits, but basically it's one volt for three-quarter inch tubing and two volts for seven-eighths inch tubing. There are two correlations, depending on the size of the tubing.

The plants -- the Westinghouse plants that this affects are either three-quarter or seven-eighths inch tubing.

DR. CATTON: And you find these with the bobbin coil.

MR. KARWOSKI: You find these indications with the bobbin coil.

DR. CATTON: Then you do a detailed evaluation with the rotating pancake coil.

MR. KARWOSKI: That's correct, and I will discuss a little more on what you do with the rotating pancake coil examination results.

You also perform these rotating pancake coil examinations at tube support plate elevations, where the dents exceed five volts. Part of the reason for doing that is because in highly dented intersections, the bobbin coil is relatively ineffective. The rotating pancake coil gives you a better inspection.

You also perform these examinations at tube support plate elevations with copper deposits. The reason for that is because the pancake coil will give you a better inspection. And also at locations with large mixed residuals, for the same reason.

DR. POWERS: Maybe you should explain what you mean by mixed residuals.

MR. KARWOSKI: What mixed residuals are are when you do these inspections at the tube support plate, depending on the frequency, you will get a response not only from the tube, but also from the support plate.

So what you do, you're using a multiple frequency probe, you will -- you take one frequency that is more sensitive to the tube and another frequency that is more sensitive to further out or the tube support plate and you essentially mix out the signals. That's not a 100 percent perfect. There are some what's called residuals and so if you have large mixed residuals, you will inspect those with the rotating pancake coil to give you a better examination.

MR. STROSNIDER: Ken, I guess that large mixed residual, it basically looks like distortion of the eddy current signal.

MR. KARWOSKI: Yes.

MR. STROSNIDER: When the analyst looks at it, it's an amount of distortion that's in the signal, because the mixing isn't perfect.

MR. KARWOSKI: So I've discussed the inspections and I've tried to give you an idea that there are detailed procedures in order to interpret the voltage and to characterize the defects, but that's only one part of the picture.

You also have to have correlations correlating that inspection parameter to the structural and leakage integrity of the tubing.

The correlations come from two primary sources, tubes removed from operating steam generators and specimens produced in model boiler facilities. The specimens produced in model boiler facilities span a larger range than the data from tubes removed from operating steam generators, because, in general, the voltages observed in the field typically aren't as great as you can produce in a model boiler.

And as Jack Strosnider pointed out, there is a periodic tube pull program for confirming the degradation mode at the plant. There's an initial tube pull that involves a couple tubes and, I believe, four intersections.

After that initial tube pull, there is a periodic tube pull requirement, which involves pulling additional intersections at a frequency of about every two or three outages.

The examinations performed on these tubes, I'll start down here. You perform a metallurgical examination to make sure that the degradation mechanism is consistent with that observed at other plants and for -- and is consistent with the other data in the database.

You also do leak testing, what's involved here is the tube is pressurized internally and on the outside. It is taken up to steam line break pressure at temperature of around 600, 650 degrees Fahrenheit.

The first thing that they do is determine whether or not it leaks or not. If the tube leaks, then they go on to measure the leakage. If the tube doesn't leak, then they just use the data in the probability of leakage correlation, which I will be discussing later.

DR. POWERS: In all cases, the testing is done at temperature?

MR. KARWOSKI: I don't know if I can say all cases, because I don't recall from that --

DR. POWERS: We'll accept 90 percent.

MR. KARWOSKI: The vast majority, and I would also like to point out, but I need to point out, it may not be the exact temperature and there may need to be some adjustments to the data. It may be taken at 2,603 PSI instead of 2,650 and maybe taken at 580 degrees F instead of 620. So there are adjustments that need to be made to the data.

The burst testing is performed at room temperature. Typically, after the leak testing, they will take that specimen, they will insert a bladder. The reason for the bladder is to prevent excessive leakage from preventing them to achieving burst.

They will burst test the tube at room temperature and they use that burst pressure in the correlations. When I get to the slides on the burst pressure correlation, all the data is normalized to a specific burst pressure and they use lower tolerance. Then they scale it up to operating temperature and take a lower bound, and I'll point that out on the correlations.

So that's the testing that is performed. Earlier, I discussed the regulatory criteria. Typically, the most limiting is that the tubes must withstand a pressure differential of three times the normal operating or 1.4 times the maximum postulated accident or steam line break.

This roughly turns out to be around 3,660 PSI. For degradation at the support plate, during normal operation, the plate is present. I should point to this one because this is the degradation mode. That plate is present.

As a result, the criteria for three times the normal operating pressure is met during normal operation. The degradation is confined to within the tube support plate region.

Given the clearances between the tube and the tube support plate, that tube will not burst. So the three delta --

DR. POWERS: I guess the question comes up, when you say it's combined within the support plate, what exactly does confined mean?

MR. KARWOSKI: The degradation, in general, does not exceed -- does not extend above or below the tube support plate.

DR. POWERS: At all.

MR. KARWOSKI: There have been pulled tube data where there has been some minor extension of the outside diameter stress corrosion cracking beyond the plate. To my knowledge, that has only been discovered as a result of destructive examination in the cases I'm familiar with and it's only on the order of .02, .03 inches beyond the plate, and it's typically attributed to some slight deposits which basically come up along the side of the tube.

DR. KRESS: If that is found by the NDE techniques to extend beyond, then that's excluded from this being confined.

MR. KARWOSKI: There is a reporting requirement in the tech specs that if they find that degradation, they have to let us know because it will draw on the question, the validity of all the arguments.

If you look at our understanding of this phenomenon, it's basically crevice corrosion in this location, all the pulled tube data has suggested that degradation is confined within that support plate region. If they find it by NDE, there is a reporting requirement to address that.

DR. KRESS: Then it's treated like a crack that's outside the confined area.

MR. KARWOSKI: Yes. We would have to question whether or not they should even implement the repair criteria not only at that location, but at other locations in the plant. That's the purpose of the reporting requirement.

DR. BONACA: But can you detect it if you have just a fraction of an inch?

MR. KARWOSKI: That pulled tube data that I was referring to where there was a minor extension, that was not detected in the field. A .02, .03 inches will not be detected in the field.

On the other hand, it probably will not have a significant effect on the burst pressure of that tube.

MR. BALLINGER: A question. Circ cracks are plugged on detection, right?

MR. KARWOSKI: Yes.

MR. BALLINGER: Getting back to this degree of circumferentiality issue, when you have a network of cracks in the TSP, in the support plate region, is there some kind of judgment that has to be applied to -- since you can't see that with a bobbin coil and the rotating pancake doesn't work too well either for that kind of situation, what happens if there is a likelihood that you've gotten an equivalent circumferential crack there?

How do you deal with that?

MR. KARWOSKI: That will show up in the burst pressure database. The correlations are all empirical. So you've pulled a variety of tubes with given voltages. You also have a number of tubes produced in model boiler specimens.

Those tubes are somewhat representative of what's out in the field or they are representative of what's out in the field. During those tests, if you were to have a, quote-unquote, limiting circumferential crack, you would have observed a circumferential failure.

That's not what we've been observing today. That's part of the reasons for the periodic tube pull examinations to confirm that that is not occurring.

MR. BALLINGER: So that would be picked up in the tube pull.

MR. KARWOSKI: Tube pull and if you do a -- when you do your rotating pancake coil examinations, which are basically of most indications above one or two volts, if you had a large circumferential extent, you would notice.

DR. CATTON: When you do a tube pull, do you literally snake that entire tube out of there?

MR. KARWOSKI: Basically, what's done is they will cut the tube at a specific location.

DR. CATTON: And pull.

MR. KARWOSKI: And pull it through the tube sheet.

DR. CATTON: What do they do with the rest of the tube, it just stays there?

MR. KARWOSKI: It just stays there. They frequently stabilize it, depending on what they believe the tube will whip around, if they believe there's going to be some damage, but they'll stabilize that tube.

MR. STROSNIDER: Ken, we might mention, too, I think there was a little discussion yesterday. During the tube pulling process, and it's not always easy to pull these tubes out. There can be some --

DR. POWERS: Is it ever easy to pull these tubes out?

MR. STROSNIDER: There's a possibility of some change in terms of the defect that you're trying to get at and, again, I don't want to get ahead of you, Ken, but I think when you look at what's plotted in the database, it's the in situ voltage versus what was tested after you pulled it. And during the pulling it, it's possible that ligaments might tear or whatever, but in general, the pulling is not going to make the burst pressures or leakage characteristics better. It's going to make it worse.

So there is some conservatism in that.

MR. KARWOSKI: Right. And if you did have a large circumferential network which was limiting, when you're pulling that tube, there's some extreme forces, it would break.

And that has occurred for some circumferential cracking at the top of the tube sheet where the licensees have attempted to get those specimens out. They go to the tube pull, they pull it and basically it rips. But that has not been observed at the support plate elevations for which this generic letter is applied.

As I discussed, there are two correlations. This doesn't have any data, it's old, so it won't match the data that I've presented or that I've provided to you, the proprietary information, but it will help me illustrate the points.

This correlation is for seven-eighths inch diameter tubing. We have the burst pressure over here and we have the bobbin voltage on a log scale over here.

If you look in your package, you will see the data point scattered throughout. You will see a mean regression curve, a lower 95 -- a mean regression curve where the data has been normalized to specific material properties, a lower 95 percent prediction interval.

DR. POWERS: It's the 95 percent confidence level for a prediction drawn from the correlation?

MR. KARWOSKI: This is the 95 percent prediction interval associated with this mean regression curve.

DR. POWERS: What I find remarkable about that curve is that as you move away from the mean of the data, those curves typically expand out a lot. In principal, they go to infinity at -- well, they go to zero and on the other side they go to infinity, if you get far enough away from the mean of the data, and this curve does not seem to do that.

MR. KARWOSKI: You asked me that question a number of years ago and I did research after. If you were to blow this up and expand the scales, you would see the exact effect that you're talking about.

You just don't notice it on the scales here, but you are absolutely correct. When you blow that up, you see that -- see the curves with that trend, the blow-up way down here.

DR. POWERS: I'm confident that -- I mean, I'm encouraged that my intuition is good. I'm surprised I don't see it, because it did look awfully scattered.

MR. KARWOSKI: Yes. But you do observe it when you blow this up.

As I mentioned, this is the mean curve adjusted to a specific set of material properties. The lower 95 percent prediction interval. Because material properties in the steam generator tubes vary, they adjust that for the lower 95 percent material properties and they get this dotted curve down here.

In order to determine the repair limit, basically, you take the intersection of this curve with your limiting regulatory guide pressure, which is around the 3660 PSI, you come down, you get a limit of 8.8 volts.

That is then consistent with Regulatory Guide 1.121. You take off allowances for growth and NDE uncertainty, and I'll discuss this in a little bit, and you get a repair limit at which tubes would need to be plugged, and I will discuss that because this is not what we've accepted as a result of some of the assumptions made in the growth and NDE uncertainty.

This next viewgraph here, I'll just discuss it from this. The industry's original proposal said basically we would like to implement a five and a half volts. Anything above five and a half volts we'll leave in service. I'm sorry. Anything less than five and a half volts we'll leave in service, anything greater than, we would plug.

The values have changed and it's evolved over time, but basically the staff was concerned with this approach given that back in the '95 timeframe, most of the data out here was from model boiler specimens. The pulled tube data was relatively scarce and it was all centered in the lower voltage regions.

Because of that, because you can have higher than average growth rates which are used in the calculations and your NDE uncertainties aren't limited and a variety of other reasons, the staff chose to use lower voltage limits.

In the case of seven-eighths inch diameter tubing, we chose two volts, and, for three-quarter inch tubing, one volt because of differences in the correlation.

MR. SIEBER: Just a quick question. The 1.4 times the steam line break differential, the other requirement is three times the normal operating differential, which I presume, for Westinghouse steam generators, is either 1550 or 1600, depending on the model.

So that would come out to be 4800 on that chart.

MR. KARWOSKI: That's right. But if you remember from this plot here, during normal operation, this plate will be in place.

MR. SIEBER: Okay.

MR. KARWOSKI: That plate is in place. That three delta P, that tube is not going to --

MR. SIEBER: So you don't consider it.

MR. KARWOSKI: Right.

MR. SIEBER: You don't consider that, okay.

MR. KARWOSKI: We don't consider it. So how are these repair limits implemented? This is not in your handout, I don't believe. This is a subsequent -- I didn't plan on getting into all of this.

Below the lower bobbin voltage repair limit, and what I mean by that is if you go in and inspect and find something less than one or two volts, you can allow those tubes to remain in service.

Those are tubes that can be left in service.

DR. POWERS: Yesterday we had several mentions of three volts.

MR. KARWOSKI: I will get into that at the very end, but I want to point out that that three volt criteria, although it is a modification of this approach, it is not the same. It is not the same as what's in Generic Letter 95-05.

There are similarities and some of the data is the same, but there are differences.

MR. HIGGINS: On the last slide, you said that you had implemented a lower repair limit as opposed to the five and a half.

MR. KARWOSKI: Right.

MR. HIGGINS: And you're saying now that it's one or two.

MR. KARWOSKI: It's one volt for three-quarter inch diameter tubing and two volts for seven-eighths inch diameter tubing.

Between the lower voltage repair limit, this one and two volts, and the upper bobbin voltage repair limit, which would be the equivalent of the 8.8 volts that I showed you -- I'm sorry -- the 5.5 volts, you need to do RPC inspections for all those indications. That confirms your degradation morphology and will give you added confidence that the degradation is within the support plate.

Any indications that are not confirmed by RPC can remain in-service. What I mean by not confirmed is the bottom coil has a certain detection threshold. The RPC has a certain detection threshold. The bobbin tends to be more influenced by noise and other masking features. You might call something that is not a flaw a flaw, or the RPC's threshold of detection is different.

If you don't confirm it by RPC, then you can leave that tube in service, and the reason is that the RPC is typically less sensitive to interfering signals. So things that you might have caught with the bobbin may not actually be flaws.

However, they may be flaws, but RPC is less sensitive to shallow crack networks, but it's at least equally sensitive to deep cracks.

So what that means is even though that the RPC isn't seeing it, it's probably not significant.

DR. POWERS: Now, that presumes that shallow crack networks are not going to coalesce and make a deep crack.

MR. KARWOSKI: That will be handled in the growth rate analysis that I'll get into in a minute, but yes and no. I will point out that even though these tubes are allowed to remain in service, the industry, I believe, originally argued that they should not include them in the probabilistic calculations that I'll be discussing.

The staff said yes, you need to include those, even though you don't believe there is degradation there.

DR. POWERS: Okay.

MR. KARWOSKI: You need to address them.

DR. POWERS: You're setting the stage for that part of your talk that will take place after the lunch break.

MR. KARWOSKI: Actually, this might be a very good ending point. Above the upper bobbin voltage repair limit, the indication must be repaired regardless of RPC results. Licensees are required to RPC even those tubes that they are going to need a plug, for the obvious reason. Those are the ones that will start probably showing circumferential extent, extending outside the support plate.

We wanted to make sure that there is nothing going on there that we need to be aware of before the repair criteria is implemented.

DR. CATTON: And repair means plug.

MR. KARWOSKI: Plug or sleeve. If the plant is licensed to sleeve.

DR. POWERS: And the extent of Ken's talk here is a prologue for all this in-depth analysis he's going to do for us. Are there any questions you want to pose to him now?

MR. STROSNIDER: If I could interrupt for just a second, Ken. I don't know if this is the best time or not, I'm going to put him on the spot here. With regard to the correlation in the database for burst pressure, I just wanted to point out that, I think you've got it in front of you, there's a substantial number of data points and part of the discussion about whether there -- first of all, it is an empirical model. So some of the things you need to look at are what sort of correlation coefficients, how much data, can you do reasonable statistics with this.

And I would suggest, if you look at the amount of data and the care that's been taken to make sure that it's the right population to compare to the steam generators, there is a good basis for doing this sort of empirical evaluation.

And you'll see more when we get into the leakage and other correlations.

MR. KARWOSKI: In the proprietary handout I gave you, you have all the data or you have all the correlations and some of the correlation coefficients that Jack was talking about, but basically here is a summary for the burst correlation, since we were discussing it, for three-quarter inch tubes and seven-eighths inch tubes.

Basically, there's 96 and 91 data points, correlation coefficient --

DR. POWERS: If I were to ask you a question, what is the probability that a random data set would produce such a high R-square given that there are 96 points, what would you answer?

MR. KARWOSKI: I would probably refer to the statistician.

DR. POWERS: R-squared values are virtually useless. The important point to understand is what's the probability the random data set would produce such a high value of R-squared.

MR. KARWOSKI: Just since we're on that point, we have had a statistician look at any of these correlations. The statistician has had a tremendous impact on the leakage analysis. We've had these correlations looked at.

DR. POWERS: I'm fascinated in what you have to say about something at the 12 percent R-squared.

MR. KARWOSKI: That's the interesting one, actually.

DR. POWERS: At this point, I want to recess, and apologize to Ken for interrupting his presentation, it's going awfully well, a very nice presentation, but let's recess and come back at 1:00. At that time, Dr. Kress will be chairing the session.

[Whereupon, at 12:05 p.m., the meeting was recessed, to reconvene this same day at 1:00 p.m.]

AFTERNOON SESSION

[1:05 p.m.]

DR. KRESS [Presiding]: Can we come back to order, please? I will be the substitute Chairman till Dana gets back, which may be not too long from now.

So we'll continue now where we left off before lunch.

MR. KARWOSKI: Thank you. Okay, just to go over quickly what I did this morning, this morning I showed the first pressure correlation.

I showed you how we determine the tube repair limits with respect to the one-volt, two-volts, and the upper repair limit. I also discussed the inspections that were performed.

That's the deterministic approach with respect to addressing structural integrity, but as I pointed out, there are several assumptions with respect to lower tolerance limits, material properties using a lower, 95-percent prediction interval.

As a result, from a structural integrity standpoint for the entire steam generator, you need to take a bigger look at what the cumulative effect of all the uncertainties and of some of your assumptions with respect to using an average growth rate or a 95-percent confidence interval.

So, to ensure structural leakage integrity, the Generic Letter requires two calculations: The first calculation is a conditional probability of burst under steam line break conditions.

And that calculation is necessary to ensure that the repairs are adequate from a structural integrity standpoint. And as I've just mentioned, it's because the values of the growth in NDE uncertainty may exceed those in the deterministic determination of the repair limits, it's because we used the lower 95-percent prediction interval for the burst pressure correlation.

It's also because we only used lower 95-percent material properties. You can have values less than those.

It's also because you're only looking at a single indication and not the entire steam generator. There is a cumulative effect.

The other calculation that we do -- and I'll be talking more about how we do that, in a minute -- is, we determine what leakage will exist under postulated accident conditions.

And that's necessary because with this voltage-based approach, there is no correlation between voltage and depth. As a result there are through-wall cracks or near-through-wall cracks, can either remain in service after the inspection, or they may develop --

service.

And as a result, we have to do a calculation to determine the leakage under those conditions.

Both of those calculations will require some knowledge of what's going to be present at the time of the next inspection, because from a conservative standpoint, you want to know, if I had that steam leak break at the end of the next operating interval, what would my probability of burst be and what would be my leakage?

That's going to be the most limiting, because the tubes will have the chance to progress the most.

Looking at this big picture, basically what you do is, you do your eddy current inspection, you find all the defects at the tube support plates, because under a 100-percent inspection, you do your RPC examinations.

With all the indications you detect, you then make a POD adjustment. We use the constant value of .6 as the POD.

And I'll discuss the basis for the .6 later today in then presentation on NDE capabilities.

DR. CATTON: What is POD?

MR. KARWOSKI: Probability of detection.

DR. BALLINGER: Is that the 40-percent level, 40-percent through-wall?

MR. KARWOSKI: We applied that .6 value to everything that you find. Remember, what you're going to have is at the beginning of the cycle -- this graph doesn't really show it.

What you're going to have, after you do your inspection, you're going to have a distribution of indications. Some of those will be RPC-confirmed; some of them won't.

They will be a function of voltage, okay? So it has nothing -- any given voltage can have a range of depths associated with it; there's no correlation.

We say that our probability of detecting a large voltage indication is the same as our probability of detecting a low voltage indication.

And we assume a constant value of .6 throughout that range. So, if you were to say the worst degradation is the largest voltage, and that the lower voltage indications are minor, we're saying that you have an equal probability of detecting both of those.

It's a very conservative assumption, which we'll get into this afternoon when I talk about the POD.

In general, you would expect to find some of the more significant flaws easier than some of the more minor flaws.

So we applied this .6 value. So everything that you find during an inspection, you divide it by .6, which is roughly equivalent to multiplying it by 1.6-something.

And then you subtract out the indications that you repaired. That will be your beginning-of-cycle distribution.

It's very conservative in the sense that if you found a ten-volt indication, you would assume that roughly -- that you have roughly .67 of those indications left in service.

DR. BONACA: Could you had a defect for which you have no indication whatsoever?

MR. KARWOSKI: Yes, where you missed it during the inspection.

DR. BONACA: So for this, really, the conservatism doesn't apply, the statement of conservatisms doesn't apply, because simply you don't detect it.

MR. KARWOSKI: But that's one of the purposes of the .6 adjustment, is to account for that. It's to account for the fact that you can miss flaws during your inspection; that's one of the purposes. There is also another purpose, and I'll get into that.

So you divide what you find during your inspection by .6, and then you subtract off the indications that you repaired.

DR. KRESS: Now, suppose you found no indications?

MR. KARWOSKI: If you found no indications, you won't be implementing the repair criteria, you would just --

DR. KRESS: You don't assume that you might not have detected --

MR. KARWOSKI: Utilities would normally not apply for this repair criterion, unless they had a high likelihood of finding it.

So, then you take this beginning-of-cycle distribution, and you need to add in growth and NDE uncertainty to get to the end-of-cycle distribution.

Then with that end-of-cycle distribution, that's what you use to calculate your probability of burst.

DR. KRESS: Voltage growth is based on extrapolating fast growth rate?

MR. KARWOSKI: The way voltage growth is determined as part of this generic letter, is, when you go in and inspect -- if today was your inspection, you would inspect you'd find a distribution of indications. You would have the voltages associated with those indications.

You then go back at that location, at your prior inspection, and see what it was then, and you take the difference, okay?

DR. KRESS: Fine.

MR. KARWOSKI: And the time, and you make the appropriate adjustments for whatever your operating cycle is, but you take the difference between those two, you adjust it for the appropriate time, and that will be your growth rate.

There are provisions in the Generic Letter that address how many datapoints you need in order to use the growth rate, the concern being is that if you didn't have enough data, how could you project?

If this is one of your first cycles, you only have 40 datapoints, how well did you know that the growth rate of the tubes?

And in those cases where there is limited data, it requires the utility to use a bounding growth rate based on other steam generators that are operated under a similar condition.

DR. KRESS: And the assumption is that if the growth rate is changing, that that change may not be enough to worry about over one cycle?

MR. KARWOSKI: Yes, the assumption is that the growth rates -- you're looking at this from a population standpoint. The assumption is that the growth rates that you observe during the course of that cycle will be -- when you operate it the next cycle, adjusted for the appropriate operating length, the growth rates will be comparable for the entire -- for the population. That is an assumption.

The other thing that we do with respect to the growth rate distribution, when you go in and do these inspections and you find, say, a 1.5 volt indication, or -- that's probably not a good example.

Say you'd found a half a volt indication; you could go back to the prior outage and notice that the voltage was actually .6 volts, and you could have a negative growth rate.

How we address that in the methodology, to be conservative, is, we make the utilities assume that the growth rate was zero. So all negative growth rates in the probabilistic analysis for determining the end-of-cycle distribution are assumed to be zero.

DR. CATTON: Because it's physically impossible?

MR. KARWOSKI: It's physically impossible.

DR. CATTON: But you don't do that with the iodine?

[Laughter.]

DR. CATTON: Just thought I'd mention that.

Between two cycles, I can see how you're going to predict the third. Do you ever go back and make a comparison of what was anticipated and what's measured?

MR. KARWOSKI: You're getting to the punch line. I will present that. That's a very important aspect of this methodology, very important.

So, with the beginning of cycle voltage distribution, and the growth rate distribution, I also need

NDE uncertainty. The NDE uncertainty stems primarily from two distributions.

I only have a picture here, but there are two distributions that get sampled. The first is analyst variability, and that's a result of two different analysts using the same procedures, could look at the indication and get different voltages.

There was a study performed by the industry when this methodology was being developed, which indicated that basically the mean of the distribution was zero, it was the normal distribution. I believe the standard deviation was about ten percent.

The important point is that they recognized that different analysts can call an indication different. There is an analyst-variability portion to the NDE uncertainty.

The other portion of the uncertainty is uncertainty associated with the wearing of a probe. If a probe wears, it will be a different distance away from the tube, and it could result in a different voltage.

As a result, the industry did some tests to assess the effect of probe wear, and there's another distribution which takes into account, the wearing of a probe.

So those are the two components of the uncertainty distribution.

To arrive at the end-of-cycle --

DR. KRESS: Are those distributions clad randomly to the original distribution?

MR. KARWOSKI: Yes. You basically --

DR. KRESS: Sort of like a Monte Carlo.

MR. KARWOSKI: It's a Monte Carlo simulation. You sample the beginning-of-cycle voltage distribution, you sample growth, you sample the two NDE uncertainty distributions.

DR. KRESS: Do that over and over till you get a new distribution?

MR. KARWOSKI: Right. And the number of indications that you leave in service is a result of whatever the -- you take your detected, divided it by .6, you will grow everything that exists, and you will also have the indications that you missed.

The one other factor, the .6 does not only account for indications that were missed during inspection, it also accounts for indications that could develop over the cycle.

Because we do not explicitly say there could be 50 more indications in the steam generator, the .6 POD accounts for both those factors, the missed indications and indications that can initiate during the course of the cycle.

DR. KRESS: What do you do with this bottom distribution, once you get it?

MR. KARWOSKI: With the end-of-cycle, that's my next slide.

DR. KRESS: Okay.

DR. BONACA: I would like to know, how does it account for those that are not detected? I mean, the .6, if I understand it, you take the reading, which is the voltage, and you multiply it by 1.6 or whatever, or you divide it by .6 to get the new voltage value.

MR. KARWOSKI: To get the number of indications. If you found one indication, and it was below the repair limit, and you could leave it in service, you would take the one indication, divide it by .6, and you'd end up with roughly 1.7 indications.

You would take that 1.7 indications that would be at that specific voltage, because you're not repairing anything. You'd sample 1.7 indications and propagate it through.

DR. BONACA: So the number of indications that you are separating then?

MR. KARWOSKI: Right. It's the number of indications at that specific voltage. So, recognize that one of the industry complaints of our model is they say that as the voltage gets -- as the voltage rises, they believe their probability of detection increases, because it's a much bigger flaw, the noise or the signal will come much clearer out of the noise and the analyst isn't going to miss it.

So, one of their criticisms is this constant POD model. If I had a 13.7 volt indication, and I detected it and I'm going to repair it, this model will require for them to leave in at the beginning of cycle, 7/10ths of an indication that is at 13.7 volts.

Their criticism is, if we had a 13.7 volt indication, we would find it. Their position is 100 percent of the time. We say you need to leave .7 of an indication there.

DR. KRESS: When you divide by the .6 and get a number, the number of indications is an integer, a whole number. Do you round it up to the next whole number, of just leave it as a fraction?

MR. KARWOSKI: The fractions are propagated.

DR. KRESS: The fractions are propagated, okay, which is all right when you're doing a Monte Carlo.

MR. KARWOSKI: But, yes, you're right. But if you're counting multiple bursts or something, it poses some challenges, but we leave all the fractional indications in service.

So what do we do with that end-of-cycle distribution? I said there's two things we do:

We do the probability of rupture calculation, and we do the conditional leak rate calculation.

With respect to the probability of rupture, we start with this end-of-cycle distribution. We have our burst pressure correlation that we discussed this morning.

It has scatter around it. We sample around it. If we picked ten volts, we'd come over and say that for a ten-volt indication, what is the range of burst pressures that we can have?

We take that sample. Then we say this has been normalized to a specific material property. We then come in here and take a sample of our material properties distribution, scale the burst pressure, determine what that burst pressure for that one indication is, and determine whether or not it's going to rupture under steam line break.

We repeat the process for all of the indications in the steam generator and determine if there was a rupture in that steam generator during that one Monte Carlo cycle.

Then we repeat it, tens, hundreds, thousands of times to determine the probability of rupture under steam line break conditions.

Leak rate calculation --

DR. CATTON: And this is done with 100-percent evaluation of the steam generator, all the tubes are checked?

MR. KARWOSKI: All the tubes are checked, 100 percent at each intersection.

Another conservatism in the model is there can be multiple indications in a tube. The model treats them all as if they were independent tubes, so if you had two indications in the same tube, theoretically you could get two bursts from that and it's just counted -- they're counted as two multiples.

DR. CATTON: The multiples can be greater than the number of tubes?

MR. KARWOSKI: Yes, yes. That's usually not the case, but in the extreme, you're correct.

DR. SIEBER: Just so I understand, it seems to me that if you repaired everything that you postulated would leak, the fact that you end up with -- break, excuse me -- the fact that you end up with a probability of burst, really comes from all these uncertainties that you have factored into this.

Otherwise, you would know it perfectly that everything would be accurate, and you could fix everything.

MR. KARWOSKI: Right, and that's one of the industry's criticisms of our model, because even if they repair everything, because of that probability of detection adjustment and the uncertainties, they can predict extreme --

DR. SIEBER: There's going to be something in there?

MR. KARWOSKI: Right.

DR. SIEBER: Okay, thank you.

MR. HIGGINS: So you could you explain the probability -- after you do the probability of rupture calculation, is there an acceptance criteria there that would then cause them to go back and make additional repairs beyond what the voltage requires them to make?

MR. KARWOSKI: Yes. The acceptance criteria -- there's a reporting requirement. If the conditional probability of burst exceeds one times ten to the minus second, then they are required to notify us.

One of the corrective actions would be to plug more tubes. Recognize, though, the other problem with -- not problem -- one of the issues with the methodology is that you could start off with a very high probability of rupture because of that POD adjustment.

MR. HIGGINS: And has that happened? What are the typical results?

MR. KARWOSKI: Typically, usually, the few high voltage indications dominate the burst probability, and so once you start leaving 7/10ths of an indication in service, that drives the probability.

MR. HIGGINS: I mean, have you had cases where you had to repair additional -- plug additional tubes beyond those that would have been called for by the one volt or two volt criteria?

MR. KARWOSKI: I don't believe that has happened, although I think that in one instance -- there is one instance that I'm definitely aware of where the probability exceeded one times ten to the minus two.

I believe it was 1.2 times ten to the minus second, so I know there was one instance where it went over.

DR. SIEBER: The limit that you're looking for is one times ten to the minus two?

MR. KARWOSKI: Yes.

DR. SIEBER: Okay, thank you.

DR. KRESS: Where did that number come from?

MR. KARWOSKI: That is on a slide towards the back, but it's 1/5th the value that was assumed in the staff's assessment of risk assessment of steam generator tubes in NUREG 0844.

The value was just 1/5th because this is only one degradation mechanism. We assumed a probability of rupture in that report of five times ten to the minus second and we said we didn't want one mechanism controlling, you know, right up to the limit. There are other mechanisms going on.

The other reason is that it gives us some insights on whether or not any tubes may not meet the Regulatory

Guide 1.121 structural criteria. Even though we're calculating the probability of burst, we wanted some insights on whether or not there are any tubes that are starting encroaching on the deterministic structural margins, the worst case tube.

DR. KRESS: Where did the number -- what's the technical basis for the number as multiplied by five?

You say you took one-fifth of the number. What's the technical basis for the --

MR. KARWOSKI: For the five times ten to the minus second?

DR. KRESS: Yes.

MR. KARWOSKI: Somebody else -- basically I think they did a risk assessment that assumed a frequency of rupture of five times ten to the minus second, propagated that through a risk assessment, and determined that that was acceptable.

That's my understanding.

MR. STROSNIDER: That's correct. The assumption in NUREG 0884 --

MR. KARWOSKI: 0844.

MR. STROSNIDER: 0844, I always get that mixed up. But the assumption was made of a conditional failure probability, given a main steam line break, of five times ten to the minus second.

And when that was worked through the whole risk assessment then, it was found that it gave an acceptable level of risk.

All right, and then we reduced it to account for the potential for other modes of degradation. I think Steve --

DR. KRESS: It gave an --

MR. STROSNIDER: -- might be able to explain that in more depth.

DR. KRESS: Okay.

MR. LONG: I wasn't involved in 0844, but Emmett can correct me if I get this wrong. Initially, they were trying to determine what they thought the conditional probability of burst would be for a main steam line break, based on experience, and largely that was experience of ruptures that had occurred and some estimate of the period of time that it would take to grow from where they could be susceptible to rupture when the depressurization occurred, to the period when they just ruptured during normal operations.

So there was sort of an exposure estimate in there. And I think it was essentially that exposure estimate, with some adjustments for things they didn't think they really observed, it was put into the risk assessment in 0844 to see if there was an acceptable or an unacceptable situation.

It wasn't intended to be a limit when they did that calculation. And it wasn't a risk assessment that recognized severe accident sensitivities or that sort of thing.

So it was done about 1985, I guess.

MR. MURPHY: The report was issued in 1988.

MR. LONG: So that's sort of the first of three risk assessments that you will hear about, the one for 1477 being the next, and 1570 being the most recent one.

DR. KRESS: We'll hear about this later, will we?

MR. LONG: To some degree. We didn't intend to go into it that way, so that's why I'm explaining it now.

The point I was trying to make, though, is, we didn't try to use the .05 as some sort of an acceptance criteria. It came from experience, and it was essentially evaluated to see if it was something that we should try to backfit.

MR. STROSNIDER: But I might add that when we were developing Generic Letter 95-05, that's the risk assessment that we had to look at.

And when we looked at it, we said, well, five times ten to the minus second, when that was propagated through the risk assessment for those sequences associated with main steam line break, it resulted in an acceptable level of risk.

So, we said, okay, we'll use that as an acceptance criteria.

DR. KRESS: Yes. What was your criteria for an acceptable level of risk?

MR. STROSNIDER: I don't know what it would have been in '88 in terms of -- we'll have to get back to you, okay?

MR. KARWOSKI: The one other item that I should point out with respect to the probability of burst calculation is that it's evaluated at a 95-percent confidence value.

It's not just whatever the probability is; it's at a 95-percent confidence value.

The other portion, the leakage integrity portion, is depicted on this viewgraph. It's a similar methodology; you do a Monte Carlo analysis. You sample the end-of-cycle distribution for a specific voltage.

You come to the probability of leakage correlation, and determine whether or not the tube will either leak or it won't leak. If it leaks, you come into the leak rate correlation and determine --

DR. KRESS: If you have a probability of leakage? That's like Schrettinger's cat; it's both dead and alive. What do you mean, it won't leak or it will leak? If it has a probability of leakage, it has a probability.

I didn't understand your statement; that's what I'm saying.

MR. LONG: Oh, for a given voltage, there is a probability that a tube will either leak or it won't leak.

DR. KRESS: It's still Schrettinger's cat. I don't quite understand. If it's a probability of a leak, then one minus that is probability that it won't leak. I'm still having trouble figuring out what you're saying.

MR. STROSNIDER: The point is that the probability depends on voltage.

DR. KRESS: Of course it does. But it's a probability --

MR. STROSNIDER: But I think the point is that it either will leak or won't leak. If it leaks, the probability is one, and those are the datapoints that are on the top.

If it doesn't leak, the probability is zero. Those are the datapoints that are on the bottom.

DR. KRESS: Yes, but that's a delta function.

MR. STROSNIDER: I'm sorry, I didn't hear you.

DR. KRESS: That's a delta function; that's not a distribution.

MR. STROSNIDER: Well, and this came up yesterday. Art Buslick raised a question about how you fit the distribution to those data.

DR. KRESS: Once you have a distribution, though, it's just a probability.

MR. KARWOSKI: Let me try it this way: You pulled these tubes, okay, and you test to determine whether or not they're going to leak under steam line break conditions.

You've got various voltages, okay?

DR. KRESS: So you plot that.

MR. KARWOSKI: When you test this tube, it either leaks or it doesn't leak; there is no -- it either leaks or it doesn't leak.

So you have the voltage associated with that indication, and you either know if it doesn't leak, or if it leaks.

DR. KRESS: Sure.

MR. KARWOSKI: Okay? There may be five tubes with the same voltage. Three of them may leak; two of them don't leak, okay, for a given voltage, okay?

So when you come in there, say, you just had indications of that voltage, roughly 60 percent of the time, 3/5ths, 60 percent of the time you will assume that that indication leaks; 40 percent of the time, you will assume that it doesn't leak. Does that --

DR. KRESS: Yes, that answers my question. It's just a probability.

MR. HIGGINS: Are you talking now about applying the methodology or the development of these curves?

MR. KARWOSKI: Also applying it. It's the development of the curves and applying it, because once you have this function -- and I'll talk about this in a minute -- once you have this function, you have a relationship and you can say that there is a certain probability that an indication with that voltage will leak, and one minus that is the probability that that indication will not leak.

So you go through this method; you sample your voltage, you sample your probability of leakage to determine whether or not the tube either leaks or it does not leak, you determine the associated leak rates with that voltage. You add it up for the entire steam generator, and you get a value for leakage.

You repeat this for all the indications in the steam generator, and you've got the one value of the leakage for that steam generator, you repeat it, tens, hundreds of thousands of times, and you will have a distribution of leak rates.

You order those, you take the 95th percentile, at the 95-percent confidence, and that's what you say your leakage is under steam line break conditions. So it's a 95/95 leak rate, okay?

There was some discussion yesterday on the probability of leakage correlation, and this one is not in your handout. What I have plotted here -- and it's probably not worth your effort to figure out which curve is which -- but it's basically looking at six different functions of the probability of leakage.

Okay, there is no theoretical basis for the log logistic curve that we are using. We've documented that in NUREG 1477. There's no basis, theoretical basis.

DR. KRESS: And there's not enough data to best-fit any of it.

MR. KARWOSKI: They all have equal -- so, why did we choose the log logistic over any of the others? Well, before I get into that, I mean, if you look at this some of these functions -- and, truthfully, I don't even know which ones the log logistic predicts a probability of leakage for very small indications, most of them don't.

These curves criss-cross. To determine which one is conservative, you're not going to be able to do it, because it's going to depend on the distribution of indications you leave in service.

You can do it only if you analyze that steam generator, because these curves are criss-crossing, and so it depends if you have a lot of low voltages, medium voltages, or high voltages.

In NUREG 1477, we did some analysis where we shifted the distribution and determined the leak rates. And in some cases, I believe it was the log co, she was conservative, and in other cases, the log logistic was conservative.

So, the staff did look at, you know, should we be using a different function or how should we implement it?

What the staff subsequently decided was that the log logistic was acceptable for several reasons, one being the POD adjustment was conservative; we're evaluating leak rate at the 95th percentile and a 95-percent confidence.

Those are two primary reasons, so the staff did look into which curve to use, and we chose the log logistic. The next few slides just basically describe what I said, conditional probability of burst. It discusses the process and the acceptance criteria. And this slide discusses the leakage distribution.

I wanted to spend a little more time on here because I think, as Dr. Powers pointed out, you know, it had very low correlation coefficients for some of those leak rate correlations, and there has been considerable study of this data.

The way the methodology works for the leak rate correlation -- and I don't believe this one is in your package. This is something that I prepared subsequently.

DR. KRESS: No wonder I couldn't find that.

MR. KARWOSKI: If the linear correlation can be developed between the leak rate and the voltage -- this isn't the actual data, so you have the data in those proprietary summaries that I passed out -- but -- well, let me start over.

When you look at this data, the original data, you could look at it and say, well, should the curve be like this? Should it be like this? Should it be like that?

The industry did a linear regression and came up with a curve. The staff was concerned, is there really a correlation there?

We had a statistician look at it. And basically, what he concluded, at least for the 7/8ths inch diameter database -- and it may have been the same with the 3/4 inch at the time, but for the 7/8 inch database, he concluded that there wasn't sufficient confidence that the slope was not zero.

So, basically he said there is no correlation. The implications of that to the utilities is that a tenth of a volt indication, the way we interpreted that is, regardless of the voltage, the indication will leak by the same amount.

And that's what the Generic Letter describes, an acceptance criteria for showing whether or not you have a correlation or you don't.

And it's a standard statistical test, a P-value test, and you have to have a 95-percent confidence that the slope of the line is not zero.

DR. KRESS: This is sort of a generic correlation that you build up a database for.

MR. KARWOSKI: Right.

DR. KRESS: And you're saying that before -- I'm not quite I understood. You're saying that before you can use this in your alternative criteria, you have to have enough data to have a correlation?

MR. KARWOSKI: No.

DR. KRESS: I'm not quite sure what you're saying.

MR. KARWOSKI: What I'm saying is, if you can just demonstrate by statistical tests that the slope of this line is non-zero --

DR. KRESS: I thought you already said that you tried that, and it wasn't.

MR. KARWOSKI: The Generic Letter is more performance-based. Instead of saying here is the correlation at this time, and there is the slope, we recognize that they're going to pull additional data. They're going to get more data which --

DR. KRESS: At some point in time, you may have a correlation?

MR. KARWOSKI: That's correct, or you may go from correlation to not. The bottom line is, when you get more data, you have to put it in these correlations and you have to use the most recent database.

We didn't want to lock in a certain database, knowing that utilities will gain more experience.

DR. KRESS: Up to the point where they statistically can't say they have a correlation, what do they do up to that point?

MR. KARWOSKI: When they don't have a correlation, they have to assume that essentially the tube will leak at a specified value.

DR. KRESS: And that value is what?

MR. KARWOSKI: They basically do a Monte Carlo analysis and model the error and say -- around the regression line, what the slope at zero. So basically they average the log of the leak rate, obtain a value, and then sample the uncertainty around that line, and basically assume that the leak rate is independent of voltage.

A tenth of a volt indication will leak the same as a 25 volt indication.

DR. KRESS: Put that line through the average of the data?

DR. CATTON: That's what it amounts to.

MR. KARWOSKI: Through the average of the log, it will leak -- they basically do a -- right.

DR. KRESS: That doesn't sound like a very interesting thing to do.

MR. KARWOSKI: Well --

DR. CATTON: It's zero physics. It's simple, too.

DR. BALLINGER: But what is the criteria, exactly, by -- what is the fence that they get over before they say it's a correlation? What is the statistical test that has to be passed?

MR. KARWOSKI: The P-value has to be less than .05. There has to be a 95 -- I hope I get this right; I'm not a statistician.

It has to be a 95-percent confidence that the slope of that regression line is not zero.

DR. KRESS: That's the sort of standard rule of thumb for statisticians, I guess.

DR. CATTON: If you don't know a damn thing, you just average everything.

MR. KARWOSKI: Well, not only do you average it, but you also model the uncertainties.

DR. CATTON: The only thing you're bringing in over here is the distribution of voltages. A tenth of a volt is probably not going to show -- you just toss out all the physics when you do that.

MR. KARWOSKI: This is an empirical --

DR. CATTON: It's not even empirical. I mean, you've just thrown everything away.

DR. KRESS: There's no physics in there anyway.

DR. BALLINGER: But it just says there's no information in the data.

DR. CATTON: That's right. So how do you pick the number that you use that's meaningful?

DR. KRESS: I think that's your point, right?

DR. CATTON: That's right, how do you know?

MR. STROSNIDER: This is Jack Strosnider. I'd suggest that one of the things that might need to be pointed out is that there is a very large uncertainty -- not uncertainty but actually variability in scatter and leakage rates through cracks.

And we're talking orders of magnitude, as I recall, three leakage values. And that's part of what drives this, is there is a large variability.

And there was discussion yesterday about what sort of particulate might be in the cracks, and what the morphology is and that sort of thing.

DR. CATTON: That's right.

MR. STROSNIDER: And even when you grow single stress corrosion cracks in a tube, you get very large variability. So trying to get a lot of information out when you've got that much scatter, that may contribute to some of the problem here.

But anyway, that's -- the industry was not happy with what was sometimes referred to as the flat earth model, where we said, okay, just take this horizontal line.

But as Ken indicated, we tried to build into it, as the database grew, if the correlations became more obvious, then it could be used.

DR. KRESS: We have an imminent --

MR. SHACK: This is Bill Shack. Let me just take a crack at it. This is a bad joke here.

DR. KRESS: Fine.

MR. SHACK: One of the reasons that -- you know, physically, I think this thing sort of works out the way you expect it to work out.

There is a rough correlation for cracks up to a half an inch between the crack length and the voltage, that is, the voltage increases rather rapidly with crack length up to about half an inch, and then it kind of flattens off.

Well, most of the cracks of interest here are on the order of something less than a half an inch, so that an increase in voltage is a measure of crack length.

Burst pressure depends only on crack length. So, I would expect to get a reasonable correlation between burst pressure and voltage.

Leak rate doesn't depend just on crack length; it depends on crack depth. And so I expect to get a hell of a lot more scatter in my voltage versus leak rate correlation, than I do in my voltage versus burst correlation, and so I get a better statistical fit for the pressure, burst pressures, than I do for the leak rate, because I'm trying to look at both.

And as Jack said, even if I knew the depth of the crack and the length of the crack, I still have more uncertainties in the leak rate, because it depends on lots of things.

DR. KRESS: Don't you have to have a through-wall crack to get a leak rate?

MR. STROSNIDER: Right, and that's probably the biggest thing here, but that's sort of accounted for in the zero to one.

Either I've got a through-wall crack or I don't.

DR. KRESS: That's sort of wrapped up in that distribution?

MR. STROSNIDER: That's wrapped up in that distribution. But then once I do that, even though I have decided I've got a through-wall crack, I really don't know how much the through-wall length is, where the burst pressure is.

I just keep upping the pressure until I rip the thing open, and I know really how long it was.

All I'm arguing is that the fact that you have lots of scatter here and you get a better correlation, the burst pressure, isn't a surprise. But the answer is, you shouldn't expect a terribly good correlation here, so what are you supposed to do?

DR. CATTON: And it's a nice physical explanation of the observations. But the question still remains, what are you supposed to do?

I guess the only thing is that you have a low-end cutoff on the voltage; don't you, with that probability of a leakage. If the voltage is less than some number, it doesn't leak.

So when you say even a tenth of a volt has a leak, it doesn't, because there is a cutoff.

MR. KARWOSKI: Right, wherever there is some --

DR. CATTON: There's a cutoff. And I bet that you're at zero at a tenth of a volt.

MR. KARWOSKI: You could be. I don't recall.

DR. CATTON: That's what that is, it's a gate that sits right in the middle.

MR. STROSNIDER: This is Jack Strosnider. Art Buslick could address this better than me, but I think all those fits go through zero. But the probabilities do become very, very small.

DR. KRESS: For the log, the logistic, you don't have a cutoff; it goes to zero asyntotically. There is still a small probability.

MR. STROSNIDER: It could be a small number, but the utility, the thing they didn't like about it is that if they had a large number of small voltages, that they could still generate some sort of leakage.

DR. CATTON: I would be concerned if I were them, too.

But from the other side, you'd probably get a too-low rate at the high voltage.

DR. KRESS: Basically what they're saying is that those two sets of data on the top and bottom, and when you get down that low, there are no overlapping, although this curve would have said there was some overlapping.

MR. STROSNIDER: I think the answer to your question with what you do with it -- and this may just spur more discussion -- but my response is that you take a conservative approach, which is what we think we did, in terms taking the constant leakage --

DR. CATTON: Where you put the line is on the high side, rather than the low side?

MR. STROSNIDER: Well, Ken can explain it.

DR. KRESS: I think we may have to.

MR. STROSNIDER: When you go through this evaluation, then you look at some 95th percentile.

MR. KARWOSKI: If you look at the type of voltages that the plants are running at, giving it the zero slope or the flat earth, however you want to look at it, that's going to be more conservative, because usually there aren't a lot of large voltage indications.

What will drive the probability is the leakage from those low voltages. You still have a finite probability that those will leak.

MR. MUSCARO: Joe Muscaro with the NRC staff, I just have a point of clarification. I think Bill mentioned the phenomenon in reverse. I think he said that the leakage depends on the length and depth and burst on length.

Well, it's the reverse, of course. Leak rate depends on the length of the flaw, but burst depends both on the length and depth.

The other point was the physical basis for whether there should be a correlation or not with respect to voltage versus crack length, which is important for leakage.

And there some studies we have done in the past and have show that the voltage starts at a crack length of about 3/10 of a inch, which makes this fairly important for the application when we're talking about cracks that are less than 3/4 of an inch long.

So there is a saturation effect on the voltage. The physics limits it. I mean, it only sees a certain portion of the crack, so there is no additional voltage beyond about a 3/10 of a inch crack with the coil that we use today. Of course, that's a function of the design of the coil.

MR. HIGGINS: Ken, you said that this is being updated, and how is that done? Is somebody the keeper of the curve for all of industry, and then people use that?

MR. KARWOSKI: Essentially, that's correct, either EPRI or NEI -- and I don't recall which one. I believe that NEI sends in the formal report, but it's an EPRI report.

But basically, annually they update. There is a specific protocol, and I believe it's annually. They incorporate all the pulled tube data from that year, they put it in the correlations, they develop revised correlations, and they give those to the industry.

The case where I said that one plant exceeded the one times ten to the minus two burst probability, what happened there is, they did their inspection, they calculated the probability of burst to be less than ten to the minus two.

They incorporated more data into the burst pressure correlation, and when they did that and reran their burst results, they ended up slightly greater than one times ten to the minus second. That's what happened in that case.

So, utilities, when they do these calculations are supposed to use the most recent database.

DR. CATTON: Is this number increasing or decreasing with time?

MR. KARWOSKI: The?

DR. CATTON: The leak rate, the uncorrelated leak rate number?

MR. KARWOSKI: I believe there is a correlation now for both the 3/4 inch and 7/8 inch database.

DR. CATTON: So it's beginning to get a little bit of a --

MR. KARWOSKI: A slope, right.

DR. KRESS: So at some magic point in time, people will quit using this flat earth average with the distribution around it, and go to this actual curve?

MR. KARWOSKI: With also distribution around it, because as was pointed out, there is a lot of scatter.

DR. KRESS: And you won't be penalizing the small cracks as bad?

MR. KARWOSKI: That's right, and it makes a -- once again, it's going to depend on your distribution of indications, but for the one plant that I'm aware of, it reduced their leakage by a factor of three.

They were predicting something on the order of 13 gpm, and when they went to this it was more like four or five. So, it reduces the leakage considerably.

Okay, one of the important aspects of this Generic

Letter is that it requires the licensees to submit certain data.

With that data, you can go back and compare the projected and actual end-of-cycle distributions. That allows you to check various things. Is the .6 POD, is that accounting for new indications and the probability of detection accordingly? Was your growth rate assumption realistic?

Was my NDE uncertainty realistic? Because you can compare what you actually found to what you projected. That's a very important aspect of this.

These comparisons have generally shown the methodology to be conservative. That's not to say that they're perfect; that's not to say that they're haven't been indications with larger voltages than we predicted, as was pointed out yesterday. I will discuss that specific example.

But the staff did do a comprehensive review of what I have termed the 90-day reports, where the utilities supply their inspection results, both their projections and their actual results.

DR. KRESS: This is one of those cases where you hope you're not gently flowing down the stream and suddenly about to go over a waterfall, a cliff-edge type thing? I don't know how you deal with that in terms of projecting forward in time, based on past history.

MR. STROSNIDER: This is Jack Strosnider. I'd just make a short comment on that, which is that I think what you're talking about is how can I foresee what's going to happen in the next cycle of operations?

DR. KRESS: Yes.

MR. STROSNIDER: That's an issue that exists, regardless of what --

DR. KRESS: No matter what.

MR. STROSNIDER: -- what repair criteria or inspection method you use. And you can go to laboratory data to get some insights, all right, but we have found, historically, we think, that the best predictor is what happened in the last cycle.

That doesn't mean that you won't some day be surprised with a new form of degradation or some higher growth rate, and you have to deal with that when it occurs.

But nobody has a crystal ball to say exactly what's going to happen in the next cycle.

CHAIRMAN POWERS: You're going to show us some examples of where the methodology has been compared?

MR. KARWOSKI: Yes, I will show you -- I have two examples. I just pulled out two.

But back in 1997, we did do a comprehensive review of, I believe it was eight 90-day reports, and there were some issues that were identified as a result of that.

One of the issues was, when you have a low number of indications, your projections typically are off, but with respect to the distribution that you find in the next cycle -- but that necessarily is not a bad thing.

When you look it from the probability of rupture or leakage, what you're finding out is, you know, you predicted a ten to minus four probability of rupture, and instead, it's five times to minus four.

In general, in all of these assessments, in not one instance did we exceed the acceptance criteria of one times ten to minus second for burst, or the applicable leakage limit at that plant.

The other thing I'd point out is that as a result of implementing this repair criteria, there has been no significant operational leakage.

DR. CATTON: Your second paragraph says comparisons have generally shown methodology to be conservative.

MR. KARWOSKI: Yes.

DR. CATTON: What's your measure?

MR. KARWOSKI: The measure is several things: We look at what the distribution of voltage is. Did we predict the peak voltage?

Did we -- how did we do with respect to burst and leakage? What was our predicted probability of burst and what was our actual probability of burst, based on the inspection findings.

DR. CATTON: I can buy into that, but now what about leakage?

MR. KARWOSKI: The same thing for leakage: What did we predict for leakage, and what will we predict, based on our actual end-of-cycle findings?

DR. CATTON: End-of-cycle findings is a measured leak rate for the steam generator? What is the end-of-cycle finding that you compare with leakage?

MR. KARWOSKI: Okay, when you go in and do an inspection today, okay, you're going to have a distribution of indications. Before I do any repairs, I say, how much leakage would I have gotten from that distribution of indications?

DR. CATTON: Okay.

MR. KARWOSKI: Okay? Now, I go back to the prior cycle. What did I predict I was going to get?

If I predicted --

DR. CATTON: But you haven't measured leakage and compared it with prediction?

MR. KARWOSKI: No.

DR. CATTON: So this is all just paper? It's an earlier estimate with the present estimate, but there is no actual comparison.

So you don't know whether you're conservative or not. You may have been non-conservative in both cases, one must more than the other or less than the other.

MR. KARWOSKI: The burst and leakage are a comparison based on the methodology. The actual findings is a comparison of what you have.

MR. STROSNIDER: This is Jack Strosnider. What you have --

MR. CATTON: I thought I asked and you said no.

MR. STROSNIDER: What you have compared is you have compared the predicted versus the measured end of cycle voltage distribution.

MR. CATTON: Yes.

MR. STROSNIDER: That is what you have compared.

In terms of what leakage would be associated with those two distributions, it uses the same methodology. As you say, if there is some bias in that methodology it would be there in both --

MR. CATTON: So you really don't know where you're at.

DR. KRESS: There's no additional information --

MR. CATTON: That's right.

DR. KRESS: -- in going to that second comparison, yes.

MR. CATTON: So this second statement, comparisons have generally shown methodology to be conservative we don't know.

MR. KARWOSKI: In predicting the end of cycle voltage.

DR. KRESS: With respect to that you can say that.

MR. CATTON: It only has to do with voltage. It has nothing to do with the leakage.

MR. KARWOSKI: Yes.

MR. CATTON: We have a model that we might believe --

DR. KRESS: You're comparing the bursts -- comparing the voltage and then comparing the leakage tells you nothing, no additional information.

MR. CATTON: Bursts I buy because they actually have correlations and so forth but the leakage is so all over the map I don't think you can come to a conclusion.

DR. KRESS: But even comparing -- you know, it makes no sense to compare predicted bursts to actual bursts or predicted leakage to actual leakage. The comparison you are getting is voltage to voltage, predicted versus actual, and that is the only information you have really.

MR. CATTON: That's right.

MR. HIGGINS: But the leakage database should be getting better as you go over the years because you are adding more and more pulled tubes that you are testing.

DR. KRESS: Yes. Yes, the database ought to get better. Your ability to convert the number in the leakage is getting better.

MR. STROSNIDER: I think I'd just comment I think it's kind of indicated there is a correlation in the leakage database now -- you know, because additional data have been added and it meets the criteria that were established but again I would come back and make the point again that when we start talking about leakage under main steam line break conditions that we are always relying on a model.

DR. KRESS: Oh, yes.

MR. STROSNIDER: Okay? We have a model here that is tied to voltages. If you had crack measurements that were accurate that you believed you would be going to something like CRACKFLOOR or one of the other models that was discussed yesterday, so yes, we are constrained by having a model. That is just a reality of it.

DR. KRESS: These leakage correlations were developed using the normal operating delta p, right?

MR. KARWOSKI: No, these are steam line break --

DR. KRESS: Those are steam line break leakages that used the actual steam line break delta p.

MR. KARWOSKI: Right, and as I mentioned this morning there are some adjustments to that data.

The test procedure calls for them to be run at operating temperature. There are some data -- I looked at lunch -- there are some data that are at room temperature and they are adjusted over here.

The delta p could differ. The delta p for the test --

DR. KRESS: You might not have tested at the delta p --

MR. KARWOSKI: At 2650 -- it could be 2500, 2400. I would have to look at each individual datapoint but there is a range of differential pressure.

DR. KRESS: They would have corrected for that some way?

MR. KARWOSKI: They would have corrected --

DR. KRESS: If they knew how leakage varied with delta p they would have corrected maybe.

CHAIRMAN POWERS: They would know leakage varies with delta p?

DR. KRESS: I don't, do you?

CHAIRMAN POWERS: I thought you knew everything.

DR. KRESS: If they were flowing through a pipe I'd know.

[Laughter.]

CHAIRMAN POWERS: So how can they do the correction?

MR. KARWOSKI: I am not aware of all the details with respect to that correction procedure. I know that they have corrected. I don't know the specific details of that. We would have to get back to you on that.

The procedure is documented in the EPRI test procedures.

MR. HOPENFELD: I'd bring to your attention that this was a major point that I was making yesterday and I wouldn't let you out of this room until you answered it.

MR. STROSNIDER: I think that the Staff committed to get back on that subject and we will. Thank you very much.

MR. HOPENFELD: Okay.

CHAIRMAN POWERS: I regret that I was called away. Have we had a chance to discuss the famous log logistics question.

DR. KRESS: We passed right through it just like that. You want to go back to it?

[Laughter.]

MR. BALLINGER: Not without getting a definition of what they defined as being proof.

CHAIRMAN POWERS: Okay, but sometime I would like to hear about the log logistic curve just because I happen not to know what a log logistic curve is.

Please continue.

DR. KRESS: We decided it was conservative for very small cracks because it doesn't have a bottom cutoff for it.

MR. KARWOSKI: Here is one example of a comparison of actual and predicted. I just -- I don't want to spend a lot of time on this one. I would rather go to the following one that was discussed yesterday.

Basically if you look here the actual is the open and the predicted with the POD of .6 is in black. This other one is an approach proposed by EPRI that the Staff hasn't approved, but if you look at this, here is the actual number of indications predicted in this voltage range versus the predicted and in general although you can't see out here, in general we have been conservative.

CHAIRMAN POWERS: Maybe I need a little more explanation.

You have plotted a number of indications versus bobbin voltage and the black bars are what you predict and the open bars are what was actually measured --

MR. KARWOSKI: Right.

CHAIRMAN POWERS: -- and when I look at the lower areas you get more low voltage indications than you predict but as you move up the voltage then you predict more than you actually observe. Is that correct?

MR. KARWOSKI: But there are exceptions but in general you tend to be conservative in this. This was just one example. You are right. There are exceptions and the Farley example

MR. STROSNIDER: Ken, I just might point out that I think it gets back to using the leakage model, okay?

When you ask yourself is that distribution conservative or not, what you do is use the predicted versus the actual, calculate the leakage and say does this end of cycle distribution result in more or less leakage than would have been predicted.

Again, I think Ken said earlier that when we do that in every case the leakage has been bounded, is that --

MR. KARWOSKI: What I said is that in every case when you do that the leakage is always less than the acceptance limit based on the dose equivalent of Iodine 131 in the tech specs.

In some cases it may have been higher than the predicted but it has always been less than the dose allowed, the leakage that will be permitted --

CHAIRMAN POWERS: Do you have the numerics on that?

MR. KARWOSKI: Numerics? Like the number of times it's happened or --

CHAIRMAN POWERS: Well, like a leakage predicted versus a leakage actual -- I'm sorry. A leakage predicted versus leakage predicted from the actual distribution?

MR. KARWOSKI: We have that data. I think I may have it for the Farley.

CHAIRMAN POWERS: Oh, good.

MR. KARWOSKI: But I don't know if I wrote it down.

This is the Farley example. Getting at the comparisons, the statement that the comparisons have generally been conservative, this is an excerpt from a Farley submittal. This would be their end of Cycle 14 projections, what they planned on, what they expected to find.

Here's the number of indications that they expected. Here's the maximum voltage that they projected. Here's the burst probability, single tube burst probability and here is the steam line break leak rate, okay?

If you look, let's look at the number of indications. In all cases the projections bounded the actuals. The max voltage, in this case they are equal.

The projection, in Steam Generator B the projection was greater. In Steam Generator C this is the indication we were talking about yesterday. It was 13.7 volts but this value is adjusted for NDE uncertainty and that is why it is listed as 14.9 but it is the same indication.

In this case we underpredicted the max in the cycle voltage.

With respect to burst probability, if you look at these values --

CHAIRMAN POWERS: These means or are these the 95 percentiles?

MR. KARWOSKI: These are the 95 percentiles.

Steam Generators A and B we conservatively predicted and the burst probability Steam Generator C we underpredicted.

What I was saying was this value was still less than 10 to the minus 2.

I would also point out that what is driving this burst probability is that single, is that large voltage indication.

With respect to leakage with Steam Generators A and B our projections were conservative compared to here. Likewise for Steam Generator C.

CHAIRMAN POWERS: Why was that?

MR. KARWOSKI: It was probably because they were using the leakage correlation which had a zero slope. That is probably what was causing that. I don't know that, but that would be my guess.

MR. BALLINGER: Do you have information on what the end of Cycle 13 actuals were?

MR. KARWOSKI: We would have to dig it up. I don't have it here.

DR. KRESS: Is there a consistent underprediction?

MR. KARWOSKI: I tried --

DR. KRESS: For two cycles or --

MR. KARWOSKI: During lunch I tried to get that information because the end of Cycle 15 has already occurred.

From what I was told and I have not reviewed this, from what I was told this did not occur in the next cycle. I would need to review that and provide that. I tried to do it during lunchtime. I just ran out of time.

MR. STROSNIDER: Again I think it might be worth pointing out too that in this methodology that 14, almost 15 volt indication would get folded back into the prediction of the next cycle's end of cycle distribution, all right? So the fact that that occurred does influence the assessment of the next operating cycle.

MR. KARWOSKI: Absolutely and if you look at the end of Cycle 15 projections for Steam Generator C, your burst probability of 9 times 6 times 10 to the minus 3 is dictated by that seven-tenths of an indication that you left in there at roughly 13.7 volts.

DR. KRESS: Will they repair that one or --

MR. KARWOSKI: Absolutely. That indication was actually --

DR. KRESS: So if they repair that one, how do you fold it back into your projection for the next --

MR. KARWOSKI: Oh, because that's -- you just do that on paper. You plug a lot of tubes but you still include them in your analysis -- through .6 POD so --

DR. KRESS: Okay. They can never escape that .6.

MR. STROSNIDER: You are assuming that you missed another one of those at some probability.

DR. KRESS: You never escape that -- that's right.

CHAIRMAN POWERS: You have an acceptance value for the burst probability. What about the leak rate?

MR. KARWOSKI: The leak rate is based on the dose equivalent Iodine 131 and whatever value that have in the tech specs there's associated leakage which they --

CHAIRMAN POWERS: Yes, I was asking do you know what the number is --

MR. KARWOSKI: For Farley?

CHAIRMAN POWERS: We are asking you a lot.

MR. KARWOSKI: I looked at all this and I have it written down in my office. I don't have it here but I think it was -- I think the value was on the order of 11 GPM, something like that. I would have to look it up. There's a history associated with that.

DR. KRESS: Let me ask you about this --

MR. KARWOSKI: I can tell you this value is less than whatever was in the tech spec at that time.

CHAIRMAN POWERS: That is really what I wanted to know.

DR. KRESS: Let me ask you about these phantom particles -- I mean phantom cracks -- regarding the .6.

Let's say I have this 14.9 voltage indication and I say all right, I am going to repair that one but I have got another one in there at .6 that I am going to carry to the next cycle and predict the voltage and the leakage.

If I go to the next cycle, if I go in and make a bunch of measurements, now I don't see this 14.9 again because I prepared it and I really didn't have that phantom crack that I thought I did. Do you now throw it away, the phantom crack? Do you throw that away although --

MR. KARWOSKI: The growth rate in this may include remnants because when you do the growth distributions you have to take the most conservative of the two consecutive cycles, so there may be a part of that 14 volt indication hidden in the growth, because in this case you had like a 12 volt --

DR. KRESS: So you don't carry these phantom cracks forever?

MR. KARWOSKI: No, they eventually will disappear.

MR. HOPENFELD: May I ask Dr. Powers a question?

I think in a case of Farley I think originally they didn't have iodine spike in their tech specs and they didn't take it into account, and I think it was something like 60 and then it was pointed out to them that they had to take iodine spike into account they backed up to whatever it was, something like 20 or something -- I think 20 to 30, roughly.

MR. KARWOSKI: That specific tube -- this viewgraph which isn't in your handout also basically just summarizes the Farley and the Cycle 14 results. Tube was pulled. Predictions bounded the actuals except for Steam Generator C. We discussed the two exceptions.

This basically says that they were still within their limits for burst and leakage.

The tube was pulled for destructive examination. It grew from 1.4 volts to 13.7 volts so 12.3 volts changed during the cycle. All the voltages used in the correlation are prepulled voltages. When you pull these tubes if there is any cellular component it can open it up and change the voltage. It can cause some damage to the crack.

The post pull voltage was 28 and a half volts. I just point that out just to show that there may have been some damage.

This tube burst, slightly higher than 1.4 steam line break, which I believe would be expected based on the correlations. This tube also leaked at three-quarters of a gallon per minute, and the other observation I would make -- they put an in situ pressure test and tried to leak test this while the tube was still in the steam generator so the support plate is still there. It's still packed with corrosion products.

During that test there was no leakage but it did leak in the laboratory test at three-quarters --

CHAIRMAN POWERS: If we had done that leak test with a guy whanging on the support plate with a 16 pound sledgehammer, would it have still not leaked?

MR. KARWOSKI: I can't say. All I can tell you is the way we assume it in the methodology is that it leaks at three-quarters a gallon per minute.

CHAIRMAN POWERS: But depending on crud and what-not to prevent it from leaking may not be applicable under a main steam line break.

MR. KARWOSKI: That's right, and our methodology assumes, and I might not have pointed this out, all these leak tests and burst tests take no credit for the support plate. It assumes the degradation is entirely free-span.

CHAIRMAN POWERS: Okay, so this last line is just a point of interest?

DR. KRESS: As a point of interest. It's a point of a lot of interest.

When you take those tubes out, do you do anything to them like clean them before you leak test them or just take them over and leak test them?

MR. KARWOSKI: There is no cleaning --

DR. KRESS: No acid wash or anything like that?

MR. KARWOSKI: They take them from the field. Whatever damage is done, you know, from the scraping through the other plates and through the tube sheet they take it and leak test it.

DR. SIEBER: Do they decon it at all?

DR. KRESS: That was really my question, decon.

MR. KARWOSKI: I don't know, to be honest.

DR. KRESS: Because that could be --

Do you have any idea why this thing went from 1.4 volts to 13.9 volts? That's that cliff edge I was worried about.

MR. KARWOSKI: The tube was pulled, and they did destructive examination. The tubes at the tube support plate intersections at most plants have intergranular stress corrosion cracking.

DR. KRESS: They did.

MR. KARWOSKI: When they did destructive examinations of this tube, they noticed two things: They noticed some transgranular stress corrosion cracking, and they also looked at -- what they have said is that they have noticed some fatigue type degradation at some fatigue striations.

What they postulated is two things: The transgranular stress corrosion cracking may have been contributed to by the presence of lead in the steam generator, which is a known mechanism.

Two, they think that they had done some pressure pulse cleaning during the prior steam generator inspection, and that this tube was at the location of where that nozzle was, and so they believe that they may have propagated that. That is what they have put in their reports.

DR. BALLINGER: This whole thing is predicated on the fact that you don't introduce an additional degradation mechanism into the system.

And what you're saying is that this datapoint is exactly that. It's a datapoint for which you don't have intergranular stress corrosion cracking and for which you -- that's not the mechanisms we're dealing with here.

This datapoint is basically useless.

MR. KARWOSKI: There was some transgranular components. I didn't mean to imply that it was predominantly intergranular stress corrosion cracking with some -- I believe they said minor transgranular.

I did not look at it in any more detail than that.

The Generic Letter 95-05 approach has some advantages: Basically, the licensees have to go in, do their inspections, make projections to the end of the next cycle, and then with those projections, you can go back and say, well, how well was my methodology performing by doing the -- when you do the next inspection.

And that's basically what condition monitoring is. It's basically monitoring and evaluating the as-found conditions in the steam generator, and comparing them to what you predicted that you would have during the -- from what you would have predicted from the prior cycle.

In addition, you take your as-found and you determine whether or not you would have satisfied your burst and leakage criteria during that cycle. And that's a backwards look to ensure that you had adequate tube integrity during the previous cycle.

CHAIRMAN POWERS: My understanding is that this condition monitoring and operational assessment is a program the licensees have committed to. It's not required by the regulations, but they have just committed to it.

MR. KARWOSKI: They have committed to it, yes, and to the NEI 9706.

MR. STROSNIDER: But in terms of -- and maybe we're moving into the next assessment here, but -- or subject.

In terms of Generic Letter 95-05, it becomes part of their tech specs.

MR. KARWOSKI: Right. The utilities, as part of 95-05, have to submit certain information. That information -- with that information, the staff can take a look at how well the methodology predicts. It can determine what your -- the staff can do that condition monitoring analysis.

The operational assessment is similar to all the projections. Basically, you're taking a forward look and saying will I be able to maintain tube integrity during the course of the next inspection?

And it required knowledge of NDE uncertainties, growth rate uncertainties, and the burst and leakage correlations.

Before I talk about crack growth, I promised earlier that I'd talk a little bit about the three-volt alternate repair criteria.

Once again, this slide is not in your package, but based on yesterday's discussion, I thought I'd clarify.

The three-volt alternate repair criteria was implemented at one plant -- or at Byron I, one utility. And it's a modification to the standard Generic Letter 95-05 approach. It's still a voltage-based approach, but it's different.

What they did in this submittal is, they said we want to take credit for our support plates being there. We want to say that that degradation doesn't get exposed.

As a result, our probability of burst at that location for axial indications will be minimal.

So what they did is, they went in and expanded selected tubes above and below the tube support plate, and essentially anchored the plate in place.

There were some small deflections. I think they were intending to limit the deflection to a tenth of an inch.

As a result, the probability of burst from an axial indication is minimal, however, these larger voltages, where you start to get concern then is axial tensile tearing of these indications.

They developed another voltage correlation which indicated that you needed voltages on the order of, you know, ten to 20 volts. I don't recall the numbers, but it would allow them to leave larger voltage indications in service.

And the voltage limit basically is based on this axial tensile tearing correlation, and that's where the three-volt limit came from.

This approach isn't identical to 95-05 in terms of the leakage assessment, because it introduces another concern. If the tube can't burst, it may try to burst, but once it hits the edges of the support plate, it will stop, what they called indications restricted from burst or ERBS.

And they had to develop another leakage correlation or another leakage database to say how much leakage can I get from these tubes that attempt to burst but don't fully open up?

And so they had to modify their leakage analysis. I didn't want to go into all the details of this methodology, but basically that's the three-volt criteria. It's a different concern than the one and two volt amendments of Generic Letter 95-05.

Byron and Braywood have subsequently replaced their steam generators. No operating plants currently have it, although I believe there is an application inhouse for us to review this again.

CHAIRMAN POWERS: How long did Byron and Braywood, how many cycles did they operate with that criterion?

MR. KARWOSKI: This amendment was approved, I believe, in the '96 timeframe, so I'm going to say two cycles. I'd have to look at it, but it's something like two cycles.

CHAIRMAN POWERS: Is the application that you have by another licensee to do that, is he also indicating that he will replace his steam generator in short order?

MR. KARWOSKI: I don't know in that case. I know that there is a different methodology. They're making different assumptions, and the staff will perform a detailed review.

DR. KRESS: There couldn't be a lot of database behind those correlations.

MR. KARWOSKI: With respect to the leakage database, they did an extensive test program as a result of proposing this criterion.

DR. KRESS: They did?

MR. KARWOSKI: So they did many tests with respect to how much leakage could I get, and they basically -- I'm not sure they took the bounding leak rate, but they took something near the bounding leak rate, and any tube that they predicted will burst, you know, but contact the plate, they would assign that limiting leakage to it.

MR. STROSNIDER: This is Jack Strosnider. Ken, I think you're going to be going on to more general discussions now with regard to crack growth rate?

MR. KARWOSKI: Yes, I was going to touch on that.

MR. STROSNIDER: I just wanted to make that clear, again, that I think we completed the presentation on Generic Letter 95-05 and the voltage-based criteria. And I just wanted to make a clear demarcation here before we move on to other issues, so they don't get confused.

MR. KARWOSKI: Okay.

MR. HIGGINS: Could I ask one final question on that to maybe wrap it up in my mind?

Do you consider that these -- by applying this Generic Letter 95-05, and applying these alternate repair criteria, and then doing these special main steam line break analyses where you assume that the tubes will be rupturing, perhaps, or leaking, did you consider that for these plants to be an accident of a new type and a change to the design basis of the licensing basis for those plants?

And, further, do you consider that you've addressed all of that by doing all these additional analyses and tech spec modifications, et cetera?

MR. KARWOSKI: I'm not an expert in that area, but I believe that our intent was to be consistent with the current licensing basis of the plant.

MR. STROSNIDER: These are processed as license amendments, changes to the technical specifications, and that becomes part of the licensing basis of the plant.

It is a provision, but it is done on the licensing basis. Does that answer your question?

MR. HIGGINS: Generally.

MR. STROSNIDER: I was just going to comment that I wrote down two things that we need to get back to the committee on: One was some additional description with regard to the adjustment methodology used for taking the leakage data and adjusting it for pressure and temperature.

And the other was, I think, a little more background on the ten to the minus fifth criteria that came from NUREG 0844. So we'll get back to you with those two things.

DR. CATTON: Do we have somewhere, a figure that shows the measurements of leak rate as a function of voltage?

MR. KARWOSKI: That would be in the proprietary handout. All the correlations of leak rate as a function of voltage are in there.

This came out of a more detailed report which has much more details with respect to, you know, crack dimensions and whatnot, but this is an excerpt of the database, so it shows the actual data plotted.

DR. KRESS: Jack, it was five times ten to the minus two.

MR. STROSNIDER: Yes, you're right, 0844 was five times ten to the minus two, and we adjusted it to ten to the minus second, so we'll --

DR. KRESS: You said ten to the minus fifth.

MR. STROSNIDER: I'm sorry.

DR. KRESS: You confused me.

MR. STROSNIDER: Thank you for telling me.

CHAIRMAN POWERS: A modest difference. Let me make sure I'm going to be able to derive everything I need to know about this log logistic curve for the rest of the members.

DR. CATTON: This looks a lot better than some.

DR. KRESS: If you have questions about that, you should ask them now.

DR. CATTON: I guess I just basically understand what the correlation is used for, and by the particular one that was selected. I mean, it's an obscure one. To say, that I don't know what a logistic is, is to overstate the case a little bit, but it's not the first one that comes to mind.

CHAIRMAN POWERS: Just to help you before you go on, is this the data that you're talking about that has no correlation?

MR. KARWOSKI: That's the 3/4 inch. There's a much stronger correlation. There should have been another handout that you had.

What I mentioned earlier, a tube either doesn't leak or it leaks and so what we have here is probability of leakage as a function of bobbin voltage. If you were to look at the actual data, you would see tubes with certain voltages that didn't leak and tubes with certain voltages that did leak, and you have the actual data in the handout.

The proposal made by the industry was to fit a log logistic curve through this data.

There is no theoretical basis for the log logistic. There could be other curves that fit through the data.

As part of NUREG 1477 the Staff looked at some of these other correlations and what this plot has are some of these other correlations.

The only thing I want to point out is you can see that for this function you will have a higher probability of leakage than these functions.

These curves criss-cross each other also, so which curve will be conservative, which is a function of what the voltage distribution is to some extent because depending on what your distribution is you -- depending on which probability of leakage curve you use, you'll get a different answer, so as part of NUREG 1477 the Staff looked at various correlations and various distributions.

I forget the exact numbers but if you have a distribution centered around one volt you may conclude that the COSHI is conservative with respect to the other models.

If you go to a higher voltage at 1.5 volts based on some of these correlations criss-crossing you may conclude that another model is conservative like the log logistic or the log normal.

What the Staff then did was say, well, which correlation should we use or which function should we use, and what the Staff concluded was given the other conservatisms in the model and the fact that each one of these functions had roughly the same degree of fit to the data that the log logistic was acceptable and that is why we used the log logistic.

CHAIRMAN POWERS: I think you may be too strong when you say there is no rationale for it, because log logistic is an extreme value distribution, so what you have got here is an on/off switch sort of phenomenon fitting in, and so I think there's more justification for it than maybe the normal distribution.

Now the question that comes to mind is that my recollection of the data -- I have to look again to remind me -- is if we take a value of, say, 4 volts, maybe 5, we will find tubes that had 5 volt indications that leaked with 100 percent probability because they were tested.

MR. KARWOSKI: Right.

CHAIRMAN POWERS: Okay, and at 4 volts you would find tubes that didn't leak with 100 percent probability because they were tested, so now I am wondering why would I want to use a continuous high entropy distribution like this at all for this.

Why wouldn't I just come through and say, okay, my minimum voltage at which I ever observed a leakage was 4.5

volts, say --

MR. KARWOSKI: The industry would probably like to do that.

Actually, I think they proposed that, below a certain voltage they didn't need to consider leakage, but that isn't necessarily conservative, so we fit a continuous distribution to it because you get into how much overlap of the data do you have -- do you have enough datapoints to sample the actual data?

Although there are a lot of datapoints in this overlap region I don't think there's enough data.

CHAIRMAN POWERS: So what you really were trying to do is put a non-zero probability for leakage on those that did not observe leakage in that voltage range where your -- all your datapoints said there was no leakage. You wanted to put a non-zero probability there.

MR. KARWOSKI: Correct.

CHAIRMAN POWERS: And you did that --

MR. KARWOSKI: This is an imperfect correlation.

CHAIRMAN POWERS: And you did that at the expense of putting a nonunity probability for -- in that voltage range where you had datapoints that said that it would leak?

MR. KARWOSKI: Right, but on the other hand this data overlaps. I think if you look at the data, I would have to look, but there's points which don't leak at like maybe 8 volts and I am making these numbers up, and things that leak at 6 volts and vice versa so you are right.

But given the other conservatisms in the methodology the Staff believed that that was an acceptable --

CHAIRMAN POWERS: Well, I may be preaching to the choir, but coming in and defending an action because there are other nonconservatisms all on top of nine conservatisms just emphasizes the fact that we end up not knowing what the conservatism of the whole is.

DR. KRESS: The log logistic has a cutoff at the upper end? It goes to one at some value?

CHAIRMAN POWERS: Well, I think it goes near one.

MR. KARWOSKI: It goes near one.

DR. KRESS: That's why they call that the infinity?

CHAIRMAN POWERS: I think my recollection is --

DR. KRESS: I thought it went to a value that you had to stick in there.

MR. KARWOSKI: There are functions like that. This one doesn't -- where you specify at what voltage you get a zero. There are functions like that and the industry has used them in POD but they did not use that here.

CHAIRMAN POWERS: I mean I think my recollection of it, it's very hard to invert because that is given a probability of what is the value on the horizontal axis it is hard to invert because it goes up so tightly --

DR. KRESS: Essentially one.

CHAIRMAN POWERS: That is my recollection on the thing but I can't swear that it doesn't actually --

DR. KRESS: Does it really have that little discontinues?

MR. KARWOSKI: It definitely does not have that.

MR. STROSNIDER: This is Jack Strosnider. I wanted to make two comments.

One is just with regard to Dr. Powers' discussion about putting a non-zero for these low voltages. Part of the discussion that the task group had when we were developing 1477 was the likelihood that you could have a low voltage indication but that voltage might be coming from one crack as opposed to a network, and the possibility that there is an outlier, more or less, and I am not sure that this completely addresses that issue, but there was some discussion about should it ever really be zero, likelihood of that sort of thing.

CHAIRMAN POWERS: I mean the statement was there was no technical basis for it and in this discussion I found at least two.

One is it's an extreme value, it's tradition, so it's appropriate for these things, and the rationale was putting on sort of probabilities for whatever reason down there.

MR. STROSNIDER: The second comment I wanted to make was come back, changing subjects on you here to the correlation issue again, and Ted Sullivan of the Staff just pointed out to me that there was actually more going on than just adding data to the database, that in fact the industry realized that they were not applying the P test correctly. They were doing a two sided test and it should have been a one sided test and they did the one sided. The correlation popped out.

If you look at the original data you might conclude it was there all the time then. I don't know, but there was a little more to the story than what I gave you before.

MR. CATTON: In looking at these figures, for the three-quarter inch tubes it looks like there is a pretty strong dependence on the voltage and it's rather weak for the seven-eighth inch. Is there any explanation for that? That's only an eighth of an inch difference.

MR. KARWOSKI: Right and they're scaled tubings with respect to the tube.

I believe the industry would argue and I would have to go back and look because it's been awhile since I have looked at that database, the industry has proposed excluding certain datapoints based on various reasons, and they would argue that some of those leak rates are inappropriate.

The Staff did not agree with all the exclusion criteria that the industry wanted to apply to the data.

MR. CATTON: Which, the seven-eighths?

MR. KARWOSKI: To both. It actually applies to both but I think the outliers were more in the seven-eighths inch database, but I would have to look it up.

MR. CATTON: So when this process is exercised, the one that you explained, probability, voltage distribution and so forth, you then come to the break flows. If it is a three-quarter inch tube they use this figure, the figure 6 that is in this document? This one?

MR. KARWOSKI: They would use that.

MR. CATTON: And if it is seven-eighths they would use this other one?

MR. KARWOSKI: Correct.

MR. CATTON: Thank you.

MR. STROSNIDER: I think it's empirical.

MR. CATTON: I can't understand the difference really, but --

MR. KARWOSKI: There's also observed differences in the burst pressure with respect to the one and two volts.

MR. CATTON: One has a thicker wall.

MR. KARWOSKI: One has a thicker wall but the diameters are different so the ratios are essentially the same.

MR. STROSNIDER: Actually there were some theoretical analytic studies done back when this was being developed to try to understand that difference, R over T ratios, et cetera, and I don't think anybody could ever put their finger on it.

I don't think we have a good answer.

MR. CATTON: Are there more generators for three-quarter than seven-eighths?

MR. KARWOSKI: Actually more -- with respect to the people who apply it, more are seven-eighths inch than three-quarter inch, the Sequoyahs, the Beaver Valleys, the Farleys, Diablo Canyon, Prairie Island -- they are all seven-eighths.

I think Watts Bar and South Texas are the only two that are right now three quarters.

MR. CATTON: Because you got some really low values, very high voltage, and those are built into all the averaging. That's not comforting.

MR. KARWOSKI: As Jack pointed out, I would like to now talk about steam generators in general rather than Generic Letter 9505, although I will pull in portions of 9505 in this discussion.

With respect to tube repair criteria that have been approved, the two dominant ones are the 40 percent depth base limit, which was developed 25 years ago and you have the Generic Letter 9505, which is the voltage-based for ODSCC at the support plate.

The crack growth assumption in the 40 percent tube repair criteria we kind of discussed this morning, but the growth rate assumption in there was that the NDE uncertainty in the growth rate was somewhere on the order of 20 percent and they assumed infinitely long degradation.

The crack growth rate for the alternate tube repair criteria is either based on plant specific data if you have enough datapoints or bounding generic data if you do not have enough datapoints.

One of the questions that cam up is why don't we use laboratory crack growth rate data. While there's many factors that influence crack growth, there's operating parameters like temperature. There's water chemistry, bulk versus crevice, how well do you know the type of crevice chemistry that you are having, the tube material affects crack growth.

Many of these factors, and it is not intended to be all inclusive, but many of these factors are not only plant specific or steam generator. They can be in some cases tube specific with respect to what is happening in the crevice.

It is difficult to apply laboratory growth data to the field because the assumptions made in the laboratory are usually conservative to try to bound a variety of conditions and they may or may not be representative of what is happening in the steam generator.

For that reason, you know, in Generic Letter 95-05, it's a voltage-based approach. There really is no, quote/unquote, crack growth; it's how much voltage progression do I have over the course of the cycle.

This basically describes the methodology for determining the growth that I previously described this morning. And there are two growth rate calculations that are performed:

One for the deterministic determination of the structural repair limit of 5.5 volts, and that's basically an average growth rate.

And then there's a growth rate distribution which is used in the Monte Carlo analysis for determining the conditional probability of burst.

It's in this where if you do not have enough datapoints, that basically you'll use the bounding generic database, rather than a plant-specific.

And based on the predictions of the end-of-cycle voltage distribution, in general, the growth rate -- I'm sorry. In the predictions of the end-of-cycle voltage distribution, negative growth rates, as we discussed before, are treated as negative in the Generic Letter 95-05 approach.

DR. BALLINGER: Is treated as zero, right?

MR. KARWOSKI: Is treated as zero.

DR. BALLINGER: You said they were treated as negative?

MR. KARWOSKI: Negative growth rates are treated as zero.

So, the real question is, with these growth rate distributions -- and really the only growth rate distributions that the staff uses is for Generic Letter 95-05.

As I pointed out earlier, those predictions are usually pretty accurate. I'm sorry, the predictions of the end-of-cycle voltage distributions tend to be conservative, as I pointed earlier, and licensees are required to evaluate their inspection results as a result of NEI 9706.

DR. SIEBER: If you were using the tech spec, the old time tech spec values for maximum flaw depth of 40 percent through-wall, that has built into it, a ten-percent growth, roughly.

MR. KARWOSKI: Roughly.

DR. SIEBER: How do the actual flaw growth data compare to the ten percent, if it's built into the 40 percent through-wall?

MR. KARWOSKI: There will be tubes that exceed it and tubes that are less than that. In general, for the wastage and wall thinning, it would be plant-specific on what growth rates they observed.

It's only until NEI 9706 that licensees now start doing those condition monitoring and operational assessments where they start doing more detailed assessments of what the conditions will be at the end of the cycle.

DR. SIEBER: Now, it would be interesting to know at some time, whether the original 40 percent through-wall was conservative or not.

MR. KARWOSKI: With respect to when you include NDE uncertainty and growth?

DR. SIEBER: That's right.

DR. BALLINGER: Well, when you look at it -- I've been thinking about that, and if you take a look at all of the steam generator tube ruptures that we've had, it's pretty much been independent of whether anybody has been applying the 40-percent criteria or not.

DR. SIEBER: That's right.

MR. MURPHY: Ken, if I might add one thing, this is Emmett Murphy.

Condition monitoring programs have been conducted by licensees routinely now for several years. In general, these condition monitoring programs are successful in demonstrating that all tubes have adequate margin at the end of the cycle.

So, this early experience indicates that a situation where implementation of the 40-percent plugging limit, in general, that approach does ensure that adequate margins are maintained at end of cycle; not always, but in the vast majority of the cases.

CHAIRMAN POWERS: I think what you said is absolutely true, but it looks to me like it's close in some cases. I mean, we're getting close to the ten to the minus two acceptance probability for burst in the Farley example.

And it looks like in the projection of the 15th cycle, you know, they were close. There's some of Tom's virtual character to that 15th cycle, I'll admit.

I mean, I guess the issue is, is there -- are the acceptance limits set sufficiently high that getting close is not a source of concern? And correct me if I'm wrong, Jack, but in the original discussion, that acceptance limit was really five times ten to the minus two?

MR. STROSNIDER: If you go back, again, talking about Generic Letter 95-05, and the original assumption in NUREG 0844 was five times ten to the minus second.

And we reduced that by a factor of five to account for the fact that there could be more than one degradation mechanism.

The thing I'd ask you to think about, though, is putting the voltage-based criteria aside, criteria that are in the licensing basis that the plants need to meet are three-delta-P on normal operating pressure, and 1.4 on main steam line break.

And what's being done now in terms of the condition monitoring is not only eddy current testing, but in situ pressure testing, all right? And I think what Emmett was indicating is that in the majority of case where they look at what they think is the worst defect in the steam generator and they do this in situ testing, it satisfies those factors of safety.

So, moving away from the voltage-based, there are other criteria that come into play.

CHAIRMAN POWERS: I guess the nagging concern here is that there are a lot of point examples, and I don't have enough collection data together to get out of the -- effect, you know, enough cases to persuade me that this isn't just a quick of nature, and that tomorrow we have one that's an egregious example.

MR. STROSNIDER: Well, I guess I can offer two -- the only response that I think I can give is, one, we'll continue to collect data, all right, and hopefully collect more confidence in that regard until the generators are replaced and nobody's dealing with this one any more.

CHAIRMAN POWERS: The real answer.

MR. STROSNIDER: Yes, but I'd also come back to the point that I made earlier, that there is always the potential for some new form of degradation or for some change in degradation mechanism in terms of growth rates.

I would point out that the industry has moved to more stringent controls, for example, in water chemistry. There is more consistency there. They are looking now in terms of corrective actions.

People, on occasion, will lower operating temperatures, so in that sense, things are being done that can be done to try to make things a little more predictable.

However, you can always get to a point where you reach the incubation time for a new type of degradation and it shows up.

CHAIRMAN POWERS: We have the famous 13.7 or 14.6 that seems to come about because of something unanticipated.

MR. STROSNIDER: Right, and if you look at the tube rupture list, you will see that there are a number on there that were caused by loose parts.

Just a few other observations there: One is that with regard to performance criteria -- and we weren't planning on getting into 9706 and the operational assessments and that sort of thing a whole lot, but we really did take a look at trying to establish performance criteria that left sufficient margins such that even if they were violated, it didn't represent the end of the world.

Similarly, I think -- and as I indicated, we'll have to get back to you with some more detail on the 0844 evaluation, but my recollection is that from a -- and let me characterize it as a risk-based point of view -- that you actually could have driven that conditional failure probability higher, an still come up with an overall acceptable risk, given the frequency argument.

And, in fact, we had that discussion with the industry where they wanted to use a much higher acceptance criteria, and we felt that in terms of maintaining margins and defense-in-depth, that we needed a lower number.

CHAIRMAN POWERS: Have you given thought to the issues that accompany --

MR. STROSNIDER: I'm sorry, I can't hear you.

CHAIRMAN POWERS: Have you given thought to what happens as we go to higher levels of burnup or higher boration of the water or higher power, operating power levels?

MR. STROSNIDER: With regard to power up-rates, a look at the steam generators is, in fact, part of our review.

I'm not aware that any significant issues have come up with regard to those reviews. I can't really go into a whole lot more detail than that.

CHAIRMAN POWERS: Does boration level cause any headaches?

MR. STROSNIDER: Not that I'm aware of.

MR. HOLOHAN: I think licensees, at least to date, have not been going to additional boration as a part of longer cycles of power uprates. They're using burnable poisons, and that goes to the poison concentrations are controlled by things like moderator temperature coefficients.

Now, if we were to relax our limits on things like moderator temperature coefficients, then I think the chemistry might -- I think the industry would like to use more soluble boron and less burnable, but we haven't done that to date.

MR. KARWOSKI: When I first started off, I said there were three issues that I was going to talk about. The first one was steam generator regulatory framework and operating experience, and I did that this morning.

The second one was Generic Letter 95-05, its technical basis, and also included a discussion of growth rates, and I just completed that.

The third topic was NDE capabilities with respect to detecting and sizing flaws, and that's what I'd like to discuss now.

The primary means for inspecting the steam generators tubes is eddy current testing. There are a variety of different probes that are used during the inspection of a steam generator tube, and I'm talking now in generic terms, not Generic Letter 95-05, specifically.

The bobbin probe is the tool that's frequently used just for screening the tubes for defects. It's relatively fast, can do 24 to 48 inches a second, but it's relatively insensitive to circumferential degradation.

Okay, it's also poor at characterizing degradation. As a result, there's an alternate probe that is used by licensees, and those are frequently referred to as rotating probes, rotating pancake coil probes.

There are various types of coils that go on the probe. It can be a pancake coil that is sensitive to axial and circumferential flaws; a plus-point coil which is sensitive to both also; and axially-wound coil that is only sensitive to circumferential flaws; and a circumferentially-wound coil that is sensitive to axial flaws.

DR. CATTON: Just a quick question: Why is it that they chose the bobbin to do these leak rate correlations, when it's the worst measurement speed.

MR. KARWOSKI: It's 100-percent inspection. It's sensitive to the axial type of degradation that occurs there.

DR. CATTON: But I thought that before they pulled the tube, they went in with the rotating probe to be sure that whatever they saw was right. So don't they have the rotating?

MR. KARWOSKI: They would have the rotating probe data for the pulled tubes, because they --

DR. CATTON: Which is the database.

MR. KARWOSKI: Yes, but they don't do rotating probe inspections at every intersection in the steam generator. And you need something -- as a result, you need something to correlate those intersections that have those bobbin indications to something, and you're not doing rotating inspections at every elevation.

DR. CATTON: But if you're trying to develop a correlation, and you've got such huge data scatter, why do you use the worst measurement for your correlation?

MR. KARWOSKI: I don't know if it's the worst. I don't know how the correlation of both RPC voltages would be compared to like burst pressure or to leakage.

DR. SIEBER: Maybe I can answer that. The bobbin coil probe moves at about two feet a second.

DR. CATTON: Oh, I see this up here.

DR. SIEBER: So it flies through the steam generator and you can examine two or three thousand tubes in seven days, eight days.

A rotating pancake coil goes a half an inch to six inches a second, and it just takes forever, and so it's a matter of what can I use to keep my outage down to the minimum, still do the inspection, and get reliable data.

DR. CATTON: I understand this, but maybe I'm missing something in the process. You do the bobbin coil, and then where it looks like you might have a problem, you check it with the rotating probe before you pull the tube, or do you pull the tube?

MR. KARWOSKI: Okay, I have to answer this in two parts: When licensees pull tubes, they will stick a variety of probes through there. So, for the tube-pull database, yes, you would probably -- you probably have some data with respect to the rotating pancake coil voltage at specific locations on that -- along that crack.

The bobbin coil integrates around the circumference, okay?

DR. CATTON: I understand.

MR. KARWOSKI: So for the pulled-tube database, I am sure that there is RPC data for the vast majority of the pulled-tube database.

When you do the inspection, though, you don't

RPC-evaluate every intersection. As a result, you would have nothing to compare to your correlation.

DR. CATTON: You don't RPC?

MR. KARWOSKI: RPC, rotating pancake coil probe. You don't do RPC probe inspections at every intersection.

DR. CATTON: You do it at the intersections that the bobbin coil told you might be a problem.

MR. KARWOSKI: Only if the voltage is above one volt for 3/4 inch tubes, and two volts for 7/8ths inch tubes, so you don't have that inspection data.

DR. CATTON: So above one volt and above two volts, you could check bobbin, RPC leak rate, and then you could generate a nice correlation.

MR. STROSNIDER: This is Jack Strosnider, and I want to point out one other thing here. It's not clear in my mind that the RPC is going to be easier to correlate to.

As Ken pointed out, the RPC is a much smaller probe, and it examines -- I mean, you get an actual profile around the tube, so you're going to have to say, you know, which part of that profile do I want to make the correlation with? The peak? The average, or something else.

The bobbin probe is sort of an integral look at that intersection. And so at first you'd have to decide that it may be possible that you could go through there and pick something off of there to correlate, but I wouldn't assume that I could necessarily get a better correlation.

DR. CATTON: But if you did -- but I might be able to explain why I got three decades variation in the data, because I would know more about what it is I'm looking at.

CHAIRMAN POWERS: You don't get a single number out of an RPC; you get a flattened out geography, 3D profile.

DR. CATTON: That might help the mechanics guy figure out why the hell the thing is leaking.

MR. STROSNIDER: And as I mentioned earlier, though, if you go run leak tests, not on intergranular stress corrosion, not on this network, ODSEC type cracks, but on single stress corrosion cracks, you will get the same sort of distributions in variability in leakage.

CHAIRMAN POWERS: If you've got an absolutely perfect correlation with the RPC, you would still have to have your bobbin coil distribution.

MR. KARWOSKI: Or we would have to change the Generic Letter and require licensees to inspect with a rotating pancake coil at every support plate.

CHAIRMAN POWERS: Yes, you could do that, but given that you didn't do that, you'd still need a bobbin, and it would still look just like it does.

MR. KARWOSKI: Right, you would need something to correlate.

CHAIRMAN POWERS: They've got to account for all of the indications that they find in carrying out the process, not just those that are over some voltage limit, be it one volt or two volts.

MR. KARWOSKI: That's right.

CHAIRMAN POWERS: You have to invert those distributions in some way to end up with a predicted leakage rate.

MR. KARWOSKI: That's right. Another probe is used. It is a Cecco probe. It's a transmit, receive and a ray-type probe. It is medium speed, around 12 inches per second. It's sensitive to axial circumferential flaws.

Some utilities choose to use the Cecco probe instead of the bobbin and rotating. Other utilities feel more comfortable using the bobbin and rotating. It is up to the utility. The tech specs do not dictate what probes to use.

The other thing that I put on this slide is that ultrasonic testing is also sometimes used to inspect steam generator tubes.

Usually it's as a supplemental technique to help characterize degradation.

CHAIRMAN POWERS: Before you launch into that, I'd just raise a question that the former Commissioner Rogers raised on several occasions.

Is there a growing technology in this area? Are people trying other kinds of technologies to do better on these things?

MR. KARWOSKI: The technology is evolving. A couple of the slides I have, there's different algorithms used.

There's new probes that are coming out. A lot of them are still based on eddy current technology but there are advances being made and you will hear about some of the work being done at one of our contractors.

The probes used to inspect steam generator tubes have changed over time. Originally in the 1970s they used the single frequency bobbin coil and I think somebody described it before where they had a oscilloscopes where they were analyzing the data.

That was good for general wall loss type of degradation mechanisms, not so good for stress corrosion cracking.

In the last late '70s and the '80s, multiple frequency bobbin coil techniques started to be developed along with rotating pancake coils probes. In the early '70s there were no rotating probes.

The multiple frequencies allowed mixing out some of the unwanted signals and it allowed you to focus at different parts of the tube while just pulling the probe through once rather than several times.

The rotating pancake coil probe, as I said earlier, was better at detecting and characterizing stress corrosion cracking. It is better in geometry changes. It is sensitive to circumferential flaws. It was initially used primarily at the expansion transitions.

Widespread use of the rotating pancake coil probe began in the late '80s, early '90s. People, licensees, really started to inspect their expansion transitions.

The plus point coil, which you probably heard about, merged in the mid-1990s. Its first major application was at Maine Yankee, where they had problems with circumferential cracking at the expansion transition and new probes and data analysis software and techniques continue to be developed.

So what drove these improvements in technology? Well, both economics and regulatory concerns. If you remember from this morning I showed a plot of the forced outages as a result of leakage. Back in the '70s and early '80s there was a number of outages. Outages are costly.

What caused those leakage outages? Well, it can be a variety of factors.

It could be the technique capability -- how good was that single frequency bobbin coil at really inspecting the tubes for the degradation mechanism of interest?

Could have been analysts' reliability. How well were those analysts trained on those techniques? A lot of those techniques were evolving at the time.

Could have been high growth rates.

It could have been any combination of those three.

In the mid-'80s the Office of Research had Pacific Northwest Laboratory analyze the removed steam generator from Surry. They shipped the steam generator from Surry up to PNL -- Hanford? -- and they did a bunch of analysis. They did leak testing, burst testing. They developed various correlations.

Under that contract they also determined some probability detections. The dominant degradation mechanism or one of the prevalent was wastage. However, as part of that program they did a mini-round robin of laboratory samples, not from the Surry steam generator -- laboratory grown samples, and determined the probability of detection.

The probability of detection varied from team to team and I don't recall the exact ratios but some of them were pretty poor -- .3 probability of detection -- to larger, maybe .8 POD independent of depth.

The average turned out to be about .6, and that is where we got the value for the generic letter 9505 correlation, so we used a POD of .6 based on techniques available in the mid to late '80s and we applied it to the

Generic letter 9505 approach.

Much has happened since then. In the '80s and early '90s the industry started developing ISI guidelines. They needed to be able to detect this degradation sooner. They didn't want to have forced outages. They wanted more reliable inspections.

These guidelines started to specify criteria for the probability of detection. Some of the versions in the early '90s said we could have an 80 percent probability of detection at 90 percent confidence and there's various criteria with respect to what are the depth distributions of the degradation.

The only thing I just wanted to point out is that the industry guidelines really started evolving in the late '80s and early '90s.

They also developed a qualified data analysts program, a rigorous training program, with the analysis qualified to detect specific types of degradation.

The other issue that was starting to emerge in the early '90s was generic versus plant specific qualification of techniques. The role of plant specific factors in the detection of flaws became a concern. Yes?

DR. SIEBER: About two hours ago you were talking about the NDE uncertainty distributions.

MR. KARWOSKI: Yes.

DR. SIEBER: And you said it was made up of two elements, analysts' variability -- that was about 10 percent? -- and physical variability, which I presume includes systematic and random calibration errors, probe wear --

MR. KARWOSKI: It was predominantly probe wear.

DR. SIEBER: Okay, because other factors were in there?

MR. KARWOSKI: It was specifically probe wear. They shaved a probe and analyzed what the change in voltage response was as a result of physically wearing down a probe.

DR. SIEBER: How did they accommodate things like calibration errors and random errors that occur just in the process of any kind of sampling technique?

MR. KARWOSKI: All of those types of errors will be captured and all of the correlations, because all those pulled tube datas will have all those different errors promulgated through it.

DR. SIEBER: How did they come up with the number for analysts' variability?

MR. KARWOSKI: Analyst variability was by having a group of analysts evaluate several different -- well, hundreds of different indications and determine what was the difference between the voltage calls between the different --

DR. SIEBER: Who is smart and who is not, right, essentially, so there is actually a basis for these distributions that go into that for every element of it?

Do you have a list of all the elements, what the distribution looks like?

MR. KARWOSKI: For the two distributions, there are only two distributions --

DR. SIEBER: Only two.

MR. KARWOSKI: -- that the probe wear --

DR. SIEBER: And the analysts.

MR. KARWOSKI: -- and the analysts' variability, and we do have that data. We can provide it to the committee.

DR. SIEBER: If it is not too lengthy --

MR. KARWOSKI: No, it is actually --

DR. SIEBER: Two pages. Thank you.

DR. BONACA: You said before that the POD, the licensees contend that the POD depended on the entity of the defect, that for a bigger signal you would have -- do you have any inputs on distributions from the licensees?

MR. KARWOSKI: Yes. I will show you several of those.

DR. BONACA: Okay.

MR. KARWOSKI: In the mid to late '90s the industry and the NRC developed additional guidelines which addressed various factors like plant specific considerations and the concern of what is the probability of detection. Is the technique capability or is it the entire system?

There is currently an evaluation of steam generator mockup samples being performed by Argonne National Laboratory as a result of a contract with the NRC. At the very end of this presentation I am going to ask Dr. Muscara to present some of that work and their results with respect to detecting and sizing flaws.

There are a number of factors that affect detection of flaws. There's equipment and technique variables. There are -- the analyst plays a role in detection. There's also plant specific considerations.

With respect to the essential variables, there are equipment variables -- what equipment do I use to get the data, what type of acquisition system, what type of probe am I using, what type of cable, is there enough shielding in that cable to prevent noise signals from being picked up.

There's technique variables. What are the frequencies, the dry voltage calibration method, how much data am I obtaining, the digitizing rate, how do I scan the tube -- what is the direction, do I gather the data on the push or pull?

There's analysis variables that also affect detection, including what the data review requirements are, the algorithms used in the software, and the calibration.

Of course, analysts' reliability plays a role in detection and there is also plant specific considerations.

There's the role of deposits. Are those deposits conducting, nonconducting, ferromagnetic. How do those deposits affect the signals? Dents and geometry changes affect your ability to detect degradation. Support structures. Do I have other interfering signals that are coming in. The crack orientation, is it axial, circumferential? Is it on the ID or the OD of the tube? Also there is noise.

CHAIRMAN POWERS: You have noise, electro: tube -- is it tube noise?

MR. KARWOSKI: Tube noise is basically the surface of the tube is irregular. Also tube noise can come from deposits. They kind of overlap. Noise comes from a variety of sources.

Okay. The industry qualifies specific probes, specific sized probes for specific degradation mechanisms under specific circumstances at specific locations, so when we say what techniques are qualified for detecting, it depends on what you are looking for and how you are looking for it.

Each one of these techniques has a list of essential variables, so what does this mean?

Basically a given size plus point coil is qualified to detect circumferential primary water stress corrosion cracking at a specific frequency in dented locations. The industry has a dataset which they believe demonstrates that they have an 80 percent probability of detection at 90 percent confidence.

The industry has lists of essential variables that go with each one of these techniques. I need to keep this probe or I need to acquire the data in this fashion and the dataset that supports qualification has this much noise. The industry has an extensive program with respect to the ability to detect degradation.

There are a number of issues that have been raised with respect to what is the probability of detecting flaws under certain circumstances, and I have listed some of them here.

One of them was axial versus circumferential cracks. How does your ability to detect degradation depend on the orientation?

Well, the best way I can answer it is it depends on the probe. A bobbin probe will not reliably detect circumferential degradation. Unless that circumferential crack has opened up a lot axially it won't seal.

The bobbin probe also has weaknesses at detecting axial degradation under certain circumstances, namely in dents, severely dented tubes, U-bends, and expansion transitions.

The pancake and plus point coil, on the other hand, are qualified for detecting those cracks which the bobbin coil would not be qualified for. For example, axial degradation at those locations or circumferential --

CHAIRMAN POWERS: Initial screening done with a bobbin doesn't detect circumferential cracks reliably, that is what you are saying?

MR. KARWOSKI: Right.

CHAIRMAN POWERS: And are those, is the rationale then for using the bobbin coil for the initial examination then that circumferential cracks are sufficiently rare that missing them if of little consequence?

MR. KARWOSKI: Licensees in general do not normally just use the bobbin coil. The industry is aware through numerous generic communications that the potential for circumferential cracking exists at various locations, namely at the expansion transition, at dents and U-bends, and in sleeves, and as a result consistent with Appendix B they use techniques that are qualified for detection.

If you look at what licensees do, they would, in those locations they would do some type of sampling program to ensure that they are detecting the forms of degradations that those tubes are susceptible to.

The Staff has issued generic communications highlighting the weaknesses.

CHAIRMAN POWERS: So I understand this better, talk about circumferential cracks in the free span area. Are those sufficiently rare that you think there is no need to go around looking for that?

MR. KARWOSKI: There have been no circumferential cracks in the free span area.

CHAIRMAN POWERS: That's why I picked the example.

MR. KARWOSKI: The answer is yes.

You would have to look at are there any stresses sufficient to induce that type of cracking in the free span, and in general the likelihood of that is very small and so -- and that supports operating experience where none have been identified either through leakage or through inspection.

The bobbin coil I did say is relatively insensitive, unless those cracks open up. If it were happening in the free span in a nondented area, you would have either, you know -- the cracks would eventually start growing and you would either detect it by bobbin or through leakage. We haven't observed that today.

The other issue is, how well can I detect with respect to isolated cracks versus cracks in clusters? The comment that I make there is that, in general, the more material you use, the less ligament paths that you have, the easier it is to detect that type of degradation, but characterization of the flaw is much more difficult.

DR. POWERS: Is there a well recognized description of the width of cracks?

MR. KARWOSKI: Of the what?

DR. POWERS: Width of cracks. I mean a very, very fine crack, different from one that is opened up a ways.

MR. KARWOSKI: I am not sure I understand what you are asking.

DR. POWERS: Well, it is this volume, missing volume. The more missing volume, the easier it is to detect clusters. What I am thinking is, suppose I have an isolated crack and have a crack that is really hairline wide and has lots of cross ligaments throughout its length, or I can have another isolated crack that actually has some canyon, open, it is a canyon, I can put something down into it.

MR. KARWOSKI: Right.

DR. POWERS: The two are very different, and I am wondering how you -- I am having a hard time describing these cracks. Is there some measure or something like that that does a better job than I am on describing these two different types of cracks?

MR. KARWOSKI: In terms of NDE, no. But you could describe what the crack opening area is, but in terms of

NDE, you know, the only thing you would notice is that you get a much larger signal from an EDM notch than from a crack. I mean the wide open one is what I am thinking of.

DR. POWERS: What I am thinking of is there are going to be some cracks, I mean I can imagine a crack, whether one actually ever existed, and which, across the length of it, there are ligaments and metal that are still there.

MR. KARWOSKI: Right.

DR. POWERS: That might be very different than one in which there were no ligaments.

MR. KARWOSKI: Absolutely.

DR. CATTON: And then if you fill those spaces with some kind of crystals of something or other, it would be completely different again.

DR. POWERS: Or if it was amorphous sludge, it would even be worse.

MR. STROSNIDER: This is Jack Strosnider. That is certainly true, and that is just describing physics and the reality, what is out there.

I guess what I think is really relevant here, though, and we have had a lot of discussions with the industry, and if you look at our Draft Reg. Guide 1.174 that was put out for public comment, when you talk about the qualification process, it is very important to use what you might characterize as prototypical cracks. And you give a perfect example where the use of EDM notches may not give a good qualification demonstration.

And the industry has made some progress in that area. We continue to push on it because it is very important. And some of what we are discussing in the 97-06 framework is, you know, the need to use cracks that have signals that are representative of what is in the field, whether they be autoclave or pulled tubes. There are some EDM notches used in the qualification database, but, you know, we push to minimize that. And I don't know if that is part of what you were thinking about, but it is an important aspect of recognizing the difference.

MR. KARWOSKI: Some other issues they came up with, how is my ability to detect cracks that are plugged? Once again, this issue depends on the nature of the deposits. In general, deposits on the tubes are on the outside surface of the tube. They are not -- these stress corrosion cracks tend to be very tight.

But it would depend on the nature of the deposits. If they are conducting, it is going to lower the eddy current response, making it more difficult to detect. Ferromagnetic deposits, you know, will increase the response, but it will mask the flaw. If the deposits are not conducting and no effect on tube noise, you probably have similar detection probability.

The other thing is, what is my ability to detect cracks relative to location with respect to tube support plates or bends? In general, the more geometry changes that you have, the harder it is to detect a flaw. However, there are techniques that are qualified for giving locations in order to provide detectability.

Operation speed was also -- how does that affect your ability to detect degradation? Utilities have done various assessments of this. The speed of the bobbin coil has gradually gone up as the ability of the hardware to process this data has gone up. Typically, the run the line tests where they change the speed and determine whether or not they would have detected the same tubes, you know, regardless of the speed.

What is interesting to note, at first you would think the higher the speed, the more difficult it is. At one of the plants, they increased the speed, I believe the ratio was 12 to 24 inches per second, but what they found out, that increasing the speed actually reduced the noise that they were observing and the detection was comparable.

That is not to say, though, well, let's go up to the next speed and it is going to be better. There is a problem with too fast a speed and that is because if you have any geometry changes, which you do, in several locations in the steam generator, that probe can jump, resulting in noisy data, missed data and cause problems. In addition, too fast a speed, because of the software that processes this data, you can have some frequency effects if you don't have proper compensating software.

The next part of the presentation shows you probably detection curves for various degradation types. These are all industry data. The staff has issues with them. I will point some of these issues out as we go along, but the committee indicated it wanted to know how we can do with respect to detection. I am going to present some industry data.

Let's start with wear, general wall loss type of thing. In general, detection is pretty good. What this has on this plot is the fraction detected as a result of the throughwall extent of wear indications, under, you know, given frequency indications. At face value, and this is the industry data, from December '93, at face value, you might say, well, I can detect 100 percent of flaws affected by wear. The staff isn't saying that, but that is what this data may tell you.

And at the end of presenting all these probability of detection curves, I will go over some of the issues that we have with respect to this data. But, in general, the staff expects that, you know, sizing or detection of general wall loss indications is pretty good.

DR. POWERS: What would it be at 99 C/L?

MR. KARWOSKI: What that is is what is the probability of detection at 90 percent confidence. So even though you detected 100 percent, you know, you only had 13 samples and there it is. Remember, the acceptance criteria that I believe the industry is still using is 80 percent probability of detection and 90 percent confidence.

Okay. This next plot is for plus point coil detection in a sludge pile region. Once again, in this case they fitted a curve to the data, you have probably a detection as a function of maximum depth. You get into the same issues, why a logistic fit versus other fits. Basically, what this says is we have got somewhat of a high probability of detecting larger flaws, a lower probability of detecting smaller flaws. That is the sludge pile.

This is also for sludge pile. That first graph was maximum depth. This one has several different curves as a result of maximum depth and average depth. And as you might expect, the probability of detection for the maximum depth is less than the average depth, and that is because you have a larger flaw when your average depth is greater than just the peak. But, in general, for the average depth, the probability of detection is larger.

The dots indicate the bobbin coil probability of detection. This data, once again, industry data, indicates that the probability of detection with the bobbin coil is better than the plus point for these low voltage indications. To what extent that is an artifact of the distributions they chose, I don't know. I am just presenting what they submitted.

This next graph is for --

DR. SIEBER: A question.

MR. KARWOSKI: Yes.

DR. SIEBER: The plus point has a whole bunch of different coils, some of which look like bobbin coils and some of which are rotating.

MR. KARWOSKI: The plus point coil is a specific coil that will fit on a rotating probe.

DR. SIEBER: Okay.

MR. KARWOSKI: The plus point coil has an axial coil and a circumferential coil.

DR. SIEBER: Okay. But you can --

MR. KARWOSKI: Okay. Wound together.

DR. SIEBER: -- mathematically play with it.

MR. KARWOSKI: Right. And one of the advantages, or the advantage of the plus point coil, theoretically, is that you have some general disturbance area, those two coils, the signal from those two coils will cancel out. That can also be a disadvantage of the crack if a crack is perfectly, you know, symmetric and hits both coils at the same time. But that is not -- that is theoretical general, those types of flaws don't exist.

Here is a plot from December '93 of probability of detection for outside diameter stress corrosion cracking at the support plates. Staff used a value of .6 for the Generic Letter 95-05 methodology. What the industry presented in December '93 is basically for everything over 40 percent throughwall, we were detecting. The issues had issues with respect to that, and that is why we used the .6. But here is what the industry presented back in December '93.

PWSCC occurs at dented tube support plate elevations. Here is a plot of the fraction detected versus maximum depth. What I will point out on here is this has the fraction detected for the plus point coil, the bobbin and the Cecco.

Just to point out some of the issues that you have with limited data sets, you would conclude here that your probability of detection may be higher in the 0 to 10 percent range than in the 10 to 20. That is an artifact of the amount of data and shows some of the difficulties that the staff has with respect to when somebody says, what is the probability of detection? It all depends on the data that supports the correlations.

The other thing to note is, you know, why would you have a lower probability of detection at the higher depths? And, generally, you would think the deeper the degradation, you are better your probability of detection. But, nonetheless, this basically shows, you know, reaching towards 100 percent when the maximum depth hits around 50 percent for PWSCC at dents inspected with the plus point. Once again, this is all industry data.

Here is another example for PWSCC at dented tube support plate intersections. Some of the data comes from that previous graph. I put this up because it compares the bobbin and plus point, and this is a result of fitting a specific function recommended by the industry through the data. And what I wanted to point out here is here a function, the bobbin POD is a function of depth, here is the plus point, the two cross. In general, that is probably a result of either the curve fit or the limited amount of data, or the fact that, you know, you missed one indication with the plus point and it results in a worse probability of detection.

In general, the industry and the staff believe that the plus point is probably better than the bobbin in that area.

So how does the staff use the probability of detection? Usually, probability of detection is only used in Generic Letter 95-05 tube repair criteria. And in that analysis, as I pointed out this morning, that we used .6, and that is based on the roundrobin analysis of the laboratory stress corrosion cracking samples that was done in the late '80s.

As I pointed out this morning, that is used not only to account for missed indications, but also indications that may initiate during the course of a cycle.

And here I have a rhetorical question, is .6 the correct value? I have showed you various data the industry would argue for deeper flaws were better than .6, and there may be some more data forthcoming from some of the Argonne roundrobin tests that may support that, I don't know, but I will say that the .6 is conservative and accounts for other things than just missed indications, and that is based on the operational assessments.

MR. HIGGINS: Do you know the breakup, breakdown between missed and the ones that would initiate during the cycle? And have you looked at end of cycle, beginning of cycle test results for the last few years on 95-05 to see if that number of those that initiate during the cycle is reasonable with the actual data?

MR. KARWOSKI: The staff has looked at the 90 day reports, and the industry has, with respect to, you know, did I detect something in the prior cycle? The industry has actually proposed an alternative way for POD where they look -- and I don't recall this methodology in detail, the staff hasn't approved it, but, basically, they do look back and say, was there a signal there? And in some cases they missed it, in which case that would be a missed indication. In some cases, they would say, well, analysis wouldn't have called that, so that would be a new indication.

I don't know if anybody has ever done a comprehensive assessment of, you know, how many indications are new versus how many indications were missed. I know the industry has proposed an alternate probability of detection model based somewhat on that type of approach, but, to date, the staff hasn't accepted that.

MR. STROSNIDER: I would just add to that, it might be difficult to separate the two, although, perhaps with a hindsight review, when you detect something in this outage, if you go back to the prior outage with some knowledge, maybe you could say that it was there. But I think the important think is you asked, is it being benchmarked, and the sort of histograms, if you will, of voltage versus number, you know, that is one of the reasons we look at that, and one of the reasons we concluded that the .6 does a reasonably good job of accounting for those that are missed in inspection and new indications.

If we saw that those distributions were radically different, one thing you could modify is that .6 factor and try to bring them back into alignment.

MR. LONG: This is Steve Long. Let me add one thing. By the time these things get to me it is usually because they are doing some sort of risk-informed license amendment request, so I guess I probably see the problem ones more than anything else.

We had a problem with Arkansas Unit 2 this past spring and their proposal to not perform another mid-cycle inspection where they would have to take two inspections in the middle of a fuel cycle. And we had a problem with reconciling what they were projecting as the flaw distribution of the tubes during the remainder of the fuel cycle with what they had found in the last inspection and what they had projected to find in the last inspection.

And it comes down to a question of a combination of the growth rate and the POD in the previous inspection, is your growth rate right or your POD right?

Arkansas was insisting that the things that they missed in their inspection before last, they could find when they did a lookback they were missed, and that their growth rates were consistent once they found those things in the lookback, that the growth from what they probably should have seen in the inspection before was what they would expect up from the last inspection.

So we asked them, then what does that say about your POD? And I believe the POD was about .4, which was one of the reasons we couldn't probably work with that, because some of the larger flaws that we thought they would need to be able to detect, there would need to be performance -- screening criteria. They really didn't have a very good probability of detecting those in the inspection before last, and we couldn't credit any change in the inspection process for improving that over the last inspection.

So there are problems occasionally.

DR. HOPENFELD: This subject was discussed yesterday and I pointed out that there is a possibility of differences between running a test in a laboratory POD, determination in a laboratory or at PNNL, versus in real life, and I think this is a very good example, except that this is only one.

And one of the questions that we didn't get to discuss yesterday, this pertained to this very item, that we need more information from plants to determine what that POD is. Keeping this in perspective, I don't think it is proper to say that what we are using is a conservative number. We don't really know it is conservative.

DR. POWERS: Let me ask how much longer you have to go on this presentation?

MR. KARWOSKI: A half hour.

DR. POWERS: Why don't we take a 15 minute break now. We will recess until five of the hour.

[Recess.]

DR. POWERS: Let's go back in session. Pardon me for interrupting you yet again, but I firmly ascribe to the belief that the mind will ultimately absorb what the body can endure.

MR. KARWOSKI: I am aware of that.

DR. CATTON: You know where the limits are, so you are going to push them.

SPEAKER: You say we are staying until what, 10:00?

DR. POWERS: There is nothing sacred about 10:00.

MR. KARWOSKI: In the interest of time, I will try to finish these last few slides. The last thing I wanted to say about POD is there is a lot of issues. Some of those curves showed it. We don't have 100 percent probability of detection. It does, you know, 5 percent degradation for some of those mechanisms.

Some of these concerns have already been raised. Lab data versus field data. Is the field data -- or is the lab data representative of the signals in the field? If not, is it appropriate to use it.

DR. CATTON: When you say lab data, is that a lab created defect as well, or is it just a lab measurement of a pulled tube?

MR. KARWOSKI: It is like a model boiler specimen, a laboratory created defect, like a stress corrosion crack developed in a model boiler. Is the signal that I get from that representative of the signal that I would have observed in the field at that location?

DR. POWERS: Ken, you didn't discuss -- one of the documents that we got, and I cannot remember which one, spoke of a rather elaborate set of measurements on a steam generator that had been removed from service at Surry, and in which there was quite a lot of effort to address the issues of probability of detection and whatnot. You didn't bring that data up. Is there any particular reason why?

MR. KARWOSKI: The only thing, the Surry steam generator, and Dr. Muscara can correct me if I am wrong, the dominant degradation mechanism there is wastage and pitting, and as a result, a lot of that was probability of detection curves. Although they may be appropriate, it is not the degradation mechanism of interest.

As part of the program, they did do that stress -- laboratory grown stress corrosion cracking mini-roundrobin, and that is where the .6 came from. With that said, towards the end of this presentation, I am going to have Dr. Muscara present the results of another roundrobin that is going on now. Results should be available at the end of this year, sometime next year, and he will describe that program, which is probably more relevant to inspection capabilities today.

The other thing is -- another POD issue is the overall system versus technique capability. When you analyze some of these tubes, it is part of the qualification program. Assume they are all field data that you are getting. These tubes are analyzed by a lot of people under controlled conditions and get a much more detailed evaluation than necessarily a production inspection where the analyst is asked to look at thousands of tubes, you know, in an outage.

So the question is, what are these curves really measuring? Are they measuring what the technique is capable of doing, or is it measuring the overall system, how the analyst is going to perform under actual field conditions?

The other POD issue is, how pertinent is this probability of detection curve to the plant that I am applying it to? There are plant-specific circumstances that may affect detection. Do I have copper deposits? Are my deposits uniform such that I may be able to mix them out? Are they spotty, causing problems in detection? Do I have a lot of denting, a lot of noise from a variety of sources? That is another issue with respect to POD.

The other is, how do I evaluate this data, and what curves do I fit through it? Is it a function of average depth, maximum depth? Do I use the log logistic or what type of curve do I use to fit the data?

The other issue that I have put up is false calls. When you do these inspections, when you pull a tube and you look at it, you may tend to over call with respect to some of these indications and say there is something there, but then it is not there. Does that count against you in the probability of detection calculation? Well, maybe it should if you are not using the exact same criteria in both applications. Those are just some of the issues that we have with respect to the POD curves.

The first part of the presentation, detection. Can techniques detect degradation? The next part is sizing, and I am going to go through this relatively quick and just provide some examples. There is a lot of examples in your handout.

The first thing I would like to point out is a technique may be qualified for detecting a form of degradation but may not be qualified for sizing it. And that is why I have the bullet -- Utilities routinely use the bobbin coil for detection and other probes such as like a rotating plus point probe for characterizing it. And sizing can be in terms of length, depth, voltage. It depends on what type of correlation you have for your structural leakage integrity.

What is considered qualified with respect to sizing? In the past the industry has used the root mean square error approach. There is limitations to that. Probably a more appealing approach might be just understanding the uncertainties in your technique and then applying those uncertainties in your condition monitoring and operational assessment.

With respect to what is considered qualified, generally, the bobbin coil is considered qualified for sizing wall loss type of indications. The bobbin coil is, quote-unquote, qualified for sizing via voltage for the Generic Letter 95-05 methodology. And the NRC has approved the use of the plus point coil for depth sizing degradation at dented tube support plates, primarily axial PWSCC.

There is several pages in your handout with respect to sizing curves, how well techniques do with sizing. Once again, this is industry submitted information. There are some issues with respect to this. I will just put a couple of them up. If you have any questions, I will be glad to answer them.

Here is a plot of the estimated throughwall depth versus the true -- or throughwall depth as a result of metallography. This is for wear indications and, in general, you have a pretty good -- you can estimate the depth of these indications pretty well, and that is generally accepted throughout the industry.

And there is various other correlations. I don't want to spend too much time on them given that we are already behind schedule. But this is for sizing sludge pile ODSCC, destructive exam maximum depth versus NDE depth, and you can see that there is a lot of scatter in the data. And there is various -- in your handout, there is various correlations with respect to how well different techniques can size specific degradation mechanisms in terms of length and depth.

The burst characteristics of a tube depend, basically, not just on, you know, a single parameter of length or depth, but more a function of a composite of the crack and there may be a limiting form -- a limiting portion of that crack.

This plot here is a plot of a circumferential crack located at an expansion transition which was pulled. The metallography, this tube was pulled in the '95-'96 time. The metallography is shown here. This was a deep circumferential crack with some ligaments in between, two deep portions of the crack, I guess it is wrapping around, but there is a ligament there. That is the destructive exam results. If you look at what the NDE analysts called this, they significantly undersized portions of this crack.

DR. POWERS: Maybe clarify what the vertical axis is.

MR. KARWOSKI: The percent through the wall.

DR. POWERS: Throughwall, yeah.

MR. KARWOSKI: So this is the crack profile. Basically, it is 100 percent through the wall, in this region there was a ligament, it went back to nearly 100 percent and then there was -- and this using an 080 pancake coil, and that is just sizing of indications, circumferential ODSCC at the expansion transition in '95-'96 timeframe.

I just point that out to show --

DR. CATTON: This doesn't say anything about the cross-sectional area.

MR. KARWOSKI: Cross-sectional area.

DR. CATTON: Well, I mean what is the flow area through a crack like this?

MR. KARWOSKI: No, it doesn't say anything. Typically, the eddy current data --

DR. CATTON: You just take a slice?

MR. KARWOSKI: Well, they look at the various ligaments and they develop, you know, somewhat of a composite type of crack. But, you are right, it is a slice.

There is a couple of more plots of that same tube with different types of probes. But just to show you a little difference, one of the qualified techniques, or a technique where we approved an alternate repair criteria, here is a tube that was sized with a plus point coil for primary water stress corrosion cracking at dents. That is not listed on here, but that is the type of degradation.

And, in general, you see the destructive examinations and the solid symbols, and you have the analysts. Much better agreement with respect to both the length and the depth of the degradation. And there are several examples of this type of degradation in the handout.

DR. POWERS: And you said this was primary water stress corrosion cracking?

MR. KARWOSKI: Primary water stress corrosion cracking at dented tube support plate elevations.

There are many factors that affect the ability to size, and a lot of them are similar to those with respect to detection. What technique, what type of coil, the location of the degradation, frequency. What are the interfering signals? Noise.

Plant-specific considerations play a role. Typically, the qualification, though, is done generically. Licensees, however, need to assess whether or not that technique is applicable, whether or not they can use that generic qualification at their plants.

One of the things that some of the tube pulls programs that have been done revealed is that, although you can't necessarily determine the quantitative size of the degradation, the general severity can be inferred from the eddy current results. And what this allows licensees to do is, although they -- was that me?

DR. POWERS: Your battery is probably going out.

MR. KARWOSKI: What this allows licensees to do is to select some of the more limiting tubes for in situ pressure testing to determine whether or not the tubes meet the required structural and leakage integrity requirements.

There are a number of questions with respect to the factors affecting the ability to size as a function of different parameters. Crack orientation. Basically, circumferential cracks are located in locations where there is geometry changes that typically will make it more difficult to size. Isolated cracks versus crack in clusters. The problem there is the eddy current coil has such a width that it may not be able to discern some of the individual cracks in a cluster of cracks.

With respect to plugged cracks or cracks occluded with crud, once again, it depends on the nature of the deposits and crack location relative to support plates. The more interfering signals that you have, the more difficult it is to size.

DR. POWERS: Well, you have indicated a couple of times that the plugging material affects it depending on its particular nature, whether it is ferromagnetic conductive or, I guess, if it is insulating sludge, it is just like air getting in there. What do you typically have? Or is there a typical?

MR. KARWOSKI: With respect to inside the crack, it tends to be very tight and there is not really a lot of deposits inside the crack. Most of the deposits are around the outside of the tube. Okay. Those deposits, if they are uniform, you know, if they are non-conducting, they may contribute noise which will make it more difficult to detect.

If they are conducting and they are uniform, you probably have a better capability to detect the flaw that if the deposits are conducting and they are not uniform, because then you are going to get a lot of individual signals where you won't be able to discern what is noise from what is a defect.

Does that answer the question?

DR. POWERS: Well, I guess the one I am interested in is the cracks are tight, there is very little material within the crack, but surely there must be some material in there. I am sure there is oxide coating on things.

MR. KARWOSKI: Right.

DR. POWERS: If indeed these are oxides or spinels, they can't be -- at least any ferromagnetic.

MR. KARWOSKI: Right. And so these deposits will interfere, but, in general, I have shown you some of the data that the industry have presented. In general, that will come out in whatever sizing curves that they develop. So it will affect it, the magnitude.

DR. POWERS: Well, I guess what you are telling me is, whereas, in principle, the cracks can be affected by the crud that is in them, or around them, in fact, it is either accounted for in the empirical determinations or it is just not a very big affect.

MR. KARWOSKI: That's correct, that would be my interpretation.

I have presented basically industry data with respect to detection and sizing. The Office of Research has a program at Argonne National Lab which is also looking at the ability to detect and size flaws. So at this point I would like to turn it over to Dr. Muscara, who is going to discuss some of the work being done at Argonne.

DR. POWERS: I have some experience with the work Argonne and we won't hold that against you.

DR. BONACA: Just I have a question I would like to ask you before. Regarding the information distributed this morning.

MR. KARWOSKI: Yes.

DR. BONACA: For those correlations. Is it all field data or is it also laboratory data?

MR. KARWOSKI: It consists of both field and laboratory data.

DR. BONACA: I looked for information on separation of the two, I couldn't -- maybe I have to look deeper.

MR. KARWOSKI: Some of the plots may discern laboratory from field. I don't know if those do. But if not, we can provide you the raw data that shows you which ones are model boiler specimens and which are field.

DR. BONACA: A little understanding of --

MR. KARWOSKI: The relative counter.

DR. BONACA: -- how preponderant one group or the other is over the other.

MR. KARWOSKI: Right. There is a lot more field data now than there was in '95, but we can provide that to you.

DR. BONACA: Okay.

DR. CATTON: Joe, what is a model boiler?

MR. KARWOSKI: When I said a model boiler, it is just -- how should I? It is a means of creating laboratory cracks, where you basically subject a tube to accelerated corrosion tests, or accelerated corrosion, so that you can develop a flaw in a subsequent test.

DR. CATTON: Boiler, okay. And what are added tubes?

MR. KARWOSKI: Added tubes. Oh, with respect to those correlations.

DR. CATTON: Yes.

MR. KARWOSKI: The industry updates the database from year to year, so the added on that means we have added new data from the previous one. So what they are doing is assessing the changes with respect to adding the new data, that is what those curves are referring to.

DR. CATTON: Okay.

DR. MUSCARA: Thank you, Ken.

I am not sure whether I should stick with my viewgraphs. I mean I have heard a lot of things that I would like to respond to, but I know we probably won't get into that. But before I do get to the viewgraphs, there are a couple of points.

I was really shocked yesterday to hear that in NUREG/CR-2336, we had data on the probability of detection that the tube support plate location, I think it was set at the tube intersection, but tube support plate location. I was surprised mainly because I had planned that work and managed it for many years, and we did no such thing. There was no work done that evaluated POD at the support plate.

And Dr. Powers was correct, we did a lot of work on the Surry program to quantify the capability of inservice inspection for the Surry generator. In general, we had, as was mentioned earlier, mostly wastage and pitting, and we did a thorough job of evaluating that type of degradation.

We had a number of industry teams inspecting the generators, the same way that they inspected field generators, and we got some valid data.

Well, this work was done essentially -- well, the entire Surry work was done in the time period between 1982 and 1986. We did have lots of cracking in the Surry generator at the support plate location due to the gross denting that was going in this generator. But at that time inspectors refused to give us information about support plate, they knew they could not inspect in that condition.

The denting we are talking about in those days was much, much more gross than what we are seeing today. So we had no attempt at evaluating probability of detection of cracks to support plate.

MR. BALLINGER: I have got four PNNL reports, one of which does contain some stuff on POD, on a little test, a roundrobin looking test where they did it.

DR. MUSCARA: Yes. Yes.

MR. BALLINGER: And that was -- it was in a report related to the Surry generator.

DR. MUSCARA: Yes. Yes. It was done during that time period. This is what I was getting to. But as far as evaluating POD at the support plate, that wasn't done. Because what did we do? In that time area, we were, of course, beginning to experience cracks, stress corrosion cracks, let's say the modern day stress corrosion cracks, where we have families of small cracks with ligaments, not the planar stress corrosion cracks that we experienced early on in the life of these generators.

So we decided that we needed to do something to provide some data on stress corrosion cracking besides having the very thorough work done on the wastage and pitting. And so we did run a small roundrobin. Now, this roundrobin contained 17 samples. There was one sample at 85 percent throughwall stress corrosion crack, one sample was throughwall. Twelve of the 17 samples had cracks above 40 percent, but many were between 40 and 50, the rest were below 40 percent. So it was not a great deal of work.

We did include in the sample set, samples that had copper-coating, because we know that this complicates the inspection. And in four of the 17 we had support plate simulation with the specimen. Again, the intent wasn't to really evaluate POD at the support plate location, the idea was to get an idea of how teams at that time might be performing on stress corrosion cracks.

So the numbers that were given for POD at the support plate location, yes, they were somewhere between .27

and .5. A little bit about what kinds of teams looked at this, at this information. We clearly had sent a small mockup to a number of inspection agencies and they used their field teams. We also asked them to look with their most advanced techniques and their researchers, not necessarily just the fuel technicians.

We had looked at the bobbin coil on these samples. Pancake probes were beginning to get developed in those days and beginning to be getting used, so we also had pancake coil inspections. And there was one probe that was under development which never really got into the commercial arena, and this was called, in our report, we called it an alternate bobbin call. This was what was called a segmented coil. The idea here was to segment the coil so that you might get some information about the location of the flaw.

As you know, the bobbin coil, the encircling coil essential integrates around the circumference of the tube, so it gives you some information about the flaw, but not necessarily the location of the flaw.

As a matter of fact, we do have some probes here that we could send around. Thank you, Bill.

This is a typical bobbin coil, you see the encircling coil there. And this probe head has three coils on it, there is a plus point and two pancake coils. The two pancake coils are different sizes. The larger size gives you more sensitivities, a larger signal, but less resolution, so they have the combination of these two, depending on whether you are interested in resolution or sensitivity.

I guess I might mention, we talked about the bobbin coil, and, you know, everything is bad about the bobbin coil, it doesn't detect circumferential cracks. Well, there are some things that are good about the bobbin coil. Of course, one was already mentioned, it is the speed. But if you are looking at a small level signal, let's say, from an actual stress corrosion crack, the bobbin coil actually gives you a larger signal than these pancake probes, which these are small coils.

They don't have the resolution. Of course, if you are trying to evaluate the size of the flaw, if you are trying to map the shape and size of the flaw, you are better off with these pancake probes. But from a point of view of detection, especially for small signals, the bobbin probe is a better probe. So in a voltage based criterion, where we are looking at accepting flaws that are less than two volts, it makes sense to use a bobbin coil. You know, we do get more sensitivity to those kinds of flaws.

Again, before I get to the viewgraphs, there are several things we could say about POD and how people do these POD tests. Just a couple of points in general, we have had the experience of doing roundrobin inspections for many different components, not just steam generator tubes, but also piping invessel. So we have done a lot of work on evaluating the POD of different techniques, say, ultrasonics, eddy current for the different inspections. And we also have been involved in the work not only in the States, but international work.

And when you talk about the reliability of inspection of POD, it is made up of at least two components. We like to talk about reliability of inspection in terms of the NDE system capability, and the system is really made up of the equipment, the procedure and the personnel, and each taken together give us some idea of the reliability of the inspection process.

So a lot of the work that we do in evaluating POD, of course, is laboratory work, so, in a sense, you know, people know they are under test conditions, so you get more or less an upper bound of what you might expect for a field inspection. Very often we do not have enough samples in sets of samples. If you are trying to evaluate POD, a high POD at a high confidence level, you need hundreds of flaws, not what we do in qualification where we are looking at a handful of flaws.

We often use notches, which, again, are not realistic when one is trying to do POD data.

In our work that we have done, where the work has been robust, we are really trying to determine reliability of inspection, we find that you never get to 100 percent POD when you use the system, the person, the procedure, the equipment.

Now, we can break this down into capability. Depending on the physics, the equipment can have the capability to detect 100 of the flaws it is supposed to detect. But then we put this into the hands of an inspector and the human reliability comes into play, and we do not get 100 percent POD. Regardless of what some of the data shows us when you look at three, four, five, ten flaws, detect all of them, therefore POD is 100 percent. That just is not the case when you use hundreds of flaws and inspection teams outside of the laboratory, more on a situation like a Surry roundrobin.

I have seen information on POD, for example, where information is shown as POD going up to 100 percent at about the 50 percent level of degradation. But when you question how this information was obtained, the number of specimens is somewhere around 30 or 40. The mean size in the sample set was 27 percent throughout wall depth, the maximum depth was around 36. Yet they show us a curve of POD, you know, at 50 percent throughout flaw, POD is 100 percent and it stays 100 percent from thereon out.

Well, in questioning about how this was developed, we used the logistic curve fit. So, you know, the data set is down -- 27 percent is the mean depth of the flaw, one flaw at 36 percent, and with logistic fit, we wind up having 100 percent POD.

DR. POWERS: How do you use a POD when you are talking about a production process? Suppose I have got a run through a particular tube and that tube has -- in some way we know absolutely that the tube has five indications in it that are, let's say, 50 percent throughwall, okay. Now, and I have a probability of detection at 50 percent throughwall of, say, 80 percent with a 90 percent confidence level. And I ask what is the probability that my analyst will find all five of them?

Is it 80 percent times 80 percent -- 80 percent to the fifth power or something like that, or is it another number?

DR. MUSCARA: I guess, like Ken said before, I have to defer this to our statistician. But we have done the statistics and determined the number of flaws that you need to evaluate POD at different levels. And the way we have set up some our roundrobins are based on this. When we report our POD data, they have a confidence level that is statistically based.

I Ken was showing some of his viewgraphs, 13 out of 13 giving 100 percent POD, but then we apply the number of samples that were use. The 90 percent confidence on that was really 80 percent.

DR. POWERS: What I am driving at is -- I think it is just what you have been saying. If you give me a small set of samples to do, and a relaxed period of time to do it in, then essentially I am doing each indication alone, it is a separate experiment and I get a particularly probability.

But now when I am running this detector through the tube in one big operation, now the question is not individual cracks but what is the probability I will detect all five of the indications that are known to be there? And is each one of them an independent event, or do I get a set?

DR. MUSCARA: Yeah. In fact, in order for the statistics to work, it has to be an independent event. And when we evaluate our roundrobin data, we essentially divide up the test section into what we call grading sample, grading units. And there are certain requirements for the flaws. For example, in order for the numbers to be independent, the signal from one flaw cannot be interfering with the signal from the other flaw. So there are a number of rules that are set up to separate these so that when we do run the statistics, we get the correct answer, that each measurement is essentially independent.

DR. POWERS: But are they independent when I am in a production run? I guess that is the question.

MR. STROSNIDER: This is Jack Strosnider. Just if I could interject just for a second, Joe, I guess. The way this actually happens in the field is typically there is two reviewers, and they are working independently. And then depending upon whether they agree on their calls, it goes to a third reviewer for disposition.

And we have had a number of discussions with the industry, and I am not sure how consistently this is being done now, but one of our concerns, first of all, you have the question, you have got two analysts looking at the same signal. Can you that their probability of detecting the flaw is truly independent? Now, you can make that assumption, but, in fact, there may be noise or something in the signal that makes it difficult for both of them. There is probably some dependency there, but hard to quantify.

The other issue that has come up in some of the reviews we have done is after they go through their comparison, they give it to this third fellow who is usually a more senior level analyst, and he makes the decision. So it comes down to, you know, what he is deciding.

So, I don't know, and staff can fill me in, that there has been any real methodical study where we can tell you what the probability of missing an indication is even with a qualified method. There is some finite probability.

It is, you know, it is not a foolproof process, that is for sure.

And without going into a lot of detail, I would just say, when you recognize that, you have to recognize that inspection is just one layer in defense-in-depth that is applied to managing steam generator tube integrity. You have leakage rate limits. You have the fact that it is a design basis accident. You have your operating procedures, and so on.

So, we recognize that inspection is not, you know, not 100 percent reliable.

DR. POWERS: Well, I guess I am just worried about the theoretical issue of whether, in a production run, looking at just one tube, it is a set of independent indications or it is a collection. And how you do the -- how I divide the probability of detection, I think the way you are applying it is all independent isolated events.

MR. STROSNIDER: Yeah. Well, let me give you one other thing there just to add some perspective on this. It might require some more discussion later. But we talked about the voltage based criteria, and you heard some of the discussion about how the probability of detection, et cetera, is applied there. Ken mentioned one other plant that has an alternate repair criteria for primary water stress corrosion cracking at the tube support plates. And they -- and I guess maybe I am jumping a little bit ahead, because it is not just POD, but it is sizing. And those are the two alternate repair criteria, we pretty much rely on that.

Most of the operational assessments that are done, all right, there is not a whole lot of calculating and the kind of stuff that we have been talking about, it is basically, do the condition monitoring at the end of the cycle, which relies to a large extent on the in situ testing. And if you show that you meet the margins when you do that, the assumption pretty much is that your probability of detection and growth rate is such that things remaining the same, without any, you know, significant changes, you ought to be able to operate the same length of time again.

Like I said, there is always the possibility that new things show up in between. But I think people have the perception that, you know, that every review and every operational assessment that is done, that people are going in and using all these numbers and stuff, and that is not really the case.

But it does come up, it came up in the Indian Point review. It came up in the Arkansas review, it came up in Farley. We will talk a little bit about those tomorrow.

So I don't know if that is helpful, but that maybe provides a little perspective on how it is actually applied.

DR. POWERS: Well, certainly, it provided a perspective on the overall problem, the challenge that Joe has.

MR. STROSNIDER: And just to follow up on that, as we move more toward these risk-informed and start doing more risk-informed amendments, there will be more reliable on this. You know, part of what we are trying to get across, working with the industry to get, is reliable data that can be used in those type of analyses. And there is certainly room for improvement at this point.

DR. BONACA: I would like to ask one more question on this issue. When they ran the bobbin coil and they get these five signals in a tube, first, that will have to the one of deciding what kind of indications these may be. For example, they are not all going to be one type of defect. There is going to be different types of defects. So I imagine that they have some techniques by which they control these defects in different bins.

And I imagine that, for example, correlating one defect with the position of the plates, and so on and so forth, will help the selection, but, you know, it is not clear to me how this complicates the process. I think we had an overhead that showed that there was some consideration of the process as one complicating factor. But I imagine it is a complicating factor.

MR. STROSNIDER: I guess you are saying you are trying to understand exactly what the type of degradation is that you are dealing with.

DR. BONACA: Yeah. Because I mean all we have talked about in these past two days is one type of defect, and characterizing it, and we have seen a distribution of that versus voltage. But, really, this is the process by which they are identifying all the defects.

MR. STROSNIDER: You can get some information from the eddy current, but, as we said, you know, it is not -- it has limitations in its ability to characterize a defect. You also, you know, based on operating experience, and as you suggest, the location of the defect, draw some inferences from that.

When people find some new things, or some things that are unusual, two pulls are the way to get some solid information, but there is a challenge here.

One other thing I would add, too, which may not have come across in all the discussions we have had so far, is that, aside from these alternate repair criteria that we talked about, which are relatively few, the industry practice it to plug on detection. So when they find a defect, a stress corrosion crack in particular, they are going to plug that.

Now, thinning and that sort of thing where they have some qualified and reliable sizing method, that is not true, but for IGSCC, basically, plug on detection.

DR. MUSCARA: There are codes that the industry uses for characterizing flaws, but, generally, they are based on past experience. A lot of it is based on location. But, generally, you can discriminate between large volume defects and small volume defects. We can discriminate between cracks and wastage or pitting. And then within the crack regime, we can discriminate the circs from the axials.

So there is some capability even with the detection probes. And then, of course, if you are doing more careful work with some of these rotating probes, you have additional capability for characterizing the flaws.

DR. BONACA: Yeah. I just was wondering, for example, if the fact that you are going through and picking up a lot of signals could confuse this detection ability.

DR. MUSCARA: Right. With the detection, the problem is if the signals are close together, then it could confuse it and you could mess up the statistics. In the tests, of course, we do make sure that we have independent measurements.

In the field, if you are finding different kinds of flaws very close by, that confuses the issue. But, generally, I don't think that is the case. We find a cluster of flaws at the support plate, we know the type of flaw they are, they are in a certain zone. The next flaw you would find maybe at the next support plate. So they are not really -- one is not really affecting the other.

I guess just to finish up what I had started on the NUREG-2336, the loan numbers were developed when we were using this, you know, I mentioned there was an experimental probe, a segmented bobbin coil, so that is where the 27 percent came from. That has never been used in the field.

I guess also I should mention that in that test, we were using single frequency eddy current at the time. So when you look at the data from the normal bobbin coil and the pancake coils, the average POD was, as was stated earlier, was .63, I think we are using .6. I think the maximum we found in that small roundrobin was about .75.

But one of our main objectives, let me just mention it, the Surry work was I think very useful, we had very valid data. That was part of an international program. The Surry part of the work cost $17 million, and the flaw types of a different nature these days, we need to do similar kind of work, but we can't afford to spend another $17 million to get these POD curves.

So what we are trying to do is set up a mockup and an inspection process that mimics what goes on in the field. And so we are trying to set up this mockup so that it has the kinds of conditions one runs into in the field, so that the flaws are typical of what is in the field, and so that the inspection process is also conducted according to the qualified procedures and so on. And I will get into some of this work.

Unfortunately, I will not be presenting a lot of new results right now. We will have information by the end of this calendar year. We are in the midst of conducting the roundrobin. We are trying to keep this a blind test. There are some other teams that we need to bring on board, so we cannot release a lot of the information, but I can give you some trends.

We also mentioned earlier some advanced techniques. I wasn't planning on talking about the work we are doing on advanced techniques, but we are doing research both in characterizing the reliability of current inspection methods, most of this is with the mockup and the roundrobin testing, but we are also doing work on advanced eddy current techniques, in particular, data analysis procedures bring some of this in, and that we are also using this for characterizing the mockup.

And I have probably mentioned already what is in the first viewgraph. The purpose, again, is to evaluate the reliability of current day inspection, both with respect to probability of detection and sizing accuracy, and we will be using a mockup.

I guess while I am going through the viewgraphs, I also mentioned a couple of items. When we talk about qualified techniques, you know, that is very soothing. When we talk about something being qualified, we think it must be good. I think we need to pay particular attention to what we mean about qualified techniques with respect to what is being qualified for inservice inspections.

When we talk about qualified detection techniques, the technique can be qualified if it passes a particular test. There are a certain number of samples that are involved. Normally, there are not a great deal, a number of samples. But the passing criteria is that you need to get 80 percent of the flaws at 90 percent confidence level for flaws that are 60 percent deep and deeper. So if we are talking about a 40 percent plugging criterion, do I always know what the probability of detecting a 40 percent flaw is when the qualification is at 60?

In the sizing arena, when we started doing qualification on sizing, the criteria had been 25 percent root mean square error. We are no longer using that. So what is implied in a qualified sizing technique is that the process has gone through the system, a test has been conducted, but there is no passing criteria. But they do record how the person and the system performed. So a qualified technique would be something that gives you a sizing accuracy of plus or minus 50 percent. If it has gone through the system, it is qualified.

So we need to understand that the qualified doesn't necessarily mean it is very good, but at least we know how it performs. And then that information, of course, is used in the operational assessments, and that is the important point. But we shouldn't be left with the idea that a qualified sizing technique may be a very accurate technique.

Just a very brief description of the mockup, we have essentially 400 tube openings. Each tube is made up of nine test sections, so there are nine individual one foot sections. They may have a flaw, they may not have a flaw. But there is the option of having 3,600 test sections in this mockup. At the top of the mockup there is a three foot run out section, and that is there so that -- well, the probe doesn't fall out of the tubes when we do inspection, but more importantly than that, we want to make sure that when the probe hits the first sample, that the probe is up to speed, so it has a constant speed throughout the inspection.

I mentioned they were trying to make this mockup realistic. We have literally hundreds of flaws. Again, since it is a blind test, I don't want to mention how many hundreds, but it is several hundreds of flaws. The types of flaws we have are mostly stress corrosion cracks from the ID, axial, circumferential. We have some IGA. There are a few EDM notches and a few fatigue cracks.

We also try to reproduce conditions in this generator, so that besides the straight sections of tubes, we have tubes that are rolled into tube sheets. We have the same roll transition in these tubes as we have in operating plants. There are dents in the tubes. We have sludge piles, we have magnetite. So that we have tried to reproduce the conditions that are important that affect an inservice inspection signal.

MR. BALLINGER: No U-bends?

DR. MUSCARA: No U-bends, correct.

DR. CATTON: 22.2 millimeter diameter is 7/8ths of an inch?

DR. MUSCARA: That is three-quarters, right next to it. So, yes, the other items that we do have an actual carbon steel support plate, we have three simulations of these in this mockup. So that is the same size as a support plate out in the field.

As I mentioned, we are trying to mimic the inspection process that goes on out in the field, and we know a lot about that inspection process. Our researchers at Argonne know something about that. But we really wanted to make sure we were doing this right, so we put together an

NDE task group. And the idea here was that we wanted to have some input that actually do these inspections, people that develop the inspection plans.

So we put together this task group and the members were from Argonne, from NRC, from EPRI, from FDI, which was the old Babcock & Wilcox, ABBCE, from Zetech, which is a major inspection company, provides inspection services and also equipment, Westinghouse, Northern States Power, Commonwealth Edison and Duke Power. We met a number of times to discuss this test. But the main input we had from the members was that we wanted to know if the signals that we had from the cracks in this mockup are typical of what they see out in the field, because we wanted to make sure that the cracks are prototypic both from the point of view of the morphology, from a metallurgical point of view, and also from the eddy current signal point of view.

And so we have compared these cracks to signals that you get out in the field, and they are typical. We also compared them to the metallography of stress corrosion cracks that we get from pulled tubes and they are quite typical. Of course, stress corrosion cracks, if you have seen one, you have seen they are all, but there are minor variations, and we cover the range of the stress corrosion cracks that you do notice in the field.

Now, we do have in the mockup cracks that come in clusters. We have single cracks, but many of them are today's type of crack where we have small cracks with ligaments in between, and these are distributed around the circumference in one more than crack and also axially along the tube.

DR. POWERS: How did you make these cracks?

DR. MUSCARA: Well, these cracks were made in the laboratory. When we started out, we were using autoclaves, high temperature caustic solution. That got to be too time-consuming and too expensive, so we have been working for a number of years, one or two years, to just develop methods for coming up with these cracks. We essentially heat treat the tube so it is sensitive to cracking and we conduct the cracking at room temperature in one more solution of sodium tetrathionate.

DR. POWERS: Tetrathionate.

DR. MUSCARA: And we can get cracking with this in time periods of the order of a day to three or four days.

Now, all these cracks are very well characterized. I mean we know where the flaws are because we are introducing them. But they undergo a battery of tests where we used advanced NDE techniques, whether it is ultrasonics, mostly eddy current, die penetrant. We do a lot of work to characterize these cracks before we accept them for the mockup.

Again, we want to make sure that they are realistic and sometimes we aim at certain kinds of cracks. We can produce fairly closely what we need, but it is a random process. So, you know, sometimes we reject some of the cracks, they may be too wide open for us.

Well, in addition to assuring that the cracks are typical and the conditions are typical, we also wanted to make sure that the roundrobin is conducted in a manner similar to inspection conducted in the field. So we effectively treated the generator -- in the field, of course, there is an owner of the generator, and the owner is responsible for what goes on with this generator. The owner is responsible for coming up with the inspection program. So we assigned Argonne National Laboratory the ownership of the generator, so they act as the owner, and they are responsible for developing the inspection program.

Now, when developing the inspection program, a number of things are taken into account. For example, the owner is responsible for doing a defect analysis, and so they are required to sit down, determine for their plant the kinds of degradation they have experienced in the past, and determine for sister plants what kind of degradation they are experiencing. And then they are required to make sure that the techniques used for inspection match the kinds of degradation that they are experiencing.

So, they are supposed to be using qualified people, but, in addition, the personnel needs to be qualified at the plant site. So they need to take a plant-specific examination, both a written examination and an examination, an actual examination of inspecting data from their plant from the past where they know what the situation of the flaws are.

So a lot of information was gathered, a lot of documentation was written, very similar to what is conducted in the field. And our task group was very helpful in providing us with a lot of this information. So we had a lot of information, for example, on inspection plans that are used at actual plants, and so we mimicked our process along that information for the mockup.

For an actual inspection, there is usually at the utility is a Level 3 inspector who is responsible for approving the inspection program. And, similarly, for this mockup, for our roundrobin, we had the task group had the responsibility to review all the information and approve it, that the techniques we are using match the requirements and that they are appropriate for the kinds of degradation that we have in the mockup.

So, what I really wanted to stress is that when you do see our information, come December or January, that you do have the feeling about how the work was done. This is not just a laboratory test. You know, clearly, I can mention laboratory tests and roundrobin that we have done in the past, some international work where they use all notches, 100 specimens, 95 which were notches, five were cracks, and they tried to develop UOD curves from that.

This work is laboratory, but it is a mockup and it is trying to reproduce the conditions of the field, and inspections are conducted in a manner similar to what is being conducted in the field.

After assembling the mockup and doing a lot of work on our own to characterize the nature of these flaws, we started the actual roundrobin in February of this year. We are dealing here really with an analysis roundrobin. In the past, if you know the work from Surry, we have done both what we call data acquisition and analysis roundrobin, and we also conducted analyses roundrobin.

What we found is that the data acquisition, regardless of who performs the data acquisition, you get the same result. It is the same procedures used, the same equipment, and the flaw is the same flaw, so that we found very little variability in having many teams gathering the data, the data was the same. So that we decided here that we needed a run, an actually roundrobin, this is where the variability in POD comes from, not from gathering of the data.

So we had a qualified team from Zetech gather the data. There was some oversight of the team, there was a proctor present making sure that the data was gathered according to the procedure. But then this data was given to a number of independent commercial teams to do the analysis, provide us the information about what they have detected, and we also required information on sizing.

At this point I believe we have five teams that have done the analysis roundrobin. We have incorporated most of the major inspection agencies that conduct inspections in this country. One clear exception at this point is Westinghouse has not been able to participate yet.

I should mention that the program I am talking about is an international program. The participants in this program are Westinghouse, EPRI, Canada -- there is one more, Korea.

So Westinghouse is very willing and has several times scheduled to do the analysis roundrobin. Unfortunately, they have been pulled away on other activities. The last one they were scheduled to do the analysis roundrobin for us in May and, unfortunately for us, they got pulled away with Indian Point 2. They are now scheduled I believe for November, and, hopefully, in November they will do the roundrobin, and we can include them with the set of information we already have.

Let me describe a little bit the teams. In effect, as I mentioned before, we conduct it the same way as the inspections are conducted in the field. So the inspectors have been tested, they are qualified inspectors. They have been qualified through the EPRI NDE Center. We use a five person analysis team, and this is what is going on today.

I also must mention that this technology is evolving and it is improving fairly rapidly. Jack mentioned we have two inspectors. In fact, when we planned the roundrobin, there were three inspectors involved in the team, now there are five. The description of the five member team is that there are two analysts, we call them primary and secondary, but, again, you know, they do the same function. A secondary team doesn't mean it is less qualified or the result is less important.

There are two independent teams that do the analysis. In the past, if these two teams did not agree, then a resolution analyst, who is a Level 3 rather than a Level 2, will do the first -- the primary and secondary can be Level 3. They are normally Level 2 and Level 3, but the resolution analyst is usually a Level 3, and he decides what the true call is if the primary and secondary can't agree. That was the past.

Now, we are using five teams. So we have two initial inspectors doing the analysis, primary and secondary. There are two resolution analysts, and they have to come to a consensus on the call. And there is a fifth member of the team which is called the independent QDA or the independent qualified data analyst. And this fifth member is usually a member of the utility rather than the inspection agency, not always, but usually.

So the makeup of our analysis team is made up of these five inspectors. The true independent who looked at the data, if there is something to be resolved, the resolution analysts look at the data. And then finally, the independent QDA has an opportunity to look at the data and provide a final answer.

We have mentioned there are not too many sizing techniques that are qualified, there may be one or two. But we are requiring these teams to provide us with sizing information, at least to give us the maximum size of the degradation, and that is a sizing technique that is based on the face angle of the indication. If you have to do sizing, this is a typical method for sizing flaws. And it is very similar, what we are requiring for the max size is very similar to one of the qualified techniques for sizing.

I must mention also that, in addition, we plan on getting a subset of this data to a number of commercial teams to provide us with sizing information, not just the maximum size, but we will ask them to do the entire mapping of the flaw. This is not something that is required or is qualified, but we are trying to get some idea about the sizing accuracy also in this work.

In order to be able to grade or to evaluate a roundrobin, we must know what the true state of the mockup is, so we must know what the actual size, and type, and location of the flaw is. Well, as far as location, we know that. The most difficult part is to try and determine what size these flaws are.

We want to be able to use this mockup in the future for evaluating emerging technology. It is very time-consuming and expensive to produce these tubes that have realistic flaws. I guess, just to mention the mockup itself, putting it together, making some of the flaws cost over a million dollars. So these tubes are very valuable and we do not want to destroy them all. So we are trying to find some techniques for getting a true state of these flaws so that we can then use in evaluating the performance of the analysts.

And I will not spend a lot of time on this, but we tried many techniques, including ultrasonics and high frequency ultrasonics, land waves, all kinds of eddy current techniques. Just to summarize, none of those worked that well for all kinds of flaws.

We are concentrating now on one technique, which is part of the research work we are doing not to evaluate the reliability, but research that we are doing on advancing data analysis techniques. And so right now we are benchmarking a technique that was developed at Argonne National Laboratory. It uses multi-frequency eddy current, but in addition to the multi-frequency eddy current, we do filtering, we do deconvolution. And we have developed a rule based smart system, that is, we are incorporated into this algorithm the kinds of things that the good inspectors do when they do an analysis and try to decide whether it is a flaw or not a flaw. So all of these rules have been incorporated into this system. So one major aspect is the multi-frequency correlations to flaw sizing, and that is incorporated in this.

And what I wanted to show you next, just very briefly, is some of the capabilities for sizing. We are validating the technique by destroying -- by inspecting this set of tubes, having the inspector provide us with the mapping of the flaw, and then we are destructively evaluating these samples to determine how well this technique is working.

It is just an example of the result we get from the technique. You have seen the kind of graph that you see on the left before. A key aspect of this is that it has no resolution. One of the key aspects of our rule based automation calibration and deconvolution that we are doing is to improve the signal to noise ratio. And the key parameter in being able to detect and also size flaws is to have a clean signal. So if we can reduce -- if we can increase the signal to noise, that helps both the detection and sizing. And one major aspect of this work is that we are really reducing the signal to noise -- increasing the signal to noise, reducing the noise a great deal.

So that shows the kind of information we get, and on the right, we just show that you can section this information, looking at the flaw either from a circumferential, from an axial, or from a longitudinal view, and you can get the profile of the flaw. And we are evaluating how accurately we are doing this by destroying samples.

Out of the 29 samples, we have finished that work. All of those have been destroyed and compared to the eddy current result. This is just a set of three samples that we have looked at and compare the eddy current profile versus the actual metallographic profile. I could spend some time on how that is done, but if you have specific questions, we can try and answer them, but in the interest of time, I will move on. I just want to say we are doing a careful job of metallographically evaluating the flaw.

DR. KRESS: How do you cut the tube?

DR. MUSCARA: Well, we don't cut the tube. The best way we have found, that gives us very good information that is more effective from a cost and time point of view, is to pressurize the tube a small amount to open up the flaw. We then look inside the flaw face, the tube is heat tinted, so we know what the prior flaw is from the heat tinting, and, also, it is an intergranular crack which is different from any quote we might have had from the pressure.

We take a digital picture, the picture is digitized and we do digital analysis. And looking at the light areas and the dark areas, we map out the flaw. It is a very difficult and time-consuming process because the flaws we have in some of these tubes are literally dozens to hundreds of flaws, small flaws with ligaments. And we are trying to evaluate those very carefully both with the metallography and with the eddy current. And to my surprise at least, we are doing a lot better than I thought we could do with the eddy current technique. We are really getting much resolution from this technique. And, as I say, we are checking it out against samples, so know that it is real.

Well, this shows a comparison of three samples, the eddy current by using this technique versus the actual destructive examination. And you can see it is quite close.

In some cases, for example, the eddy current here does not pick up the full length of the flaw, and that is fairly typical. The probability of detecting shallow flaws is small. So in these matters, if we turn to a length sizing, we often undersize because we do not pick up the part of the flaw that is shallow. That is a fact.

The interesting thing is that when you try to evaluate these flaws and calculate a burst pressure, you don't really need to know the shallow part of the flaw because that does not contribute to the failure pressure of these complex flaws. In general, what we found by running a number of tests, and Bill will talk about some of this tomorrow, but the portion of the flaw that is less than 70 percent throughwall usually does not participate in determining the failure pressure.

So when we look at these flaws and we go through a process of characterizing the flaw where we look at the equivalent area or an equivalent flaw, length and depth, because these are not rectangular flaws, and then use that in our integrity correlations, we can estimate the burst pressure very well by using these profiles, even though we might miss the shallow part of the flaw from the NDE.

DR. KRESS: When you do the destructive test, is there any chance that you change the flaw characteristics by doing that? It looks like you --

DR. MUSCARA: Well, of course, there is always the chance, but, again, we are careful. We do not open these up a great deal, we just want to open them enough so that we can look into the face of the flaw.

DR. KRESS: Yeah, and you can tell where you might have changed it.

DR. MUSCARA: Right. And if we do change it, then we know that that is different from the heat tinted area, number one, from the intergranular nature of the crack.

DR. KRESS: And you can see what has changed.

DR. MUSCARA: So we can see. We do look at these things on the scanning microscope, so that --

DR. KRESS: Oh, you look at them on a scanning microscope.

DR. MUSCARA: Oh, yeah. Yeah, when we need to. I mean some cases we don't need to. But in many cases we do look at the surfaces on the scanning microscope before we decide what the profile is.

Well, again, as I said, I wasn't going to -- I really wanted to come in and show you some example PODs, and we are not doing that, partially because we haven't fully characterized the generator. We are still in the process of evaluating the data. We are shaking down a statistical package we have for conducting these analyses, a number of reasons. And, also, you know, it is a blind test and I really didn't want to have in the public some of these POD curves.

But I can tell you, qualitatively, we remember the Surry kind of information. We are not too far from that. If you remember at Surry, we had some fairly high PODs for large flaws, but never went to 100 percent. Real teams miss flaws even though they are big. All they have to do is blink while they are looking through the record. So that in reality, some teams miss flaws. And we know in this case also, some teams missed once in a while a large flaw. So the POD does not go up to 100 percent, nor is it 60 percent for the large flaws. So we are doing better than 60 percent.

I say in general I think we are doing quite well, but you will see all this information in several months in a much more quantitative way.

DR. KRESS: Is your objective to get a POD versus flaw size?

DR. MUSCARA: POD versus flaw size as a function of the technique that was used, which is qualified, so it is POD as a function of the flaw type, the technique, the location in the generator. We are going beyond that. I mean in the past we looked at POD as a function of the maximum depth. Well, maximum depth is not a really good parameter for determining burst strength of these tubes when we have a complex flaw. It is if it is a simple rectangular flaw, but for these complex flaws it is not a good parameter. So we will be looking at POD as a function of other things.

One of the items that works very well, that we find worked well for predicting burst pressure is this M sub P. M sub P is a correlation factor, it is almost a stress magnification factor that describes the stress at the ligament of the flaw that is used for predicting burst pressure of different types and sizes of flaws. So it takes into account the geometry of the flaw. And Bill will cover a lot of this development tomorrow.

But one of the important parameters here for describing the severity of the flaw is M sub P. So one of the things that makes a lot of sense to us is to try plot POD as a function of M sub P. And it is not just the research work, I mean even in the field, we are moving towards, we are using this evaluation, we are using M sub P for predicting burst pressures.

Besides the voltage criteria, and you have heard there are a few other criteria out there, one of the most recent ones is a criterion where you are actually using length and depth of the flaw to predict its burst pressure. And it is not just length and depth, in fact, it is what I showed you before, it is the profile and how to calculate the burst pressure of those tubes. Well, you need to have a severity factor which is this M sub P.

So, you know, the laboratory work, yes, is leading some of this, but it is winding up the field, and we are doing those kinds of analysis. And, in fact, we have an ultimate plugging criterion at the support plate and dented region where we use this kind of an evaluation for calculating a burst pressure and making sure that it meets the 3 delta P.

And so with that, you come up with something other than 40 percent, depending on the reliability of sizing, the crack growth rate and the strength of these tubes.

So one of the things that makes a lot of sense for us is to evaluate POD as a function of M sub P. And since we are using voltage and we get that free, the voltage always comes with the signal, we will be plotting, I am sure, POD as a function of voltage for these different kinds of cracks.

And then again, for the first time, we will have a comprehensive data set where we know what POD as a function of voltage is. When we looked at this POD of .6, it was as a function of a handful of flaws of varying sizes. And normally POD is a function of size, it is not POD is a function of voltage. But we will have that information once we are done with these analyses.

I am not sure if I should go through this. I mean you can read it as well as I can. But we find is that the POD for the larger flaws, or for the large segments of the flaw can be fairly high, above 80 percent. Again, it is not 100 percent. We have missed sometimes large flaws, but it is more than .6. And so there is -- we realize that .6 is conservative, and in a voltage based criterion, .6 covers, as we mentioned earlier, a number of things, not just the POD but the crack's initiator in cycle, for example.

So the POD will have detailed data, can get fairly high. On the other hand, it is very low for flaws that are smaller than 40-50 percent, and that is not a surprise, I think we expect this.

I think you can read the rest at your leisure.

DR. POWERS: Everything else is pretty much as expected.

DR. MUSCARA: Right. Just very briefly, we have been talking about sizing and the difficulty sizing. Sizing is usually based on a calibration, so you have a set of standards with different depths of holes or notches, and you look at the face angle for each one of these notches, and you have a calibration curve for sizing.

We also have indicated -- maybe we haven't, but sizing ID flaws is more troublesome than sizing OD flaws, and this graph shows you the reason why that is. If you are looking at the ID flaws, that is the portion of the curve in red. You can got from 100 percent -- from zero ID flaw size to 100 percent through the wall size, and you are just using up 30 degrees of face shift. So within 30 degrees, we have the full span from nothing to throughwall. And when the signals are complex and complicated by noise, and it is difficult to pick out where one should measure the face angle, then you get into a problem with getting good, accurate sizing.

For OD cracks, they normally can be sized a little bit more accurately. There is a larger span than covers from 0 percent depth to 100 percent. But, at any rate, so sizing normally is conducted with a calibration curve. We know whether it is ID or OD based on which, what quadrant the signal fall from, from 0 to 30 percent, it is ID. From 30 percent on up, it is an OD.

Well, this is similar to the second to last viewgraph. Stress corrosion cracking depths less than 50 throughwall, we find that is not reliable, and it is not unexpected. Smaller flaws give small signals and they are complicated by other conditions, and it is difficult to select the proper face angles. But what you find in general is that these flaws are overestimated, but we see they are unreliable because that is not always the case, sometimes they are underestimated.

Well, and the orientation we have found is quite difficult. Circumferential cracks at the top of the tube sheet, when they are small cracks, they are really difficult for the teams to get a good sizing on.

This is all that I had. I had prepared to, again, give you a view of the work that is in progress and the kind of data we were looking forward to getting. It will be quite useful in evaluating submittals that come in and getting a feeling for what the real probability of detection of these flaws is.

MR. STROSNIDER: This is Jack Strosnider. I don't know if you had any additional questions, but thanks, Joe.

This is some really useful work which I think is going to help NRR in terms of our review of licensing amendments and activities that come in. And I think the industry, and I mentioned earlier this sort of simplified approach to operational assessments, but I don't want to give the wrong impression, I think licensees may actually be out there doing these calculations and this is going to help them do their work. I was talking about the sort of sanity check that we give those evaluations.

In terms of schedule, we are almost finished with Item 10. Actually, under Item 10-G, Number 1, it talks about laboratory studies and why these are applicable in light of vibrations induced by blowdown, et cetera. We interpreted that as wanting to hear some more about the issue that Mr. Spence discussed yesterday with regard to blowdown effects.

And Jack Rosenthal from the Office of Research is here. You know, this issue has been -- Research has been asked to take a look at it in terms of the GSI process, and so to address that issue, we are going to ask Jack to give you a little status on where that is at. And I don't know how much he has got, but when we finish that, that will conclude Item 10, and then we can decide how to go forward I guess with the rest of the agenda.

DR. POWERS: Well, I will tell you what the decision there is. We will take a little break after Jack and then we will trudge right ahead.

MR. STROSNIDER: Okay.

MR. ROSENTHAL: Really, my comments are programmatic and short, so I will go fast. Okay. My name is Jack Rosenthal, I am the Branch Chief of the Regulatory Effectiveness Assessment and Human Factor Branch in the Office of Research, and one of the teams in my branch is responsible for working generic issues.

Yesterday that was some discussion that we have been slow about working some generic issues, and that is true. But since 1981, we have approached 632 issues, prioritized them, et cetera, 283 of them actually were worked as generic issues with some sort of technical approach to them.

At least of recent, I think we are doing much better at working the issues. So from 600 issues, of course, the tough ones that take years, we resolved five in '99, six in Fiscal 2000. There is seven on the books right now. Our real viable process has new issues coming in and old ones getting resolved, but the big backlog of prior years is no longer.

The ACRS has been kind to us, and you will see we have, with some regularity, been coming forward to you with the issues as we resolve each technically. And you are familiar with these because we have discussed these with you recently.

Before us now is -- that is the list of current issues, and monthly we tell Pete Domenici where we stand on resolving issues. There is some thrust to get them resolved. When this slide was made up, we didn't have GI-188, which is the most recent one.

I was going to talk about 163, but looking through the material, Jack Strosnider did a good timeline review at the beginning of the day, so I won't do it again. You will find all the information on the NRC web, and there is a commitment there that following this panel's deliberations, we will figure out what to do in terms of a program plan for resolution of the multiple tube rupture issue. And in that document sitting on the web page, it says, within a month of you finishing your work, we will come up with a plan to finish ours.

The last slide is 188, it is resonance vibrations of steam generators tubes in a main steamline break event. That is just a title that has been given to it. It has been entered into our system, and we are starting to work the issue. And I just -- as I understand, and this is what needs to be worked out, the postulate is that, and it is not surprising, if you have a fluid system and you suddenly open that system, or you suddenly close that system, yes, one would have pressure pulses in the system which would induce mechanical motion in the system, et cetera. That is really not a surprise.

At least a preliminary look, would the vibrations be -- or the number of fatigue cycles that you put on it be bounded by the current design of the steam generators? Well, it is not so obvious because just the amplitude might be different. It is something where we can't dismiss it out of hand. It does appear to warrant some technical work.

We are following the pilot application management directive 6.4, which we brought before the ACRS, and the ACRS was very kind to us in retrospect when you said, look, why don't you try it out for a year before you adopt it.

And as is proven, we have had some lessons learned from that, so thank you.

In that process, --

DR. POWERS: We will help you, Jack. Come back to us again and we will still harass you.

MR. ROSENTHAL: In that process, the big change is the management directive, is to say that for issues that we current worked, resolve really means resolved as somebody in the street would understand the term "resolved."

DR. POWERS: Best move you can possibly make.

MR. ROSENTHAL: Okay. And that is that you bring it through, figure out what you want to do and actually do it and figure out, okay. And some of the activities upfront are handled by RES, and then some of the issues, I mean NRR has responsibility for writing rules, doing inspections, et cetera, so it is a joint effort. But what we have said is in terms of the public, that resolved ought to be mean resolved all the way through verification.

We are in the identification stage of this issue. Initial screening. We have a panel of experts, Milos Chochki is the panel chairman on that, and the next meeting is scheduled for 10/18.

What we do is, based on what we have heard here, and the information that is brought forward, we are going to try to write down what we think is the issue, get agreement on the issue. And then -- not so simple. Because we want to know upfront whether we are going to include things like motion of the lowest support plate or not. Are we only talking about the tubes? Are we talking about other mechanical aspects of the steam generator? And just what is the issue?

And what we have learned from other go-rounds is that defining the issue is quintessential. Then within the sense of the Generic Issue process, you decide if it is a compliance issue, it was already covered by the regulations. Is it an adequate safety issue? Typically associated with what you think of as this 3 to the minus 3 delta CDF type issues, which I don't perceive this to be. Or is it a safety enhancement issue?

Following that, we would then develop a program plan for how we would attack the issue, and then everything the NRC does goes through a PBM process and we would get resources to work the issues. That is where we stand.

DR. POWERS: I mean that is great, and I am glad to see that the process is being exercised, and we will be anxious to hear how it comes out. But we are left with a problem now. We have a contention that says, gee, when you set up this Generic Letter 95-05, you guys didn't take into account the fact that you are going to get these violent pressure pulses and vibrations in here that could lead to a couple of things, growth of cracks that otherwise wouldn't have grown, and enhanced leakage, and unplugging of cracks that have been plugged by corrosion products, okay, and that would give you enhanced leakage. So your leakage estimates that you had in mind when you set up Generic Letter 95-05 just don't take into account this physical phenomena.

And the question is, what is the response to that? Now, what we heard from Ken is he says the cracks are very tight, and there isn't much in those things, so the unplugging cracks may be not such a major issue as it is other contexts. But the growth of cracks due to the violent vibrations is still, I think, an open issue here.

MR. STROSNIDER: You are looking for a response.

DR. POWERS: Yes.

MR. STROSNIDER: This is Jack Strosnider. I guess the answer to that is, number one, I think, yeah, we do need to do some work to understand what this phenomena is. I don't think there is anybody here right now that say how significant it is or isn't, you know, what it is going to do, and it is just going to require some technical work to go figure it out.

You know, we have emerging issues in regulatory space all the time, all right, and that is why these processes are set up to deal with them.

The other point I would make is that, based on what I heard yesterday, and some of the concerns that have been expressed, it is not clear to me that this is just a Generic Letter 95-05 issue. You know, some of the suggestions with regard to the significance of this transient, you know, if some of what we heard is, in fact, what we find out when we go look at the technical aspect of this, it is broader that voltage based repair criteria, it has some much more fundamental issues.

DR. POWERS: That's fine. But right now I want to work on what it has to do with alternate repair criteria.

MR. STROSNIDER: Yeah, and I think, you know, my response is, like I say, we have emerging issues that come all the time in regulatory space. We have a process for dealing with them. When we talk about going out and changing the licensing basis for plants, et cetera, we need to do that, you know, in a methodological way, and that is what the process is there for.

MR. ROSENTHAL: Can I make a comment? Let me just make one more comment and then I will give you the mike. And that is that, depending on what goes on with this panel, okay, we have options to incorporate the resonance issue in with 163 into a major -- into one big issue. We could parse it out amongst its pieces. And we just haven't made a decision pending hearing out the results of this work, plus the panel meeting to discuss in greater depth that technical work. And then we just have to put together.

But we are dismissing the issue. And as we look at it, we see that there is interesting technical aspects.

DR. HOPENFELD: Let me relate to you my 40 years of experience in Research. You don't look, you don't find. Ten years ago, nine years ago, the broad spectrum of problems were really identified. We didn't go into the detail in that GSI-163 in the DPO, but NRR chose not to look at it, chose to set it aside.

And now you tell me -- I am positive that somebody, that if that work had started then, all these problems would have been identified. So I am kind of a little bit frustrated in you telling me that this is a new thing that you are discovering today. That is water over the bridge. The point is that with this kind of attitude, I think we should start this new vibration program.

But if you are going to proceed in the same way that we have done before, there will be other things here, because it is a very complex problem. You ask yourself --

DR. POWERS: That deals with issues of management and whatnot that are out of our spectrum. I think we are interested in the technical issues here, and how it impacts.

MR. STROSNIDER: I would provide two additional comments, too. I mean this is -- obviously, you as the special subcommittee have been tasked with dealing with the issue, and so this is just my perspective, okay, and take it for what it is worth. I think, you know, there is a question, and Dr. Hopenfeld just pointed to it, you know, is this issue, was it part of the original DPO or not? And you can take a look at that, you know, it is open to the discussion probably or debate.

But the more important thing is, and I tried to talk about this this morning in terms of what it takes to resolve a DPO, or any other, you know, emerging issue and how we deal with them, okay, saying that, you know, that in an ideal situation you come up with "the" technical answer. You know, we would all like to have a lot more information on what transpired after the event down there as described yesterday and, you know, all sorts of analyses, and we could look at them today and say this is the answer. You know, we don't have that.

The resolution to many DPOs, if you go back and look at it is to say, we are going to go. You know, we acknowledge that it is an appropriate issue for further study and that is what we are going to do.

So that is just my perspective on it. You know, you as a committee have to decide how you want to deal with that.

MR. HOLAHAN: Let me just add something. This is Gary Holahan. When an issue is referred to the Generic

Issue Program, in effect, you have made a judgment already that you don't need to take immediate regulatory action. I know, you know, Jack referred to the judgment about this as concern associated with not a very high probability event, and I think that is part of the judgment.

And I think, Jack didn't mention it, but part of his panel's responsibility as they get into the issue is, in fact, to identify for themselves whether this is an immediate safety problem which could be kicked back into the regulatory process for a Bulletin or a Generic Letter, or calling in an Owners Group or dealing with on a more immediate basis.

By its very nature, these things are judgmental, because you haven't done the research work and you haven't put all the information together, okay. But there is, within the process, a judgment being made about this is an issue that should be worked, and it is reasonable to take some time to do it. And we have these sort of issues, you know, every once in a while.

I think back to when we had problems with, you know, fire barriers, and we talked to people, and we tried to figure out whether that was a concern or not, and we dealt with it a while. And then we observed the test, and in the test, there was an immediate and obvious failure, and two days later we wrote a Bulletin that told the industry they had to do something in the meantime.

So when an issue moves from a concern, you know, to a clearly known problem, we can deal with that. I think this issue is at the concern stage. We realize that, you know, I mean we have 2,000 years of operating experience and we, you know, haven't had any main steamline breaks, you know. So this issue is something that needs attention but doesn't need attention today or this week, and can go through a deliberate process. But as part of that process, people have a responsibility to say this looks like more and more like it will be resolved and it is not a problem, or it looks like the evidence is building up that it is a real problem. And the process has to deal with that.

DR. POWERS: I think I would have liked to seen what the thinking about it was. Even if the outcome was exactly what was described, are you going to put it into the Generic Issue process?

Why don't we go ahead and take a 15 minute break. And then we will come back, and I guess we are doing damage propagation at that point, is that correct?

SPEAKER: Yeah, that is Item 11 on the agenda, that's right.

[Recess.]

DR. POWERS: Let's come back into session. I think at this time we are going to turn to the issue of damage propagation, in particular, the subject of jet cutting. Okay. And I have Joe and Steve listed down here. I usually ask Steve why he is not working on the human performance program plan, but I won't ask him this time.

So, whomsoever is leading off, please lead off.

DR. MUSCARA: In the interest of time, I could mention, as you said yesterday, you can all read. We just need the viewgraphs. We would rather answer questions.

DR. POWERS: Well, to tell you the truth, this one involves CFD calculations and whatnot, and I don't read CFD to be honest with you.

DR. MUSCARA: I don't either, that is why we have Steve here.

Okay. So I guess we are going to be talking about the agenda items 11 and 14, damage propagation actually. Item 14 will be done tomorrow morning. To do this section of the agenda, we have Steve Arndt, Steve Long and Bill Shack will be contributing parts of the presentation.

Quickly, I will talk a little bit about our jet impingement work that we have planned or are in the midst of. Jet velocities --

SPEAKER: Is your mike turned on?

DR. MUSCARA: Thank you. Jet velocity and particle motion, Steve will cover, Steve Arndt. The quarter-inch -- the basis for the quarter-inch crack, Steve

Long will talk about that. He is here. Good. And as I mentioned, Bill Shack will present work on different models for predicting behavior of cracked tubes under different conditions.

Well, the issue of the jet cutting was brought up in NUREG-1570. I think at this point the staff really had some concern, a lot of it based on some samples we had seen. One of our staff members had done some work at a fossil plant, and he was doing a failure analysis of some tubes that had seen some jet cutting in a fossil plant, and very impressive tubes. In fact, they did cut through -- these tubes, I believe they are stainless steel, they are about .44 inches think, and a jet from this fossil plant did cut through a number of tubes. So there was some concern there.

Well, based on this experience, the staff looked for some data that they could try to relate to the behavior of steam generator tubes under severe accident conditions, found some data on coal gasification and used this data to come up with some estimates. In fact, if you look at the NUREG, some fairly high ablation rates were estimated with that work, where the ablation or erosion is due to mechanical processes or corrosion processes, or a combination of these two.

In particle droplet impingement, generally, we are looking at a mechanism that is driven by mechanical processes, either by jet cutting or by fatigue of the surface layers.

For particulates in a corrosive atmosphere, the removal mechanism can be either by the mechanical methods or by corrosion. At low velocities, normally it is driven by the corrosion. Intermediate velocity is a combination of the two. And at high velocities, even in a corrosive atmosphere, the ablation is driven by the mechanical processes.

So we were interested in looking at this issue again to try and relate it more closely to the conditions that we have during severe accidents. I guess I should mention, we will address both erosion under severe accident conditions and under steam line break conditions, but we need to separate those two. Certainly, under severe accident conditions, we are dealing with high temperature superheated steam. A compressible fluid in the steamline break, we are dealing with water droplets and possibly steam.

So we are going to separate those two. We are planning some work on the severe accident conditions. That work is underway. We are planning work also under the steam line break conditions, that is just in the planning stages.

So to address this a bit further, we decided to do a number of things. One was to do a literature search and the second step was to bring together a group of experts to talk about the issues that might be involved, in particular with respect to the severe accident conditions and the ablation expected under those conditions, and also to talk about, say, the leak rates or, in particular, the creep crack opening of the steam generator tubes under the creep conditions we might experience in the severe accidents.

So we held a specialists meeting at Argonne National Lab on November 19th, '99. We do put minutes together and those are available to the public. They were sent to the Public Document Room on December 10th. At the meeting, it was open to the public, but we particularly invited a selected number of experts. Among those in the erosion area, we had Ian Wright from Oak Ridge National Lab and John Stringer from EPRI. I guess I should also mention that the data that was used in the NUREG from the coal gasification work was data developed by Ian Wright, among some others, but that was a major part of the data that was used.

In the severe accident area we had Jason Schaperow from NRC and Mati Merilo from EPRI. In the high temperature fracture mechanics we had Professor Saxenna from Georgia Tech, and then the various other from NRC and ANL, including staff from Combustion and B&W, or ABB and FDI.

When we discussed these issues, certainly a number of things came up as being important in this area, and a number of clarifications were provided by the experts. Both Stringer and Wright felt very strongly that the fossil experience with the superheated tube could not be used or extrapolated to the steam generator case.

In particular, the fireside atmosphere in a fossil plant contains a heavy load of ash particles, sand and the large particle sizes. And what happened in the cutting there is that the jet entrains these very abrasive particles, their large size, and they cause the cutting. So, you know, we don't think this kind of particle is really present in the generator.

DR. POWERS: Numerous speakers have spoken of sludge piles and whatnot.

DR. MUSCARA: Yes.

DR. POWERS: The oxide itself is spinel. It seems to me that there are some fairly hard particles in there.

DR. MUSCARA: Yeah, we actually discussed this with the experts, you know, the possibility of the jet picking up particles as it exits the tube. Well, there are several locations in the generator where this could be possible. One place in particular where you have sludge probably would be the top of the tube sheet. Another place might be at the support plate. I don't think we get a great deal of sludge there, you know, not as much as we get at top of tube sheet.

The consensus was that, if you know the nature of the sludge, there is sludge lancing that goes on periodically to get rid of the sludge at the top of the tube sheet, and the loose particles are usually taken away by the sludge lancing. But, in fact, the majority of the sludge is not even able to be lanced off. The stuff is cementatious and it is very hard and sticks to the tubes. So we thought even if the jet worked its way through a piece of sludge, it may pick up a few particles, it would tunnel through there and would not really pick up much more beyond that.

DR. KRESS: Under severe accident conditions that generated a lot of aerosols.

DR. MUSCARA: Yes, I will get to that, sure.

DR. KRESS: You are going to get to that later.

DR. MUSCARA: Sure. Yeah.

DR. POWERS: Under design basis accidents, won't you be carrying in lots of the crud particles from the primary piping system in the jet?

DR. MUSCARA: Yes, generally you do get corrosion of the carbon steel area. I mean much of the primary system is clad, but there is some carbon steel, you do get some product. We have done some work in the past trying to characterize leak rates through cracked pipes, and, you know, we try to do a search and get information on the kinds of crud that you get from the primary side. It is really not crud. You may have some very small particles, and even if you have those, the loading isn't that great.

So, you know, we are not -- we haven't quantified that. Our feeling is that you do not have a large amount of crud due to the corrosion products that gets carried by the primary side fluid.

Also, Stringer felt that the droplet erosion during design basis accident was unlikely, and the reasoning is that the erosion rate is dependent on the droplet size, and it is related to the diameter to the third power. We have noted water droplet erosion in steam turbine, but this occurs because of fine droplets condensed in the turbine, and when they enter the turbine, they become larger drops and then the spinning blades hit these drops and you get erosion, which the insiders call baseball bat erosion. But, again, you know, these are large droplets that the finer droplets are condensed and then are picked up by the blades.

DR. CATTON: So do the smaller droplets cause more problems?

DR. MUSCARA: No, less. The big droplets to the third power.

The NUREG-1570, extrapolation of the data from the coal gasification plants, they assume that the ablation rate will be proportional to the density of the fluid and to the cube of the velocity. The temperature affected the extrapolation only as it changed the density of the fluid. But, in effect, the work that was done for coal gasification, the gas mixture is very oxidizing. In particular, it is 1 percent H2S in this mixture, and nickel alloys under corrosion in these kinds of atmospheres.

So what we are looking in the coal gasification data is one at done at high temperatures, much higher than we expect, and at higher temperatures the corrosion, you get a greater amount of corrosion. In addition, the work was done at low velocities, 10 to -- I believe they had a set of data at 10 feet per second and a set of data at 100 feet per second. And then, of course, this was extrapolated up to about 1,000 feet per second, using the correlation to the third power.

DR. CATTON: Can I go back to that first paragraph for a moment? What is the scenario that you are looking at? Isn't it the high pressure inside the tubes and its water?

DR. MUSCARA: Under steamline conditions, yes.

DR. CATTON: And isn't that what we are talking about?

DR. MUSCARA: Yes.

DR. CATTON: So the jet would expand from, I don't know what, 2,500 psi down to 1,000? Isn't this what leads to the erosion, so it is the droplet sizes associated with the fragmenting jet, liquid jet?

DR. MUSCARA: Right.

DR. CATTON: So where does this fine droplet business come from?

DR. MUSCARA: In the turbine case, the water droplets coalesce, become large droplets.

DR. CATTON: But here you are starting with a liquid jet.

DR. MUSCARA: Right.

DR. CATTON: And it is going to fragment into small droplets.

DR. MUSCARA: Right. So they are small --

DR. CATTON: How fine are the droplets?

DR. MUSCARA: Right.

DR. CATTON: They can be coarse.

DR. MUSCARA: We will be addressing the area of the jet behavior later. But let me just mention right now, we --

DR. SHACK: This is just somebody's opinion, an opinion.

DR. KRESS: It is experts telling what they think.

DR. CATTON: Okay.

DR. MUSCARA: In the literature. But let me just --

DR. CATTON: At the agency, we know quite a bit about this kind of process because this is what is associated with combustion. So you don't have to think it, you could base it on something that is real.

DR. MUSCARA: Right. Right now the first step was to concentrate on the severe accident condition.

DR. CATTON: Okay. It just seemed you were throwing that one away.

DR. MUSCARA: Right. I am bringing this up also because we will be doing some work in this area. So my feeling is we need to understand the dependencies of the jet and how it expands, the particle size, particle density, et cetera, for the severe accident case because we can't really conduct tests under those conditions.

We are also trying to understand how the droplet erosion would work under steamline break conditions, and we will try to understand that from the literature as much as we can. However, we have developed a very nice facility at Argonne National Laboratory for conducting tests under prototypical conditions. So regardless of what the theory tells us, my first step is to conduct -- well, they are concurrent steps, but we are conducting actual test under prototypic conditions with cracks, and whatever jets that are produced impinging on a sample. So there we get some data on the prototypic conditions.

Meanwhile, we will also try to understand it from a theoretical basis.

For the severe accident case, there is no way that we can develop a rig to produce the kinds of conditions that you get under severe accidents. So here we are depending more on whatever knowledge is there, what other research has been done. So this is the one I would like to address first.

DR. HOPENFELD: If you want, I will make my comment later, but it is pertinent to this point, if it is okay with you. I will make it very fast.

Three years before the DPO, I asked Los Alamos to do a study as to what happens when a jet 2200 flushes into water and flushes into steam, into air. To come up with some kind of estimate, what kind of particles, particle size you have. They have done a very considerable amount of work on that. The conclusion was, with all due respect to the expert, that you cannot really predict what size you have. You can come up with sizes from one micron all the way to a fraction of a millimeter.

So what I am kind of seeing here, that you are starting a new program without really looking at what happens based on a meeting. Now that is not how you do research.

DR. MUSCARA: So when one extrapolates the coal gasification data, this is data really that is based on corrosion, not ablation, and the dependency to the third power doesn't hold. In fact, when you look at the data itself, work was done at different velocities, different temperatures, it is inconsistent with their extrapolation. And, also, the effect of temperature on corrosion was ignored.

Based on the literature review, and the experts meeting, we identified some of the key parameters. Two of these were the jet velocities and the associated particle motion that were some of the most important parameters, including the particle size.

Having this background, we asked for some assistance from our Division of System Analysis and Regulatory Effectiveness to carry out calculations to better define the jet velocities and the particle motion that we would expect under severe accident conditions. This work has been completed, and I think I would like to break at this point and ask Steve Arndt to address some of the findings from this work. Steve.

MR. ARNDT: Thank you. As Joe mentioned, my name is Steve Arndt. This week, literally, I am the Assistant Branch Chief for the Safety Margins and Systems Analysis Branch in the Office of Research. I am going to go through some of the work fairly quickly that we were asked by the Division of Engineering to look at to support some of their work in characterizing the kinds of damage propagation you can get.

There have been several attempts to come up with an appropriate velocity impinging on the adjacent tube to this kind of damage mechanism. We were asked to look at this for basically three primary reasons. One, to get a better fundamental understanding of what is going on. The previous analysis were fairly simple analysis. Two, to understand what the particles within the fluid were doing, which had not been looked at previously, because the computational tools weren't available. And, also, to support the work that the Division of Engineering is going to be doing at the University of Cincinnati, or is actually in the process of doing at the University of Cincinnati, and to benchmark the velocities that they need to test at.

And when we originally discussed it, one of the things was, is 1,000 feet per second an acceptable number? Will it give you the numbers?

So, as you can see, this work was done by Professor Piomelli, who is a professor of mechanical engineering at the University of Maryland who does CFD calculations. The code that we used was the NPARK code. I believe Professor Catton is familiar with that. It is an Air Force developed code that is specifically for high velocity flows.

Because we were trying to understand the phenomenon and also provide input to the Division of Engineering, we wanted to look at, based on our computational study, what not only the important parameters were and the actual numbers, but what the sensitivities were. So we looked at variations in the temperature, the pressure, the various steam geometries and the crack thickness. I will show you that in a moment.

For this particular study, we did a two-dimensional study, so we have a crack, we looked at two different thicknesses and assumed an infinitely long crack. The particle size and densities were developed from a Victoria calculation, and this is a slightly misleading phraseology, Charlie pointed this out to me, we assume an equal distribution in the tube at the time of the crack. It is not in the entire primary system, it is the particular density at the primary side of the crack. And we also assumed that the particle velocities were calculated along with -- I'm sorry. One of the goals was to calculate the particle velocities.

DR. POWERS: So you are looking really at the severe accident scenario?

MR. ARNDT: Yes, this is a support of the severe accident scenario.

DR. KRESS: Are these Victoria calculations, were they using the natural convection recirculating countercurrent flow conditions?

MR. ARNDT: That's correct.

DR. KRESS: They actually used those.

MR. ARNDT: We used the SKDEP calculation to develop the accident scenario and then used the Victoria to actually propagate the aerosols.

DR. KRESS: So these aerosols went back and forth with some residence time that may be relatively long while you are heating up and agglomerating perhaps and changing size, and you got all that out of Victoria?

MR. ARNDT: Charlie.

MR. TINKLER: Yes. Charlie Tinkler from the NRC staff. As it turns out, most of the larger aerosols are out of the stream by the time they get to the steam generator.

DR. KRESS: They fall out --

MR. TINKLER: They fall out. So we are left with a distribution, I think it was 1 to 5, on the order of 1 to 5 microns, something like that.

MR. ARNDT: Yeah, I will show the distribution here. Actually, I will show it now just because we are talking about it.

MR. TINKLER: You know, we had a large, a relatively large inventory of non-radioactive aerosols that were floating through the system.

DR. KRESS: That was going to be my next question. What was your source term for non-radioactive aerosols?

MR. TINKLER: I think we have three or four hundred kilograms worth.

DR. KRESS: Coming from the cladding?

MR. TINKLER: Coming from the cladding and structural materials in the core. I think I actually show the number in one of my viewgraphs in tomorrow's presentation. I think it is about three or four hundred.

DR. HOPENFELD: Can I just make one comment? Because what I said yesterday, you cannot do those because of the agglomeration that you get, because the thermophoresis forces in the plenum. So unless you have, in the mixing plenum, unless you have the temperature distribution, you can't figure out the residence time. Right now they don't have it, it is a perfect mixing.

DR. KRESS: Do you include thermal phoresis at all in the calculation?

MR. TINKLER: Yes. Yeah, we have thermophoresis in the model, and, typically, in the tubes themselves, thermophoresis is relatively small effect because the temperature difference between the vapor and the thin steam generator tubes is pretty small. But in other parts of the system, you know, we use the SKDEP RELAP boundary conditions, you know, through a rather tedious process of imposing as boundary conditions on the Victoria. We use their thermal hydraulic conditions, and, yes, we do have thermophoresis.

MR. ARNDT: This is the relative particle size, out of particle mass, particle size and density of the distribution that we had at the time of the opening. And, as you can see, the mean is in this couple of micron area.

You might want to remember this because, for various reasons associated with the computation, we refer the mass as opposed to the particle size, which is not quite as intuitive in later calculations. But I will keep that handy in case anyone needs it.

Like I said, we looked at a variation of several different things, temperature, size of the crack -- I'm sorry, size of the crack which is this -- these are actually half-heights, because we use a symmetric system. Two different sizes of a rectangular grid and a triangular grid. These were designed to be similar in configuration to a Model 51 D type steam generator and a triangular grid from a combustion engineering.

And what you have here, and, by the way, all the details are in this handout that you got. I believe you now have the color version of this, with all the gory details. What you see, and these are the physical properties. Let me actually skip to the next couple of slides later, because it is a little easier to explain what is going on. This is two slides later. From the velocity curves as opposed to the thermal properties curves.

What you have is a thin hole, a small hone, very high pressure, low pressure. This is 16 megapascals, this is atmospheric, so you have an expansion pressure ratio of 160, which produces a very under-expanded jet.

You go through rapid pressure drop and velocity increase and you, because you have a blunt body here, form a shockwave. The expansion would have continued out into, if you didn't have this actual blunt body here, to a typical, very high, under-expanded jet kind of phenomena. We did some sensitivity studies basically by removing that and looking at what would have happened had it not been there and got pretty much what you would expect from a theoretical standpoint.

If these had been parallel plates, you would expect it to expand, then drop, the pressure would drop rapidly and then as you go over here, would increase almost to the original pressure. The same for the velocity, you would start it at a lower velocity, expand rapidly to a very high velocity, the mock numbers are very high, as you can see, and then drop to almost zero here.

DR. KRESS: I presume these are steadystate calculations where you kept the primary side pressure conditions constant?

MR. ARNDT: Yes, that's correct.

DR. KRESS: Okay.

MR. ARNDT: Given the flow rates and the availability of steam on this side, basically, you can't deplete this in the kind of timeframes we are talking about. And this sets up very quickly. You are on the order of a couple of microseconds to set up that kind of steadystate.

Because both of these barriers are curving away, the flow does not stagnate here, but actually shoots off in this direction. In the rectangular grid, of course, there is another tube up here, and you would have another shockwave up here. In the triangular grid, there is another tube over here, and, basically, what you have is a second nozzle type effect where you are shooting off fluid off this direction and off this direction.

If you go back to the original graph and look at the thermal properties, you can see, after the shockwave, well, the jet shoots out very high density, expands rapidly. After the shockwave, the pressure goes back up, and the density goes back up, but not nearly as high as the original pressure. You go through a very rapid drop in temperature as well on the other side of the shock. Because you are compressing the fluid, you increase temperature again. And you can see that from this particular graph. The density drops down dramatically, the temperature drops and then goes back up again. This is the velocity in the X direction and the pressure.

DR. CATTON: You approach velocities of about 3600 feet per second in that. You have 1200 meters per second, about 3600 feet per second.

MR. ARNDT: Yes. And if you look at the variations associated with the different geometries, you can see the basic phenomenology is very similar. You expand rapidly, the pressure drops, the velocity goes up. You go through the shock front, the velocities drop, the pressure goes up.

DR. KRESS: Where is the 1,000 feet per second on that?

MR. ARNDT: This is -- 1,000 feet per second, it would be right in here.

DR. KRESS: So that is where the number comes from?

MR. ARNDT: Well, I will show you where that comes from.

DR. KRESS: Okay.

DR. CATTON: Normally, you don't get such nice shocks when you use this code. Did he do something to smooth them?

MR. ARNDT: We looked at --

DR. CATTON: You get more spikes, unless you do special effects.

MR. ARNDT: Yeah. These are all center line calculations, along the --

DR. KRESS: Along the line of symmetry.

MR. ARNDT: Along the line of symmetry. We will look at in a minute what it looks like off line of symmetry, and you will see it is a little more --

DR. CATTON: Anybody who gets such beautiful shocks, I don't trust it. And I have used NPARK.

DR. KRESS: A shocking statement.

DR. CATTON: Usually you get wiggles, you get the wiggles just because the codes can't really treat the shock that well. You have to do special effects in order to treat the shock.

DR. KRESS: Well, he had an extremely fine grid.

MR. ARNDT: Yeah.

DR. KRESS: And he had very, very small time plates.

DR. CATTON: You get spikes.

MR. ARNDT: We also, like I mentioned, --

DR. CATTON: Magnitudes don't change a whole lot.

MR. ARNDT: -- did several sensitivities based on things like temperature. We varied the temperature by 100 degrees, and we saw fairly small changes. We varied the size of the hole quite a bit. We see significant amplitude changes, particularly in the pressure, because you are putting out a whole lot more fluid, so the pressure behind the shockwave will be considerably higher, but the basic phenomenology is fairly similar.

Now, of particular interest is what is happening along the streamlines, and that is because one of the things we are really interested in is what is happening to the particles along this flowpath. If you look at jet cutting tools, they are very high colonated, usually gets of water. Reasonably high velocities, very high pressures, very high particle loading. And they, of course, will turn when they hit the piece of metal you are cutting to, but the braces, as well as the actual water, will actually go forward.

So, one of the things we wanted to look at was along the line -- not along the line of symmetry, but actually various jet streams. The first is actually outside of this jet, and you can see it goes through a much different velocity and mock number profile. If you look along these two, particularly, the third one here, which is fairly close to the center line, but off of it and does the turn, what you see is it, of course, accelerate, decelerates through the shock, comes to a steadystate during its turning here, and then as it goes up through here and starts accelerating again into the nozzle between the two, you get the accelerating again.

This is basically where we got our 1,000 feet per second. Somewhere in this range will be right off or right near the center line.

Now, if you want to look at the particle velocities, you have to look at what the particles are doing in the fluid. They are going to be accelerated with the fluid based on, in essence, a relative flow rate between the particles and the fluid, that gives you the driving force to accelerate them. And in these particular cases, you can get all sorts of different forces associated with them, but the dominant one is the drag force on the particle.

And if you use the basic Reynolds number for particles, the stand Stokes Law type calculation, what you get is kind of what you expect, although when we did it, we didn't think it was going to be this dramatic. The smaller the particle, and if you will remember, our mean particle density was right in this area, a couple of microns -- I'm sorry, micrometers, what would happen is it would accelerate as it goes through the expansion, then it would decelerate, but the amount of acceleration and deceleration basically depending upon the size because you had -- you are driving the drag based on size. We, as it turned out, for computational convenience, used the drag on a sphere. We later looked at what would actually happen if it was not a sphere, which, obviously, a lot of aerosol particles aren't, and I will talk about that in a minute.

Of particular interest, of course, is it takes a little longer for the heavier particles to accelerate, as you would think. It also takes longer for them to decelerate. Again, the particles that we were looking at, by and large were in this range. And you see they drop off rather dramatically. We have a few particles up in this higher range, but this wasn't real satisfying, so we want to look at this a little bit closer.

If you look at the data for very high Reynolds numbers for particles -- let's see if I can find my graph here, you find that the standard and Stokes Law doesn't really apply very well. And use the Stokes solution for the drag coefficient on a sphere with a relative Reynolds number, it will predict something like this. If you go back and look at some of the experimental data in this, it doesn't really do that. So if you go -- what we did was, for very low velocities, we used the Stokes solution. For this intermediate range, we used an older solution, and then we also used the experimental data to try and redo this.

What happens when you do that is the larger particles, even though they do slow down -- rather, speed up and slow down at a slower rate, they are considerably more dependent upon the fluid velocity than if you used the straight Stokes Law.

So the real issue here, as we wanted to really find out, was what was the fluid velocity doing? What was the particle velocity doing? And then we can give that to our friends in --

DR. CATTON: Are these particles solid or what?

MR. ARNDT: They are assumed to be solid.

DR. KRESS: So your major conclusion is that the particles are going the same velocity as the fluid.

MR. ARNDT: They are going the same velocity as the fluid and, more importantly, they are moving with the fluid. If you go back to the streamline analysis, they are moving with the fluid, and they are decelerating here, and they are also turning. If they weren't moving with the fluid, they would have the tendency to go forward. They would basically maintain their momentum and go forward in this direction. So they are moving with the fluid and they are also moving at the fluid velocity. So they do have the tendency to turn with the fluid due to the blunt body.

DR. KRESS: The first calculation using Stokes Law.

MR. ARNDT: Yes.

DR. KRESS: They didn't follow the fluid.

MR. ARNDT: They didn't follow the fluid as much. They tended to not be decelerated with the fluid because the drag coefficients were lower, so they would decelerate slower and also turn less.

DR. KRESS: Does that imply there might be an optimum drag coefficient?

MR. ARNDT: There probably would be. Because aerosols are not nice perfect spheres, we also did a sensitivity study that increased the drag coefficient and decreased the drag coefficient by a factor of 10. And I didn't plot it up for you because it is not in the report as a plot, but what basically happens is, by doing that, you move this up a little bit. It comes in kind of like that.

DR. CATTON: So what this is saying is basically you can't erode the adjacent tube with a compressible flow.

MR. ARNDT: With this kind of compressible flow, with these kind of particles.

DR. CATTON: Is it because of the size of the particles?

MR. ARNDT: That is primarily --

DR. CATTON: There are examples, in the old Nike Zeus program, they were going to steer it with the Cunard, and they just chewed a hole right through it. In that case the particles were actually liquid, less penetrated.

MR. ARNDT: Yeah.

DR. CATTON: And the velocities weren't near these because it was still, it was in the nozzle, it was still turning. And that kind of -- maybe the particles were bigger or smaller, I don't know.

DR. POWERS: I guess the question I have for Dr. Kress is, what impactors work? That sonic jet is coming in on plates?

DR. KRESS: They work because the particles --

DR. POWERS: Can't make the turn.

DR. KRESS: Yeah, can't make the turn, that's right.

DR. POWERS: And don't stay with the flow velocities.

DR. KRESS: That's right.

DR. POWERS: And here they are tracking, this impactor won't work. But one micron particles, one micron particles are the easiest particles in the world to get an impactor to work on, because they don't stay with the stream velocities at sonic levels.

DR. KRESS: Flow velocities through an impactor are smaller.

DR. POWERS: No, they can be sonic, but just sonic.

MR. ARNDT: Now, if this didn't --

DR. CATTON: The shock standoff distance seems to be quite large also.

MR. ARNDT: Yeah, it is. If, for example, this was at sonic, or subsonic, you wouldn't have this kind of shock standoff. Sonic velocities are down in this range for this particular fluid at this particular temperature. Also, you have comparatively low particle loading in this particular case, which is something that we are -- I think Bill is going to talk about a little later.

The sonic velocity is down in this range. So if you assume that you have very near sonic velocity, say, for example, up in the next tube over, say, for example, this will expand and contract. Then you are down here, but you are also at a much lower density not of the fluid, but of the particles themselves.

DR. KRESS: What do you mean lower density of the particles?

MR. ARNDT: Well, as you spray this out, you are expanding the jet.

DR. KRESS: Oh, you are talking about number density.

MR. ARNDT: Number density, yes. I'm sorry.

DR. POWERS: I also know its impact better when the number density is lower.

MR. ARNDT: Yeah. This is the study we did and these are the conclusions we came up with.

DR. HOPENFELD: I would just like to make a quick comment.

MR. ARNDT: Yes, sir.

DR. POWERS: Is this an item of verification?

DR. HOPENFELD: No, it is just a clarification, that is all. For two phase flow, in subsonic flow, if you have gas and particles, the loading of the particles is an important factor because there is an interaction there, they tend to stack up. That affects the standoff distance and their loading rate. And I was just wondering, I am just making a suggestion, so I am not criticizing anybody here, maybe you should look also, look at it as a two-phase flow, set up the basic equation for it, and this way you can find out what the effect of concentration is, just like a two-phase flow equation basically, particles and gas.

DR. KRESS: I suspect your number density of these particles is so small, you are not going to affect the sonic velocity in this case. It is a pretty small number density there, so it looks pretty -- it is mostly acting like a gas.

MR. ARNDT: And, of course, if you have any additional questions, I am available tomorrow, and, of course, we can provide additional input.

MR. HIGGINS: You didn't have a slide on the conclusions, but you said verbally that the conclusion from that was that they would not, the particles would not erode the other tube. That wasn't really the objective of this part of the study. Part of the study objective was to provide particle and velocity -- particle and fluid velocity calculations to the Division of Engineering so they can look at what particles moving at those kind of velocities, in those kind of densities would do, and they are going to talk about that next.

DR. KRESS: The particles that are right on the line of symmetry have nowhere to go except impact the tube -- so those at least will go on and impact.

MR. ARNDT: Yes. And we would expect them to impact in the two or three hundred meters per second kind of time velocity.

DR. KRESS: I guess the question may boil down to, do those particular particles do some sort of damage to the next tube?

MR. ARNDT: Right. And Joe is going to talk about some of the experimental work he is doing on velocity, particles of that velocity and that size on actual pieces of zinc alloy.

DR. MUSCARA: Okay. So, based on the information we have been gathering, we have set up some tests, tests to address the jet impingement under severe accident conditions to be conducted at the University of Cincinnati with Professor Tabakoff, and we are also in the process of planning and running some tests at Argonne National Laboratory under steamline break conditions.

The conditions we are considering for the severe accident conditions, a temperature of 700 degrees centigrade and a pressure 2350 psi. The particle loadings, we discussed earlier, taken from the code. You see most of the particles are silver, about 85 percent of the particles are silver. There are some oxides, 10 oxides dominant and indium oxide.

The total loading is 115 grams per cubic meter. The medium --

DR. POWERS: It is a little surprising you don't have any urania in that mix.

DR. MUSCARA: Any?

DR. POWERS: Urania.

DR. MUSCARA: You know, I suspect it has to do with its melting and volatilization temperatures. If it is not volatilized, it doesn't get picked up in the stream and condensed later on into an aerosol.

Charlie.

MR. TINKLER: I am not sure we are claiming it is zero. I think we have just listed the dominant species and compounds that might be present. I doubt very seriously if it was zero UO2 in there.

DR. KRESS: It looks an awfully lot like it is at the stage of the accident where you have just failed the control rods and attacked the plant a little bit, but haven't gotten --

MR. TINKLER: That is also true. You know, in past presentations, I have indicated that typically these are the conditions at the time that we normally predict the surge line or hotleg to fail.

DR. KRESS: Okay. So you haven't really --

MR. TINKLER: They are still relatively early in the core degradation, overall rows core degradation process.

DR. KRESS: You have probably entered the high rate of steam zirc reaction.

MR. TINKLER: Yes.

DR. KRESS: Just barely probably.

MR. TINKLER: We are into the temperature escalation of the cladding and the core, but we haven't gotten to the formation of a large molten pool or things like that yet.

DR. MUSCARA: That is why we try about 700 degrees centigrade, it reflects the temperature of the tubes at the time of surge line failure, which is at 684 degrees under the 6 RU scenario that is described in many of Charlie's reports.

DR. SIEBER: Once the surge line fails, the driving force can make the jet --

DR. MUSCARA: This is why we are concentrating here, yeah.

DR. SIEBER: So that is a reasonable assumption.

DR. MUSCARA: So the mean particle diameter is 1.5 microns, most of the particles were less than 3 microns. I think the distribution I saw, there might have been a few particles at 5, but nothing at all beyond 5.

So before we are planning this work, we looked for places where you could conduct some experiments. And it turns out there are a couple of rigs around the country. At the University of Cincinnati, there is this apparatus that has been used for many years and many erosion studies conducted by Professor Tabakoff. We decided that this was a place we could conduct some experiments.

This is the rig that is used. Essentially, there is a propane burner atop of the rig. Air is mixed with the fuel. Below this there is a preheater for the particles, so the particles are fed out to the preheater, with some time in residence to pick up temperature. And then the particles are injected in the stream and there is a fairly long tunnel, acceleration tunnel. And at the bottom, here is where the test specimen is. There is a capability for changing the angle of the specimen with respect to the fluid.

Beyond that, there is an exhaust tank where effectively these gases cool, the particles can drop out and recover.

Well, the atmosphere certainly is not pressurized steam, but it is an oxidizing atmosphere. Most of the combustion products would be CO_2 and steam.

DR. KRESS: What particles do you use?

DR. MUSCARA: Well, that is one of the following viewgraphs. The predominant particles that we had in the aerosol, as mentioned, was silver. And we can't use silver for these tests, in particular because the silver would melt in the combuster. So we were looking for a surrogate material we could use for silver and still be conservative. So what is important here, of course, is the density and the size of the particles, and essentially the hardness of the particles at that temperature.

So when we compared different materials to the major particles in the aerosol, we settled on using nickel for the majority of the particles and to simulate the oxides with nickel oxide. And, also, to be even more conservative, we were looking at some aluminum oxide in conjunction with the nickel.

We did need information on the velocities, however, when we run these experiments, we like to go beyond the particular velocity that was calculated, just to make sure that we have enough information. So we run tests from a lower velocity, about 300 feet per second, up to 1800 feet per second. The initial series of tests were aimed at determining the worst conditions, so we ran a number of tests at 1,000 feet per second by changing the angle of the target. We looked at, I believe, 20, 30 and 45 degrees, and the maximum wear rates were obtained at 30 degrees. This is similar to many other tests that have been conducted. This is a similar angle that produces the worst results.

So the subsequent tests were run at 30 degrees and we varied the velocity and also the particle mix.

Again, I must say that these tests are not complete, but we have completed tests with the nickel powders at 300, 600, 1,000, 1,800 feet per second, 100 percent nickel. The particle size, 3 to 7 microns. And there is the initial data on the erosion rates.

We are also planning on running tests with nickel plus 15 percent nickel oxide and nickel plus 15 percent aluminum oxide, and have completed some tests with the 100 percent nickel oxides, thinking that this would be as conservative as one could get, all of the particles are the hard oxide.

DR. POWERS: Your data seemed to indicate that something unusual happens between 1,000 and 1,800 feet per second.

DR. MUSCARA: Well, there is the relation of the wear with respect to velocity. Professor Tabakoff had some tests some years ago using 70 micron quartz and, clearly, he gets higher wear rates, but the velocity dependence is similar, and it is representative of what happens with mechanical abrasion.

DR. KRESS: Now, the units on this are cubic centimeter per gram per second.

DR. MUSCARA: Yeah, actually, those are converted numbers. What we get from Professor Tabakoff from the tests are milligrams of material lost per milligram or gram of particles impacting the surface. So we converted those numbers and are taking into account the temperatures. We convert to get an estimate of the amount, depth of material that is worn.

And for the 300 meter -- well, 1,000 feet per second, and with the nickel powder, the wear rate on this material is about 4 mils per hour.

Now, I am not sure if I have a viewgraph on this, but the experiments we have conducted with the nickel oxide, in fact, we did not get anywhere. We effectively had deposition. There is the sample weight more at the end than it did before. But at these temperatures, of course, the material is fairly soft, and the hard, abrasive particles embed themselves into the material. They plow the material, they don't cut the material, so we had -- we didn't have any wear there. So now we are going back and trying the nickel with 15 percent nickel oxide and also nickel with 15 percent aluminum oxide just to see whether there is synergistic effect there, but I suspect limiting data will be the data with just the nickel powder.

You know, I have indicated that we will be running some tests for the steam line conditions, but in some of our prior work for different purposes, we are running some tests on different size orifices. We did have one test where one 32nd inch hole was impinging on the inconnel target. This was a test at room temperature, with 2500 psi pressure, a four hour duration test. At the end of the test we noticed only very light burnishing of the tube.

We will run tests under more prototypic conditions. Well, first, conservative tests at 2500 psi and 300 degrees C, and then following those tests, we will consider running some tests that more closely reproduce the accident scenario. There is the actual pressures and time relationships.

DR. POWERS: And these are liquid tests that you are talking about?

DR. MUSCARA: This would be tests where we effectively, on the primary side, we have high temperature water. The secondary side will be dry and we will see what happens.

Well, I think quickly to conclude, we believe the damage by jet impingement, due to severe accident conditions in particular, is not going to be a concern. We are doing additional tests, I think that will be confirmed.

I guess, also, we conclude for the rest of this section, which has to do with the work that Bill will talk about tomorrow, but effectively develop models for predicting the structural behavior of a good tube, as well as degraded tubes under normal operating design basis accident conditions and severe accident conditions. We will see all of that tomorrow. But the models have been very good in predicting behavior and we have quite a bit of test data to validate those models.

So I think at this point, I am finished unless there are some questions.

DR. POWERS: Any other questions on this presentation?

I don't know these results are all stunning -- I mean surprising, are they, to you? I mean, typically, when you use jets to cut things, you work with much higher velocities?

DR. MUSCARA: Much higher loading on the particles and much larger particle sizes. Our meeting with the experts, they really felt there would not be much erosion under those conditions. We felt that there was enough evidence from the literature and from the meeting that that would be the case, but we wanted to run some tests to verify it.

DR. CATTON: I think the use of a CFD code like NPARK, which is really, it is a code that has been around for a long time, it was originally developed by Los Alamos and then the Air Force picked it up, and they actually have an office in St. Louis whose only purpose in life is to incorporate everybody's experience. It is a reliable code. The only question one might have is the shock standoff distance might not be quite right. But looking at the particle sizes, they are correct, and the drag coefficient is anywhere near correct, the tracking of the particles to the fluid velocity, you could have that shock standoff distance and it still wouldn't cause much of a problem. I think they did a fairly substantial job in demonstrating that this particular aspect is not a problem.

DR. POWERS: My concern remains the same, it is counter-intuitive to me to have particles like that tracking the velocity so closely. Because usually we rely on the fact that they don't track velocity closely in order to sample and trap them.

DR. CATTON: That's right. That's right.

DR. POWERS: But I mean I have no experience that at these kinds of velocities.

DR. CATTON: My only experience was the opposite, and I cut a hole right through the device, but it was a lot hotter.

DR. KRESS: I would follow up a little on Dana's comment. The code you are talking about doesn't have particles in it, it calculates the stream lines.

DR. CATTON: It is pure and simple a compressible flow code.

DR. KRESS: The question I have then is how did one translate the drag coefficients in order to see whether the particles followed the stream lines or not.

DR. CATTON: Well, you have to make an assumption when you do this. The assumption is that the particle density is low enough that it doesn't impact the flow.

DR. KRESS: That is a reasonable assumption.

DR. CATTON: If the loading starts to go up.

DR. KRESS: No, that is a reasonable assumption on his loading we have here.

DR. CATTON: If you can make that assumption, then it is just a matter of fitting the particles into the flow field and asking, where do they go?

DR. KRESS: Do you have the capability of putting them in the flow field and changing the drag coefficient?

DR. CATTON: Not with NPARK, no.

DR. KRESS: As you move from one spot to the other.

DR. CATTON: I suspect what they did is they just have a data set with a velocity field in it, and they stuck the particle in and said, where do you go? Is that correct?

SPEAKER: Yeah.

DR. CATTON: Yeah. So once they have the velocity temperature and pressure fields.

DR. KRESS: So all you have to do is put one particle of each size at a given spot and watch it go?

DR. CATTON: That's correct. That's correct. And then just look at its trajectory.

DR. POWERS: I mean the place where we run into problems with that is so much higher that I mean I don't have any trouble with these assumptions. I have troubles with the conclusion because I mean, how do we move particles across boundary layers? They don't track fluid velocities. And especially when you get up to a micron, I mean it is a micron where we have impaction problems. You get much below a micron, then, yeah, they track the stream velocities pretty well. Certainly, if you get down as low as a tenth of a micron, then they really track stream velocities well.

DR. CATTON: What are the drag coefficients? These are --

DR. POWERS: It is a drag curve, a lot like the one he showed with the dots that come across there. I mean the drag coefficients you had were about -- seemed all very rational to me. So I am perplexed, I mean my experience in shockwaves is exactly zero.

DR. MUSCARA: I had almost forgotten, but there is another item on the agenda, and that is to discuss the behavior, the reasoning for the selection of the quarter-inch crack.

DR. POWERS: Right.

DR. MUSCARA: Steve.

MR. LONG: After what just transpired, I think I need to remember everybody that I am about to talk about what happened first.

DR. POWERS: In the beginning there were quarter-inch cracks, right.

DR. KRESS: Your first test is to see if you can turn the thing on.

DR. POWERS: If you can't turn that thing on, then we are not going to believe a word you say.

DR. KRESS: It is the big long bar on the front.

MR. LONG: There we go.

DR. POWERS: Now we have to listen to him.

MR. LONG: Okay. To put this in context, when we did NUREG-1477, we were dealing with really just measurements of cracks in terms of voltage, and we didn't have length and depth information. We were somewhat concerned about severe accident issues where there would be high temperature flowthrough cracks in tubes. We didn't really -- I'm sorry. We didn't really have any way of working with that until we got some distributions of crack sizes. So, through a research contract, Dominion Engineering produced some correlations of crack sizes and we could proceed when we did NUREG-1570 work.

DR. POWERS: Correlations of crack sizes with?

MR. LONG: Well, basically, they were giving us results of a lot of different analyses that the utility companies had done with their distributions in their plants, separated out by distribution -- by degradation type, and then trying to give us a distribution of lengths and a distribution of depths.

DR. POWERS: This is just the database they maintain on what people find.

MR. LONG: I don't know if they maintained it or produced it, but we received it through the subcontract. They didn't correlate the length and the depth, and when they did the correlations to the depths with the data, they used gamma functions that basically fit the data in the exponential part of the function and had offscale low in depth and in length, extremely high artifact peaks that didn't fit any of the data.

So the difficulty was we ended up, when we tried to combine the two to get a distribution of physical cracks estimated, we would end up with a very large number of extremely short but extremely deep cracks.

Well, when we looked at the temperatures and what we expected the cracks to behave like at the temperatures the tubes would reach before RCS pressure boundary failure in core damage accidents, the type of behavior we were seeing would have said a crack that was maybe 4/10ths of an inch long would be about as short as you would expect to rupture with a typical limit load analysis based on the flow stress, as best we could extrapolate it.

So, we were aware of the DPO concern about cutting. I think there was also some consideration of cutting when NUREG-1150, accident progression expert elicitation was done. So we wanted to try to represent the cutting somehow assuming the cracks that were still too short to rupture would open significantly and might be able to cut adjacent tubes, or for that matter, erode the hole in that tube that the flow was going through.

But the problem was we had a large number of cracks that could be throughwall that were perhaps less than a hundredth of the inch long in the correlation, and if I just put that in there, I had a probable certainty that those cracks would be present. Should I assume they cut?

Talking to the materials people, this was seemingly resolved with sort of the classical back of the envelope argument that stress corrosion cracking had typically as aspect ratio that was at least five times length to depth and, therefore, for a 50 mil tube, we really shouldn't see throughwall things much shorter than a quarter of an inch.

That sort of became the cut-off in NUREG-1570 for looking at the distributions that were given to us by Dominion.

That picture sort of stayed in vogue until we really started doing profiles of cracks that were coming from plus point analyses of flaws found in power plants. And the first one I was afflicted with was from Farley when they requested essentially a waiver of a mid-cycle inspection on the last fuel cycle before they replaced their steam generator tubes. And the way this data was being treated is that the eddy current signals were being treated as planar cracks with jagged shapes, and then these were being projected over the cycle to grow, at least in depth, and I have forgotten if they were growing theirs in length. Sometimes they are not grown in length, sometimes they are, depending on who is doing the analysis.

At any rate, once they are grown in a Monte Carlo process by depth, they are analyzed for the fraction of the crack that might go throughwall and perhaps create a leak or go throughwall and burst. And this is done by mathematically taking a rectangle, taking a small length and moving it along the crack, taking the average depth within that length, calculating the stress magnification factor, then taking a slightly longer length and doing the same thing until the find the part of the crack that has the maximum stress magnification factor for pop-through and the maximum for burst.

What Farley found, very late in the review process, actually, Westinghouse was doing the analysis, was that they had a near certainty that they would have something go throughwall by growth during that cycle. The concern was that they thought it was very short, and did we want to treat that as a complete failure of the primary to secondary boundary for the risk analysis?

This was a difficulty because now we weren't talking about a whole crack that went throughwall, it was sort of a simplified rectangular approximation to the crack shape. Now, we were talking about long cracks, they might be half an inch, an inch long, but we were only talking about a small segment, so we couldn't argue they don't exist, they probably do exist.

So we had to figure out, did we believe a crack of a certain length that was throughwall would really open significantly and leak significantly? We didn't really have data that would allow us to do this back when we were doing the Farley analysis.

So it ended up with a telephone conversation late in the game. Bill Shack was on it, Joe Muscara was on it. I am sure Bob Keating was on it. I think Tom Pitterly was on it, but I have forgotten. I was, some people from the Farley site, and we were trying to figure out what we would do in this case. Would we basically reject the application on the idea that some crack, not matter how short, would penetrate the wall during the fuel cycle? Or would we stick with the quarter-inch or set some other value?

And I think the general feeling was, from the people that had some experimental evidence but hadn't really been able to quantify it, so it was a qualitative feeling, that the quarter-inch crack was probably not going to be severely cutting, not in the timeframe that we thought we had before some other part of the pressure boundary would fail, if, in fact, this didn't fail first and relieve pressure.

We didn't really know what number to pick. It wasn't a quantified judgment. So at this point what we decided to do was leave the crack at the quarter-inch as the threshold for what we would consider to be a gross failure and proceed with the application review.

We wrote the SER and pointed out that the results would be sensitive to this conclusion. I will go into the practices of risk-informed decision-making on my slides tomorrow, so at this point I will just say that one of the principles is to look at the things that your decision is sensitive to and the uncertainties.

We put in that this was a judgment, and I know that that upset the DPO author because he felt should have something better to make a relaxation. The best we could do at the time was to make that clear, that it was not an analytical result, it was a judgment, that we were sensitive to that judgment, and to modify the DPO considerations document as well so it was clear in there that this was an issue that we needed more information on.

Subsequently to that, RES started the work that you just heard about. That was actually before we formally got the letter over to them to request them to do that. And as you have heard, at least from the cutting angle, it doesn't seem as though there is a real problem with the quarter-inch cracks. And there is another aspect of this that has to do with how much those cracks would open up and leak, and Joe Donohue is not here today to present that, so I guess we will have to deal with part of that tomorrow.

There is still then, now we are talking about quarter-inch openings that might exist, there is still the potential issue about how much leakage would you get through them. And there what we would like to do is make sure that we are at least staying below the 1 GPM that is the current tech spec limit.

In that regard, you would want to have, you know, very few cracks. And another part of the work that I didn't hear about was to try to add the creep aspect of the crack opening, so that you could get a crack area and get a more valid calculation of leak rate tomorrow.

I hope that explains the history of not answering all the technical questions.

DR. POWERS: Okay. Any questions on this? We have a quarter-inch cut-off for cutting, which may not occur at all, but we still, we don't have any cut-off for leakage?

MR. LONG: The leakage has not been handled quantitatively at this point for very small cracks, in terms of looking at a distribution and quantifying it through that.

DR. POWERS: Any other comments that the members would like to make?

[No response.]

DR. POWERS: Well, on that note, I will thank everyone for some very nice presentations, very informative, and invite you all to reappear at about 8:30 tomorrow for some more of this fun.

[Whereupon, at 7:00 p.m., the meeting was recessed, to reconvene at 8:30 a.m., Friday, October 13, 2000.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

NAME OF PROCEEDING: MEETING WITH THE AD HOC SUBCOMMITTEE ON DIFFERING PROFESSIONAL OPINION ISSUES

PLACE OF PROCEEDING: ROCKVILLE, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Mike Paulus

Mike Paulus
Official Reporter
Ann Riley & Associates, Ltd.

GL 95-05: CONDITIONAL PROBABILITY OF BURST

Calculation methodology

Sample projected EOC voltage distribution

Sample burst pressure correlation with that voltage

Sample material properties distribution (based on pulled tube data)

Repeat for all indications in the steam generator and record if any of samples had burst pressures less than the MSLB pressure

Repeat this process many times for the SG

Divide the number of SGs which had indications which burst by the total number of times the SG was sampled

Limit conditional probability of burst to 1×10^{-2}

1/5th of value assumed in NUREG-0844

Provides indication that a tube may exceed Regulatory Guide 1.121 structural limits

GL 95-05: LEAKAGE DURING POSTULATED ACCIDENTS

Calculation of leakage involves combining EOC voltage distribution with

- Probability of leakage correlation
- Conditional leak rate correlation

Conditional leak rate correlation

- If linear correlation can not be demonstrated based on statistical analysis, leakage is treated as independent of voltage
- If linear correlation can be demonstrated, leakage is treated as a function of voltage
- Parametric uncertainty must be accounted for in either approach

Licensing basis assumptions are used to determine offsite doses associated with primary-to-secondary leakage

- Offsite doses are evaluated against applicable regulatory limits (part 100, GDC19)
- If doses are unacceptable, options include, reducing iodine limits and/or repairing additional steam generator tubes

FARLEY EOC 14 RESULTS

Predictions bounded actual results except for SG C

Under predicted the peak EOC voltage (7.6 volts versus 13.7 volts)

Under predicted the probability of burst ($1.4x10^{-3}$ versus $5.2x10^{-3}$)

Even though licensee under predicted the probability of burst and peak EOC voltage for SG C, the licensee was still within "limits" for tube burst and SLB leakage

Tube with 13.7 volt indication was removed from SG for destructive analysis

Grew from 1.4 volts (12.3 volt change from prior cycle)

Post-pull voltage was 28.5 volts

Burst slightly higher than 1.4xMSLB

Leaked at 0.72 gpm (no leakage during in-situ pressure test)

GL 95-05 CORRELATION STATISTICS

GL 95-05 review involved detailed evaluation of correlations and probabilistic methodology (reviewed by statistician at PNL)

Coordinate system, Residuals, Parametric Uncertainty

Correlations submitted to NRC on periodic basis to reflect additional data

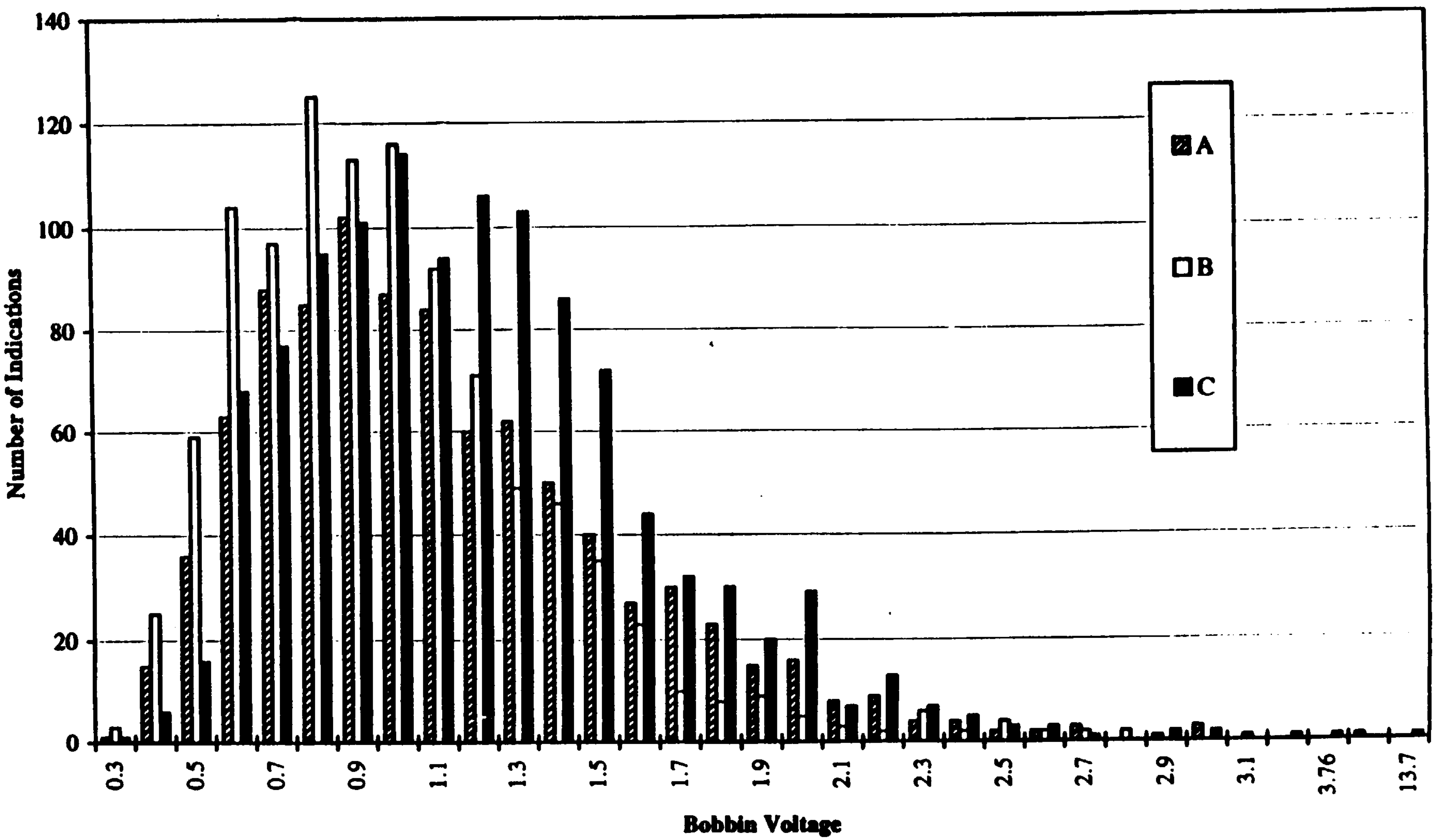
Burst Correlation

	3/4"	7/8"
N	96	91
σ	0.86	0.82
r^2	82%	82%
p-value	10^{-36}	10^{-35}

Leak Correlation

	3/4"	7/8"
N	48	29
σ	0.61	0.81
r^2	62%	12%
p-value	10^{-11}	.035

Figure 1
Farley Unit 1 April 97 Outage
Bobbin Voltage Distributions at EOC-14 for Tubes in Service During Cycle 14



Bobarpc xls-Fig11

Table 3
Farley Unit 1 1997 EOC-14 Outage
Summary of Calculations of Tube Leak Rate and Burst Probability

Steam Generator	POD	Number of Indications	Max. Volts[(1)]	Burst Probability		SLB Leak Rate (gpm)
				1 Tube	2 Tubes	
EOC - 14 PROJECTIONS						
A	0.6	1276	6.9	7.5×10^{-4}	2.5×10^{-5}	7.1
B	0.6	1310	6.7	7.1×10^{-4}	1.9×10^{-5}	6.0
C	0.6	1545	7.6	1.4×10^{-3}	1.9×10^{-5}	10.2
EOC - 14 ACTUALS						
A	1	921	6.9	6.2×10^{-4}	1.9×10^{-5}	5.3
B	1	1014	3.7	1.9×10^{-4}	$< 4 \times 10^{-6}$	4.2
C	1	1140	14.9	5.2×10^{-3}	$< 4 \times 10^{-6}$	7.6
EOC - 15 PROJECTIONS[(3)]						
A	0.6	1411[(2)]	13.5	2.5×10^{-3}	5.9×10^{-5}	10.7
B	0.6	1563[(2)]	13.4	2.1×10^{-3}	$< 4 \times 10^{-6}$	9.3
C	0.6	1733[(2)]	14.7	9.6×10^{-3}	1.2×10^{-4}	15.6
C	0.6 ≤ 10Volts 1.0 > 10 volts	1732[(2)]	14.1	6.3×10^{-3}	6.6×10^{-5}	15.4
C	POPCD	1199[(2)]	13.0	4.4×10^{-3}	3.1×10^{-5}	9.6

Note:
(1) Voltages include NDE uncertainties from Monte Carlo analyses and exceed measured voltages.
(2) Adjusted for POD.
(3) Based on a Projected Cycle 15 length of 486 EFPD (1.33 EFPY)

3 VOLT ALTERNATE REPAIR CRITERIA

At Byron 1 and Braidwood 1, a modification to the standard GL 95-05 repair criteria was implemented

This involved expanding tubes above and below the tube support plate to limit the deflection to ~0.1-inch

Axial burst probability is minimal

Axial tensile tearing needed to be addressed because of the higher voltage limits

Modified leakage approach to account for indications which attempt to burst but don't fully open because of the presence of the TSP

Byron 1 and Braidwood 1 subsequently replaced their SGs

No operating plants currently implement this criteria - an application to implement has been submitted by 1 licensee