UNITED STATES NUCLEAR REGULATORY COMMISSION
REGION 1

In re: CONSOLIDATED EDISON COMPANY OF NEW YORK
INDIAN POINT 2

An Enforcement Conference was held before Loretta B. Devery, Registered Professional Reporter and Notary Public, at the offices of the United States Nuclear Regulatory Commission, Region 1, 475 Allendale Road, King of Prussia, Pennsylvania, on Tuesday, September 26, 2000, commencing at 1:00 P.M.

ALL POINTS REPORTING
723 Erlen Road
Plymouth Meeting, PA 19462
(610) 272-6731

ORIGINAL

PRESENT:

HUBERT MILLER
JOHN ZWOLINSKI
BRIAN HOLIAN
DAVID LEW
JIM TRAPP
WAYNE SCHMIDT
BILL RAYMOND
EMMETT MURPHY
JACK STROSNIDER
EDMUND SULLIVAN
STEVEN LONG
PETER ESELGROTH
ALAN MADISON
TOM SHEDLOSKY
RICK URBAN
DAN HOLODY
A. RANDOLPH BLOUGH
J. BRADLEY FEWELL, ESQ.
MICHAEL RAFKY, ESQ.
MITZI YOUNG, ESQ.
PATRICK MILANO
PETE HABIGHORST
JOE SHEA
G. SCOTT BARBER
JIMI YEROKUN
GREG CRANSTON
RICH PINNEY
JOHN ROBERTS
JOHN GROTH
JIM BAUMSTARK
JACK PARRY
TOM ESSELMAN
DOUG GAYNOR
JOHN McCANN
JOHN ASHCRAFT
BOB HENRY
BILL McBRINE
RONALD BALLINGER
TOM PITTERLE
BRENT BRANDENBURG, ESQ.
TOM POINDEXTER, ESQ.
BART COWAN
D. C. ADAMONIS
DAN SALTER
JIM MARIS
S. M. IRA

MR. HOLIAN: Good afternoon. My name is Brian Holian. I'm the Director for the Division of Reactor Safety here at NRC Region 1. And we'll go through introductions in a brief minute here.

The purpose of today's meeting is to conduct a regulatory conference on the inspection findings from the Indian Point 2 Special Inspection concerning the performance of Con Edison during the 1997 steam generator inspections.

This meeting is between the NRC and Con Edison and it is open for public observation. The meeting will be transcribed. And along that line, I would ask, as we have questions or comments from the NRC's perspective or responses from Con Edison and it's not evident who's talking, that you do state your name for the transcription.

Copies of the NRC inspection report and the presentation slides are available on the back table, and there is an attendance sheet going around the room. So I ask you to get your name on that.

And with that, I'd like maybe to start up at this end of the table, go right down the NRC side.

MR. SCHMIDT: Wayne Schmidt, NRC Region 1.

MR. TRAPP: Jim Trapp, Senior Reactor Analyst.

MR. LEW: David Lew, Region 1.

MR. MILLER: I'm Hub Miller, Regional Administrator.

MR. ZWOLINSKI: I'm Jack Zwolinski. I'm the Director of the Division of Licensing and Project Management, Headquarters.

MR. STROSNIDER: Jack Strosnider, Director of the Division of Engineering, NRC Headquarters.

MR. MURPHY: Emmett Murphy, Materials Engineer with the NRR.

MR. RAYMOND: Bill Raymond, NRC Senior Resident at the site.

MR. ROBERTS: John Roberts, New York State Public Service Commission.

MR. McCANN: Good afternoon. My name is John McCann. I'm the Manager of Nuclear Safety and Licensing.

MR. GAYNOR: Doug Gaynor. I'm with the Risk Assessment Group.

MR. ESSELMAN: Tom Esselman, consulting with Con Ed.

MR. GROTH: John Groth, Chief of Nuclear Operations, Con Ed.

MR. BAUMSTARK: Jim Baumstark, Vice President, Nuclear Safety.

MR. PARRY: Jack Parry, Project Manager.

MR. ASHCRAFT: John Ashcraft, Con Ed.

MR. POINDEXTER: Tom Poindexter, Winston and Strawn, counsel for Con Ed.

MR. BRANDENBURG: Brent Brandenburg, Assistant General Counsel, Con Ed.

MR. PITTERLE: Tom Pitterle, Westinghouse.

MR. BALLINGER: Ron Ballinger, Altran.

MR. McBRINE: Bill McBrine, Altran.

MR. HENRY: I'm Bob Henry, Fauske and Associates.

MR. COWAN: Bart Cowan, Eckert Seamans.

MR. ADAMONIS: D. C. Adamonis from Westinghouse.

MR. MARIS: Jim Maris, Westinghouse.

MR. IRA: Steve Ira, Westinghouse.

AUDIENCE: Members of the public.

MR. YEROKUN: Jimi Yerokun, NRR, Region 1.

MR. BARBER: Scott Barber, DRP, NRC.

MR. RAFKY: Michael Rafky, Office of the General Counsel.

MS. YOUNG: Mitzi Young, Office of the General Counsel.

MR. MILANO: Pat Milano, NRR Projects.

MR. HABIGHORST: Pete Habighorst, NRC Resident Inspector, Indian Point.

MR. LEHMAN: Jim Lehman, I'm the Deputy Director, Office of Enforcement.

MR. SULLIVAN: Edmund Sullivan, NRC.

MR. LONG: Steve Long, NRR.

MR. ESELGROTH: Peter Eselgroth, Branch Chief for IP 2 in the Region.

MR. BLOUGH: Randy Blough, Director of Reactor Projects, Region 1.

MR. MADISON: Alan Madison, Special Programming, NRR.

MR. HOLODY: Don Holody, Team Leader, Region 1 Enforcement/Allegation Staff.

MR. SHEDLOSKY: Tom Shedlosky, Senior

Reactor Specialist.

MR. URBAN: Rick Urban, Senior Enforcement Specialist, Region 1.

MR. FEWELL: I'm Brad Fewell. I'm the Regional Counsel, Region 1.

MR. HOLIAN: I'm briefly going to cover the agenda on the slide which today is as follows: Following these opening comments, Dan Holody of NRC will present background information on the regulatory conference process and the relationship of the new reactor oversight program.

Dave Lew here to my right will then provide a summary of the steam generator tube leak event, the NRC event response, and the NRC inspection findings.

Jim Trapp, our Senior Reactor Analyst, one of our two analysts, will present the NRC assessment of risk.

And a majority of the meeting will be the licensee's presentation by Con Edison. There will most probably be a short caucus and a brief wrap-up from the NRC perspective.

I presume sometime during the Con Edison presentation, especially if it's like the evening meeting on the 11th, we might need a five-minute break in the middle of that.

At this time, I'll just give a brief few sentence summary of the inspection report that was conducted earlier this year.

The NRC found that the 1997 steam generator inservice examinations were program deficient in several respects, as summarized in the cover letter for that August 31st event. Despite opportunities, Con Edison did not recognize and take appropriate corrective actions for significant conditions adverse to quality that affected the steam generator inspection program.

Con Edison did not adequately account for conditions that adversely affected the detectability of and increased susceptibility to tube flaws.

Dave Lew will talk in a little more detail about those inspection findings in his presentation. The NRC did present these inspection findings and a summary of our risk assessment of the condition of the generator during this period in our August 31st inspection report.

The main purpose of today's regulatory

conference is to provide an opportunity for Con Edison to present information that may affect the NRC conclusions with regard to both the inspection findings and our risk assessment.

No decisions will be made at today's meeting. These regulatory conference meetings are an opportunity to obtain information on what we've published and put out following our inspection. The NRC will consider the information we obtain today and transmit by letter the final NRC conclusions regarding both the inspection findings and the risk assessments.

Additional opening comments?

MR. MILLER: No, just that as Brian said, our main purpose here is to hear from you. And we will, for completeness really of the record, review again our findings. I think you will recognize them. They're the issues and the findings that were documented in our inspection report, but I think it's important we go through that, and we'll do that briefly. But our main purpose is to listen to you. And in that regard, there are quite a few people here from NRC, but we're here to make sure that we do hear what you have to say.

And what we will likely do, in order to assure that we get the information that's needed and we understand what you present to us, we'll likely caucus -- I think Brian mentioned that, I don't recall -- and make sure that we ask all of the questions we can possibly ask here today, if that's possible.

And so again, there are quite a few people here, but it's for a reason. It's to make sure we really get all of what we need and not have to do a follow-up.

MR. HOLIAN: The caucus will be for those final questions. We will be interrupting you during your presentation on certain slides no doubt.

MR. MILLER: Just the last small thing, and that is if there are members of the public here, I don't want to insist that you do it, but if you want to introduce yourself, I want to give you the opportunity. It's not required. Let's go ahead.

MR. HOLODY: Good afternoon. My name is Dan Holody. I'm the Team Leader of the Region 1 Enforcement/Allegation Staff.

As Mr. Holian had indicated, today the NRC is conducting a regulatory conference with Con

Edison. The conference will cover the NRC findings as well as an apparent violation associated with Con Edison's conduct of its 1997 inspection of its steam generators at Indian Point 2.

Since the meeting is open for public observation, and there is at least a member of the public here today, and since the meeting is transcribed, a copy of which will be made public, I'll take just a few minutes to briefly provide some background on the regulatory conference as well as the role or the place in the NRC evaluation of the NRC inspection findings.

This regulatory conference is part of the NRC reactor oversight process for dealing with inspection findings and performance issues at nuclear facilities. Using a process for determining the significance of findings called the Significance Determination Process, or SDP, the findings are assessed based on safety and risk significance. As part of this process, the findings are primarily assigned a color of green, white, yellow, or red, with green being the least significant and red being the most significant.

Whenever a potentially risk significant

finding is identified and characterized by the Significance Determination Process as white, yellow, or red, the licensee, Consolidated Edison in this case, is provided an opportunity to attend a regulatory conference.

The purpose of this conference is to discuss the findings, the performance issues, the preliminary significance determination, the related potential violations, root causes, and corrective actions.

In the NRC inspection report issued on August 31st of this year, the NRC preliminarily categorized the significance of the deficient Con Edison 1997 steam generator inspections at Indian Point 2 as red. Mr. James Trapp, our Senior Reactor Analyst, will later discuss our bases for this conclusion.

The NRC report also described the apparent violations as Con Edison's failure to identify and either adjust or modify the inspection methods and analyses so as to account for significant conditions that affected the quality of Con Edison's 1997 steam generator inspections. Con Edison subsequently requested this regulatory conference be held to discuss its position on the finding and its significance, including the bases for its position.

This regulatory conference is essentially the last step of the process before the NRC makes its final decision on the significance of the inspection findings. The purpose of this conference is not to negotiate the significance of the finding or any resulting enforcement action. Rather, the purpose is to allow the NRC to obtain information from Con Edison that will assist us in determining the appropriate significance determination.

We will seek to assure a common understanding of the facts and a common understanding of the assumptions and factors used to determine the significance of the findings.

During the conference, the licensee can disagree with the NRC policies and positions and provide its bases. Con Edison may also provide any other information it deems relevant to the significance determination of this case, including its position on the content and accuracy of the NRC inspection report findings. The basis for any such challenges will be discussed.

It is important to emphasize that the decision to conduct this conference does not mean that the NRC has made a final decision with respect to the significance determination or the apparent violation or what enforcement action is warranted. The apparent violation is subject to further review. The NRC will evaluate the information presented today, along with the inspection report findings, and then make a final significance determination and enforcement decision.

We will strive to issue our final decision A.S.A.P., in the worst case, within 30 days. It is possible, however, that given the nature of these findings, issuance of the final decision in this case may take longer than desired.

Finally, prior to turning this meeting over to Mr. Lew, I note that any statements or opinions made by NRC staff at this conference should not be viewed as a final NRC position. Similarly, the lack of an NRC response to a statement by Con Edison should not be viewed as NRC acceptance of that position.

With that said, I'll turn the meeting

over to Mr. Lew.

MR. LEW: My name is David Lew. I was the manager for the NRC steam generator special inspection. I plan to briefly cover the event, the NRC response, and NRC findings for the steam generator special inspection.

I guess first I'll start off with the event itself. On February 15th, 2000, the steam generator tube failure occurred at the Indian Point 2 reactor facility. This resulted in an initial primary-to-secondary leak of about a hundred 46 gallons per minute. Con Edison declared an alert and initiated a manual reactor trip before identifying and isolating the source of the leak.

Con Edison successfully mitigated the event and placed the plant in cold shutdown. While there was a minor radiological release to the environment, this release was well within regulatory limits, was not detected offsite, and the event did not impact public health and safety.

Subsequent to the event, Con Edison conducted inspections of the steam generators. They found that the failure was located at the apex of tube R2C5, which is a low row tube. As they continued eddy current testing of the steam generator, the number of defects identified placed two of the steam generators in technical specification category of C-3, which required NRC approval for restart with the existing steam generators.

The NRC immediately responded to the event with follow-up and monitoring by the residents and the region based inspectors and also by NRC managers and technical staff in the Region 1 office.

An Augmented Inspection Team, AIT, was conducted from February 18th through March 3rd, and an AIT follow-up was conducted from May 15th to May 26th to review the safety implications and the associated licensee actions in response to the steam generator tube failure.

The cause of the tube failure itself was not reviewed by the AIT and the AIT follow-up inspection. Instead, the NRC conducted a special inspection from March 7th to July 20th to review the causes of the failure and the adequacy of Con Edison's performance during the 1997 steam generator inservice inspections. This inspection consisted of personnel from Region 1, the Office of Nuclear Reactor Regulation, as well as NRC contractor specialists in steam generator eddy current testing. The findings of this inspection are the topic of this regulatory conference.

Overall, we concluded that the direction and execution of the 1997 steam generator inservice examinations were deficient in several respects. Despite opportunities, Con Edison did not recognize and take appropriate corrective actions for significant conditions adverse to quality that affected the steam generator inspection program.

Con Edison did not adequately account for conditions which adversely affected the detectability of and increase the susceptibility of tube flaws.

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During the 1997 inspections, a primary water stress corrosion cracking, PWSCC, defect was found for the first time at the apex of a row two U-tube. The significance was not understood by Con Edison. The appearance of one defect signifies the potential for similar cracks in other low row tubes. Such PWSCC apex flaws are considered significant because they have been associated with through wall leakage and bursting.

Con Edison did not perform an adequate

evaluation of the cause and the susceptibility of low row tubes to PWSCC, the extent to which this degradation existed, and the increased probability of such a defect to rupture during operation.

Con Edison did not adequately evaluate the increased possibility of hour-glassing, which is a precursor for PWSCC in the apex of low volt tube, or question the adequacy of visual inspections for hour-glassing. The issue was not entered into the corrective action system and the tube was simply plugged.

Also during the 1997 inspections, tube denting in low row tubes was identified for the first time. Restrictions were encountered at the upper support plates as eddy current test probes were inserted into 19 tubes. This can indicate the possibility of hour-glassing. This hour-glassing increased the stresses at the U-bend apex of the tubes, which in turn are the leading contributor to low row U-bend apex PWSCC.

Con Edison did not perform an adequate evaluation for potential of hour-glassing, nor did they establish procedures or examination criteria to determine if such hour-glassing was occurring.

Again, this issue was not entered into the corrective action program.

Significant eddy current testing noise interference was encountered with the data of the low row tubes. This significant noise level reduced the probability of identifying an existing PWSCC tube defect. However, the 1997 steam generator inspection program was not adjusted to compensate for the negative effects of this noise in detecting flaws, particularly when conditions existed indicating an increased susceptibility to PWSCC.

For example, a more careful examination of available data was not performed. Detailed careful review of the 1997 data could have identified four defects, including the one tube that failed, which was R2C5.

Collectively, the failure to adequately address these conditions contributed to the tubes with PWSCC flaws in small radius U-bends being left in service until the failure of one of these tubes on February 15th.

That basically covers the inspection findings. I'll turn it over to Jim Trapp to discuss the risks associated with those findings.

MR. TRAPP: Good afternoon. My name is Jim Trapp, and I'm one of the Senior Reactor Analysts in Region 1. And I'm going to briefly discuss the risk significance evaluation performed to determine the risk associated with these findings.

The risk assessment was performed in accordance with the revised oversight program inspection manual chapter 0609. The inspection manual chapter provides three phases of risk assessments that increase in sophistication.

The phase 1 screen is performed to determine if additional analysis of the finding is necessary. Phase 2 utilizes pre-established sequences from the IPE to quantify the risk. Phase 3 evaluations are performed using best available risk information to more accurately characterize the risk of the findings. All three phases of the SDP were performed for these findings.

The SDP determines the potential risk associated with existing conditions. It is not limited to evaluating only actual consequences. For example, if all the diesel generators are found inoperable for a significant duration, yet offsite power is not lost during the period that the diesels are inoperable, the actual consequences of this condition would be negligible.

However, the change in core damage frequency, delta CDF, and overall risk of this condition would be significant. In the case of IP2 steam generator findings, poor quality steam generator tube inspections in 1997 would increase the likelihood of a steam generator tube rupture, which is a significant event, and therefore, these findings would be risk significant.

Steam generator tube rupture events are significant because by their nature, this type of accident degrades both the RCS and containment fission product boundaries. Therefore, they will increase both the probability of core damage and the release of radiation to the environment.

The NRC phase one and two SDP, significant determination process evaluations determined that these findings were potentially highly risk significant. Therefore, a phase 3 evaluation was performed by the PRA branch of NRR. The key assumptions in the phase 3 analysis are:

Number one, that the initiating event

frequency for a steam generator tube failure is one per year. This assumption is based on the as-left condition of the steam generator tubes in 1997 and the actual steam generator tube failure history.

Number 2, half the steam generator tube failures will result in a steam generator tube rupture. This assumption is based roughly on in service tube failures experienced throughout the industry.

The third assumption is that the delta CDF is approximately equivalent to the delta LERF, or the large early release frequency is approximately equal to the change in the core damage frequency. This assumption is based on observations made by the NRC in NUREG 1560 that while most steam generator tube rupture core damage events result from a stuck open secondary steam relief valve which allows a direct fission product flow path from the core to the environment.

In addition to spontaneous steam generator tube failures, phase 3 analysis evaluation also included evaluation of other initiators which could induce a steam generator tube failure. These are events that increase the pressure differential across a cracked steam generator tube which could induce the tube to rupture. The accident initiators considered were secondary side system faults, anticipated transients without scram, and severe accidents. In addition, core damage results from other causes such as station blackout which can result in elevated tube temperatures that can also lead to failure of degraded steam generator tubes were also considered.

Also inspection manual chapter 0609 establishes four risk thresholds for risk significance for both the delta CDF and delta LERF. The findings are assigned a color based on risk significance with green being the least risk significant and red being the most risk significant. The risk threshold for a red finding is delta CDF greater than 1E-4 or a delta LERF of greater than 1E-5. Each decade reduction in delta CDF or LERF will result in a reduction of this significance color.

The results of the NRC's phase 3 risk assessment were documented in Attachment 2 of our inspection report 2000-7. The delta CDF and delta LERF were determined to be 1 times 10 to the minus

4. This would be indicative of a high risk significant or a red finding.

This concludes my comments regarding the NRC's risk determination for these findings.

MR. HOLIAN: Are there any initial issues from NRC, NRR, anything else? Mr. Groth.

MR. GROTH: Good afternoon. We appreciate the opportunity to come and present information. Particularly, we appreciate this opportunity to come and discuss with the Nuclear Regulatory Commission particularly the phase 3 self-assessment or risk assessment in regards to plant specific information.

We'll spend quite a bit of time talking about that this afternoon and try and share with you our perspective on plant specific information, and that will deal with Cycle 14 risk, and also with the risk on February 15th.

With that, Jim, if you would, please.

MR. BAUMSTARK: Good afternoon. The purpose of Con Ed's position in this meeting this afternoon is to present our phase 3 analysis in accordance with the significance determination process.

As stated in manual chapter 609, we will provide plant specific risk perspectives and related information, including alternative risk and engineering analysis, to support reducing the significance issues associated with our recent steam generator tubing.

Concerning the 15 February event itself, using probabilistic risk assessment insights, we are in agreement that there was low to moderate risk to the health and safety of the public. Today's discussion will focus on the risks associated with Cycle 14 operation. We will describe why from a fracture mechanic and probabilistic perspective, a leak in row 2 column 5 was not going to progress to a rupture.

We will describe why other tubes with defects as determined in 2000 were not going to rupture based on in situ testing. We will describe why the probability of a rupture based on tubes left in service is much lower by an order of magnitude than that described in the inspection report.

And finally, we will describe why, for the spontaneous rupture case, delta core damage frequency is not equal to delta large early release frequency.

We will also -- as discussed in our 20 July inspection debrief and in the inspection report, we remain convinced that the 1997 inspection met the requirements of then current industry guidelines. Based on lessons learned as a result of that inspection, we will outline measures to prevent recurrence.

The inspection report itself states that the affected tube row 2 column 5 did not rupture as the leakage was observed to be about a hundred 50 gallons per minute and leak flow from ruptures leading to core damage is on the order of magnitude of 4 to 6 hundred gallons per minute.

We will show that the probability of the leak we experienced progressing to a level exceeding the capacity of our charging pumps or some 225 gallons per minute is an order of magnitude less than that stated in the inspection report. As a result, our analysis shows a white delta core damage frequency and a yellow delta large early release frequency for Cycle 14 operation.

A key factor which helps put the event in perspective is the extensive in situ testing conducted in 2000. This real world end of cycle demonstration was conducted on 51 tubes, 48 with indications, including all 23 tubes with crack-like indications in both U-bends and straight leg segments.

This testing program went well beyond industry guidelines, and included the three tubes with no indications to see if we could grow cracks under test pressure conditions. All tubes met three delta P burst margin criteria with negligible leakage at steam line break conditions. This testing demonstrated that the row 2 column 5 leakage was not indicative of multiple pending tube leaks.

Therefore, since the leak from row 2 column 5 did not and would not progress to a rupture, and since in situ testing demonstrated that no tube with crack-like indications detected in 2000 would have burst, we are convinced that the probability of leaving a tube in service during Cycle 14 that could have burst is far less than the .5 discussed in the inspection report.

Tom Esselman will begin with a discussion this afternoon of the mechanics of the row 2 column 5 failure and the likelihood of tube failure. Tom?

MR. ESSELMAN: The progression from an understanding of the mechanisms and an understanding of the experience that has occurred over at Indian Point 2 over the last couple of years is really an important part of leading to the probability of failure calculation.

What I would like to do is to discuss and identify and define the primary water stress corrosion cracking mechanisms that have occurred in the steam generator tubes, then I want to take the Indian Point 2 specific experience with crack growth and the failure in R2C5, and to look at that plus the results of the eddy current test plus the results of all the stress analyses that we've done that we reported over the last several meetings to try to provide a foundation to allow us to look at the likelihood of tube rupture and to draw some reasonable conclusions.

It's clear that to discuss the row 2 U-bend behavior, we need to relate it to the occurrence of hour-glassing. Only to provide a very brief description of what's going on, if I take a single tube and assume a line of symmetry, what's happening is at approximately two inches below the transition region is the top of the top tube support plate.

The top of the tube support plate, the top tube support plate, because of denting that's occurring, is causing the tube support plate points to move together. It's really that motion together that's giving us a condition -- giving us a state of stress that we've been most interested in up near the apex that we can relate to the occurrence of primary water stress corrosion cracking. And we can also relate to the extent of cracking. We're interested not only in the occurrence, but in the extent over which that cracking has occurred.

So we really want to take the stresses and the mechanism and talk about the behavior, especially R2 -- row 2 column 5, where we have some evidence of what's happening.

The primary water stress corrosion cracking mechanism is known to have cracks initiating at multiple sites. It occurs in a chemistry that induces it with susceptible material with a state of stress.

We've previously discussed at the

meeting the relationships that have been developed for returns of stress corrosion cracking versus the stress as a function of ratio of stress to yield stress. And we will talk a little bit more about that today. But you need to have sufficient stress, and the stress is occurring at the apex of the tube.

The cracks initiate at multiple sites. They're known to initiate as small cracks, much like thumbnail cracks. They don't initiate as two-inch long cracks that are 30 inches, three mills through wall, they initiate as thumbnail cracks and then eventually they will grow and link to form larger cracks. That is they'll grow deeper and longer and they'll eventually link. And I'll show you a picture of this in a minute.

Eventually, you get to the point where the higher aspect ratio cracks, these are linked cracks that now are still shallow but yet pick up length -- the aspect being the ratio of the length to the depth -- will grow until the stress in the remaining ligament exceeds the material failure stress. So you get to the point where these are growing through the tube thickness to the point where the remaining ligament is capable, just capable of withstanding the load or the stress that's induced. And when that stress equals a failure stress, you'll get that crack to penetrate the wall.

We've worked with Professor Ron Ballinger, who's here today, who's worked with us, also who is at MIT, who spent the last 15 or 20 years or so working on stress corrosion cracking in inconnel tube, both from a laboratory point of view where he's been inducing cracks in the lab and also from looking at experiences from the field.

There are a lot of examples of this, but this is an example of a mill annealed inconnel 600 tube that was cracked by primary water stress corrosion cracking in a primary water condition but from the OD of the tube.

You can see the tube outside diameter. This was in a primary water environment. And this tube, even though in this photograph appears to be circumferential shape, these are axial cracks.

Now, if you look at this, you can see the thumbnail cracks that I'm referring to or individual cracks that form and they will link. Because when these two cracks A and B are linked, there is adjacent to, but not yet linked with C. But as these grow through the wall -- and this is the outside surface of these look like they're about 30 percent through wall at this point -- as they grow, they will continue to link. The result will be a fairly flat crack, obviously with this kind of shape, but it will grow through wall and have associated with it -- and this is important as we look at the probability -- it will have associated with it a depth of crack and also a length of crack. And it's a length of crack that will be important as we look to see how long a crack will open to give us a flow rate.

Evidence of this linking of cracks can be seen in several different ways from the Indian Point tubes. I've shown an EC scan from -- an eddy current scan from a row 2 column 69 tube where this is along -- this is the apex, this is along the axial orientation, and this is the depth of the crack. If you look at these cracks that are noted by eddy current testing, you can see evidence of linking of these cracks. And these are multiple cracks that will form. They'll generally be colinear because of the stress, but they may be offset slightly from one another. And that is -- that's evident as we look further, particularly at row 2 column 5.

Could we turn the lights off for a few minutes?

This is a montage of row 2 column 5 failure. This is taken from a video that was taken from the inside of the tube. And this shows the failure from one end of the tube out to the other end of the tube. The failure was approximately 2 and a half inches long, 2.4 inches I believe.

And you can see, number one, a primary failure zone. This also is a straight crack. The apparent turning is really a video image issue in developing a montage. It's generally a straight crack across the apex.

You can see one primary flaw where you can see evidence of individual -- the roughness here and the slight linear differences are evidence of cracks that have formed. And then when the failure has progressed, it's gone through wall at one point, and when it's progressed, there's a linkage of these cracks that are partial through wall.

What's clear in looking at the primary

crack -- two things are clear. Number one, this image is obviously a depressurized condition. In the plant, when that is pressurized and leaking, we expect that this was open at the mid point probably twice as much, roughly twice as much as it is here.

But what's evident even in this image that the ligaments as you look across here are all broken. Particularly when this is expanded further, we believe that it's clear and, you know, obviously looking at this thing in more detail than what you can do here, by looking and relooking at the videotape, it's clear that the ligaments in the primary crack are broken. That is, that in the primary crack, the opening that you achieve from one end to the other was maximum.

What you can see at the other end of this though is a shorter crack that is -- that progresses -- and you can visually see this coming out here just a bit -- progresses in its darker -- but it's not linked to the same level where these cracks are. This lack of linkage means that this crack -- also it's not perfectly clear whether this crack is through wall or just visually appearing like this. In fact, it could be through wall in the

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fact that it's open so much and not particularly tight indicates that it could be. And that is linked on the outside diameter of the wall.

What's clear is that this crack, which flowed through, clearly through this primary crack region, could have been several tenths of an inch longer.

The other thing that's clear when we've looked at the videotape in detail and looked at the results of eddy current, particularly clear at the right-hand side is that you have a region where the crack clearly has stopped. You can see plasticity at that end. And we'll talk a little bit about crack behavior in the presence of a sharp tip. But yet in the review of this end, it's clear that there's plasticity. And in reviewing the images in that direction, there's no evidence that is all evident of any additional cracking being available. It appears like it has gotten to the point where there's no additional cracks -- that no existing additional preexisting cracks that that would have run into it. Question?

MR. LONG: Steve Long, NRC. I understood you to say that the crack was essentially axial with no circumferential aspect to it at all. And is that what you meant? Because it looks even in say that frame right there like that's the axial direction, there's just within that frame quite a change in direction.

MR. ESSELMAN: In looking at the videotape, it's difficult to be absolutely precise as to where you are circumferentially.

We are confident for two reasons. Number one, just because of the linear nature of this and based upon the expectation of where the cracks need to be that provide the mechanism for linkage, as you go around the wall, and for instance this went 45 degrees or 90 degrees around the wall, we know that the stresses are not sufficient that far around the wall to give you the preexisting cracks.

So it's a challenge to maintain circumferential orientation as you look at this. We believe that this is linking of cracks that are around the apex.

MR. LONG: I don't want to pursue this very long, but when we were looking at this before, we were under the impression that the way the camera took pictures essentially gave you, for a frame, some idea that this was along the axis. You're saying that the axis of the tube was actually like this?

MR. ESSELMAN: That's what we believe, yes.

MR. LONG: Okay.

MR. MURPHY: This is Emmett Murphy of NRC. There were striation markings, imperfections, linear imperfections on the tube surface that appeared to be axial against which you could compare the orientation of this crack.

MR. ESSELMAN: That would be the result of forming.

MR. MURPHY: Perhaps as a result of forming, but which would suggest that this crack does have a skew as described. And it becomes pronounced, perhaps approaching 45 percent on the right-hand side of the picture, the montage here.

MR. ESSELMAN: Okay, I guess we could go back and re-review that again and look for that. I guess we believe that, number one, we believe that it's axial. Frankly, whether it's axial or skewed a bit is less important, except that it's related to the stress distribution in the presence of cracks. We believe that even down in this region, there's evidence of linking of cracks as you go down to that end.

MR. MILLER: You're saying that that tail -- just to ask a question as a layman here --

MR. ESSELMAN: On the right-hand side?

MR. MILLER: -- is not really a tail. It's some artifact of the way the camera worked, that in your view that this thing is a straight line more or less along the axial direction at the top?

MR. ESSELMAN: More or less along the axis.

MR. MILLER: It's not sort of a component that you do a vector and transverse it. It's all kind of in line?

MR. ESSELMAN: We believe that it's primarily longitudinal. We think that there's some stepping and non-colinearity, but primarily we believe that it's longitudinal, and that is around the axis, around the apex.

What's evident at the right-hand end of this is that you have -- is that you have a crack arrest with a region of plasticity. And there's no evidence of additional cracking at that end.

At the left-hand side, there is evidence of a crack that was not linked, but that crack appears to go a relatively short distance past the primary crack and would be -- could have added several tenths of an inch to the length of that crack.

We also reviewed eddy current data that was taken to look at the regions to the upstream and downstream of this crack. Also saw no evidence of any cracking that was existing at either end of this flaw.

We know -- and there's numbers that are used relative to the flow rate. It was -- the flow rate that was calculated from the plant was a hundred 9 GPM from this flaw. We also can relate that flow rate -- and I will talk later about the relationship between that flow rate to that length, this length to that flow rate to the flow rate that have come from other U-bend cracks that will allow us then also, as we look at this further, to relate this crack to other U-bend cracks, particularly with respect to the flow rate that's coming out of the crack.

MR. ZWOLINSKI: Did I hear you mention a figure, a hundred 9?

MR. ESSELMAN: Yes.

MR. ZWOLINSKI: Is that at the beginning of crack initiation or is that at the end when you terminated the event?

MR. HOLIAN: I'll back up on that question also that John is asking. And you mentioned it in your opening comments about being different than the flow rate in the inspection report. And I was going to address it then, but I let it go.

The AIT report which was previous to this '97 inspection report was to three different flow rates for the event. Which I think what John Zwolinski is getting at, you have one flow rate when the plant is at primary pressure, and which according to the AIT report, two charging pumps could maintain.

Then you had the plant dropping the pressure where there was a less of a DP through the crack. And by that time, there was a water balance being done with changing temperature, and that was where you get that approximate flow rate of a hundred 40 gallons a minute. And you're also saying a hundred 9. It's a different number than we've gotten. Whether that's down at a lower pressure or it's down at a lower rate at less of a pressure. Similar question.

The only other thought I had, when you read the '97 inspection report, the recent one, you mention an order of magnitude down. And I think what you were referring to was the tube rupture flow rate that's used in the SDP risk assessment.

MR. BAUMSTARK: That's correct. And that's why we arbitrarily -- because we knew the numbers were out there and they were a little bit different. The 109 and the 141 assumed a value of 225 for the cutoff point that we established when we did our analysis.

MR. HOLIAN: We want to revisit a little bit. And John, I don't know if that answers your question. But in any event, I think it's important just for this discussion here that the SDP analysis assumes a flow rate based on a tube rupture. And that's taken into account with what probability that a tube failure, which is less of a flow rate, would propagate. I mean you had to make that assumption first and then you're still dealing within risk space and the risk of a full tube rupture, if it were to go to that.

MR. BAUMSTARK: For purposes of discussion, we're going to start off with that leak and show that is the order of significance of .5 and above.

MR. HOLIAN: I guess just for the record, whatever the leak rate is for this individual event is one issue. And the issue is under risk phase for the condition of what tube failure might have progressed to is another issue.

Does that answer it?

MR. ZWOLINSKI: Yes.

MR. LONG: This is Steve Long again. Since the hundred 9 or whatever is going to be used as data in their development of the distribution of leak rates, I still want to pursue it for a minute.

Can you explain to us how you developed the estimate of a hundred 9? We understand I guess it was in the log that at the time the plant was tripped, there were two charging pumps running and that the pressurizer level was increasing at that point, indicating that those two charging pumps would not have been keeping up with the flow rate through the failure in the tube. So what I'm looking for is an explanation of the derivation of a hundred 9 and the relationship to the log rates.

MR. BAUMSTARK: I don't know that we're prepared to discuss that. We'd have to go back and look at the log entry and review that as part of the process. Off the top of my head, I'm not prepared to discuss whether it was a hundred 9 or a hundred 40.

MR. LONG: Do you know where the hundred 9 came from?

MR. BAUMSTARK: In our SL1 report, in our investigation of that, that's what we came up with, 109 gallons. AIT came in later and determined a different value. I'd have to go back and contrast the elements that were taken into account to develop those numbers.

MR. LONG: Can you just tell me how you developed a hundred 9 independent of what we did?

MR. BAUMSTARK: Not without a review of the SL1 report.

MR. LONG: Okay.

MR. ESSELMAN: The question in

reviewing this, as we go back to the mechanism, is really why does it behave that way. We know that mill annealed alloy 600 is susceptible to PWSCC, again when you get the stress up near or above the yield stress.

However, from a crack propagation point of view, the material is extremely ductile. It has a very high toughness value, which is a measure of its resistance to critical crack growth or unstable crack growth. And it is clear that in the presence of a sharp flaw that this material will blunt, and that is that you get a region of a large plasticity -- a large plastic region at the crack tip that will have the crack tip not behave as a crack through propagation but as a material with the effect of the crack diminished greatly.

With crack blunting and with high toughness, you can determine that crack propagation -- and this is through a remaining ligament or from a running crack -- will be through overload. And that is when the stress in the material, in the ligaments, exceed the failure stress and not by unstable crack growth.

In order to demonstrate this, we took a

piece of mill annealed 600 material, it's very similar to the material that is in these tubes. It happens to be 62 mills thick instead of 50 mills thick. But we took a piece of this, we machined a notch in the center of this approximately three-quarters of an inch it looks like, half inch, maybe a half inch, and then we fatigued a crack into this specimen by putting cyclic load on it so that we ended up with a fatigue crack by fatigue, but a sharp crack at each end of the notch.

We then took that and began to load it so that the load in the section went above yield. And in fact, the behavior of the crack which is first shown here, the fatigue crack is very tight. And it was evident microscopically, but was not evident in this picture. As we started to load it, the fatigue crack opened. And at a very early part of the process, you can see that you get what is really a classical region of yield at the crack tip. This is the way that you would expect it to yield at the crack tip.

And as we loaded it further, you can see that the crack, which never extended, opened up and continued to open until it eventually was as wide as the initial notch and behaved much like a notch without a crack would behave -- much as a notch without a crack.

What's happening here is that the effect of the crack tip is greatly blunted by this material, and that the failure of the remaining sections, that is the section -- the failure of the remaining section from the crack tip on out occurs when the stress in that section reaches the failure stress.

If you have a less tough material, obviously you can get to the point where this crack will run unstably and give you crack propagation without a great deal of plasticity. This material is a very tough material and it behaves.

MR. STROSNIDER: Was the test that you're illustrating here, was this run under a displacement control or a load control situation?

MR. BALLINGER: Load control.

MR. STROSNIDER: This was load control. I was curious, because typically, when you do this sort of analysis, the length -- or experiment, the length to which the crack will propagate is depending upon the compliance with the system and the loading. And I'm wondering if you're suggesting that there's some compliance effects when you introduce and limit the length of the crack.

Because if you hung a weight on that and you reached a point where that thing started to tear, if it were totally load controlled, it would tear all the way through. And this is a simple tearing modulus comparing sort of evaluation.

So I understand what you're saying, but I'm curious if you're going to suggest that there's something in the introduction of the U-bend in the pipe due to compliance, given the pressure is pretty much in a controlled situation.

MR. ESSELMAN: The suggestion that we will make is that the extent of the crack propagation would be dependent upon the stress in the ligament that it will run into. The suggestion and belief that we have is that the crack will propagate through regions that are precracked, where there's existing cracks.

And as evident by R2C5, it ran through existing cracks, linked cracks, and stopped when you get to a region where there's no preexisting cracks, where you know that just the pressure stresses are low and the pressure stresses, even with the effect of the wedging that you get from a crack, are also below the failure stress.

MR. STROSNIDER: Not to belabor this, but I guess the point you want to make is that this crack really didn't have a critical crack length when it was through the wall.

MR. ESSELMAN: Correct. That failure didn't occur because of fracture mechanics, it occurred because the stress in the uncracked ligament, the uncracked region exceeded the failure of stress, the ultimate stress.

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MR. MURPHY: Let me -- I'm not quite sure of the point of this discussion. You're saying that U-bends are incapable of undergoing a classical burst type failure? Is that --

MR. ESSELMAN: No, it's not what I'm suggesting. I'm suggesting that if you pressurize the U-bend absent preexisting cracks, you can get the stress in the whole section to the point where it's high enough that you can get a crack to run, because, though, the stress in the section is initially high because it's an unflocked section.

If you're running, though, with a

preexisting crack, with an 80 or 90 percent depth -- and an average depth is up in the 80 percent region -- the uncracked ligament, the stresses are the pressure stress where in fact they're 10 or 12 or 15,000 PSI, and that crack will not progress at those pressures into that uncracked ligament.

MR. MURPHY: Perhaps let me ask a follow-up question.

Tom, Westinghouse, in its reports on behalf of Indian Point, have described a series of burst tests conducted on U-bend sections, conducted on the EDM notch, for purposes of demonstrating a higher burst strength for the U-bend section when compared to a similar crack in a straight length portion of tubing. Can I infer from the report of these tests that you were in fact achieving a burst configuration for the U-bend notch?

MR. PITTERLE: There was some tearing at the tips of the crack. There was not a lot of opening in the crack in those tests. It was a little bit through the semantics as to how you're going to define the condition of the tube after that test.

MR. MURPHY: You got some crack

extension, but you didn't get a large opening; is what you're saying?

MR. PITTERLE: Right.

MR. ESSELMAN: You also can take R2C5, and if you were able to seal the crack and pressurize it, you can get that crack to extend, but yet it will extend by pressurizing it, again in a burst test, when the yield stress or the stress in the uncracked ligaments again will exceed the ultimate.

And I think the burst testing that was done where you have an EDM notch that's sealed so that you can pressurize it, you again get the bulk material away from the notch up to a very high stress. So that when you reinitiate cracking, your section is at a very high stress level.

When you're in operation and you have a preexisting flaw, the stress is away from the flaw, particularly in the U-bend where you have compression at the outer fibers. For instance, where you have a state of bending in addition through the extrados, the state of stress away from the flaw is not conducive to that flaw running into it in an unstable fashion.

MR. MURPHY: One additional question. We didn't have any photographs available to us of what the Surry ruptured tube looked like. Could you just briefly characterize what the cracked tube looked like in that case?

MR. PITTERLE: The IDN load is very much like the picture shown, and the extrados was about four inches in length, but there is no -- it's just a separation.

MR. MURPHY: Okay.

MR. MILLER: Is there a contention here -- again asking as a layman almost -- unless you have preexisting cracks, you won't have a tear much more than what the preexisting cracks exists? That you don't believe that it's a sort of thing where you have cracks and then the initial opening and then the linkage, you have small ligaments and that won't take the pressure and tearing beyond that, unzips, if you will, to use a gross term here, unzips beyond the boundaries of the initial crack? Is that your contention here?

MR. BALLINGER: Correct, correct. In other words, the material is just simply tough enough so that for the operating pressures that you have here, that when you exceed the region where you have preexisting IBSCC cracks, the material thickness and toughness are such as that the crack just won't run.

MR. SCHMIDT: So then it's critical to find the cracks before they can link up.

MR. ESSELMAN: Yes. Number one. But number two, it also allows you, if I can try to take the statement of what we're looking at, it also allows you then to look at the extent of preexisting cracks to define the potential extent of the crack should it go through wall and extend across the entire length of the preexisting cracks that would link in that mechanism.

MR. LONG: Okay. Well, then I guess what we're really interested in is how long a preexisting crack could you really have gotten. You know, primary water stress corrosion cracking in the apex, is there some limit to the stress field or something that will give you a maximum length there?

MR. ESSELMAN: Yes. Yes. And we want to go in a couple of overheads to exactly that. We would like to -- we believe that that's meaningful to look at the extent of cracking that exists and why it exists that way as a way to define how long the crack might be. So we will go there.

MR. MURPHY: Just one final question for you -- and this is anticipating where you're going with this discussion -- I think part of the point you're making up until this point is that under normal operating conditions, it's unlikely that you could achieve a classic vision of burst configuration. Are you also saying that this situation is applied or may be seen in break conditions?

And let me explain why I ask the question. I note that one might expect on the basis of relationships that have been published relating burst pressure as a function of crack length, through wall crack length, that that relationship reaches an asymptote at around 1,000 PSI, based on 1,100 PSI, based on average material properties and straight length tubing, and considering that you have a strength hardening effect in the U-bends and the effective yield is higher, and therefore the burst strength would be expected to be higher, that asymptote then would be probably very close to normal operating pressure conditions and may not be perhaps much of a surprise from that standpoint, that you might not get a fish mouth configuration under normal operating conditions, but perhaps you could get a fish mouth configuration under mainstream conditions.

MR. ESSELMAN: I would need to look at your asymptotes and the limits in a little more detail before I would say that.

But I will comment more later about the behavior of a crack running around the U-bend relative to it opening up in a fish mouth. A two and a half inch crack -- only to jump ahead -- a two and a half inch long crack like the one we had here, if it occurred in a straight leg, would have opened up more and give you a higher flow rate than it will if it opened in a strength hardened and in a geometrically constrained region like the apex of a U-bend. And I'll show you data also that shows that from the U-bend area that we have.

MR. LONG: Steve Long again. Just one more thing. Would that also apply to a crack if it had a significant circumferential aspect to it? In other words, if this crack was not running axially, but going partially around the tube?

MR. ESSELMAN: Short of circumferential, I guess I would hate to take that to a limit. If that had some circumferential aspect where it ran around 10 or 20 or 30 degrees, that would apply then also. Clearly, if you're running it circumferentially, you're into a different mechanism, but I believe that that would apply also.

So we've together developed the fact that where I'm going is to take the length of preexisting cracks to relate that to the potential length of a crack once it ran.

We've taken the -- I'm sorry, there's one intermediate point. Evident again in the videotape, the behavior around the crack head is also evident. And it's clearer in the videotape, and we were not able to grab it. But yet this kind of plasticity with the 45-degree regions of clear plasticity is also evident at the crack tip in the tube in the montage. And it's clearer in the videotape where you can see the images of this plasticity occurring.

So the -- of interest is the extent of cracking, the axial length of cracking. Number one, where did we see it, what was the extent of what we saw. And number two, what was the -- what's the potential limit, why is it occurring where it's occurring.

What I've done is taken a row 2 U-bend. All the eddy current testing is performed so that flaws and indications in the U-bend are zeroed against the top of the tube support plate, which gives a clear indication so that you can see that as we develop the entire region, we start at a 10-degree point at 2.7 inches -- actually the transition is about 2.06 inches above the top of the tube support plate. So that as we develop this from 10 degrees around to a hundred 70 degrees or zero to a hundred 80, we're running in distances from the top of the number 6 tube support plate.

What I've done in this chart is plotted, relative to the apex or the 90-degree point, which is at 7.4 inches, all of the flaws that were detected, including R2C5, which was a 2.4 inch flaw. So what I've done is I've developed, if you will, the U-bend, and I plotted relative to the apex the location of all the flaws and the length. And this is from -- the data that I've taken is from the 800 kilohertz eddy current tests that were performed in 2000.

The 2.4 inches was the longest. We have flaws that if connected were 2.1 inches, 2.0 inches, and then many of the other flaws were --
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four of the other flaws were much shorter.

The occurrence of these flaws which are due to stress corrosion cracking are related to the stress. If we go back to the discussion that we had at the last meeting, or one of the last meetings, we've taken a U-bend and have analyzed it with hour-glassing. And I plotted here the extrados hoop stress -- and this is on the inside surface. The stress that's going to initiate cracking is the stress on the inside surface of the extrados in the hoop direction, because that's what's going to open up the axial cracks that have been seen.

This is a plot of that stress. And the yield stress limits of this material -- this is an elastic-plastic analysis of 0.476 hour-glassing. The yield stress of this material was around 66,000. So that you can see that the yielding is occurring where this is flattening out. And in fact, the yielding in the high stress region is around -- centered around the apex. Again, 7.4 inches is at the apex where this is the maximum, and then it's centered around there.

If we look back at the cracks, we can see that they're also centered around the apex with a slight skew, especially in R2C5, to the hot leg side. This is the hot leg side and that's the cold leg side.

We've also performed analyses that --

MR. LEW: Just a question. Going back to the previous slide where you were trying to plot the links, it looks like 2.4 inches, R2C5 obviously was about 800 kilohertz eddy current testing. But basically the rest of the data there is based on the capability of detectability of flaws basically above the threshold.

I'm just curious, you know, whether or not that would be -- you actually -- if you can detect some of the flaws and a certain probability of flaws which may extend the links that are shown up there. How much was that?

MR. ESSELMAN: There certainly is a possibility that there could be smaller flaws on the side of these.

What we did in looking at the eddy

current data and the plots that showed the depth versus length, as we looked at the last recorded points as the flaws went down to the zero, and the last recorded points or points that were noted at a given depth were approximately 30 percent through wall. We believe that the existence of additional cracks beyond these regions could exist, but they would be generally below 30 percent through wall.

Eventually we will consider, even though this crack is 2.4 inches, as we look at the likelihood of having a tube rupture, we have considered cracks that have greater length than these cracks in the determination of probability of failure. So we'll talk again about the potential for having cracks longer than these.

MR. LEW: But this is the input into determination of the distribution?

MR. ESSELMAN: This was a part of the input to determine that distribution. What we also did though was, understanding that there is a potential for them to be longer, was that we added many more cracks that were longer than the 2.4 inches that are shown here.

MR. RAYMOND: Are you going to speak to

why you picked .476 as the assumed hour-glassing?

MR. ESSELMAN: I certainly can. We measured two locations in the steam generators. .476 was the maximum that was measured.

At the last meeting, I believe the New York meeting, we, following a request to look at larger hour-glassing, because we did not look at every flow slot, we looked at hour-glassing I believe up to .6 inches, or -- I think it was either .6 or .7, I don't remember precisely. But yet we've looked at larger amounts of hour-glassing to assure that the mechanism and the behavior is very similar.

And with larger amount of hour-glassing, given the facts that we're dealing with an elastic-plastic analysis, the conclusions are the same even if the hour-glassing is slightly larger.

MR. SCHMIDT: By looked at, you mean analyzed?

MR. ESSELMAN: We analyzed larger, yes. We measured .476, and that's why the analysis that I reported was .476, but we analyzed -- we looked at the effects of hour-glassing my recollection is up to .6, maybe a little bit larger.

MR. BLOUGH: On the other slide, do you have a theory as to why the two other largest cracks besides the one that failed are more or less symmetrical around the apex, whereas the one that failed is more canted toward the hot leg side? And again, I'm just wondering about, you know, what the detectability is for the front end crack.

MR. ESSELMAN: We would expect from the analyses for these to be slightly skewed to the hot leg side. The reason for that is because -- the reason for that is this: In this analysis, we've taken uniform hot leg/cold leg rows. In fact, what occurs in the plant is you get ununiform hot leg/cold leg rows. And that is the hot leg grows more than the cold leg, especially in a predented condition. And so it will grow slightly.

And we know that by superimposing this differential growth on top of the growth that we just showed, again this is with an elastic summation. So this somewhat over-emphasizes the effects that this will have. But because of that, we know that there will be a slight skewing to the hot leg side, not a significant skewing, but yet we've looked at this and have understood that you would expect the stresses on the hot leg side to be slightly larger than the cold leg side.

Clearly though, your inner region up here that's very flat, and you would expect to be a distribution around the apex with what we've seen is a slight skew to the hot leg side.

MR. BLOUGH: Just so I understand the answer, your explanation as to why the failed one is more skewed to the hot leg than the other ones was just random distribution.

You would expect them all to be canted a little bit toward the hot leg side, but have a distribution around that point. And so the two other longest ones, there's not as much of a cant toward the hot leg side, if any. But the failed one is substantially canted toward the hot leg side. You would say that that's probabilistic.

MR. ESSELMAN: I think that that grew that way. There's a relatively small number of cracks, a relatively small number that grew, and that one just happened to grow to the hot leg side. As we look at it though, we believe that there should be a slight preference to the hot leg side.

MR. SCHMIDT: On the R2C5, which

section is the section away from the extrados or towards the extrados, do you know? Do you understand the question?

MR. ESSELMAN: Yeah, I believe that they're all on the extrados. I don't know which end is the hot leg side and which -- I don't know which end is the hot leg and which end is the cold leg, and I don't recall which is which.

MR. MILLER: Extrados is what?

MR. ESSELMAN: That's the top edge of the tube, which is the extrados. And we struggled with this a great deal. We're dealing with the inside surface on the extrados where the cracks are initiating. And in the axial direction, they're occurring axially. So that what we're dealing with is approximately one inch on one side, one inch on the other side of the row 2 tube.

MR. MILLER: Okay.

MR. ESSELMAN: We've gone back and forth enough on that. I think that this is a little bit redundant, but what we've done is plotted those flaws on top of the stress distribution and have seen that where you would expect -- where there's a potential to have stress corrosion cracks is where the stress is relatively high.

In this behavior, there's a point where you fall off a cliff, if you will, in stress, where basically you would not expect to see any additional stress corrosion cracking once you go beyond here, for two reasons.

Number one, the progression is going to be from the high stress to the low stress. And what happened is that we found this, this had manifested itself before it had grown out to the edges of these, but yet there's also a threshold that's just very close to this yield stress point where you would not expect -- and like 90 percent of the yield stress -- where you would not expect the material to be susceptible to stress corrosion cracking. So where we're seeing the cracks is a region where we have high hoop stresses that would induce the axial cracks that we've been seeing.

The other thing that -- if I could go one more step and then take a question -- the other thing that's important is that even though this looks relatively flat, again we know that we're dealing with a stress to the fourth relationship or initiation of cracking.

What I've done in the next overhead is just taken that middle plot and plotted it as a stress to the fourth power to show that in fact this isn't a region from a PWSCC susceptibility or stress corrosion cracking susceptibility. This isn't a flat region of susceptibility based on the stress and the relationship of the yield stress. You would expect this to start at the apex and then grow in the same direction but have a marked relationship as it moves away from the apex.

MR. STROSNIDER: That's for initiation, right?

MR. ESSELMAN: Yes.

MR. STROSNIDER: Have you looked at the susceptibility of growth using a stress intensity threshold?

MR. ESSELMAN: We've looked at the stress distribution, and once a crack initiates, you get a state of tension on the ID and compression on the OD. And the crack grows across the area. You're going for altering the stress.

Once it initiates, there's not a great tendency one way or the other based on the stress within. It's really the stress initiation that is required in order to get the crack started. Once it starts, the crack will be similar in this couple of examples. Any other questions?

MR. BARBER: Scott Barber. A general materials properties question. I thought you had said earlier that the actual yield stress in the material was, I don't know, in the mid 60,000 PSI range. And yet what you're showing is, by my reading of your figures, is that in fact the normal stress levels exceed that.

From a design standpoint, I'm not sure why the -- maybe there's something unique about the configuration, the denting, what have you, but it almost appeared as if that was part of the original design. And wouldn't that normally -- wouldn't it normally be the case if you were designing a tube that you would design it to operate in the elastic range and not the plastic range?

MR. ESSELMAN: What's really clear in this region is absent hour-glassing that has occurred only with progression with time, the pressure stresses up in this region are in the region of 15,000 to 18,000 PSI. So it's really the effects of hour-glassing that have occurred over time that give you stresses that approach this.

You're pinching the tube and you're inducing high stress, bending stresses really across the tube.

MR. BARBER: Wouldn't that also lead credence to our contention or our finding that hour-glassing had a significant effect that should have been, you know, evaluated in detail? Because I mean what you're describing is in fact -- I guess I'm envisioning if you transpose the curve, it would be much lower, same shape but much lower, yet you have a phenomenon that once it occurs causes a significant change in the way the material behaves.

MR. ESSELMAN: Clearly, hour-glassing has contributed to the stress condition that's allowed you to have cracks in these tubes that have led to this failure. Absent the hour-glassing, the stresses are not high enough to give you this kind of stress corrosion cracking.

So from a crack stability point of view, what we've talked about is the cracks initiating, they grow, and they link. We believe that linked cracks will grow through wall and then extend axially by linking with adjacent cracks.

We believe that the extent of cracking seen in R2C5 with the apparent absence of any additional cracks with the 2000 eddy current test data showing cracks in the two-inch range and smaller, clearly with a potential of having undetectable flaws beyond that. But we think that the potential to grow through wall and then extend by axially linking with adjacent cracks is limited to the approximately two and a half inch region.

We also know that the high toughness will inhibit crack propagation in the areas that have no cracks or in areas that we believe have 20 or 30 percent through wall penetration.

We've taken 30 percent deep cracks adjacent to a through wall crack and looked at the stress. And the stresses in those regions are not sufficient to cause that to propagate. So we believe that the high toughness of the material will inhibit crack propagation beyond the regions where you have relatively deep preexisting flaws.

It is with that that we provide a limitation both in the stress distribution which you have through around the axial -- around the axial orientation of the extrados, and you also have limitation due to the high toughness of the material.

Given that though, as a foundation to the probability analysis, we've also considered the likelihood -- and I'll talk in more detail about how we've chosen a distribution to consider probability of failures.

But with the next thing that is required though, once you have an indication of the length of the crack -- and we've touched on this a little bit earlier -- is to correlate the flow rate through a crack with the length of a crack. What we know is that an equivalent U-bend crack will result in a smaller flow rate than the same length of a straight leg crack.

There's two things that are happening. The geometry constraint is clear in a straight length where if you have a two or three-inch long crack, that the flaps, if you will, or the edges are relatively unconstrained to fish mouth and yield. In this where you are actually taking a crack and having it run along the surface, you're getting a lot of out-of-plane or bending resistance, bending stiffness that lowers the likelihood of that fish mouthing. And so the amount of opening that you'll get in a U-bend crack is less than what you'll get in a straight leg.

Also as was pointed out, these are work hardened due to the bending so that the material properties in the U-bend -- the yield stresses which you have to exceed in order to get fish mouthing is much higher than it is in the straight leg due to the work hardening due to bending, approximately 50 percent higher.

So for both of those reasons, you get less flow out of a U-bend crack than a straight leg. And if we look at industry data on that, we've taken the stress corrosion cracks that were reported in NUREG 6365, which looked back over 20 years of history, and chose -- and reported -- described both the mechanisms of the cracking and reported the length and flow rate out of the cracks. We've eliminated from this cracks like cracks that resulted from ABB wear and loose parts and things like that.

What we've reported here is cracks in the straight tubes have followed this curve. And these are three data points that are reported there.

Prior to the Indian Point 2 experience, there's been a Doel 2 row 1 U-bend and a Surry 2 row 1 U-bend failure. The Indian Point data fits, with approximately 2.4 inches, and again into a range of flow rates.

MR. MURPHY: The one observation, one question you've shown. Your 109 GPM for the IP-2 failure there -- and of course our estimate is up around 140, 150.

Secondly, do we know how the Doel leakage is calculated? Do we know enough to put the Doel leak rate on this chart?

MR. ESSELMAN: The Doel leak rate was reported in the NUREG, and that was an Idaho National Engineering Labs, and I know they spent a lot of time.

MR. MURPHY: Did the Idaho Lab calculate that leak rate or just using a leak rate reported by the Belgians?

MR. ESSELMAN: I don't know.

MR. MURPHY: To the extent that this curve is the basis for the analysis, I guess I'd have to wonder about including the Doel. Maybe we should know more about how the Doel number was determined.

MR. ESSELMAN: I agree. In fact, what we have done -- and this is to step ahead a bit only to answer this question -- what we've done, when we've used this data to calculate flow rate, we have essentially eliminated Doel and run the line through Surry 2 and Indian Point so as to skew the line upward by about 25 percent. So the Doel data point does fall below that, and the relationship that we use eventually to link crack rate to flow rate is actually a line that's higher than the one shown here.

MR. MURPHY: Okay. I guess just to revisit where we've been then, because this is kind of -- we've reached here I think a major point in your presentation -- we have the experience at Indian Point 2 and we have the experience at Surry. One involved a two and a half inch long crack, the other involved a four-inch long crack.

I think your point is basically neither one of these resulted in a classic burst with a fish mouth, and that each one of these was much less than that, basically just a plastic opening with a crack, period. But it's two data points.

And again, I would refer you to that EPRI burst data where the burst pressure is related to crack -- through wall crack length. And it would seem from that information, and considering the strength hardening, that perhaps there is a good chance that a given tube with a crack that's quite long, in the order of two inches or three inches, couldn't burst under normal operating conditions no matter how long the crack was.

On the other hand, it wouldn't seem clear from that data that the probability might not be more than 25, or something on that order, that two out of two are not bursts here, perhaps the third or the fourth could be of a burst based on what one might infer from that EPRI burst pressure relationship.

MR. PITTERLE: I don't think that the pressure is high enough that you would not be above the bursting. That's pretty high. In any of these lengths and gaps, you're going to be above and expect it to burst.

MR. MURPHY: As I recall, the normal operating pressure for Indian Point is 1530, as I recall. And if you refer to the EPRI curve at 2.4

inches and you apply the spring adjustment involved in the COA report, you come up with a burst pressure of 1470 PSI. And that's for a nominal, average flow stress.

So the fact that we have two long cracks that have been experienced, and neither one of them resulted in a rupture in a normal operating experience is not inconsistent with that chart. On the other hand, one would not infer from that chart that the probability of getting a rupture -- a burst was zero as opposed to .5. I would think that one would still assume that, based on that curve, that the probability was on the order of .5 based on the proximity of 1470 to 1530. And so based on the proximity of the nominal burst pressure of 1470 to the nominate operating pressure differential of 1530.

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MR. ESSELMAN: Excuse me. You get a burst pressure of 1470 based upon what depth and length of crack?

MR. MURPHY: Based on a through wall, a hundred percent through wall, 2.4 inches long.

MR. PITTERLE: It could not have been through wall prior to its tearing, because the leak rate was two and a half GPM.

MR. MURPHY: Okay, but it opened. And now you've got a big leak. And that crack opened up. The liquid is gone. And now you have a through wall crack that's 2.4 inches long. So the question is why didn't it burst.

It's being presented that it can't burst. And I'm just observing that sure, it's entirely possible that in two specific cases that you might well have gotten a burst, but that doesn't necessarily mean that it's not where some situations might not result in a classic burst.

MR. PITTERLE: I think we'll be able to show on the zero and the three, but we do have to confirm it.

MR. MILLER: Recognizing that correlating flow rates with size of the ultimate tear or crack, was there any examination that you know of of these other cases, Fort Calhoun, Palo Verde, McGuire, looking back at preexisting conditions in a manner similar to what you've done here to add confidence to this view that you have that it's the preexisting condition that determines length?

MR. ESSELMAN: All of these -- all five of these, other than Indian Point, were linked to the presence of stress corrosion cracks. And there was a basis developed in the NUREG for the extent of the stress corrosion cracking and the reason why it was there. And why, for instance, this one crack in that leak rate, the stress corrosion cracking occurred or existed at a region where there was a score mark or a defect that was in the tube. And it was actually along that where there was an OD stress corrosion crack. And that occurred at a defect in the tube.

The NUREG goes through in good detail and discusses these. These are all related to stress corrosion cracking, in the presence of stress corrosion cracking.

MR. MILLER: Did you tell me that yes, there were looks at the preexisting conditions on those two, knowing what cracks were there in the eddy current testing done prior to those cracks, looked at it, and it was lined up with the final length of the crack was no greater than the connection of the individual cracks for the rest of the point where you don't have -- the answer might be no. I just want to know if there is anything else.

MR. McBRINE: Which points for those?

MR. ZWOLINSKI: For these points.

MR. MILLER: I recognize this curve has nothing to do, it just causes me to think they're making comparisons with other plants, and so I'm asking the question does Westinghouse or anybody else have data that looked back at these other plants, a similar analysis to what you've done at Indian Point 2? Do you have other data that buttresses your contention here that the ultimate crack is a long, preexisting defect?

MR. ESSELMAN: We've looked at -- we've relied on the NUREG and the description that's there. We know that many of these were pulled, many of these had great detail failure analyses done on them. And it was really the summary of those analyses with the references that were put into the NUREG. So I believe that they were all not casually analyzed, but were linked back to the mechanisms. I know that all of them were removed. I believe that the rest of them were. I know Surry 2 was removed also.

MR. MILLER: The answer that I'm hearing is that they were related generally to PWSCC, but you don't know the question of whether or not there was a study done of preexisting cracks as they would have indicated -- as they would have been indicated in eddy current testing in the manner done here and then make a correlation. I'm not challenging one way or the other, I'm just asking the question.

MR. ESSELMAN: I'd have to look.

MR. PITTERLE: We didn't do it at Palo Verde. The McGuire tube had substantial shallow cracking beyond the length of what's shown here. It did not tear all the way through the cracking. It was probably measured in feet in that particular case. Is that what you're looking for?

MR. STROSNIDER: I think so. You've raised an interesting point with the possibility that, because of the geometry of the U-bend, there may be some different failure mode for cracks here. You know -- and so I think you made some possibility there. I mean listening to this, there may be some geometry effect or something involved.

But I guess what I'd suggest is we

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ought to move on and see how you plan to apply that. I'm not sure how significant it is when you get to the risk assessment.

MR. ESSELMAN: If you can only make one additional comment. Emmett, your comment is that well, these could have been fish mouth. I don't think that this was an accident that these were down here and not up here where fish mouth is. I believe that the geometric constraints and the fact that they're in the U-bend does limit the amount that they were open. The fact that there is strength hardening and they're much stronger. The fact that, as you run any of the distances around the apex, you're going to get a lot of geometric constraints. It's different and it's not going to open.

MR. MURPHY: I agree. My comments did not reflect directly the geometric constraints. It only accounted for strength hardening. But again, your demonstration of the geometry effect applies to normal operating conditions, and we don't have much corroborating information to go to a higher pressure.

MR. ESSELMAN: So what are we going to do? I guess what I wanted to do is to describe a basic mechanism that's associated with what's going on at Indian point 2.

We talked about the PWSCC mechanisms. We've talked about R2C5. And really the important point from this is not necessarily the link back to the existence of stress corrosion cracking, but it's a link to what the flow is out of a crack, a full crack of a given length. And we've used this only for a flow rate comparison.

The real probability of failure question is given what we know now, what was the probability of a steam generator tube rupture during the last operating cycle. So you partially need to take 2000 data and use it to infer what was in existence in 1997, and then ask about the likelihood that those conditions could have led to a tube rupture.

We did not have a tube rupture. R2C5, the leakage, whichever leakage you use, was below the leakage that would have led to a tube rupture. The 225 GPM threshold certainly is well below a much higher flow rate.

Given the relative flatness of this curve, if it had been a couple of a tenths of an inch longer and opened fully, there also would not have been a tube rupture. So the task really is to infer a set of conditions going back to 1997 that the conditions were such that they could have occurred so that a tube rupture, that is a break greater than 225 GPM, could have occurred.

And our task and what we wanted to do was to try to walk through the process from having a tube with a defect, with the behavior that we've linked, and get to the point where we're going to say what's the likelihood that those tubes would have a tube rupture.

What we've done in order to do that is that we've defined a series of progressive events, if you will, that are associated with tube failure. Again, this starts with a tube in service with or without a defect leading to a through wall crack that extends a certain length and gives you a certain flow rate. What we've done for each of those progressive events is we've defined a probability associated with each of those based upon the IP-2 conditions.

Now, it is, to define the conditions in 1997, the most accurate inspection data that we have and the most accurate data that exists was performed in 2000, and we've used a lot of 2000 data to infer what the conditions were in 1997. But in fact we want to go back to 1997, knowing what we know now, and predict or calculate a probability of steam generator tube rupture.

What we've done with this series of events, and we've performed a Monte Carlo analysis to ask the question how often does that go through wall, how often does the flow rate exceed 225 GPM.

And what I would like to do is to walk through that process.

MR. BLOUGH: Can I ask a question about the basic process used? What was the end point of the analysis? Was it February 15th when the cycle ended by the tube failure, or did you actually project ahead to June, which would have been the end of the cycle if it hadn't been ended prematurely?

MR. ESSELMAN: We ran it for the full cycle. So we took -- we took the potential of having two full years, or approximately two years of crack growth on a preexisting flaw to see whether it would go through wall or not.

So the events that we've defined in the

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Monte Carlo analysis is to first start with the number of tubes with undetected cracks. And I will -- let me just define the events, and we'll come back and talk about each one, and then I'll have a summary table that will relate what we've assumed in the analysis versus what we've seen.

We've presumed then in a tube that has an undetected crack the depth of the crack. So we have a distribution associated with the depth of the crack. We then -- and this is the depth of the cracks that exist at the beginning of cycle. You then have a crack growth rate. The crack will grow at a certain rate over the entire cycle. And what that will get you to on a single tube is the depth of a crack with a crack growth rate. And you then can have a criteria, actually a distribution for whether that crack will penetrate the wall or not. And I'll describe that also.

Once it penetrates the wall, again the mechanism -- this process will begin by a single deep crack that can be very limited in length penetrating the wall. But then the question is given then you create the potential that that will unzip, we believe that the unzipping will go as far as the distance that you have preexisting cracks. So we ask then what's the potential axial length of that crack based upon the mechanisms that we've seen and based upon the possible lengths of the cracks based on the stress distribution. And then we've calculated the flow rate through that crack given the length of the crack.

Let me take each of these and briefly describe the distribution and then I have a summary table.

Postulated number of tubes with undetected cracks. Again, this is the beginning of the cycle. We have seven U-bends with axial indications identified in the 2000 inspection. In the Monte Carlo analysis, we've put in a hundred tubes with undetected cracks. We chose a number that we felt was very conservative in that we don't believe that there are a hundred tubes that have undetected cracks.

Why don't we believe that? We also know, and we've defined in other meetings, the fact that all the tubes don't have uniform susceptibility to cracking. There's a stress to yield stress relationship that we've talked about before. And there are a series of -- a set of tubes in row two that are very unsusceptible to cracking because of the high strength of the tubes and the relatively low stress. We do believe that cracking will start in the most susceptible tubes and then progress through sequential tubes as they become susceptible.

So having a small number of indications is what we've seen and we believe put in a very, very large number of tubes that have cracks.

We've put in initial crack depth from zero percent to 90 percent. The population of the over 50 percent through wall cracks that we've allowed to exist in the Monte Carlo analysis has greatly exceeded the number of 50 percent through wall cracks that we found in the 2000 inspection. The 2000 inspection, even given that those cracks added two extra years of crack growth. And I'll provide you some numbers in a minute.

The postulated crack growth rate, we've assumed crack growth rate of 4 percent to 20 percent through wall per year. With the 2000 inspection data, we've reported CMOA crack growth rates of zero percent to 16 percent through wall per effective full power year, with most cracks growing below eight percent per year. I believe 60 percent or so of those crack growth rates were below eight percent per year. We've used a crack growth rate that's slightly larger than that noted.

Postulated crack penetration of wall. We've taken what we believe is a conservative probability, saying that a hundred percent probability of through wall penetration for a crack that is 80 percent -- that has grown to be 80 percent through wall in depth. We also have a lower probability that even lower cracks could penetrate the wall. Yes, sir.

MR. LONG: Okay, this seems like it might be key. Steve Long. If you can explain this to me a little bit more.

You're saying as you go through the Monte Carlo, I guess you're assuming that when you reach 80 percent through wall, I guess average depth for the crack, you have a hundred percent probability of detecting a leak?

MR. ESSELMAN: You have a hundred percent probability of it penetrating the wall.

MR. STROSNIDER: Is that the same as saying the remaining ligament fails?

MR. ESSELMAN: Yes. You haven't yet defined though how long it is. What you've defined at that point in the Monte Carlo analysis is that you have a leak. You have a crack that is through wall. And then the question you have to ask is what then is the axial length. But yet you basically have a failure at that point.

MR. LONG: So what you're saying is basically you will shutdown for some reason, detectable leakage or some sort of gross failure at an average depth of 80 percent through wall?

MR. ESSELMAN: Not quite. We're saying in the Monte Carlo analysis that when this crack which we're artificially growing in the Monte Carlo analysis to see how long it becomes, when it reaches 80 percent through wall, it's going to go through wall. That then will give you a leakage rate that is, again given the extent and the length of these, is such that you will detect it and will give you a longer shutdown.

MR. LONG: It will give you an observable event in operation; is that what you're saying?

MR. ESSELMAN: Yes.

MR. LONG: Remind me what the average depth of the crack was in 1997.

MR. ESSELMAN: I'll get to that in a minute, if I can. Because there were many -- there were a number of cracks that were greater than 50 percent through wall on average depth.

And then relative to axial length of crack, we talked about this, the cracks being no longer, what we've seen being no longer than two and a half inches. We've assumed a distribution of axial cracks that range from zero up to four and a half inches long. So we've basically allowed the cracks, once it goes through wall, once we've exceeded 80 percent, it penetrates the wall, we've then allowed it to go up to four and a half inches long. The highest probability is for crack in the two to two and a half inch range.

But 37 percent of the cracks, that is that once we have this distribution, 37 percent of the cracks would have lengths greater than two and a half inches.

MR. STROSNIDER: You've given the ranges of the distribution. What are the shapes for these, uniform, normal?

MR. ESSELMAN: I can describe them in a minute. Let me give you parameters, then I'll describe them.

And then the flow rate, once you have the length of the crack, you then have a flow rate that we've used the NUREG data. As I indicated, we skewed it up to the Indian Point and Surry data. And then the question you asked if leakage occurred, was it above or below 225 GPM.

MR. MURPHY: And NUREG 6365 was which NUREG?

MR. ESSELMAN: That's the INEL report. That reports on all the tube ruptures that have occurred over the last 10 years. That's the one where we got that flow rate data from.

MR. MURPHY: Oh, okay. So you're using the curve in that data?

MR. ESSELMAN: Yes. The only thing that we've done is adjusted it so we skewed it from that line that we've drawn in the curve up so it bounds the Indian Point and Surry points.

If we look at and compare what was found at Indian Point 2 in 2000 to what was put into the Monte Carlo analysis, and also as I go through this and discuss the distributions a little bit, we have found in Indian Point 2, seven U-tubes that had cracks, we've provided for a hundred in the distribution. Relative the depth of cracks, we have the number of tubes exceeding 50 percent through wall are four tubes. What we've done in the distribution is allowed 19 tubes or 19 percent of the tubes to exceed 50 percent through wall.

The distribution had approximately 50 percent of the tubes in the zero to 30 percent region, and it went out to probably 3 percent or 4 percent, that were up in the 70 to 90 percent region. The total tubes that we allowed to exceed 50 percent through wall was 19.

The details of the distributions I don't have at my finger tips right now, but it was basically went out to 70 to 90 percent through wall at roughly 3 percent. 50 to 70 would have been about 17 or 16 percent of the tubes would be 50 to 70 degree range.

MR. MURPHY: What would have been the associated probability of leaving the indication that was measured for R2C5 in 1997, what would be the probability for being out of service in this scenario?

MR. ESSELMAN: That was approximately -- the 1997 depth was probably --

MR. PITTERLE: The full length was 70. The shorter segment was probably around 80.

MR. ESSELMAN: Around 70, that would have been in the 50 to 70 range. It would have been like 19 percent or maybe a little bit lower.

MR. LONG: That's 19 percent probability as to the depth. How about combining that with the probability for length as well?

MR. ESSELMAN: Which is the axial length of the flaw, we've allowed there to be 37 tubes or 37 percent of the tubes greater than two and a half inches and no distribution growth up to a smaller number greater than four, four and a half inch range.

MR. LONG: So you're saying 37 percent of the cracks are longer than 2.5 and about -- what was that 19 percent were more than 70 percent through wall?

MR. ESSELMAN: 19 percent greater than 50 percent through wall. And when they penetrated the wall, 37 percent of them would go longer than two and a half inches.

MR. LONG: So I guess a probability of about seven percent that something might be worse than what you had instead of better.

MR. ESSELMAN: If you take the probability that you had 70 percent and it was longer than two and a half inches, that's probably right.

MR. LONG: As opposed to something like 50-50 worse or better than you observed. You're saying that what you observed is a fairly unlikely outcome of not knowing what you left in service because of the noise?

MR. ESSELMAN: Given the relatively small number of flaws and given that that was a singular flaw, then we would conclude, yeah, that it was unlikely. But you know, I can -- I wouldn't want to do the arithmetic, but your number was seven percent or 10 percent or something like that.

MR. LONG: I'm saying of the flaws that you're running through the Monte Carlo, you're running more than the found in service, so there's a normalization issue.

MR. ESSELMAN: Right.

MR. LONG: Where you're trying to be conservative, but of the ones that you run through the Monte Carlo, it looks like seven percent of them would be on the worse side of what you observed with that one flaw and, you know, the other 93 percent would be better in either length or depth.

MR. ESSELMAN: Obviously there's a lot more flaws than we postulated that are not as bad as R2C5. The fact also is that when you get to the calculation of the leakage rate, R2C5 also wasn't a tube rupture and that you needed to have a flaw longer than that in order to have it exceed the 225 GPM threshold.

MR. LONG: We'll get to that in a minute.

MR. MURPHY: So what you're doing is you're not evaluating the significance, the risk significance of leaving a tube that looks like R2C5 in service, you're doing something different.

MR. ESSELMAN: If you just leave R2C5 in service, you will get what we got from R2C5, and that's a leak. What we've needed to do in order to get the probability of a steam rupture --

MR. MURPHY: They were uncertain. You

don't know the crack growth. But in the other analysis, you haven't applied it yet, but if you postulate that you start the cycle with a flaw that looks like R2C5 in '97 and then, you know, if you just start a cycle with a flaw that looks like that and then you try to estimate, you know, what are the potential consequences of leaving that flaw in service, given that I have it, you know, there's potential for growth rates that might be applied for that kind of thing. The flaw may grow in many different ways. But that's not what you've done here.

MR. ESSELMAN: Well, I believe it is, because what we've done is we've allowed flaws that are worse than R2C5 to occur.

MR. MURPHY: There's only a seven percent chance though that we would have a flaw equal to or worse than R2C5.

MR. ESSELMAN: Let me also say that I'm uncomfortable, you know, picking numbers that way, because I guess I'd like to verify that number or at least withhold my ability to verify it later.

But yet we create the ability, and if that's the percentage with a certain percentage, then we can have something worse than R2C5, yes.

MR. LONG: Let me help here a minute. What they've done is taken a hundred flaws per Monte Carlo, per iteration in the Monte Carlo analysis, and assumed that seven of those are worse than or the same as the one that failed. So that's not necessarily saying they've got a seven percent chance of one being worse than what failed, they're saying there's -- they're putting seven like that in each iteration of the analysis, as I understand it.

MR. ESSELMAN: Well, yeah, except that you first have to put one in that that's deep, and then you enter the length of the flaw. And you do have to look at the combination of the likelihood of the remaining Monte Carlo steps to get to the likelihood that there's a flaw worse than R2C5.

The fact that on the next overhead I'll present to you the probability of a leakage rate greater than 225 GPM. Every one of those was worse than R2C5. And so I believe that what we're doing is creating the potential in each Monte Carlo run, because you're right, every 10,000 times you take a hundred flaws and you assign depth to every tube, there are a hundred tubes you assign depth, a crack growth rate, you ask whether it goes through wall and you assign an axial rate and get a flow rate.

So given that we know what R2C5 was -- we need to create the potential to get flaws that were worse than R2C5 that will give us --

MR. LONG: I understand what you're doing. I think I just want to make the correction that it wasn't seven percent. You were putting in seven flaws like that per iteration on the average.

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MR. ESSELMAN: No, I don't think that that's right. Because frankly this, relative to the crack growth rate, this is less interesting than this is relative to the flow rate. What's important is the length. And the important thing about R2C5 was not necessarily that it -- well, there's two things that are important. It went through wall and it leaked, but the length of R2C5 was important.

We have allowed there to be a lot of flaws go through wall. Whether it's R2C5 or not R2C5, we have created -- you take tubes exceeding 50 percent through wall and up to 20 percent but yet greater than eight with a distribution that goes out greater than eight percent, you have a lot of tubes go through wall.

What's important for R2C5 is the axial length of the flaw. And we've let 37 percent of the flaws that go through wall to be longer than R2C5.

MR. LONG: Right, but some of those may have been zero percent deep.

MR. ESSELMAN: Well, you don't assign this until it goes through wall. And a lot of these go through wall. We've assigned this probability to a lot of tubes that go through wall, and that's 37 percent.

MR. LONG: All right. You're assigning -- let's try to get this straight one more time.

You start, with each Monte Carlo iteration, you start off with a hundred tubes. You're going to assign some depths, maybe zero, may go up to 90 percent. And then you're going to grow those hundred tubes at some variable growth rate, each one.

The ones that go through wall somewhere -- I've got two questions -- they're kind of confusing here. The ones that go through wall you're going to assign a length to and then you'll decide what the leak rate is.

Now, one of the things that's bothering me is you're starting with 90 percent through wall and you're going to grow them for two years and a minimum of four percent. So it's pretty hard to have them not go through wall if they start at 90. But you also said they have a hundred percent probability of leakage of being observed at 80.

MR. ESSELMAN: We've obviously got some overlap that says that we put some flaws in a relatively low number with an average depth up to 90 percent that immediately pass the other criteria. So we've conservatively, I think, probably looking at the numbers, left some overlap in there. So clearly, if you have anything that's in the 70 to 90 percent region, they go through wall with near certainty.

MR. LONG: But the 90 percenters would have to start short.

MR. ESSELMAN: The length of these is not particularly important. It's secondarily important to whether they go through wall or not. You can have a half inch flaw that's 80 percent through wall that will go through because of a ligament. What you do get is a stiffening effect.

So if it's an eighth of an inch, it won't go through wall.

MR. LONG: Let me just make the point so you understand and let's go ahead. My concern is that in the mathematics you're doing, you're doing something that's not realistic. You're starting with some fraction of the flaws that would be immediately observed as leaks or ruptures because you're starting with some of them at 90 percent through wall. And then you're assigning a length, I guess. I'm not sure exactly where that goes in terms of being conservative or non-conservative in your analysis. It's going to depend on exactly how you made the decisions in the Monte Carlo. But it seems pretty unrealistic, and that's why I was asking.

MR. ESSELMAN: The overlap that we've created is unrealistic that says that we've put in a flaw that will fail the criteria for through wall penetration. More appropriate would be something that didn't rupture the day before you shut down that you missed that could be up to 80 percent through wall that you put it back in service and it would go right away.

MR. LONG: The question is what's the shape of the distribution percent through wall.

MR. ESSELMAN: I could dig that out as we talk. The shape of the distribution of the percent through wall -- again my recollection is that we have 50 percent in the zero to 30 percent. We have greater -- 50 percent that are greater than that. We probably have three or four percent that are 70 to 90. And I believe -- well, this is 19 percent exceeding 50. We probably have 16 percent or 15 percent that are 50 to 70, and 38 or 40 percent that's 70 to 90. And then --

MR. LONG: I can't write that fast. Let me request that we get those distributions, okay?

MR. ESSELMAN: I had expected that through this we would be able to supply those.

MR. BARBER: Scott Barber. I just have a general question about the assumed values for the depths and the ratio. I mean obviously you have -- here you're showing that the number of U-bends that had cracks in the '97 inspection were seven, and yet in four out of seven of those, they were greater than 50 percent. Why wouldn't you have a similar correlation for your Monte Carlo? Why would you assume that instead of having 56 or 57 or 58 percent greater than 50 percent through wall, only 19 percent are through wall because you've, in fact by doing that, you've skewed the results, you know, downward.

MR. ESSELMAN: But yet in the inspection in 2000, the flaws that are most likely to be missed are not the deep flaws. It's very likely that we have found in the 2000 inspection all the deep flaws. What we really need to account for in the analysis are shallower flaws that are in the zero to 30, 30 to 50, lower than the ones that are greater than 50 percent.

I think it's appropriate, given that most of these have been found not to use this ratio but to use this number and to say as we do this we want to put in many more tubes that have flaws that deep. Because there's a much higher likelihood that those have been detected.

MR. BARBER: Weren't those seven tubes though that you're identifying on your viewgraph there, weren't they in fact not found in 1997 but were found in 2000 and also characterized in 2000 with exceeding 50 percent through wall?

MR. ESSELMAN: As I said when we started, we relied a great deal on 2000 data, which we knew was much more indicative of what was there to infer what it was like in 1997. So yes, we took 2000 data to infer what was there.

MR. BARBER: Well, you're also, by your Monte Carlo analysis, aren't you also trying to infer what existed through the cycle from '97 onward? So wouldn't it be appropriate to make a comparison between what you saw in the 2000 inspection and what your Monte Carlo analysis is going to try and show?

MR. PARRY: This is Jack Parry. We took the results of the 2000 inspection and assumed that's what we had in the beginning of the '97 cycle from a conservative standpoint. It couldn't be worse than what we found in 2000, so we assumed that's where we started off the cycle for the risk analysis.

MR. BARBER: My point is a simple one, but if you just do a simple ratio, you had seven tubes that had flaws that were left undetected from '97, four of which had crack depths or depths greater than 50 percent, if you use the same ratio with your Monte Carlo analysis, you'd have a number like maybe 56, 57, 58. And I think your end result would be quite a bit different than what it's going to end up being.

MR. GROTH: The ratio in that case is the total number of tubes inspected as opposed to the number determined to have problems. If you use that ratio and say in 2000 we inspected -- how many tubes --

MR. PARRY: 11,000.

MR. GROTH: 11,000 tubes, and we found seven that in fact had cracks. If we use that ratio, there would be a much lower number than 19. I understand your point.

MR. BARBER: Okay.

MR. LONG: Not all of those tubes are subject to this phenomenon.

MR. GROTH: I agree with that. Even if you take the number of tubes that are subjected to that phenomenon and use it as a ratio, you'd find a much different number than 50 percent.

MR. ESSELMAN: It's inappropriate to presume for purposes of choosing these numbers that a 30 percent flaw is equally likely to be found as a 70 percent flaw. We know that's not the case.

And what we've tried to do is we've tried to include many more of the flaws that are less likely to have been found and amplified by we think are a great deal the flaws that were found in 2000 to reflect what we don't think existed in 1997, because we think that we found all of these at a greater than 50 percent through wall. But we've amplified that by over a factor of four to represent the number of flaws in 2000, which I believe is accurate and very conservative.

MR. HOLIAN: You can go on to your next slide. We'll revisit it.

MR. ESSELMAN: So with the 10,000 trials performed, we have obtained -- we performed analyses for greater than 225 GPM, which we've talked about. We've also tracked a number that we'll use subsequently for flow out of these leaks that are greater than 75 GPM and less than 225 GPM. The probability for spontaneous failure that we've gotten from the Monte Carlo analysis is .039 per year. The probability per year for leakage between 75 GPM and 225 GPM is .275 percent year.

We've also performed work for steam line break. And given two things, number one, the high number of flaws that we've artificially allowed to penetrate out of these samples and the relatively small difference between normal operating pressures and steam line pressure, we've seen no statistical difference in the probability for steam line break. And we've calculated the same probabilities using the different delta Ps that you would have. So the numbers that we've calculated for probability of tube ruptures are presented here.

MR. LONG: I'm sort of trying to do a reality check on this. Basically, you're saying that -- I'm trying to figure out what, but you have about 31 percent of your Monte Carlo runs come up with a failure that is greater than 75 GPM for a spontaneous failure. Are you saying that the other 69 percent are observable leakage below 75 GPM, or is there some that have no operational leakage at all?

MR. ESSELMAN: There's some, and I don't know what that number is. There's some that have no operational leakage at all.

MR. LONG: Well, those really need to

be subtracted out. I mean you found an apex crack in 1997, and you went back to service. So in your Monte Carlo situation, if you came up with no observable operational effect, basically you do it again. So we need to know what fraction came up in that zero observable effect.

MR. ESSELMAN: I can certainly get that, but I guess I don't know that I agree that if you take a hundred tubes and one happens not to penetrate the wall, that's not a valid data point.

MR. LONG: But out of your trial involving a hundred tubes, if there's no observable operational effect, presumably you do the inspection the same way you've been doing the inspection. And if you find something, if you plug it and you try again but you keep trying with this problem, you have in the inspection process. So the reason --

MR. ESSELMAN: Except this is -- I understand your point. This is not really related to the inspection process. This is for the last cycle, given where we were in 1997, what was the probability of getting a tube rupture.

If we take a hundred tubes and none of them went through wall, that's a valid data point.

MR. LONG: I understand your position, but we have a different position. Our position is that in your inspection, you had a warning that there was something occurring that did not lead you to an inspection process that precluded the event. Therefore, one of the inputs to our process was that you essentially needed an operational event in order to terminate the process of taking the chance that an operational event would occur.

So what we're trying to figure out is the distribution of those operational events between, you know, for the observable operational events between leakage and, you know, if you want to break the gross failures let's call it into the 75 to 225, greater than 225, I think that's a legitimate thing to do, you know, because you can calculate success criteria differently for those two flow rates. But the point is that if you had no flow rate, no observation, we'd essentially say that's not going to terminate the problem that was occurring.

MR. ESSELMAN: We can track that also and we can measure that and we can let you know what that is. I think it's a relatively small number.

We'll get it.

MR. LONG: Okay.

MR. ZWOLINSKI: When you were doing this analysis just for sensitivity, if the 19 tubes was 20 tubes and you run the Monte Carlo analysis through, what would happen with the numbers as far as the .38 and the .275? Would they become larger, if 19 became 20?

MR. ESSELMAN: I wouldn't expect it to change substantially. Again --

MR. ZWOLINSKI: Okay.

MR. ESSELMAN: I'm sorry.

MR. ZWOLINSKI: Is that number not in the equation and therefore --

MR. ESSELMAN: It is in the equation. What you're doing is you're only slightly increasing the number of tubes that go through.

MR. ZWOLINSKI: I'm trying to figure out with the sensitivity which number is driving the Monte Carlo equation. When I do my mathematics, there's some numbers that are more equal than others. I was just trying to figure out which number is more equal.

MR. ESSELMAN: We've done some of the

parametric studies. I believe that this is an important parameter because that says once you go through wall and how long is the flaw and because of its importance we tried to skew it in a very conservative direction, if you look at the rest of these, what's important is that we've chosen distributions so that a lot of these go through wall.

If you substantially change them so that you change two percent of samples of a hundred tube samples that went through wall, then you would start having a sensitivity study. Those kinds of things we can do relative to 19 tubes versus 20, I believe it would not make any substantial difference.

MR. LEW: How sensitive is it to assume flow rate when you took -- the question was how sensitive is this analysis with respect to you seeing Indian Point 2 as a data point over input into the analysis of a hundred 9 gallons per minute versus a hundred 46 gallons per minute?

MR. ESSELMAN: As I described, we did not assign a conditional probability to that curve, really not knowing how to do it. So that was deterministic. So if you skewed that upward or downward, you would increase or decrease the probability that you would calculate.

MR. MURPHY: And you skewed it upward?

MR. ESSELMAN: We skewed it upward from mean up to the point where it included basically information for three data points that we had. Yes, sir.

MR. LONG: The other point that I'm trying to understand is you said that there's really no difference in the probability of having a tube fail with a flow rate of let's say 225 GPM. When you have the normal operational delta P, which is -- what was it, Emmett -- about 1540 PSI, or if you had a depressurization on the secondary side so that if you're less than that you'd be able to get, depending on what your operator did, you'd be able to get the full operating pressure on the primary while you had a pressurized secondary. In that case, you'd have -- I've forgotten what your operating pressure is -- it's like 2,000, 2,200.

MR. GAYNOR: In our plant, we don't have high pressurization safety pumps.

MR. LONG: I understand your point, but

we're talking about less than 225 GPM initially. So you would be able, with your charging pumps, to get it back up to 225 PSI? You can put in something like 250 GPM as the charging, right, 225?

MR. GAYNOR: Over a substantial period of time. Once you --

MR. ESSELMAN: But no, just because you have a flow rate doesn't mean you can develop a head. Your pumps have a head.

MR. LONG: They're PD pumps.

MR. ZWOLINSKI: All three?

MR. GAYNOR: Yes. But normally you wouldn't have all three operating. Normally you would have one operating. Now, if you get an SI, I believe those pumps are stripped, and I think at most they're normally --

MR. LONG: We're not talking about the situation that you are. We're talking about a situation where you start off less than 225 PSI, maybe zero PSI -- I'm sorry, GPM, less than 225 GPM -- I hate to do this to the court reporter -- and the pressure changes, the differential pressure changes.

Now you do have some data, but let's

assume for the moment you don't. But if you do, it's less than you can keep up with for your charging system. So you can take the charging pressure up to what your normal operating pressure would be, or hot standby pressure would be, and I'm asking what that is.

I think the lowest one I know of is 2050 and they're usually up around 2235 PSI. So your at 2235 PSI potentially with not much pressure on the secondary side. So are you telling me that when you looked at whether or not you could rupture an apex flaw at 2235 PSI pressure differential, you still didn't see any difference compared to the 1530 PSI pressure differential?

MR. McCANN: I think what Doug is saying, when we looked at the NUREG that describes the classic main steam line break accident and where the pressures go during that accident, the assumption is that the operator will fail to secure safety injection and therefore you could wind up with a flow -- with the reactor coolant system repressurizing and the reactor coolant pressure going to SI pressure. We don't have high head SI pumps. We used 1,800 pounds as a cut off point for that analysis.

MR. LONG: So what you assumed was there would not be an error that would get you above 1,800 PSI.

MR. McCANN: That would actually take more errors, right, because now the operators would have to start the extra pump. It would progress much more slowly. I think it's a different situation, but we did not look at that.

MR. LONG: Okay. You took a classic main steam line break. There are other ways of depressurizing secondaries so you can stick open a valve somewhere.

MR. McCANN: To depressurize?

MR. LONG: To depressurize, you can trip the plant, but you may not get a massive coolant with SI.

MR. McCANN: Again, what he used was the classic main steam line break accident that was described in -- I forget the NUREG -- and that assumes that the operator neglects to secure a safety injection and therefore brings the active coolant system back up to safety coolant pressure. And we used the SI pressure of our safety injection pumps.

MR. GAYNOR: Also the lower pressurization rate will also reduce the delta P in terms of timing between primary and secondary as well. One is following the other fairly closely.

MR. LONG: If you have a rapid cooldown. At any rate, I understand what you did.

MR. HOLIAN: Let's take a five-minute break before we go on to the next presentation.

(Recess.)

MR. GAYNOR: My name is Doug Gaynor. I'm with the PRA Group for Indian Point. And what I'd like to do today is to take the results of what Tom has given you and show you how we've applied that to give you some of our phase 3 plant specific assessment.

The assessment that was done by the NRC in the inspection report cited four different postulated scenarios: The spontaneous steam generator tube rupture, which is the normal operating steam generator tube rupture, an induced steam generator tube rupture as a result of high pressures, hazardous type of events on the primary side, secondary side depressurization events, and steam generator tube ruptures that are induced by conditions after core damage where you might get some very high temperatures in the primary side.

What I'd like to do is -- the two events that the assessment indicated were not as significant as the depressurization and the spontaneous rupture, go through those first.

The overpressurization event, as indicated in the assessment, the aqueous type of an event, doesn't have any impact on core damage frequency. And we agree with that because essentially you're at the point where you're assuming the aqueous event has caused the core damage event.

The assessment also indicated that basically from an aqueous point of view, your impact really can't be any greater than the contribution of aqueous to core damage frequency to begin with. In the individual plant inspection for Indian Point 2, that frequency was approximately 1.8 times 10 to the minus 6.

The only thing I'd like to point out is that since the time of the individual plant examination, we have gone back and modified and created a more robust reactor protection system model and looked at the logic and the channel in a little more detail and at the signal diversity.

As a result of that, the current model for Indian Point 2 has a contribution to core damage frequency from anticipated transient without scram of just less than 5 times 10 to the minus 7. So in fact the contribution cannot be more than that in terms of what we're looking at here.

With regard to temperature induced steam generator tube rupture postulated scenarios, the Indian Point 2 individual plant examination did look at these, and in fact used the NUREG 1150 information, which I guess actually came out of CR 4551, which indicated that for high and dry type sequences, there was approximately a 1.8 percent chance of an induced rupture where you have some flaws in your steam generator tubes.

For the purposes of this evaluation, we took a look at the more recent technical basis, including the in situ tests that we've done here for Indian Point 2, the 1/7th steam generator experiments, the TMI experience, the industry analyses that have been done since then, and the NRC analyses that have been done.

Based on that, a couple things came up. As far as the in situ test, as we mentioned previously, the tubes that were found with flaws were tested and other than R2C5. They all in fact passed the three delta P test. As far as situations where you don't have secondary side depressurization along with the high temperature, the temperatures and pressures on the tubes do not rise to the levels where you would start to see the material strength start to be reduced to the 800 K area, the 800 degrees K area.

When you do have some depressurization with regard to the locations of the tubes where we saw flaws, the temperatures still would not exceed this point at which you'd start to see the material strength being reduced. This is specifically true with regard to the R2C5 location in the apex due to the inflow/outflows. And you would see a very limited temperature increase, nowhere near that value for the R2C5.

Based on the work that's been done to take a look at this, we believe that there was no additional susceptibility to the phenomenon during the Cycle 14. And in fact, since there was consideration of this in the original IPE, individual plant examination, that that NUREG 1150 still applies and would not really impact the Cycle 14 operation. Yes.

MR. LONG: I think that's probably where we have a big difference. You've listed a bunch of things on a technical basis there, but I'm not sure exactly what if anything additional to the material I'm familiar with that you have done.

You, first of all, you say the 1.8 percent came from essentially the probability there would a susceptible flaw, so the question really is is this a susceptible flaw?

And you're saying if you need secondary depressurization, even though this flaw at some point was going to rupture, or not rupture, grossly fail at a temperature that was normal operating temperature, so I guess I'm having a little bit of trouble saying that at least for some period of time, elevating the temperature in the steam generator wouldn't have caused this flaw to fail.

I think I hear you saying it's not in

the hottest part of the generator. You looked at the 1/7th scale test, did you feel that this -- I'm trying to remember exactly where this flaw was with respect to the event, it was row 2 and column 5, so it's out near the edge, right?

In looking at the transient tests, the tubes that carry the flow from the inner plenum to the outer plenum sometimes get out there and sometimes don't. One doesn't or two or three don't, what kind of -- I guess what I'm looking for is what kind of analysis did you really do to say that the temperature you would get during these transients wouldn't be sufficient to cause that flaw to fail without essentially going into the secondary depressurization delta D part?

MR. GAYNOR: Well, I'm not the expert on it. Let me turn you over to the gentleman that did that work for us.

MR. HENRY: My name is Bob Henry. Steve, as usual your memory is very good about the 1/7th scale tests. And we particularly looked at the test -- the one that you're talking about SGHT4, to sum it up, the outgoing flow gets to the outside of the generator. We particularly looked at what the temperature was when it got there. The temperature of the fluid when it finally gets there, because it's going through all the rest of the turn flow coming back, is roughly the temperature in the outlet plenum.

So when that tube -- when that temperature goes through that tube, that flow goes through that tube, in essence it almost looks like a return flow tube in terms of the peak temperature. So our focus was are we looking at any temperatures that would exceed 800 Kelvin. Because 800 Kelvin and below, the strength is, just like you reviewed it in NUREG 1570, is just as strong as it was under normal operation. So that was the whole focus. And in this case, that temperature won't get above 800 Kelvin.

MR. LONG: What happens if the flow of that tube that has the apex flaw problem is over more towards the center of the -- not the bundle, but along the -- the center of the bundle as far as the inner row is concerned?

MR. HENRY: Given the 1/7th scale test, their inner row, the first row represents the first three to four rows in the generators, however you want to do it. So all of these that had these flaws are all in row two. So except for the one that looks like R2C5, they're always in return flow systems. They're always the cold tubes. So their temperatures again are below 800 Kelvin.

The place that you have to specifically look at is those areas that are plainer views just essentially right above where the hot link nozzle comes in. And they had one of those tubes that they looked at, and that also passed the three delta P test. With that three delta P, it will hold together longer than other things that are part of the analysis, just like you have in the NUREG 1570.

MR. LONG: I hear what you're saying.

MR. GAYNOR: Using the information that Tom discussed with you earlier, we looked at the spontaneous tube rupture in two cases. I think you're aware at this point that the cutoff was 225 GPM. And that is the reason for that was to look at the different success criteria or backup for safety injection should it fail to allow us to inject the primary system and make up for the primary system. It also allowed us to look at the additional time that was available for response by the operators to the EOPs.

The way that we did this, even though we broke it into greater than 225 GPM and less than 225 GPM, when we took the frequency for 225 GPM or greater, we used the steam generator tube rupture model that we have for full steam generator tube ruptures. So in fact the rest of the response to it assumes a full 400 plus steam generator tube rupture in terms of the modeling and the timing. The frequency is the only thing that we use the 225 GPM for.

MR. BLOUGH: When you did that analysis that came up with .039 per reactor unit year, you were looking at a two-year cycle. And when you did Monte Carlo, was there a difference between -- I assume there's a difference between the first year and the second year and that most of those ruptures would be in the second two years you looked at.

MR. ESSELMAN: We got the number by taking a full cycle. And as the crack grows over two years, we're really asking if in the cycle does it go through wall. We got to frequency per cycle which was two years and we took half of it for the frequency per year.

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So we looked at the full cycle, but then we said instead of using frequency per cycle, we just took it and turned it into a per year.

MR. BLOUGH: So the data doesn't really tell you what percentage of those failures are in the first year or second year.

MR. ESSELMAN: No.

MR. LONG: You essentially divided by two, is what you're saying?

MR. ESSELMAN: Yes.

MR. BARBER: Did you make any adjustment to your frequency for your steam generator inspection program? Your frequency is probably just something directly out of the IPE I presume. Did you adjust your frequency?

MR. GAYNOR: This is the number that came from the Monte Carlo.

MR. BARBER: I'm sorry, I thought it was out of the IPE.

MR. GAYNOR: No. As I mentioned, for this particular case, we used the full steam generator tube rupture model at that point.

MR. TRAPP: How does that compare to the IPE?

MR. GAYNOR: The frequency in the IPE is 1.3 times 10 to the minus 2.

MR. TRAPP: Slightly more.

MR. GAYNOR: Yes, about triple it, yes.

MR. LONG: I guess one thing we need to know is what fraction of the spontaneous ruptures occur in the second year.

MR. GAYNOR: The other thing that's indicated in the --

MR. ZWOLINSKI: Just so I understand, Steve, that's the number -- why isn't that the number on day one and at the end of cycle?

MR. LONG: What they did was figure out from a Monte Carlo process, with all of the distributions that we don't have, what fraction of the time they would have a tube fail during the two-year period. Where the flow from the failure would be more than 225 GPM, and that was the fraction, 3 point something percent, almost 4 percent. So that's the number they're using as the spontaneous tube rupture frequency. It's an average for two years. You presume it's mostly in the second year, so it's a factor of two there perhaps.

MR. STROSNIDER: The crack growth rate

is --

MR. LONG: Right.

MR. GAYNOR: The other point that was made in the assessment is that the STP process and the guidance in it assumes that for steam generator tube ruptures, you basically have an unscrubbed pathway on the secondary side and uses a large early release frequency equal to the core damage frequency.

The Indian Point model separates out the isolated steam generators or steam generators where the relief valves stick open versus where they do not stick open -- it's two separate plant damage states -- and looks at both of those plant damage states, 48A and B basically, and it looks at the radionucleid releases as a result of those and the level of the release fraction for both of those.

Basically if you look at the ASME standard out there now, it proposes a reasonable break point of 10 percent of iodine inventory as a large release.

The Indian Point examination looked at -- of course, that was done prior to the current work -- but it looked at the difference in release fraction for the stuck open valve versus the non-stuck open valve cases. And for the non-stuck open valves, the release fractions were below the data set point cutoffs.

So we believe that the plant damage states, the two different plant damage states, in the case of the stuck open valve would be the large early release, but the case with the not non-stuck open valves would be not an early release. And when you look at the sequences, in the sequence frequency is approximately 13 percent of the core damage frequencies are frequencies where the steam generator is open and is released to the atmosphere without it being scrubbed in any way in the steam generator.

MR. LONG: Steve Long. Can you describe the other 87 percent of your core damage frequency due to spontaneous rupture that is not from a pressurized secondary? Give me some idea of what the mechanism is that leads to core damage when you don't have the secondary depressurized.

MR. GAYNOR: We've got sequences where basically you either cannot make up the primary side and you lose auxiliary feed water as a result of the -- and cannot remove to get heat. Many of them are long term type where, for the purposes of the analysis, even though you have isolated, you have not established any kind of a long term AE removal. You have to run through the individual sequences. But basically, as long as you have not isolated either the short term or long term, you have the ability to release unscrubbed.

MR. LONG: So these are presumed to be dry generators that maintain pressure or are depressurized?

MR. GAYNOR: The sequences that we're talking about?

MR. LONG: Yes.

MR. GAYNOR: Not necessarily. You could in fact not be making up to the primary side, and even though you are able to relieve -- you're able to remove the K heat, eventually your inventory is lost and you've got a potential for core damage. There are some assumptions in there in terms of not being able to open your PROVs in order to do certain functions in response to the event.

MR. LONG: You operate with your PROVs blocked I believe.

MR. GAYNOR: Yes, but they have an open signal on a pressure.

MR. LONG: I guess another thing we're going to need to be able to understand is why you're saying that the majority of your core damage frequency from spontaneous rupture would not be to essentially an open secondary. We'll have to see something about the sequences there.

MR. GAYNOR: Well, in the earlier work that we did on the event, there were sequences that were provided. We'd be glad to do that for this.

MR. LONG: We're going to caucus when you're finished, so we'll talk about that. Thanks.

MR. GAYNOR: The other point that I just wanted to make is in fact that, although it's not in the analysis here, many of the sequences in terms of steam generator tube rupture occurs, the core damage would occur fairly late in the process. It could be 12 hours, it could be even further along if you achieve some kind of depressurization.

So that these sequences would be out in time quite a bit, and there would be significant time for taking actions. Now, we have not included that in this specific analysis because of the difficultes in terms of establishing exactly when the actions would be taken.

MR. LONG: But your .13 doesn't include lateness?

MR. GAYNOR: No. For the less than 225 GPM cases, again the frequency was taken from the work that was previously described to you. We evaluated this using a steam generator tube rupture model, assuming that it was -- the response was to a 225 GPM leak. The frequency is for anything basically above 75 GPM to 225 GPM, but it was -- it is calculated based on assuming a 225 GPM leak and the response time that would be available for that.

MR. TRAPP: Are these major changes?

MR. GAYNOR: The fact that we've got the ability to make up their primary side, the human reliability, additional time available as far as human reliability is concerned, and potentially there are some recovery actions in there as well. Especially for the cases where the core damage is late in the sequence and would be as a result of, for instance, an inability to fully depressurize and having to do something. Yes.

MR. LONG: I think I know the answer to

this because of what you said earlier about not considering a higher delta P as potentially changing the leak rate, but did you look at any human error or potential for increasing the delta P across the tube such as that you would increase the leak rate?

Some of the human errors are failures that will leave tubes under stress for a longer period or actually increase the stress level for tubes by depressurizing somewhere, and actually in the secondary, without depressurizing the primary. And since you're keeping up with the flow rate here with the charging system, I'm wondering did you look in any way at the dependence of the leak rate flow on the human errors that were in the sequences?

MR. GAYNOR: We did not directly feedback from the human error analysis that was done for a steam generator tube rupture back in to assume a greater flow rate other than what's in the normal steam generator tube rupture model.

MR. LONG: For the ones that are less than 225 and greater than 75?

MR. GAYNOR: No, we did not. But again, it assumes anything -- it assumes the original, the original flow rate is 225 GPM.

MR. LONG: And stays there?

MR. GAYNOR: Well, that's the assumption. Well, that -- it was the assumption in the model. It does not assume any additional time for the flow rate going down either as a result of the depressurization by the operator.

So there's a time frame in which the operator needs to take action to depressurize and before he completes the fuel storage tank. And in fact that would tend to stretch out much further also. It doesn't assume that.

MR. LONG: You're in a situation where you can throttle the input essentially at this point?

MR. GAYNOR: Yes. Because we've looked at it in terms of both above and below 225 GPM, we basically combined the result of the two analyses. And the change in core damage frequency, looking at that, it was 3.85 times 10 to the minus 6. The change in LERF was 1.1 times 10 to the minus 6 for this case.

For the case of induced steam generator tube ruptures as a result of secondary side depressurization, we utilized the frequency that was indicated in the inspection report for depressurization. We did not reduce it by the factor of 4 that's in the inspection report because we were looking at all of the steam generators and not just the steam generator 24.

Again, we used the information from the analysis that was previously presented to you in terms of initiating -- excuse me, in terms of the conditional probability that a steam generator tube would fail in some way as a result of an event.

The result of this was that we modified the steam generator tube rupture model to reflect the fact that, for these particular cases, you could not isolate the secondary side. And so we had to set those particular actions to fail in the model.

Since we were again looking at this in terms of the breakdown between full rupture and the 225 GPM, we made the adjustments where necessary for the lower flow rates here as well and ran it through our steam generator tube rupture model.

There was a concern in the analysis -- in the NRC assessment that, as a result of this being a more complicated event and potentially the operators not drilling on the event, that it assigned it a human error probability and a conditional probability of core damage of .01.

At Indian Point, we took a look in fact at the Indian Point emergency operating procedures. And the emergency operating procedures for a faulted generator followed by a ruptured generator are straightforward and clear. It takes you from zero, which is the main procedure, and there are basically two steps, the first of which will take you to a faulted steam generator and ask you whether or not you have a faulted steam generator. And assuming you have that, it will send you to the procedure for faulted steam generator that asks you to basically take the same kind of actions to isolate the generator that you would take at the beginning of the steam generator tube rupture.

Once you've done some of those actions, it questions again whether or not you have a steam generator tube rupture and will then send you to the procedure for steam generator tube ruptures at that point to continue the actions. Eventually it will get you to the proper procedure, which in this case would be ECA 3.1, which as you see is steam generator tube rupture with loss of reactor coolant.

There was in fact a similar analysis or evaluation that was done during the NUREG 1477 where they looked at the Westinghouse emergency operating procedures and training, and basically came to the same conclusion, that the guidance in the emergency operating procedures is relatively straightforward for these type of events, this combination of events, and that there is training on it.

We in fact do operator training on a faulted steam generator followed by a ruptured steam generator. We train -- there is simulator training. It's a dynamic demonstration of the operator's ability to respond to the event. It's required as part of the two-year requalification cycle. So it's given at a minimum every two years. It recently was done in 1996. It was done in 1998. It was done twice in '99 and again in 2000. Yes.

MR. LONG: I'm trying to remember which order you said when you said you --

MR. GAYNOR: Faulted and then ruptured.

MR. LONG: You do the faulted and then ruptured?

MR. GAYNOR: Starts with the faulted and then there's rupture. There are other scenarios where it goes from ruptured to faulted, but the particular ones that I'm talking about here, the simulated demonstration goes to faulted steam generator and then taken to a ruptured steam generator. Yes.

MR. BARBER: Just a question about from what you've laid out here from a training standpoint.

MR. GAYNOR: I'm sorry, can you speak up?

MR. BARBER: A question of what you've laid out from a training standpoint, a procedure standpoint. You seem to be implying that the procedures are good and that the training is of high quality such that, you know, you wouldn't expect to have a lot of problems during a steam generator tube failure or tube rupture. Yet during the actual event, there were a number of problems.

Did you somehow factor that into the risk assessment? Wasn't there a situation where safety injection was in fact caused by uncontrolled cooldown during this event?

MR. GAYNOR: I believe that was later on in the sequence and not a part of it.

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MR. BARBER: The point I'm trying to make is did you factor actual experience during the event from a human performance standpoint, take lessons learned from that, and put that into your risk assessment?

MR. GAYNOR: Specifically with regard to this event?

MR. BARBER: Yes.

MR. GAYNOR: One point on the event?

MR. BARBER: Well, with the points that were highlighted. I think there were some other human performance issues that were identified in the AIT. I just picked that one at random.

MR. GAYNOR: Well, I guess the point here is that what we're looking at is, compared to the steps that they would go through for a steam generator tube rupture as we've got it, we're looking at the delta for this particular event. And the lessons learned should certainly apply to what we're doing. But they should be applied equally whether we're talking about running through the procedure in this particular case or running through the procedure where you have to go through the rupture anyway and potentially have to deal with a case where you have a faulted steam generator as well.

Looking at the delta, it should apply to both cases. So being able to utilize the model that exists now for this particular case of a faulted ruptured generator is not unduly complicated in comparison. And that was really the point of what we were looking at in terms of whether we needed to consider this to be a complicated event or an event that we don't train on so that we would need to look that much higher.

MR. McCANN: I think the point, Scott, is in the inspection report, it indicated for this particular event it's complicated and we don't train on it. And I think the way you do the probablistic risk assessment, you assign a higher probability to operator error in those cases if it's something that is complicated and you don't train on it. And I think all Doug is saying is in this case, we don't think that that's appropriate for this event.

MR. BARBER: That's my point too, because I'm raising an issue where there were conditions in the plant that should have allowed for normal operator action to take place to stabilize the plant, and they didn't. And they weren't even high stress situations.

I'm really just asking a specific question if you adjusted your human performance error rates to account for that, or did you even consider that?

MR. GAYNOR: In the risk analysis, we did not adjust the human error performance rates based on one specific response, no, we did not do that.

MR. LONG: A follow-up on that a little bit. The situation that really surprises me, if you were training this many times, was that the actual event that you had in February, when you got to the point where you were trying to initiate RHR, which you would have to do in this kind of situation because you don't terminate the event in the case of a cold shutdown, you found a problem with your procedures and you hung up there until Westinghouse told you you really didn't have to satisfy these procedures in these conditions. And then you went away and initiated RHR and cooled down. So that's the kind of problem that we sort of hope gets worked out in training and doesn't seem to have been found in the training process.

Maybe because you didn't train that far along in the sequence or perhaps the simulator doesn't behave like that at the plant during the cooldown, I don't know why. But that's the kind of problem. But you're still losing water and you're kind of hung up in the procedure.

MR. GAYNOR: That was true more in the recovery phase of it rather than in the initial response to it. And you know, I can't respond right here.

MR. GROTH: I can. We found in fact that we had not gone through that far in the scenario to train on that area.

MR. LONG: So you just didn't train that?

MR. GROTH: We had not trained, we did not -- we had not at that point.

MR. GAYNOR: As I mentioned, we did evaluate it using the model with the adjustments that were necessary for showing that we did not have isolated capability. And the change in core damage frequency for that particular postulated scenario was 2.9 times 10 to the minus 6. We agree for this particular scenario that core damage frequency and the large early release frequency would be the same. There's also a possibility that the stuck open valve is closed as you get to a lower pressure, but we did not consider that.

As a result, this is basically the run up of our assessment. And the bottom line is a change in core damage frequency of 6.7 times 10 to the minus 6, which would be a white, and a large early release frequency of less than 4.5 times 10 to the minus 6, which would be a yellow.

MR. LONG: To go back and revisit item number two up there, we talked earlier about whether or not the high temperature of the tube, there's also the concern that there's a higher delta T during the event. And it doesn't look like you credited that as potentially opening up a primary/secondary at that point prior to core damage.

MR. GAYNOR: In terms of the way that that would be depressurized, you would have to have depressurization as a result of that basically. The secondary side depressurization would be already included in the fourth case. Primary side, the pressure builds up, you would have to basically have, in addition to your core damage event, you would have to have a failure of the secondary side to isolate it.

MR. LONG: I don't think you're following the part I'm asking about. In item 2, not item 4, if you have a core damage event, let's say due to something like station blackout where you can't feed the secondary or the primary. For whatever reason, you can't feed the secondary, the battery is depleted or the turbine driven aux feed failed to begin with, whatever, you tended to boil the secondaries dry.

And then sometime like an hour later, you will finish boiling the primary dry. If you stuck open a valve on the secondary side, that's usually in the model, over what's typically not in the model is any other depreciation for the secondary, like sticking SIDs or other valves that will depressurize the secondary. That's not typically tested.

As a matter of fact, I understand Indian Point had an event quite sometime ago where during startup, you failed to feed two of your generators and they both had a lot of leaks and they boiled down quite a bit. One boiled dry and depressurized. So you ended up with improprer hydro in your secondary in the past.

But that kind of leakage from the secondary side is not necessary for core damage and is at this point not included in your PRA when you're looking for core damage. But it is important if you want to ask about a damage site and whether or not you can induce a tube rupture. Did you do anything to try to assess that particular probability for item number 2?

MR. GAYNOR: Beyond the failure of a valve to reset where you get a significant and very quick depressurization of the primary side, the more -- the slower depressurization events were not specifically looked at where you get some kind of small leakage and slow leak on the secondary side.

MR. BLOUGH: Regarding the last case where you talked at some length about the risk of possibly operator error or an operator error consequence, you're suggesting the value we used might not be appropriate. What value did you use? Did you use industry generic or better operator performance than the industry generic?

MR. GAYNOR: No. Basically what we did there is, based on the fact that the operator, in responding to a steam generator tube rupture event, would have to also respond to a faulted steam generator and go through that same process, the actions that he would have to take under the conditions where he has a faulted secondary side, which is built into the model, would be basically the same type of actions that you would have to take here.

Given that the secondary side is open, some of the early actions where the focus is to try to prevent the valve from sticking open are basically not as meaningful because you already have an opening and it's not closing on you to begin with. So you have a lot of your actions are further down. And basically given that he's got to respond to it in the same way, our contention is that you should be able to use the same analysis that we used for the steam generator tube rupture. We're not being optimistic. We're just using the same analysis.

MR. TRAPP: What did you get when you

used the old model? Did you run it with the old model and old HRAs and come up with the old model and revise it?

MR. GAYNOR: What we did there is we used the information from the full model, except for the 225 GPM case. What we did in the model, however, is we eliminated any ability to isolate. So anything that the operator could do to isolate or anything that we could do in response would impact the ability to isolate is set to fail. So those things are not in the model.

MR. BLOUGH: You can't compare how the number you're using for operator error in the analysis compares to what the rest of industry is using?

MR. GAYNOR: I don't have any comparison on that. I can say that when we did the work, we used the human cognitive reliability model, Reg 1278 model, to develop those numbers. But I don't have a comparison for those.

MR. SCHMIDT: Is the .01 that you felt was too high or .01 is what you used?

MR. GAYNOR: No, the .01 is what was used in the assessment. And that was actually --

it's not exactly clear. The assessment indicates a high human reliability as a .01 as core damage. It wasn't an absolute given that that being human reliability and that being the condition of probability. That .01 is the initial probability that's in the assessment for basically that steam generator response.

That's the end of my presentation. And I'd like to turn it over to Jack to talk about the inspection measures.

MR. PARRY: Good afternoon. My name is Jack Parry. I'm the project manager for the steam generator inspection that was performed in the refueling outage this year.

Today there's two topics I will try to address. The first is to say a few statements about the '97 inspection that we've been discussing. And second is to try to address some of the measures that have been taken to prevent a similar type of event from occurring at Indian Point.

On the 20th of July of this year, Indian Point representatives met with the inspection team that was evaluating the '97 steam generator inspection. During that meeting, there was a difference of opinion between Indian Point and the inspection team of how well the '97 steam generator inspection was performed. And since that meeting, the differences still have not been resolved.

Indian Point's position remains the '97 inspection was performed in accordance with the industry guidelines and requirements in kind in place at that time. This consisted of our technical specifications, and primarily Revision 4 of the EPRI primary water PWR steam generator examination guidelines.

While we have no additional information to present in that regard, I'd like to highlight a few facts to summarize our position.

When we planned the scope of the '97 inspection, one of the things that's allowed is to be able to sample the steam generator, sample a number of tubes in lieu of looking at the whole steam generator. When you sample it, if indications are detected, then you have to increase the sample size.

In '97, we planned and executed the inspection, including a hundred percent inspection of the tubes in all four steam generators. And this was done primarily for two reasons. In '95, when we were planning it, we looked back and saw what did we have to inspect in '95. In that cycle, we inspected two generators at 50 percent, one at 75, and another at a hundred percent.

So in planning the '97 inspection, we chose to conservatively inspect a hundred percent of all four steam generators both for safety and for better planning scope of the outage. Along those same lines, in a steam generator inspection, you may have certain areas of interest, such as inspecting row 2 and row 3 U-bends that have to have additional inspections. Again, in '97, the guidelines allowed us to sample those type of areas.

But in planning and executing our inspection in '97, we performed inspection of a hundred percent of the row 2 and row 3 tubes at all four generators to perform a conservative analysis.

In looking at what kind of probes to use for those areas, one of the things we wanted to look at, what would be the best probe for that type of inspection of the U-bends. Previously, the row 2 and row 3 tubes had been sampled and had been inspected with what was called a bobbin probe, which was the instrument of the time. And it still has some application. But in '97, we identified an approved probe called a Plus Point Probe. And we used that to inspect all of the row 2 and row 3 tubes.

Another example of additional effort taken in '97 in our inspection was to qualify a probe called a Checco probe. And this is used for inspecting the intersections between steam generator tubes and support plates in the generators.

In regards to the oversight of the inspection, to assist with our oversight, we contracted and obtained what we call an independent Level III QDA, a person qualified to analyze the steam generator data that comes out of the inspection.

As many of you know, during a steam generator inspection, we have two technicians. They separately look at the data, analyze it, a primary and a secondary. If they agree, that's how the tube goes through its dispensation. If they disagree, it goes to a resolution process, which is another qualified person that looks at those.

And in '97, what we did is we took our independent Level III QDA and assigned him to work with a resolution team so that he was provided a third party review of all the calls the primary and the secondary analysts did not agree on.

Now, in the '97 time frame, obtaining an independent third party analyst was a practice used at other sites. However, it wasn't a requirement in the Reg. 4 guidelines. It was a step we felt we took to try and have a conservative and adequate inspection for that outage.

The last area I wanted to just highlight is some independent engineering studies we had done. Before the '97 inspection and after it, okay, we had an independent engineering evaluation done on the results of the steam generator inspections. Each type of the mechanisms that caused a tube to be failed, whether it was primary water stress corrosion cracking or ODSCC, was reviewed and determined the frequency of occurrence and to make a prediction of what to anticipate at the upcoming inspection.

This evaluation took into account the lower operating temperature, industry experience with the rate for these corrosion mechanisms, and the results for the steam generator inspections. This type of study was performed to assist Indian Point in understanding the overall status of their generators.

The results of the studies in '95 and '97 were reasonable with the results of the '97 generator inspection. The identification of one case of primary water stress corrosion cracking was consistent with the review performed after the '97 inspection in part due to our lower operating temperature that Indian Point operates at.

MR. BARBER: Jack, I've just got a question about your independent engineering evaluation. Did they do anything to review your noise issues that came up in '97? I know that was one of the things that we looked at in 2000. We said, you know, you had a number of indications or evaluations or examinations that had noisy profiles, if you will, and --

MR. PARRY: In 2000, we did look back as far as the noise levels in 2000. In '97, we did not. What we did in '97 is we chose probes like the Plus Point, like the bobbin, like the Checco, that we felt were qualified through EPRI and went through that.

At that time -- and I'll highlight this a little bit -- but at that time, you didn't have to go through a site specific qualification. You had to use industry probes that met industry standards for use.

Now, in looking at the noise, our review from 2000 looking back to '97, okay, we still felt, still feel, that the noise levels were bounded. If you look at what the EPRI has in their samples in the noise levels and the signal noise ratios, some of our test results were on the high end. But we felt that they were bounded in looking back at what was in '97.

I don't know if I answered your question. We didn't do it in '97. We followed the industry standard for using qualified probes, then we looked back in 2000. We still feel it was bounded by what was back there.

MR. BARBER: My question really relates to from looking back at your '97 data, do you feel that the indications that you had, the noise that was present, was there the potential to mask a plugable indication?

MR. PARRY: Was there a potential?

MR. BARBER: Right, because of the amplitude or magnitude of the noise. In other words, if you're looking for a plugable indication of a certain value, was noise at or greater than that value?

MR. PARRY: I'd have to say there may be a potential. Probability of detection is not a hundred percent for any process. So yeah, I'd have to say there had to have been a potential for any of the process.

MR. HOLIAN: I'll just add these points. You said there isn't any new information here. You're restating the inspection report. We documented these issues. We documented these issues with your perspective in our inspection report. We reiterated that we looked at these issues and still felt that the actions raised to a level of Appendix B corrective actions, even stating EPRI guidelines and those type of issues, that responsibility was on Con Ed to take those issues and apply the guidelines that are generically across the industry to specific problems at your plant. So we think we can move on.

MR. PARRY: Okay. Some of the steps that have been taken since February to help prevent a similar event from reoccurring is the industry as a whole has implemented additional guidelines. In 19 -- December of 1997, after our inspection, some industry representatives and NEI issued a document called NEI-97-06, that improved the guidance of how you perform steam generator inspection programs. These requirements were put in place January 1, '99. And all facilities doing outages after that were expected to implement those and use them in their outages. And we did that at our site.

Under the guidance of NEI-97-06 has been put into our administrative procedure. Some of the things it requires is a dedicated steam generator project manager. And that's one of my main responsibilities right now.

We also established what we call a steam generator management committee. This is a committee of senior managers of various disciplines throughout the plant, similar to the expertise in the safety review group. And they're responsible for overviewing and reviewing the operation and implementation of the steam generators. That committee's chaired by the Vice President of Engineering. And it's been in effect since early in this year, around March.

Some other items that have been implemented is prior to an outage -- and this is in our administrative procedures -- to develop what we call a degradation assessment. And what this does is look at your failure mechanisms, what can cause your tubes to have problems. You have to make sure you have the right probes to identify that, have the right qualifications for it, do site specific demonstrations, and then train your analysts on it, what you have at your particular plant.

Another item that is used generically and at our place is the analyst performance tracking. It's a computer process to help both an analyst itself and the utility know how an individual is doing in reviewing steam generator eddy current inspections.

Some of the things that we've implemented outside and above 97-06 is in the 2000 outage, with the old steam generators and in an inspection we did for the replacements, we provided what we called enhanced analyst training. For the inspection of the old steam generators, that focused a lot on primary water stress corrosion tracking, 800 kilohertz probe, site specific issues. And us working with our vendor, put the analysts through that training. Now, that training then was also used for the analysts that did the inspection of the replacement generators.

One of the things we've done is we went through and did a hundred percent eddy current inspection of our replacement steam generators as a preservice and benchmark so we know where we stand. We also use 800 kilohertz probe, as you know, an instrument that we've helped develop for the industry. That was applied in the old steam generators and we used it on a sampling basis in the new steam generators. We looked at a sample of rows 1, 2, and 3 tubes in the new steam generators with an 800 kilohertz probe.

We've also, for our outage and for the inspection of the replacement steam generators, improved the data quality criteria. Now, this is something that the industry is adopting in the Reg. 6 version of the EPRI guidelines. It's coming out next year. But it was implemented for our outage and also for the inspection of the replacement steam generators.

So all the items I mentioned, that we have improved our program. We implemented them in both the original inspection of the old generators and also the one we have in the replacement steam generators.

Another area we've worked is to try and help disseminate this information to the industry. There's been four seminars we've given talks at. I presented at two, Jim presented at one, another representative of the plant presented information to try and disseminate this to the industry. And I believe we have helped a few of the sites, like Northern States Power, maybe avoid a similar event.

This came up in the EPRI NDE Conference. One of the people that was at our three assist visits was a representative from there. They had an outage coming up. Based on what he learned at our site, he used the high frequency probe. I believe he was a little more questioning in some of the results that he got. And in one case, they performed an eddy in situ test on a tube that was questionable and it leaked slightly. It still had its performance criteria, but we may have helped one of our peers with a near-miss in trying to disseminate the information.

So being able, these three assist visits provided us a real time capability to disseminate this information for peers I think helped them in some of their inspections for this year.

With that, I'd like to turn it over to Jim Baumstark to provide some summary comments.

MR. BAUMSTARK: The purpose in being here this afternoon -- and it's again in accordance with the phase 3 process of the significance determination process -- is to provide plant specific risk perspectives and related information, including where we thought it was appropriate, alternative risks and engineering analysis to support reducing the significance of issues associated with our recent steam generator tube plan.

We began with a fracture mechanics discussion of the row 2 column 5 failure to demonstrate why we believe that failure would not be expected to progress to a rupture. We then developed a Monte Carlo model based on inspection of recent eddy current examination. The Monte Carlo model produced the probability of a leak progressing to a rupture for both the spontaneous rupture and the secondary depressurization cases.

We then took the results of the Monte Carlo analysis, and using site specific probabilistic risk assessment models, calculated values which support a white delta core damage frequency and a yellow delta large early release frequency as conclusions.

In addition to the analysis work we have presented, we believe there is a real world compelling reason why no other tube was likely to rupture during the cycle. And that's based on the extensive in situ testing we conducted this year. We believe this testing demonstrated the soundness of the tube which had crack-like indications at the beginning of the cycle and remained in service through the cycle. Therefore, our site specific analysis does not support a potential red finding in the inspection report, and more properly supports a yellow finding. John.

MR. GROTH: We understand at this point

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the staff would like to caucus. And as a result of that caucus, we expect to find some additional questions we should address. We have kept some notes to determine some of those questions that we believe will be asked, but in deference to reading what I think the questions are, we'll wait until you find what the questions are.

At some point in the future, I think it would be beneficial if we could explore with the staff what tubes we might pull in the steam generator 24 to better understand the conditions that exist within that steam generator. It would help us all better understand the phenomenon that existed during 1997 and 2000. Subject to your questions, that concludes our comments.

MR. MILLER: Okay, we'll caucus now. And we'll leave the room and you can stay here and we'll be back.

(Recess.)

MR. HOLIAN: Well, let's start. I'll make some brief closing comments. Hub will finish. A couple staff members might jump in if on my scribbled notes I missed a few of the issues.

In general, I'd like to start off first back on the issue of the '97 inspection and the '97 performance. You didn't hear much from us in comment to that. In general, we rest on the inspection report. You did counter with some of the information, that was information that we knew about during the inspection report and the exit, and those issues on using the EPRI guidelines, following the EPRI guidelines, and the type of inspection you did then.

We didn't discuss much today from the NRC perspective differences and disagreements we have with how you analyzed the data or what in our view should have been done or could have been done in analyzing that data and setting up a corrective action process that would have accounted for those issues that led to the conditions. So I just state that for the record.

Jumping to the two major issues that you brought in, which was using plant specific data to I'll say factor down initiating event frequencies and the difference between LERF and delta CDF being exactly equal, those are the two main issues that we see taking the bigger chunk out of the NRC's analysis, to almost an order of magnitude for each one. We do recognize that you still end up with a yellow color.

And before I get back into the questions we had, I would like to just briefly mention that in the Agency's response to the steam generator issues, that will be done through the action matrix, which is an Agency response to where the plant is overall, that is with the EP issues and other degree of cornerstones that you have.

So in some ways, the difference between a red and a yellow finding from Agency actions might be minimal, but that doesn't minimize us trying to get the right criteria on this.

So I will now progress into some of the questions that we have. And that is most of them center on these two issues. How you factored the initiating frequency and how you changed the LERF assumptions. In general, we'd like to have a description of the Monte Carlo model that was used, including any comparisons with benchmarks that you've done along that line. And that would be -- we would envision along your response to many of these issues, that would be information you already have. We're not necessarily asking you to create anything additionally. It's just additional information that we believe you have in coming to the conclusions from the slides today.

When you do provide this information, I think it would be easy enough to get it through the residents in whatever format you want. And we will attach it to a meeting summary. That will be whatever information you give, your slides from today, and the transcript from the meeting will go out in the public forum since this is a public meeting.

Back on the Monte Carlo analysis, we expect some discussion on the depth and length comparisons, the specific aspects of the model, and the distributions also. So that we can make some assessment of what you sampled from what distributions.

The second area was a frequency of leaks. We got into a discussion of the leaks from zero to 75 GPM and 75 to 225 GPM. We'd like a better understanding on the frequency of how you sampled from those two populations.

Next was a question of the fraction of

the spontaneous rupture. Next is what fraction of the spontaneous ruptures occur in the second year. We had a discussion and questioned you on that aspect.

The RSDP analysis that the process uses concentrates on the second year, the year of operation. You have -- Apparently, your answer was that you averaged that fraction over the two years, but we'd like a clarification of how you did that assessment.

MR. LONG: Going back, I want to clarify. One of the things we're looking for is the breakdown of the part you didn't give us. You gave us the frequency from 75 to 225 and 225 and above. We're looking for zero to 75 and we're looking for essentially zero no observable.

MR. HOLIAN: Next we had a discussion on the basis for the gallons per minute flow rate that you used as an assumption for your curve, the basis for that, the gallons per minute.

Those are the points that I had to cover. Anything else?

MR. LONG: Just one other thing. You had mentioned from operating experience any knowledge of apex primary water stress corrosion cracking was observed by a leakage event as opposed to, you know, a leakage event that's within the one GPM range.

MR. MURPHY: It's our understanding from the documentation submitted that it's our understanding from the CEBO A report and other supporting information that there haven't been any apex leaking tubes.

MR. HOLIAN: We want your verification that that's not the case.

MR. MURPHY: Apart from the rupture.

MR. HOLIAN: And your verification that that's not the case. And the context of that question is that if we were to look at the Monte Carlo and our understanding of that analysis today, we would see many more leakage events than we've seen in the industry and issues that will go with that rupture.

MR. MILLER: That's one piece. The other pieces are ones that we assume you have, because they're all questions that relate to the basis for the things that you presented. This piece might be a bit of a look-up. And we'll talk a moment about the timing of this.

MR. HOLIAN: In general, as I mentioned earlier, you know, the NRC will be taking actions in general on where you fall in the action matrix on this specific aspect. As we mentioned in the beginning of the session, we would hope to finalize a determination within a 30-day time frame, approximately 30-day time frame. To follow that along on our schedule that if you could get us that in the time frame of about a week, we can deal with anything on that order.

MR. MILLER: I would like to specifically ask for, you know, if there's some problem with that, you get back to us. I think it's best for you and for us if we just be specific on that. We believe this is information that you have, you should have.

Just a couple things. First of all, we didn't question you, as Brian said, really at all on the '97 inspection. I think the one thing that it is important to say here, and that is that our issue there really has to do with how you deal with adverse conditions. It's a broad issue at the station. It's how do you proceed when you're faced with conditions that give indication of a problem when you've got complicating factors. Do you proceed with the idea that some broad or some industry code doesn't specifically tell you to do something or is there a questioning attitude? And I think that's a fundamental point.

I won't ask you to elaborate on that, but that's a lot of where we're coming from and a lot of what is at issue here.

With respect to the other, and the bulk of our discussion today, which had to do with the determination process, I think you all certainly know that we're in the early full implementation of what's really a radically different reactor oversight program. And this new process features a lot of very good things, greater objectivity, greater scrutibility as far as our processes are concerned, and very much -- very importantly, a focus on risk. And one feature in that respect that's, you know, prominent of course is the significance determination process.

And in this respect, we are very much pioneers. You're pioneers along with us. It is a process that centers on the probabilistic risk assessment, IPE process. And that process is what it is. It's a tedious process. And it's why we need experts like Steve Long or Jim Trapp and Tom Shedlosky and Mr. Gaynor and others. It's a tedious process.

And so I say that because this meeting has been in many respects, you know, very detailed in the discussions, a lot of detailed questioning from us. I think it is important -- we feel it's important to work this process through in a very rigorous way, to test it. Because we are pioneering things. And I think the Commission and the other stakeholders deserve for us to do this in a fairly rigorous way so that we know how well this thing works.

And so this is all in the way of really thanking the people here on both sides who engaged in what I think is a very constructive, and at times tedious, but a constructive discussion. And it's why we -- while in the end, we will make decisions in the action matrix, and we're not going to get overly hung up -- we're not going to overcook this thing in terms of getting in perfect agreement, you know, we don't need that necessarily to go forward.

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