OFFICIAL TRANSCRIPT OF PROCEEDINGS

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

Title: NRC AND CONSOLIDATED EDISON TECHNICAL MEETING REGARDING IP2 STEAM GENERATOR

Work Order No.: NRC-1263

LOCATION: Rockville, MD

DATE: Wednesday, May 3, 2000

PAGES: 1 - 157

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

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

***

NRC AND CONSOLIDATED EDISON TECHNICAL

MEETING REGARDING IP2 STEAM GENERATOR

Nuclear Regulatory Commission
Two White Flint North
Room 3B-45
11545 Rockville Pike
Rockville, Maryland

Wednesday, May 3, 2000

The above-entitled meeting commenced, pursuant to notice, at 11:05 a.m.

PARTICIPANTS:

JEFF ALLAN, NRC Project Manager for Indian Point-2

PARTICIPANTS: [continued]

STEPHANIE KAUFMANN, Materials and Chemical Engineering, NRR

EMMETT MURPHY, Material and Chemical Engineering, NRR

JACK STROSNIDER, Director of the Division of Engineering

MARSHA GAMBORINI, Division of Licensing Project Management, acting Section Chief, IP-2

DAVID LU, Region I

TED SULLIVAN, Section Chief in the Materials and Chemical Engineering Branch

TOM PITTERLE, Westinghouse, Steam Generator Engineering

TOM ESSELMAN, ALTRAN Corporation

ANDY NEFF, Independent QDA for Con Ed

JIMMY MARK, Con Edison Engineering

JACK PARRY, Project Management, Con Edison

JIM BAUMSTARK, Vice President of Engineering for Con Edison

DON McADAM, Director of Nuclear Safety and Licensing

DON ADANONIS, Westinghouse

GARY HENRY, EPRI

PROCEEDINGS

[11:05 a.m.]

MR. ALLAN: My name is Jeff Allan. I'm the Project Manager for Indian Point-2, the NRC Project Manager, and I'm here to open up a meeting here between Con Ed and NRC. This is a technical meeting between NRC technical staff and Consolidated Edison, the licensee for Indian Point-2.

This meeting is going to be different from other meetings we've held in the past in that the other meetings have pretty much been management level meetings. This one is going to be a very technical discussion between us and Con Ed, in which we're going to -- we've put some topics of discussion, last week, on the 28th of April, which had topics related to their root cause evaluation that they performed and submitted to the NRC on April 14.

We'll have conversations and questions regarding the root cause evaluation and other topics that Con Ed wants to discuss related to inspection of the steam generators.

There are some handouts. Con Ed is going to have a presentation with slides. We're having copies made as we sit now, and they will be handed out to the members of the public as we go through the meeting.

This meeting is open to the public for observation and that means that this is going to be between the tech staff here and Con Ed. We're scheduled for 11:00 until 4:00 this afternoon. We're going to take a break approximately around 1:00 to allow lunch.

At the end of the technical meeting, if time allows, we're going to have a question and answer session between NRC staff and members of the public who are interested in asking questions of us. That's if we have time at the end.

This meeting is being transcribed and for the participants here, I'd ask that when you state questions, at least the first time, that you state your name for the purposes of the transcription. And for members of the public, this transcript will be made available on the NRC's web page, along with the slides from this particular presentation.

Just a little about the web, in case you haven't had an opportunity to view it, we started an initiative with this particular response to this event on February 15. We have an NRC IP-2 event web page. This web page can be found, if you go to the NRC's home page, which is www.nrc.gov, and under "What's New," you can go to the link that says Indian Point-2 event.

There we've taken efforts to put all the correspondence related to the event that's been transferred between Con Ed and NRC on that site. We are trying to maintain it to give you guys the best access we can to all the documentation that we have.

Just a little bit about the room and this floor. This room, there's no food or beverages allowed in this room. Just outside the doorway, there is a public telephone and in the elevator lobby, there are restrooms just towards the television. The men's room is to the left, the ladies' room is to the right.

With that, do you have any opening comments you'd like to make?

MR. SHARRON: No, not really, Jim. I just want to thank you and your staff for coming down here and making this presentation. I know you're still busy trying to get the inspection finished up and everything, so we appreciate that.

It might be worthwhile if we go around and at least identify who is at the table here. My name is Brian Sharron. I'm the Associate Director for Project Licensing and Technical Assessment. Stephanie?

MS. KAUFMANN: Stephanie Kaufmann, Materials and Chemical Engineering, NRR.

MR. MURPHY: Emmett Murphy, the same.

MR. STROSNIDER: I'm Jack Strosnider, Director of the Division of Engineering.

MS. GAMBORINI: I'm Marsha Gamborini, Division of

Licensing Project Management, acting Section Chief, IP-2.

MR. LU: I'm David Lu, Region I.

MR. SULLIVAN: I'm Ted Sullivan. I'm a Section Chief in the Materials and Chemical Engineering Branch.

MR. PITTERLE: Tom Pitterle, Westinghouse, Steam Generator Engineering.

MR. ESSELMAN: Tom Esselman, ALTRAN Corporation.

MR. NEFF: Andy Neff. I'm the independent QDA for Con Ed.

MR. MARK: Jimmy Mark, Con Edison Engineering.

MR. PARRY: Jack Parry, Project Management, Con Edison.

MR. BAUMSTARK: I'm Jim Baumstark, Vice President of Engineering for Con Edison.

MR. McADAM: Don McAdam, Director of Nuclear Safety and Licensing.

MR. ADANONIS: I'm Don Adanonis, with Westinghouse.

MR. HENRY: I'm Gary Henry, with EPRI.

MR. ALLAN: And Region I may join us via videoconferencing as we go. With that, I'll turn it over to Jim Baumstark, Con Ed, for Con Ed's presentation.

MR. BAUMSTARK: Thank you, Jeff. Dr. Sharron, members of NRC staff, thank you for providing us with the opportunity today to discuss our steam generators with you.

Last Friday, you provided us with 17 topics for discussion in today's meeting.

Sorting through these topics, we came up with four presentations, which will form the basis for our discussion. We have attempted to weave the 17 topics into the four presentations.

Please don't hold me strictly accountable for that, because a number of the topics will overlap the various presentations we have prepared.

Jack Parry will begin with an overview of our root cause analysis. He will address discussion topics four, six, 12, 13 through 15, and 17.

Jimmy Mark and Andy Neff will follow with a further discussion of the '97 inspection program and how it contrasts with the 2000 inspection program. They will cover topic numbers one and seven through ten.

Tom Esselman will describe the finite element analysis used to predict stresses in lower row U-bends. He will address topics three, five, 11 and 16.

And, lastly, Tom Pitterle will discuss key elements of our condition monitoring operational assessment, which is final states of development. He will cover topic two.

Without questions, these presentations will cover about two and a half hours. So if possible, I'd like to see if we could hold questions to a minimum until we conclude our presentations.

Before we begin, I would like to state that our industry has learned much about steam generators and their inspection methodology over the last two and a half months.

Had we, as an industry, known, in 1997, what we know now because of recently evolved technology, the event most probably would have been preventable. We conducted an adequate inspection in 1997 based on the then current equipment, processes and procedures. In fact, we believe we exceeded the industry standard at the time by examining all tubes in 1997 and conservatively plugging both indications we understood, as well as those indications which could not be resolved as to a specific tube deterioration mechanism.

In retrospect, we also need to be as critical of our 1997 methods as possible. We need to learn as much as we possibly can and pass that information on to others in our industry, so that similar events can be prevented.

Working with the NRC, that is our common resolve. Jack Parry will now begin with an overview of our root cause analysis.

MR. PARRY: As Jim indicated, my name is Jack Parry, with Consolidated Edison. In this job, I was the Project Manager in the Steam Generator Inspection Program.

There are a number of topics rolled into my presentation. I will provide an overview of the inspection program for the U-bends and how involved. I'm going to try and address some of the questions that were put to us; in particular, primary to secondary leakage, history of the tube restrictions in the steam generators, other issues. We were asked about the primary water chemistry, for example.

I'll try and put in perspective what we knew about row two tubes from the eddy current inspection in 1997 and also highlight the study done by Dominion Engineering for us from '95 to '97 and 2000 and what we should expect for various types of defects in the steam generators.

Starting off with February 15. At 1950 hours, or 7:15 p.m. that night, primary to secondary leakage was three and a half gallons per day, which was below any action point that we had in our procedures.

At 1929 hours, approximately 7:30 that night, primary to secondary leakage escalated to 75 to 100 gallons per minute, and we shut the plant down. We went through a number events in the cool-down process. From the steam generator perspective, the first step is we identified a tube in 24 steam generator, row two, column five, that was leaking, and this occurred February 27 this year.

The first inspection step was to put a Welch-Allyn, which is a small video probe camera, into the tube, and we identified a large axial. By large, I mean approximately two inches, at the top of the tube.

Next, you have to try and quantify that indication with a +Point probe into tube R2C5, from the cold leg side, and try to analyze the indication. The shoes on the probe got caught on the indication at the top of the tube. We only obtained partial data.

From that data, looking at the crack tip, the estimated locations is at the extrados of the tube.

Our next step for that tube was we put a bobbin probe, a flexible bobbin probe into the area and were able to quantify the length a little better. We felt it was 1.7 to 1.8 inches long, again, at the apex of the tube.

To continue to try and quantify the data, what we did is something a little innovative. We took a +Point probe and glued the shoes down. The shoes are what keeps it being a surface-gliding probe and by securing those down, that prevented it from catching on the indication of the tube, and we were then able to obtain an eddy current analysis of the indication.

So by doing that, we confirmed this at the extrados, determined the axial indication, length about two inches to two and a half inches long, and, at the same time all this was going, we were also performing inspections on the secondary side of the steam generators.

Once we identified where it was, we also went back and started looking historically at what the last inspection program can tell us. We went back and we looked in the '97 data and using 20/20 hindsight and knowing some indications probably in that tube, we identified the indication at the U-bend at the apex in tube R2C5.

We then concluded the problem was primary water stress corrosion, based on looking at the phase angle of the analysis. We also wanted to look and determine what weaknesses and how we've improved from '97 to 2000. Some of the things we identified was the signal-to-noise issue, which is significant in determining, analyzing the eddy current program, sources of the signal-to-noise problems, the outside tube deposits and mass signals, and also we had some concern with probe location speed and varying the tube as the +Point probes examined the tube.

Examined the data, looked at the setup used in 1997, we developed noise criteria and in '97, the industry did not have, per se, noise criteria. There was an individual assessment done by each analyst. So what we developed was noise criteria, about when a tube will be called too noisy. We analyzed and determined that 400 kilohertz frequency in the probes has a better signal-to-noise ratio, and we set up procedures such that if a tube similar to the one R2C5, shown in this '97 indication, would be classified for re-analysis in the 2000 program.

We took all this information we learned in the '97 program and incorporated it into the 2000 inspection program and then began. We started the 2000 program following the EPRI-5, Rev. 5 guidelines, particularly for the phase setup for the inspection.

In the next discussion, Jimmy Mark and Andy Neff will provide additional information on that.

We provided instructions to the analysis on when an inspection should be called, perhaps reanalysis due to signal-to-noise ratio, and concluded that that should be rejected.

We set up a program to analyze the tube due to the noisy data. We provided training to all the analysts, approximately three days, the first phase of the program, and, again, provided additional information and training evolved as we went through this.

We developed lessons learned as the whole program evolved and we kept improving the program as we went through.

Once we established that, initial inspection began. Initial inspection of the U-bends in the low row tubes consisted of 100 percent of the U-bends, looking at rows two, three and four. Row one's were plugged initially before the plant started up, so we do not have those in service.

During that examination, we identified three additional tubes that had indication of primary water stress corrosion cracking and the extrados of the U-bend in the row two tubes. We found no indications of primary water stress corrosion cracking in row three or four tubes.

We conducted extensive re-testing on tubes that we considered noisy and many of our tubes, and I'll give you some numbers in just a moment, we still considered unacceptable from being initially re-analyzed. That was at the end of the first phase of our inspection program.

Our second phase began with an independent analysis of the information again. What we did, in looking at what we had just learned from the first phase, we updated the training program. We brought in new additional analysts who had not seen this project before. We set up another primary, secondary and resolution team for the eddy current data and we re-analyzed all of the 2000 U-bend midrange +Point information. Midrange would be midrange frequency.

During that review, we found no new indications. The three row two indications were reconfirmed by the separate team. The result was we still had 53 percent of our tubes being classified as unacceptable due to a low signal-to-noise ratio.

As that was going on, we also looked at other options that were not available to us in 1997. Two of them we pursued was looking at the midrange probe using a higher frequency, 750 kilohertz, and also a new probe that was recommended, 800 kilohertz, high frequency +Point probe. We tested both of those probes looking at indications that knew were present in tubes and in our evaluation, we determined the 800 kilohertz, high frequency probe was the best one to use to expand and continue our examination program.

The site and EPRI conducted a program and that 800 kilohertz frequency probe was qualified for use at the site and in the industry the first time on March 20 this year.

We then, again, went back and looked at the same low row tubes. We re-tested row two and all of row three and all of the row four tubes that we had classified as questionable data.

During that phase, we found four new indications of primary water stress corrosion cracking in tubes. These indications were found in tubes that were previously classified as noisy and had eventually been plugged. We hadn't gone this extra step.

There were no new indications found in tubes that we had classified as no defect. So the high frequency probe did not find anything in a row two tube that we had cleared with our program. And, again, using a high frequency probe, we found no indications in rows three or four tubes.

What this resulted in is we were able to recover 450 tubes for use that we would have plugged. The data in the high frequency probe was much better. Of those 452, five we still felt the analysis was not what we wanted and we put those on our plugging list to take out of service.

Of those row two tubes, we also used in situ pressure tests, which I know you know is high pressure test of all the axial indications in the U-bends. That resulted in seven row two tubes being tested. This exceeded the number we had to do, if you follow strictly the EPRI guidelines. Of those seven, three of the U-bends leaked or they met the burst criteria, and I believe Tom Pitterle will discuss that later on in his presentation.

At the end of the program, all the row two tubes, whether we found indications or not, are plugged and taken out of service to isolate this concern.

Jimmy Mark and Tom Pitterle will also talk about the inspection program, provide more details for you, but I wanted to try and start off giving you a chronology of events, how we started, our first steps, how we moved into high frequency probe, what the final result was.

From an overview standpoint, while the primary side was going on, we also had a number of events going on in the secondary side. One of the actions we took is we pressurized the secondary side of each generator, where we had cameras in the primary side to look for any leaks.

During that process, in 23 generator, we found three plugs that had small leaks. Those were drilled out and replaced during this outage. We also performed secondary side component inspections, and that this is is visual inspections, looking inside the generators, both in the hand-holes at the bottom of the secondary side, looking up, then the ports at the top, looking in and down.

Two of our four generators, we had ports already that had been observed at the support plate, 22 and 23. The 24 generator, we have a port, but it wasn't exactly where we wanted, where we wanted to look at. So this outage, we ended up installing two ports, one in 21 generator and one in 24 generator, to give us a better view of the six support plate at the top of the steam generator. So that was going on at the same time.

In doing that, then we also performed measurements on 24 generator, which is the one that had the leaking tube, to quantify hourglassing, which is an inward movement of the tubes. In row one is a device, a field gauge type device. We measured about half an inch hourglassing between the low one tubes in 24 generator, approximately even with the tube that leaked. That was then put in our counts for our stress evaluation, and Tom Esselman will be bringing that up in his talk later on.

Then, finally, as a routine measure, we performed Sludge Lancing and Fosar at all four generators.

There will be a lot of other discussions as far as what's going on in the generators, but, again, that's an overview for you of what we did from the U-bend inspection program.

The other topics we wanted to talk about was primary to secondary leakage. On February 15, the start of the shutdown, nitrogen-16 rad monitors, which is one for each individual line for each steam generator, were monitoring and in-service. Alarm points in those monitors are ten gallons per day, 25 gallons per day, and 150 gallons per day, which is our administrative limit to shut down. Our technical specifications are approximately 450 gallons per day. Our administrative limits are lower than that.

There was a recorder on the instrument that was not working, but those instruments do have a common alarm for their control room, what we call the accident assessment panel. That comes up, the operators respond locally to see what was causing the alarm, and it also is the local alarm, the N-16 monitors, to help an operator qualify that in his investigation. Those alarms did not go off.

Another thing I'll put up here is the leak rate trend that we have. I know some of you may have seen this. This is our primary to secondary leakage, January 1, '99 up until February 15. You can see it's four gallons per day. At the height of the event, we're monitoring it and it went from 3.4 gallons per day to about 75 to 100 gallons per minute at the start of the event.

MR. MURPHY: Excuse me. Jack, you indicated the alarms didn't go off. They did go off.

MR. PARRY: You're right. I'm saying normally, for 3.4, they did not. But they did go off when we spiked up to the 100 gallons per minute. Thank you.

Also, in the primary to secondary leakage program, chemistry, as a routine, when they check these monitors once per shift. Another indication we have to monitor primary to secondary leakage is what we call our error check, the radiation monitor. That quantifies total. It does not isolate it to a single generator. Chemistry also pulls samples from that halfway, performs laboratory analysis using approved procedures, and, again, at about 7:15, just before the event occurred on February 15, the leak rate was approximately 3.4 gallons per day on the N-16 monitor.

One of the things this event caused us to do is we're looking at our limits for administrative limits for primary to secondary leakage. EPRI has issued new guidance as of February of this year. It's approximately half of what's in place now. There's also two limits. There's one for total leakage per day and also rate of change.

We at least implement those EPRI guidelines and that will be one of the actions we'll follow up on. This was a review by our steam generator committee, which was put into place, and then also we had to change the operation procedures and implement these.

Another issue we looked at is tube restrictions. One of the parameters we have in doing the steam generator inspection is any tube that won't pass a probe that's called a 610, which is the size of the probe, has to be plugged. So we look at how many tubes each outage we have to plug that won't pass a 610 probe.

This outage, there were two tubes that would not pass a 610 probe. To try and give you a perspective from a cracking standpoint, let me put up a graph. This is the tubes plugged for 610 probe for the life of the plant. Earlier in the life of the plant, there was an issue with denting, which is a corrosion phenomenon that increases the rate. This occurs due to some operational changes, changing the temperature we operate the primary side at.

This process, as you can see, after about 1989, was reduced. In 1997, we went to inspecting the generators 100 percent for the first time. We had an increase in how many tubes we plugged due to not passing the 610 probe up to 20. So, again, in this outage, we only identified two additional ones that we had to plug due to not being able to pass the 610 probe.

Now, that in itself doesn't give you a full perspective.

MR. SULLIVAN: Can I ask a question? What you just put up, the support plates?

MR. PARRY: Anywhere. Because if it couldn't pass a 610 probe, whether it was through the first support plate or the sixth one, then you couldn't inspect the full length of the tube, that caused us to take it out of service. We had to take it out of service.

Now, historically, '97 and 2000 were the first times we inspected 100 percent of all tubes and all restrictions. So we performed a comparison this time, looking at what was the change for each restriction and each support plate. Now, this is for 23 steam generator.

What you can see is, looking at each, 6H means the top support plate on the hot side, 6C is the top support plate on the cold side, and you work down through the six support plates.

What you can see is about 14 percent of the restrictions decreased, 70 percent stayed the same, but the one piece of information we never had before from being able to do this comparison is about 16 percent were able to get a larger probe through them.

For example, in '97, we had a 640 probe go through it, now we can get a 680, or last time we had a 620 get through it, we had a 640. That's the first time I've been able to compare that. I don't know the history of what the other outages would be. So '97 to 2000 is the first time we had the data collected to be able to make this comparison on a restriction by tube, by support plate, for all four generators.

MR. SULLIVAN: Is this a comparison based on exactly the same components?

MR. PARRY: Yes. For example, if indication for an intersection passes 700 and then this time it wouldn't pass a 700, had to use a 680, that would go into the smaller column. If it was the same, no change, that before, I could get a 620 through it, and now I could not get a 620 through it, and now I can, that would be in the larger column.

There's a couple of issues I've lumped together. One of them was the susceptibility of our Alloy-600 to primary water stress corrosion cracking. As you all know, primary water stress corrosion cracking is a function of material stressing the environment.

We looked at the material, the yield temperatures it's manufactured at, which is about 1,850 degrees Fahrenheit, this is higher than a lot of the other tubes in the industry, which is about 1,700. So this makes it less susceptible to primary water stress corrosion cracking due to the initial annealing temperatures it was manufactured at.

The environment has to do with the temperature that the primary side operates at, T-hot it's called. We operate for the past ten years at approximately 590 degrees Fahrenheit. Before that, we operated even lower, approximately 575.

A number of the units in the industry operate at 600 or higher. So, again, this is a parameter that makes our tubes less susceptible to primary water stress corrosion cracking while operating at the lower temperature.

We also look at primary water chemistry, because that would be an effect. Looking at these parameters, our assessment was these were not an impact on this event occurring.

Stress was also evaluated, and that will be talked upon in Tom Esselman's presentation, as far as the material annealing, the environment and the primary water chemistry. Our assessment is these were not a factor in this event occurring.

Assessing the primary water chemistry, we follow the industry guidelines. Some of the main parameters you look at are boron and lithium curves, which then matches your pH and your hydrogen concentrations. And, again, we assessed this as not an issue.

I'll have a graph here in a second that I'll put up.

The last issue we looked at in what I call other issues is we looked at outside diameter stress corrosion cracking potential in the row two tubes. There's some industry data, at least one event that cites this potential for happening. This was a consideration in plugging our row two tubes.

It was also a consideration in using a high frequency probe. What we did is we checked the high frequency probe for tubes that we knew had indications and against test samples to convince ourselves that a high frequency probe could see outside diameter stress corrosion cracking in the analysis.

The chemistry. The graph, the upper one shows the band we keep our boron/lithium between and the bottom one shows our hydrogen concentration, and, in both cases, we have maintained between those boundaries.

The next topic I want to try and highlight is from the perspective of what we knew about row two of some of the steam generators back in '97. One of the things we look at each outage is the secondary side support plates. In 1997, on 22 and 23 steam generators, we do have the ports that allow us to see the six support plates. Those were inspected.

One of the things we looked for were any indications of cracking in the flow slots. There were no indications of cracking in the sixth support plate of the flow slots of the 22 and 23 generators in 1997.

And looking back on the videos, the engineers, at the time, we assessed there was no observable hourglassing in 22 and 23 steam generators at the sixth support plate.

MR. MURPHY: Jack, when looking at, I think, the flow slot from steam generator 23, some hourglassing was observed and it was reported in this inspection that this condition was unchanged from previous inspections.

MR. PARRY: What we looked at, those flow slots, there may have been a slight one, but in '97, when the engineers looked at it and what they reported was their observation, our observation was it was viewed as having no cracks in the flow slots and no hourglassing.

I know what you're talking about for 23 and it may be present, okay, but I'm trying to put in perspective what was analyzed in '97 and then how we looked at it for back then.

The '97 inspection program, we felt it was consistent with industry standards. Looking at the row two and row three tubes in '97, we used the best probe available. It was the +Point probe for looking at those tubes. The probe did detect one indication of primary water stress corrosion cracking.

Our interpretation was that it could see that method of tube degradation. There was no indications of row three primary water stress corrosion cracking in '97.

Another perspective we had back then is there was a company called Dominion Engineering and one of their main expertise is looking at your analysis from your inspections, looking at frequency for different types of failures you see, and then predicting what you should see in future outages.

We contracted with Dominion in '95, '97, and also in 2000 to look at our steam generator program, help us predict what we should see each outage. Then, again, this outage, they were asked to update the information, and I'll highlight that here in a second.

Starting with their review in '95, there were no U-bend cracks detected. The prediction they performed, looking at industry data, they defined a slope which correlates to how many indications you should see in the future, called a Weibull slope. Based on having no cracks seen in 1995, predictions were we should see none in '97 or 2000.

Two of their main factors for making that assessment was the lower temperature we operate our primary system at compared to other plants and what we call the

Huntington tubes, which were the tubes that were annealed at a higher temperature, at 1,850 versus 1,700, I mentioned earlier.

In '97, they, again, looked at our data. In '97, we detected the one indication of primary water stress corrosion cracking in the U-bend. They then assessed that, assigned what they call a Weibull slope of four, which is consistent with the industry, and predicted that we should see potentially one new flaw in our next operating cycle of primary water stress corrosion cracking in the U-bend tubes.

What we also had them do is come back and look at the data in 2000, from two perspectives. One of the things they do is they take a consistent inspection method, one using the midrange, what we should have seen for indications, the midrange in '97, looking forward, and then as a check, what we saw in 2000, with the high frequency probe and back-calculating and seeing if the slope, the Weibull slope is consistent for both processes.

Starting off, what they did is they looked at the rate of increase at the U-bend, the primary water stress corrosion cracking, using the midrange data, what we think we should have seen with the midrange probe in '97 and 2000. Their analysis shows, '97 should have shown three flaws, with a cumulative five in 2000, which gives a Weibull slope of 4.7, which, again, is consistent with the industry.

Using that Weibull slope of 4.7, we then looked at what we found in 2000 using the high frequency probe, back-calculated it, and again confirmed that this slope, this rate of expectation of the primary water stress corrosion cracking is consistent.

Now, when I say industry data, the two main reports that they used were these two EPRI reports, which Dominion had a major hand in writing, and developed a lot of the industry data on this.

Again, in saying it's within the industry standards, what Dominion uses is a median of four for a slope, with a range of two to six, which the higher the slope is, the higher your frequency of steam the next time will be.

Again, the industry perspective, stress corrosion cracking has a large scatter value with it, depending, site-to-site. Normally you'll see small numbers, failures early, and then an increase as time progresses. What this process does is it takes your frequency, matches it to the what the industry has seen, and then helps you predict what you expect to see the next time.

Using that, what they told us in '97, seeing one indication, in our perspective, was reasonable. If we were to keep row two in service, which we were not, we're plugging it, their prediction is we see a modest increase in the number of primary water stress corrosion cracking flaws in row two.

The other issue that we asked them to look at is row three tubes. This will also be talked of by Tom Esselman. We asked them what's the potential for row three tubes developing primary water stress corrosion cracking. There's no known case of row three tubes having that. Our inspections detected no indication of primary water stress corrosion cracking in row three tubes.

The crack initiation in row three would be a lot lower, due to lower stress and the lower cold work in row three versus row two. The crack growth rates are expected to be a lot lower in row three versus row two, due to the lower stress and lower cold work.

MR. STROSNIDER: Just a quick clarification point on the first bullet there. You indicated no leaks in row three U-bends. But have there been any indications?

MR. PARRY: I spoke to Jeff yesterday on this. I don't remember. He stressed to me that there were no leaks. I don't know if he -- I don't remember if he told me there were no indications. I can follow up for you.

MR. STROSNIDER: Okay. Appreciate that, thank you.

MR. PARRY: The conclusion they reached for us is there is very low likelihood of any flaws developing, large flaws developing from the next inspection. There is a low potential for crack initiation, a low growth rate for the row three tubes, and for a modified operating interval.

I went through a lot of information in a very short period of time. We've got three more people who are going to be just as concentrated. So I appreciate your attention, and hope I've helped try to answer some of the questions.

MR. MURPHY: Jack, early on, you indicated that the magnitude of the leak associated with the failure was on the order of 75 to 100 gpm. Haven't you folks done more recent calculations indicating the number is up around 150?

MR. PARRY: The number I remember, and I've got to go back and look, because to be honest, I've been focused on this inspection program and may have lost some of the details. I've heard a number like of 103. Again, I can follow up on that for you. I know it wasn't 200, 300, 400 or anything like that. The 75 to 100, I thought, is in the right range. Maybe it's 103 or something like that, but that was the last number I heard, was around 75 to 100 gpm is where we keep that.

I would like to next introduce Jimmy Mark, who will talk about the inspection programs in '97 and 2000, with Andy Neff, also.

MR. MARK: With me will be Andy Neff. He is our independent QDA. My name is Jimmy Mark, Con Ed Engineering. I will be reviewing the low row U-bend examinations that we did in 1997, the qualification work, and also the same in the 2000 outage.

The technique qualification that we did leading up to the 1997 examination for the row two and row three U-bends was based on Rev. 4 of the EPRI guidelines. That included the work that was done with the midrange probe. We did qualification using 150, 300 and 400 kilohertz. Our qualification sample set that we worked with had two pulled tubes from our plants. We also had 24 EDM samples and in all cases, we were able to detect these 26 flaws, which gives us a POD of 91.5 percent and at a 90 percent confidence level.

At that time, with Rev. 4 of the guidelines, there was no requirement for site-specific technique qualification. This '97 use of the +Point probe was the first time that we had used this probe at Indian Point.

For the examinations, we did a site-specific performance demonstration, also in accordance with Rev. 4 of the guidelines. We used a practical exam that consisted of data from the industry, because at that point, row two U-bend was not detected at Indian Point.

The calibration setup that we used was within the industry variance. The data quality was non-existent at that time. There was no specific written requirement for data quality. So it was left up to the analysts, and these analysts are highly trained personnel. They're level three eddy current people, as well as EPRI qualified QDA, qualified data analyst.

There's a very rigorous training program, exam program that they go through, and within the whole word, there's about 300 of these people that go through all the outages and view all this data for us.

The +Point noise level that they found was similar to other steam generators that had Alloy-600 mil-anneal tubing. So the data that we saw at Indian Point as far as noise goes was not any different than what we saw at some other plants that had mil-anneal Alloy-600, there's some noise, also.

PWSCC was identified in the one tube, steam generator 24, row two, column 67. That gave us a level of confidence that the technique that we were using had a qualification that was able to detect the flaw.
Andy Neff is next and is going to review some of the differences between the analysis setup that we used in 1997 and 2000 for the midrange +Point probe.

MR. NEFF: These graphs illustrate the comparison between the '97 and 2000 data. We looked at the ETASS, which is the document that defines the technique, and found that in '97, the phase was about five to six degrees lower than is currently required.

We also looked at the various frequencies to determine if one frequency would provide better detection than the others. Then, finally, we looked back at the effect of applying the differences to see if some conclusion could be made regarding the detectability versus the setup and the frequency used.

This graphic shows the defect as it was originally discovered. As you can see, there are patterns along here from the noise and this is the defect. This is the same, this is using the 1997 setup. This is using the 400 kilohertz. As you can see, the change in the noise level is not very apparent. It is slightly better, but it is not drastically better.

This shows the same data using the year 2000 setup at 300 kilohertz and at 400 kilohertz. Our conclusion is that the setup changes are marginally better, but not greatly better.

Next, we looked at the setups in row two, column five. This is the tube that failed. This is called NDD in 1997. This level of noise is not uncommon. It would not probably be recognized in 1997 as being defective. However, with the lessons learned, we would now call that bad data.

This is the 400 kilohertz in 1997. This is all

1997 data. And this is with the 2000 setup and with the 2000 setup at 400 kilohertz.

So during this current outage, the 2000 outage, we qualified the midrange +Point probe again under Rev. 5 to the guidelines. We did a site-specific performance demonstration and we had a written training supplement that was developed based on information that we now know. We also developed a document called the IP-2 spring 2000 outage U-bend +Point analysis training, and, along with that, we looked at the setup for the 20 degree, 20 percent ID EDM notch and it was visible at six to ten degrees phase rotation, which is consistent with the industry variance.

There was now a requirement for data quality. We put that in based on the data that we just saw and the review with you on the row two, column five situation. So now we included a data quality requirement.

What we did was examined all row two, all row three, and all row four U-bends with this +Point probe and we identified three PWSCC indications in three low row U-bends. In the 21 generator, we had one, and we had a tube in steam generator 24, and the data quality evolved as we conducted the exam.

BDA was the call that we gave and it's the call that the analysts made and it was data that was not clearly clean and there's some question and it should be re-reviewed.

After we had the initial team review of the data, the primary, the secondary and the reso team, while this was going on and all this learning about data quality was evolving, we said, okay, let's set up another team to review everything.

So we set up a team called a tertiary review team and this team was a new group of analysts coming in. These were senior people that have been doing this type of work for a long time and very experienced, and we gave them additional information that we learned along the way.

They reviewed all the data from two, three and four, and they came up with the same three indications that the initial team found and just confirmed it. And as far as the data quality goes, they called 457 out of the 863 tubes would have BDA, or bad data, because there would be low signal-to-noise ratio.

Because of the signal-to-noise ratio being the way that it was on watch point of your tube, we did an assessment of the high frequency +Point probe to see if it would enhance the detection of PWSCC.

So we took the midrange probe, qualified it for 750 kilohertz, and we also qualified an 800 kilohertz probe. The 800 kilohertz probe was the prototype probe that we had Ztech build for us. We ran it on two tubes that we had indications and we see that the indication was much clearer. The high frequency showed a better signal-to-noise ratio.

So based on that, we decided to re-inspect all row two, row three, and all the row four data that had low signal-to-noise ratios using the high frequency probe.

The qualification program for this high frequency probe was also based on Rev. 5 of the guidelines. The qualification document covered the qualification at 800 kilohertz and also 1,000 kilohertz. The sample set that we used for the qualification was the same sample set that we used for the initial midrange qual. Those were the two pulled U-bends from other plants, as well as the .4 EDM nodules, and just like the other probe, all flaws were detected. We had the same 91.5 percent POD at 90 percent confidence level.

We also did a deposit simulation with copper foil and it showed that there was no effect on the detectability of the flaw. So by doing this, the high frequency probe was qualified for our particular site.

After we did the testing -- correction. This is the listing of the flaws in the qualification data set. We had two pulled tubes, with 40 percent flaws, and a variety of laboratory EDM notches, the lowest in depth being 27 percent, going all the way to thruwall.

This is just a graphic presentation of the POD and the confidence level.

So now we had a qualified 800 kilohertz probe for our row two U-bends. We were the first ones in the industry to use the 800 kilohertz probe for these low row U-bends and like I just said, we re-inspected all the row two and row three, we reexamined all the row fours that had the BDA or bad data classification.

We applied all the data quality requirements as far as signal-to-noise ratio based on what we had learned from the midrange probe that we had used up to now.

The high frequency +Point probe identified PWSCC in four tubes that were previously classified as BDA or RST. And five of the tubes remained, even after testing with the high frequency probe, as DBA.

This table is a summary of midrange work that was done by the first team, as well as the work that was done with the high frequency probe. It's a little busy, but it has a lot of information on it.

We have each generator. We have, under the tertiary team or independent review team, a list of all the acceptable data that we have. The low signal-to-noise data on the BDAs and what we did was reexamined all the row twos and all the row threes with the high frequency probe. So if you add up 40 for steam generator 21, row two, 40 plus 32 gives you 71 plus the one, and out of this 72, 71 had acceptable data using the high frequency probe and one was below signal-to-noise ratio which ended up getting plugged.

Under row four, we only took low signal-to-noise data, like I just said, and we reanalyzed it with the high frequency probe, and, hence, the 46 shows up here. When you add up all these numbers and you add up the acceptable and low signal-to-noise data, the difference in the total will be the difference in the row fours that had acceptable data.

So just to point out, again, we have 53 percent of BDA under the original midrange +Point probe and now we have .8 percent BDA under the 800 kilohertz high frequency probe. This is an improvement in signal-to-noise ratio and also quality of the data.

This is a listing of all the row two U-bends with defects that we will be plugging. These were the findings with the midrange probe. We have SAI, single axial indications. We have BDAs, and we have one with an RST, or a restriction.

This one was tested with the midrange probe, but was restricted at the sixth support plate on the hot leg and the U-bend. But when we put the 800 kilohertz probe and managed to make it through, so it was called based on the high frequency probe. So that was SAI. The BDAs, like I said, were resolved with the high frequency probe and the signal showed much clearer and they were called as single axial indications.

In conclusion, the 1997 examination met industry guidelines and at that time, it was Revision 4 of the EPRI guidelines. It was an industry qualified technique at that time. It was a site-specific performance demonstration that was conducted. The calibration setup that we used was within industry variance and the U-bend +Point data was similar to other plants that had steam generators with Alloy-600 mil-anneal tubing that has some noise in it.

The current 2000 examination met the industry requirements. It's now called requirements because of NEI-9706, which mandates following the EPRI guidelines, now with Rev. 5. We have a site-specific qualified technique. We have a site-specific performance demonstration. Calibration setups that we use now were also still within the industry variance.

We have new data quality requirements, based on what we know now from our earlier examinations with the midrange probe this year.

MS. KAUFMANN: Andy, I wanted to ask you a couple of questions. Earlier on in the presentation, you said that the ETSS is developed through the midrange +Point probe. You talked about the 150, 300 and 400 kilohertz frequencies.

MR. MARK: Right.

MS. KAUFMANN: Did your plant-specific guidelines include looking at all thicknesses when you did the inspection in 1997?

MR. MARK: I'm not sure about that. Yes.

MS. KAUFMANN: And your signal-to-noise ratio, do you have a specific number that you put in your guidelines for your noise criteria? How did you work that?

MR. MARK: Back in those days, it was left up to the analysts. We have the written guidelines that we wrote, plus the information that was picked up from the earlier testing that we did.

MS. KAUFMANN: Is it a specific criteria or not? I'm not quite sure how the analysts -- what they looked at to decide.

MR. NEFF: The thing that we were focusing on, if we go back to the row two, column five, was the registry. If you see this little part, this mound that seems to be sticking out from the others, this is an indication that possibly the rotational -- the probe rotation is interrupted or not steady. So whenever -- and this was a key feature of row two, column five. So if we saw any of that, then it would have been bad data for that reason.

Some of the other things that they were looking at was the end plot, if there were speed variations which you can see along this line. If we had any kind of deposits that were sticking up about the size of what we would expect a defect to be, they would have been called bad data.

MR. MURPHY: You referred to the U-bend +Point data quality in 1997 as being similar to the industry. You're saying that the quality of the data that you were obtaining in the U-bends in 1997 was comparable to what we would typically see at PWRs in the U.S. industry today or in 1997?

MR. MARK: PWRs.

MR. MURPHY: Yes, PWRs.

MR. MARK: Some generators have noisier tubes than others. What we saw when we looked at our noise and compared it to some of the other plants that had noise, it's not much different than what they have.

MR. MURPHY: Are these plants that have been long since retired or are you speaking about a number of plants that continue to be in service?

MR. MARK: This is all historical data, for older plants.

MR. MURPHY: That have been retired.

MR. MARK: I believe so.

MR. MURPHY: Thank you.

MR. STROSNIDER: Just one follow-up question in that area. You're indicating that the quality of the data may be the same. Was your analysis the same as some of these other licensees may have used in terms of how you evaluated those data?

MR. MARK: Well, those older plants never had the benefit of the +Point probe. The +Point probe is a fairly new probe, because within the last five, eight years.

So the data that those earlier plants saw is not really the same quality of probe because the technology has changed quite a bit.

MR. STROSNIDER: It's an important point here. I want to make sure we're comparing apples and apples in terms of probes that are used, quality of the data, and how those data were analyzed.

I'm a little confused with your response.

MR. MURPHY: The root cause report that you submitted took note of the improved noise quality obtained with the high frequency probe and indicated that that kind of indicated that most of your noise problem with the midrange probe was coming from the surface deposits, the copper and the magnetite.

Is it fair to say that in recent years, that your plant is relatively unique in this respect?

What we're trying to get at it, from the root cause report, you have indicated that it was the surface deposits rather than ovality or geometry considerations that was largely responsible for the noise.

These surface deposits are uniquely a -- in terms of the degree of problem, degree of obfuscation, this problem is relatively unique to Indian Point, is it not?

MR. ADANONIS: Don Adanonis, from Westinghouse. Just as a means of clarification, the types of signals that we see here due to deposit influence and effects or probe riding are not atypical to Indian Point, as an operating plant.

MR. MURPHY: Could you repeat that, please?

MR. ADANONIS: They are not unique to Indian Point.

MR. MURPHY: Due to surface deposits.

MR. ADANONIS: Well, the combination of deposits and probe riding effects.

MR. MURPHY: Does the degree of noise associated with the surface deposits, is that comparable to other steam generators that we commonly see in the field today?

MR. ADANONIS: Yes.

MR. STROSNIDER: And, again, is there any difference in the way the data are then analyzed from site to site in terms of using different frequencies or different guidance to the examiners?

MR. ADANONIS: This at Indian Point-2 was the first recognition that this kind of situation could lead to some masking of potential for degradation. So this is really the first in the industry, that I'm aware, attempt to begin to put criteria in place that would identify such conditions and then take actions to mitigate it, that action to mitigate it was moving forward with the 800 kilohertz probe.

In my mind, that's the solution. The setup, I know there was a question of setup in some of the questions that came from the NRC, we don't see a significant improvement based on setup. There was a question on frequency. We don't see a significant improvement based on frequency, the frequency of the analysis.

The frequency of the examination going from the 400 kilohertz, the midrange probe, to the 800 kilohertz probe, is the action that handles this problem, and that's where we had to go.

In fact, I think Jimmy made the point, and I'll reiterate it, that this 800 kilohertz probe wasn't -- has not been used before in the industry and probably is only an idea in somebody's mind, until we move forward and apply it out there.

MS. KAUFMANN: For your qualification effort for both the midrange and the +Point, you have a data set. How did you evaluate that set as being applicable to IP-2 in terms of noise levels of those samples compared to what you see at IP-2?

MR. ADANONIS: When we said that this was site-qualified, Stephanie, we based that on our ability to go in not only the qualification that was done by Gary Henry at EPRI, but also based on then taking the information that we had collected through the qualification process, going in and looking at two flaws that we knew to exist, saw the improvement in results, went on to examine the rest of the tubes, identify new flaws, and we're able to -- and I think we have two other graphics that can show you, more clearly than these show you, the effect of application of the 800 kilohertz probe in eliminating the signal-to-noise issues that you see in these graphics. I think it may be helpful to show it now.

MS. KAUFMANN: I'm familiar with that. I'm just asking, was the 26 data set, the 26 sample data set, do you think that's representative of the conditions at IP-2?

MR. NEFF: We also used the copper to qualify the response to the certain flaws. We put copper foil over flaws on the standards, which we determined did not affect the detectability of defects. So this was an additional step.

MR. ADANONIS: I guess --

MS KAUFMANN: Still looking for an answer.

MR. ADANONIS: Copper, to the extent that we could on the laboratory samples, yes, we represented the conditions. Did we represent the exact conditions relative to probe ride? We didn't know that, so we took it to the field.

We believe that, based on the results that we've seen, the improved detectability, our ability to eliminate that noise, and given the results of an additional four PWSCC indications in row two tubes, that, yes, we have a technique now that we consider site qualified at Indian Point-2.

MR. MURPHY: Are there any efforts underway within the industry to demonstrate performance on a statistically significant set of U-bend specimens with real cracks?

MR. ADANONIS: At this point, I would have to ask Gary Henry.

MR. HENRY: There are none right at this moment. There are some that are proposed right now, but we don't know what the status is going to be of that program.

MR. ADANONIS: This is pretty new news. I think if you look at when our first delivery of a high frequency probe was March 20, but certainly that's something that we'll be looking into.

MR. MURPHY: When you state that the calibration setups were within industry variance, what specifically do you mean?

MR. NEFF: When the ETSS sheet, that specifies the technique, requires that you set the 40 percent IDE ten degrees. There was no 40 percent ID flaw on Rev. 4 versus the 1997 inspection. So another flaw was referenced.

When we used the setup on 2000 data and experimented to try to find out how much that altered the setup, we determined that the phase rotation was about five to six degrees, more shallow than it should have been.

Normally, we have a three degree setup error that is common in the industry. So this is not far removed from being within normal industry standards. We found that we did improve slightly the detection of IDE by rotating the phase.

MR. MURPHY: Thank you. The site specific performance demo guidelines in Rev. 4, these guidelines in Rev. 4 apply to the analysts rather than to techniques.

MR. NEFF: They apply to the techniques.

MR. MURPHY: And analysts? I'm talking about Rev. 4, the guidelines that you --

MR. NEFF: You define a technique which we then have to incorporate in our inspection program.

MR. ADANONIS: I think there might be a little confusion. The site specific performance demonstration program is basically the test that you administer to the analysts and it usually includes question relative to the training that you've given them, relative to plant specific conditions, what are the forms of degradation, there's a written examination that goes along with it, and then they are required to take a practical test, which includes samples or data from tubes.

If you've seen degradation in a plant, they will have plant specific data. As Jimmy mentioned, in 1997, we had not -- number one, had not applied a +Point probe at this point at Indian Point, nor had we seen any PWSCC in U-bends.

So the site specific performance demonstration consisted of a test on industry data that had been collected with the +Point probe on U-bends.

Now, where I say there is some confusion, is your question relative to site specific performance demonstration or to the site technique qualification or the site validation of the technique? It is included in Rev. 5 as a requirement, but it was not a requirement in Rev. 4.

MR. MURPHY: Well, I was asking whether Rev. 4, the performance demo requirements, applied strictly to the analysts or whether those guidelines also applied to techniques that were being --

MR. HENRY: Gary Henry, with EPRI. They're using qualified techniques and you're applying an analyst performance demonstration to those qualified techniques. So it applies to both.

MR. MURPHY: But the purpose of even the Rev. 4

performance demo would have been to address the applicability of the generically qualified techniques for the plant specific conditions.

MR. HENRY: It's implied in Rev. 4, but it's not specifically stated as a requirement in Rev. 4. That's why, in Rev. 5, when we wrote Rev. 5, that we explicitly put it as a requirement in Rev. 5 to address those issues.

MR. MARK: Also, one of our conclusions, when all the lessons learned were evaluated, the use of the high frequency probe was determined to be the most significant thing that we did to improve the POD and these are a couple of examples that show the difference between the midrange probe and the high frequency probe, as far as improving POD.

MR. NEFF: As you can see, there are lots of deviations from the presentation on the 400 kilohertz. And here it is much clearer on the 800 kilohertz. This is typical of all the data that we looked at.

MR. MARK: So earlier, when I talked about the setup rotations and the 300 kilohertz versus the 400 kilohertz midrange data, it really only provided minimal improvement. The high frequency probe has now been site validated for Indian Point-2, and the technology and the other things that we've learned based on these inspections are being passed on to industry and vendors involved in this kind of work.

Next, Tom Esselman is going to review the finite element work that we did on the support plates.

MR. ESSELMAN: Good afternoon. The subject I would like to discuss is the susceptibility of the small radius U-tubes in the U-bend region to primary water stress corrosion cracking. In doing this, I will touch upon the effects, the behavior of the tube support plates, primarily the top tube support plate, look at modality, and we will look at the differences between primarily the row two tube, a stiffer rube than a row three tube, and how those two tubes behave relative to each other and what we would expect to see in row three.

The approach that we've used is to look at, first, the tube support plates and determine, for the amount of hourglassing that was measured, how much each of the tubes in the vicinity of the tube support plate moves.

That will allow us to then move to see how that tube support plate motion affects the individual tubes. We then want to determine the stresses in the U-bend due to that tube support plate deformation and all the other operating conditions. We've done work on residual stresses that I will report to you and discuss residual stresses. We have included the effect of residual stresses in the row two and three tube, and we want to use that to assess the time or indicate the time to initiate cracking in the row two and the row three tubes.

We use that to discuss and provide an indicator of the expected lifetime of a row three and a row two tube. We've performed both analyses and we performed tests on tubes that we've received from EPRI. I will discuss those tubes, the as-received condition, and then we'll discuss the tests that we performed on the tubes.

We've also performed stress analyses on row two and row three tubes.

The first step is to determine how much of the hourglassing is affecting individual tubes in the vicinity of a flow slot, because, in fact, the displacements are different.

We wanted to quantify the motion of all of the row three -- all of the row two and three tubes for a given amount of hourglassing. We performed a finite element analysis of a quarter plate model. We applied corrosion packing load, and I will describe this, inside the tube holes, so as to induce the in-plane compression that's caused by the denting process.

This is denting that, by primarily looking at the lower tube support plates and the behavior of them over the life of the plant, denting that occurred in the late '70s and early '80s, and that by tracking the lower support plates, which have been measured, appears to be leveled off and appears to be relatively constant. Probably not stopped, but yet clearly behaving on a plateau. That corrosion packing load causes in-plane compression and that's what causes the tube support plates to move into the flexibility, if you will, or the opening at the flow slot.

We've taken that analysis and I will show you some details of it, but the conclusion of it is that the tubes that go across a flow slot, this is focused on row six, a row two tube will move 62 percent, which would represent a tube at the end of the flow slot, to 97 percent, which would represent a tube at the center of the flow slot.

A row three tube in that range would be from 63 to 92 percent. So as you move away from the flow slot, we included row one, of course, in the analysis, it moves 100 percent, and as we move away, the row two tube moves more in the center than does the row three tube, when you take different motion into account.

The model that we've included is shown here, it's a quarter plate model. On half of the plate, in the vicinity of the flow slot, and there's three flow slots, there's six flow slots across the whole tube support plate, in the region of these flow slots, we've included a detailed finite element array, and on the rest of the plate, we've smeared it because they're really away from the effects of the flow slot.

A detail in the region of the flow slot shows that, in fact, we model the flow slots and then each tube hole and each flow hole is represented, but inside the tube holes, we've put elements that we used to expand basically inside the hole to induce this in-plane compression.

We then increased that loading, and, again, we've allowed the plasticity to occur in the plate, in a greatly exaggerated view of the behavior, as you increase this corrosion packing load, the flow slot moves into the -- the tube support plate moves into the flow slot.

Now, this is greatly exaggerated for purposes of visualizing the behavior. We've been able to take the flow slot and calibrate this to the -- basically increase it so that we get the motion that was measured in the plant.

There is a measurement that was mentioned earlier of a flow slot in steam generator 24 that indicated, and I will show this in a minute, that the total motion was measured at just under a half an inch. So the total closure was measured at just under half an inch, 475 mils, and we've increased this to calibrate it, so it moved that much.

We then took the region of that flow slot and we've tabulated for all the column numbers across a flow slot, approximately ten or 11 columned numbers or rows across a flow slot, and then for each of the first four rows, we've tabulated the percentages, all normalized to the worst tubes, the center tubes in row one.

So this is the percentage of motion that every tube will see relative to its position across the flow slot.

This is a piece of input for the tube analysis. We're interested in taking these displacements in row two primarily and in row three and incorporating those into the analysis that we're doing in tubes to see how the tubes are behaving.

With that information, we're able to consider the U-tube itself and the objective of this analysis is to quantify the row two and row three tube stresses due to the hourglassing of the top tube support plate, the number six tube support plate.

This also is a 3D elastic/plastic finite element model. We included temperature and pressure. We measured the tubes that we received, we measured the wall thinning that you would see at the extrados of the tube, and the thickening that you would see at the introdos of the tube, and we've incorporated that wall thinning due to the bending process into the model.

We've incorporated residual stresses that I will discuss. We've imposed U-bend length displacements to correspond to those displacements that I just described from the tube support plate model. We also included the effects of strain hardening, because as you bend the tubes, you strain harden them and you're going to yield them, because you're bending them permanently. You're going to push them up the stress strain curve, so that locally, on the U-bend, you're going to have a higher yield stress that will affect the behavior.

The strain that's induced is calculatable. We calculated it. We also took one of the tubes that we saw and did a test on an apex, extrados apex sample and a tube straight-leg sample and saw approximately a 50 percent increase in the strength, which corresponded to the amount that you would expect to get by looking at the strain that you induce in order to bend the tubes.

The analysis --

MR. MURPHY: Is this 50 percent increase associated with the fabrication bending process?

MR. ESSELMAN: Only with the fabrication. Again, that's input into the analysis. We performed the analysis considering the real condition of the tubes because of fabrication. So we incorporate the hourglassing in the flow slot and then reduce it as we go out to different tubes.

We also went into the records for these steam generators and we obtained the CMTRs, the certified material test reports, for every tube in row two and every tube in row three, and the CMTRs for all the row two and all the row three tubes were available, so we're able to know what the temperature material properties were, and we were able to also use that and use that range in order to make sure we were looking at the weakest tube, one with the lowest strain and one with the highest strain.

The thing we also did prior to the analysis is we took -- we were interested in ovality and we did some work on ovality. We received three row two tubes and three row three tubes from the EPRI archives. These were tubes that were original Huntington alloys. They were bent in the early 1980s, to simulate the bending process for a row two and a row three tube.

These we believe are very unusual tubes and, in fact, that they were located and that they were well tagged and that the -- that heat numbers that take them all the way back to their original formation was available for five of the six tubes that we had.

They were 7/8 inch tubes, with 50 mil walls. They were Inconel 600 mil-anneal tubes. We had performed mechanical material composition, mechanical property testing, mostly in the tubes that didn't have the heat number, to make sure that it was in the family, and it was.

We also did ovality measurements, wall thickness measurements, and then measurement of yield stress in a bent versus a straight run for the tubes as we received them.

The six samples are shown here. This is the as-received tube samples. We had three row two tubes, the wall thinning, and three row three tubes, wall thinning is, as I indicated, three mils to four mils; in the row three tubes, it was four mils to two mils. This is, again, on the extrados or on the outside up towards the apex, which would be the thinnest portion.

We also measured the ovality in the tubes and the ovality in the tubes were basically on five percent range, except for a single row two tube that had lower ovality.

With respect to ovality, we were interested in starting with an ovality like this and considering how a tube that was initially five percent ovalized would behave differently than a tube that had a circular cross-section. The analyses that we've performed had a tube with a circular cross-section and we did a finite element analysis to consider the effects of the as-manufactured ovality and the U-tube leg displacements on the apex stress.

We ran a row two, three and four tube. We ran with ovalities of zero, five percent and ten percent, and leg displacements, and this is a half displacement, so the quarter inch here corresponds to a half-inch total displacement. The lateral displacement is up to a quarter of an inch.

The conclusion that we had is that a U-bend analysis model with a circular cross-section, for a circular tube, when it is bent due to hourglassing, would give you higher stresses than a tube that has initially as-manufactured ovalization in them.

The results are shown here for a tube that has a yield stress of 40 KSI, and the diamonds represent a row two tube. What you can see is a zero oval tube gives you this number.

There's two things. I also want to talk about the behavior of this. But if you look at a quarter of an inch, half leg displacement or single leg displacement, with zero ovality, you can see a stress here approximately 46 or 47 thousand psi. With five percent ovality, you can see that drops to about 40,000 psi, and with ten percent ovality, that drops to about 32,000 psi.

So, in fact, the two that are initially oval, when it's hourglassed, will give you lower stresses than if it's circular. We were intersted in this to investigate the behavior and also to justify our use of a circular cross sectoin, starting with both row two and three, as that gives us a slightly conservative stress.

The stresses we're interested in are the stresses occurring at the inside, across the tube thickness, mostly at the ID stress and the OD. So this was important to us as we looked at the effects of modality.

MR. MURPHY: Yield stress is the initial yield stress before fabrication of the U-bends.

MR. ESSELMAN: In this analysis, this is the yeild stress that was put into the model in the bent condition, and that was for this study. As we went to analyze all the cases, if this were 50 or 60 KSI, the trend would be the same.

As we went to individual analyses, that I will describe, we took and extended the CMTR data, which was room temperature data, to width temperature and then strain hardened property, also.

MR. MURPHY: You're assuming the same yield stress for each row. Why do you get a difference in comparing yeild for each row of tubes?

MR. ESSELMAN: You're also applying pressure, which has an effect, a large effect in the ovality tubes, and you can see that at the zero leg displacement, as you pressurize an oval tube, it's going to flatten out, so that skews your ID.

This is the ID hoop stress, the stress inside, at the extrados, on the inside of the tube. The initial pressurization of the tube is going to induce compression on the inside of the tube. So that's what skews this like that mostly.

The other thing that we wanted to do with the tubes we received from EPRI were to perform some U-bend leg displacement tests. We designed a fixture that would allow us to take these tubes and simulate the application of a displacement and then compose displacement at the location of the top tube support plate.

Our intent was to develop a fixture that would limit the rotation at the tube support plate, because in the plant, of course, your top tube support plate has an hourglass that is going to remain flat. So we wanted to get as low a rotation as was reasonable.

We developed a fixture. We applied internal pressure and incrementally squeezed the tube or displaced the legs and measured ovalization as we imposed the leg displacement. We also measured strain in displacements.

The fixture and the tube in the fixture is shown here. Basically, the fixture was designed with a three quarter inch plate. This plate was split and we were able to simulate denting by squeezing down on the tube at the tube support plate. The fixture was designed with bolt-throughs and tightened to the point where we could impose a displacement and limit the rotation, and we were successful in greatly limiting rotation, but not eliminating rotation.

But you can see we measured strains around the extrados and the introdos at about the apex and about the 30 degree point, pressurized it and imposed displacement, and as we imposed the displacement, we stopped at increasing displacements and measured tube diameters around the tubes, so that we could look at ovality.

The ovality tests were for the row two tubes that we tested and are shown here. It was the tube that started with an initial ovality of two and a half percent. That ovality, as we went up to a -- and this is a leg-to-leg, so this is a two-leg displacement, if you will -- went up to .55 inches, which is approximately a 75 mils greater than what was measured in the plant.

You could see that at the apex, the ovality went up to approximately seven percent, a five percent increase, and at the 45 percent point, you could see that the ovality change was lower.

This is the step changes and then we move below, we then release the sample, the clamping, and then we depressurize the sample.

With the row three tube, we started with a five and a half percent or approximately a five percent oval tube and the data was taken by hand and it's a very small change, this was a smaller ovalization change, but this was from five percent and this went up to three quarters of an inch, approximately a quarter of an inch more than what we've seen in the plant.

Your ovality, in that case, changed, went up to approximately eight percent at the place where they are in the clamp, which is just under half an inch, then ovality would be on the order of six, maybe six and a half, to seven percent.

So the ovality change that we saw in a row three tube, as you would expect, as you go to larger tubes, reducing and was relatively controlled without severe flattening of the tube.

We also have strain data that we took that we've been able to use and correlate to the finite element model of the tube to show that the behavior of the tube, originally circular, but it is allowed to ovalize, and the strain that you're seeing here also includes ovalization.

We did those correlations to make sure that we were accurately predicting the behavior of the tube.

We also took a tube, clearly the tube that was not the tube that we ran this test on, but a tube that was as received and we measured the residual stresses on the tubes.

We measured the residual stresses using a very classical technique, which is to apply strain gauges, then to release the constraint or the restraint by cutting the tube, and then measuring the strain, as you cut the tube and relieve the constraint.

We did that with OD gauges, applied to both a row two and a row three tube. Those are shown here for a row three tube. You can see that we applied extrados, obviously outside diameter, while the tube is in this configuration, tubes at three locations. There are also introdos gauges at these three locations.

And then we cut the tube into pieces on each side of these; first of all, making only circumferential cuts, releasing a section here, a section here, and a section here. We then took those sections and applied internal gauges, prior to releasing the flags, if you will, and letting the tube expand in the circumferential direction, and you can see the ID gauges here.

We also have the OD gauges, and once we have this gauge, which is the OD on the inside of the tube, the extrados, we then cut the flanks and measured the strain change there.

The data that we saw show that for the row two tube, the total ID hoop strain that was measured is 380 micro-inches and the row three is 360 micro-inches. Now, this is the measured data. When you do this test, what you measure is opposite to what is in there, because you're relieving the strain, so what you're measuring is basically the strain release.

So what you see is opposite in sign. What this means is that if, on the ID, you have positive stresses, strains that were measured as you release the constraint, which means that you initially have compressive hoop stresses on the ID of the tube at the extrados.

The equivalent elastic stress is approximately 10,000 psi. Compression on the ID, the row two and the row three tubes were very close to each other. They were nearly on top of each other.

At the outside wall thickness, this is across the wall thickness, plus or minus 25 mils, at the outside wall thickness, you had a tensile stress of around eight or 9,000 psi.

Clearly, what you know here is you know these points and you know these points. We're drawn a line to represent what that distribution probably looked like, in probably nearly linear across there.

This was important data because this allowed us, in the stress analysis, then to incorporate the effects of the residual stresses.

MR. MURPHY: So at the apex, we're talking about the extrados here at the apex.

MR. ESSELMAN: This is the apex. It's the extrados through thickness at the apex extrados. This is the ID surface and this is the OD surface.

MR. MURPHY: Okay. So without the help of hourglassing effects, the ID surface is in a state of compression.

MR. ESSELMAN: When it was first bent, before it was installed or as it was installed, before it was started up, this is the state of stress, residual stress that you had.

MR. MURPHY: Now, it's my understanding that the rows one and two tubes were fabricated a little differently than the row three.

MR. ESSELMAN: Right.

MR. MURPHY: Was that taken into account?

MR. ESSELMAN: The row three -- we've traced these six tubes very carefully. We've collected what we think is all the information that we can. The row three tubes were manufactured without a mandrill, which is the way that the Con Ed, that the IP-2 tubes were manufactured. The row three tubes classically were manufactured without a mandrill.

The IP-2 tubes, the row two tubes were manufactured with a mandrill and we believe that these row two tubes, at least two of the row two tubes were manufactured without a mandrill. We believe the third tube, the one with lower ovality, was manufactured with a mandrill.

So these row two tubes --

MR. MURPHY: Are you talking about tubes at IP or are you talking about your sample?

MR. ESSELMAN: IP-2 was our -- and it's reported that row two was made with an internal mandrill, row three was made without an internal mandrill. These row three tubes we know were made that way, without an internal mandrill. We believe that we have both a with and without.

In the row two tubes, although we're reasonably confident that two of the row two tubes were made without an internal mandrill. We believe the third was.

That would make them slightly different than the IP-2 tubes. The row three tubes, though, are manufactured in a very similar manner to what we know is --

MR. MURPHY: I'm maybe misunderstanding you. Among the population of IP-2 tubing, row two, there may be one or two tubes that were made without a mandrill.

MR. ESSELMAN: Well, IP-2 row two tubes were made with an internal mandrill. Row three tubes were made without an internal mandrill.

MR. MURPHY: Okay.

MR. ESSELMAN: In the three row two tubes that we tested, that we had available for testing, we believe that two of them were made without an internal mandrill and had higher ovality. One of them, we believe, was made with an internal mandrill, which is a tube that had the two percent ovality as opposed to the five percent.

MR. MURPHY: These were row three or row two?

MR. ESSELMAN: The row three tubes that we tested, all were made without an internal mandrill. So we're sure that the row three tubes that we tested are very similar to the row three tubes at Indian Point.

MR. STROSNIDER: Just out of curiosity, when you did these residual stress measurements, did you do any strain gauging at the tangent point? Did you look at that area at all or just did you look at the apex?

MR. ESSELMAN: We have the apex and then we had two other gauges.

MR. MURPHY: Just one more question then. Among your sample, then, two of the three row two tubes, your lab sample, two of the three row two tubes were made with the mandrill and all three row three's were not made with a mandrill.

MR. ESSELMAN: Internal mandrill, yes.

MR. MURPHY: Internal mandrill. That doesn't affect the stress distribution that we see here on the screen, whether they were. The fact whether a mandrill was used or not doesn't affect this internal stress distribution.

MR. ESSELMAN: The single tube in row -- this is two tubes that we believe -- a row three tube that was made without an internal mandrill, also a row two tube that we believe was made without an internal mandrill.

MR. MURPHY: Okay.

MR. ESSELMAN: The tube that was made with an internal mandrill was used for the leg displacement test.

What is important here is -- what's mostly important is the magnitudes. If we get ten more of these, we're very -- despite the fact that these were very close to each other. What's important is the sign of this and I think that we had believed that the internal stresses in the circumferential direction would be compressive.

MR. MURPHY: Do we know -- did you do an analysis or a test to figure out what the stress distribution looks like for a row two tube with an internal mandrill?

MR. ESSELMAN: No. We believe that it's not dramatically different. We looked at that and we believe that it's not dramatically different, because, in fact, what you're doing with the internal mandrill is restraining ovalization a bit, but yet much of this is coming from the stretching that you're doing axially and the stretching that accompanies that circumferential -- you need to think about all that's happening circumferentially, as you're bending the tube axially.

We think that the internal mandrill gives you lower -- controls ovalization a bit more. We don't believe that the internal mandrill on row two would have a significant effect on the results.

I mentioned the stress strain properties of the tubes. The row two and row three yield stresses will be higher than normal due to strain hardening. That's the stretching to the manufacturing process.

We made a yield strength adjustment that has been determined from the elongation induced during bending. Row two strain hardening, so it's bent to a tighter radius, is greater than row three.

For the purposes of analyses, we made that extrapolation from the stress strain curve knowing that the strain that was induced in these two tubes. We did tests, though, on a tube where we took the apex -- an apex and a straight leg, and we saw that the yield strength in the apex is approximately 50 percent higher.

I'll talk more about the CMTR records. We performed the analysis with a variation. The CMTRs, the yield stresses from the CMTRs have a large variation in them, all above the minimum specified, and I will show you those.

But because of the large variation, number one, large variation, and, number two, you don't know where the higher strength tubes might be in row two. You know that they're in row three. We analyzed both the lower yeild strength and the higher yield strength and a median or an average yield strength.

The analysis was performed with pressure, temperature, residual stress and leg displacements. These yield stresses shown here are corrected for temperature. These are the actual CMTR yield stresses reported, corrected for temperature, because that had already been done a little bit, and also corrected for strain hardening.

That is, pushing up the row two tubes because of the additional strain a little bit higher in yield stress, and you can see the variation driven largely by the original yield stresses are really quite large.

What we did was we ran analyses for row two and row three using lower yeild, average yield, and higher yield, and report those stresses.

I have some plots and some more charts that will show you the distribution of the stresses. We have both the ID and OD stresses, on the inside surface and the outside surface.

We tabulated the stresses that we talked about, which were from residual stress. We looked at -- we tabulated separately the stress that would come from only applying differential pressure, plus thermal expansion, and this is without the residual stress. There's also less in plastic, so this is not -- the residual stresses are incorporated on a strain basis and not just added on the stress basis.

And then for row two, the range of ID -- this is inside diameter, circumferential stress on the extrados ranged from approximately 51,000 psi up to 92,000 psi, always compresses on the ID, always -- I'm sorry -- always tensile on the ID, always compressive on the OD. And in row three, calculated the same stress, as you can see here, that the -- for the -- these are, again, the stresses that are in the tube for the lower yield strain, materials range from 40 to 65 KSI in the row three tubes.

So these are all the effects incorporated. The lower stress is caused by several things, mostly by greater flexibility and also has lower displacement because it's one tube further out.

You have lower strain hardening in those tubes, also. So it's a number of effects that are being incorporated so that we're looking at the range of stresses that exist in the row two and the row three tubes.

We've taken these stresses and we've asked what does this mean relative to how these tubes would be expected to be susceptible to stress corrosion cracking.

MR. STROSNIDER: Before you leave that slide.

MR. ESSELMAN: Yes.

MR. STROSNIDER: I want to make sure I understand that. Where you show total loading and for the three different cases of yield stress, that includes some amount of hourglassing.

MR. ESSELMAN: It includes a maximum amount of hourglassing. It includes residual stresses, it includes the effects of strain hardening. So that is all of the effects.

MR. STROSNIDER: And as you say, this is not a linear thing. What is the delta between adding the hourglassing and without the hourglassing?

MR. ESSELMAN: If you didn't -- the increase between differential pressure plus thermal expansion is almost all recognized. There is no hourglassing, you have this stress and, frankly, that would be elastic, so you would just subtract the residual stresses, so you have a very low stress state on the ID surface, without any hourglassing.

So this is the pressure effects, plus the thermal expansion.

MR. MURPHY: And you've assumed the amount of hourglassing is, what, the 48?

MR. ESSELMAN: It's 480 mils. I wanted to point out, on the curve, and this is very important, on the curve, where we look at the ovalization effects, you saw that that went up to a plateau of about 100 mils.

In fact, what we see because of the actual behavior is that once you pass a threshold, that is, once you go across a flow slot, the stresses in the tubes don't change very dramatically, even as your displacements drop down.

So once you pass the threshold, you get some plasticity in the tubes. In fact, the tube stresses are leveled out on the plateau, which also means, as we increase the displacements across the flow slot, you don't have a very dramatic difference in stress across the flow slot even.

But this is with a 480 mil total displacement. The important point, though, is that it's not very sensitive to that, whether that's 450 mils or 520 mils.

So the question we asked is what can we do with this to relate row two behavior to row three behavior, since row three behavior is -- well, behavior of both rows is of primary importance.

We know that -- going to crack initiation, a number of references over a number of years, and a number of studies that have indicated that time to crack initiation, and that's the time from the application of the stress to the time that action would initiate, is proportional to the applied stress rates to the fourth power.

What this is most useful for is the two tubes that have different stresses applied to them and it allows you to relate the behavior of a single tube to another tube.

So what we're saying is if you have two tubes, the time to initiating of cracking of a single tube, in one tube, is related to the ratio between a different tube and the tube that you're in, raised to the fourth power.

So this isn't a linear unit. It's not an increase of stress rate of 20 percent. This is 20 percent longer. It's really a fourth power relationship, so there is a very strong relationship due to the ratio of stresses.

We believe that the denting occurred in the late '70s or early '80s. We don't know when a crack initiated in the row two tube, but yet we can use this to assess how the other row two tubes would be expected to be behave and how the row three tubes would be expected to be behave relative to the row two.

I'll just talk a little bit about this yield stress distribution again, the apex stress distribution, and what does the row two and row three comparison mean.

The CMTR yield stress distribution for the row two tubes, this is the room temperature data taken right off the certified material test report, are shown here, where this is the number of tubes and this is the yield strength and this is 300-some, 380-some tubes. I'd have to go back and look at the total number of tubes, but this represents all the row two tubes that are there and you can see it ranges from 38 KSI up to 66 KSI. This is different heats. There's a lot of different heats that will --

MR. MURPHY: Is this information you knew on a tube by tube basis?

MR. ESSELMAN: You know it on a tube by tube basis. You don't know where each individual tube goes, though. So you know the CMTRs for every row two tube. You don't know the location of any individual tube, though. But this is not a distribution. This is every data point for every row two tube.

If we take -- if you recall, we did stress analysis for low yield strength, medium yield strength, and the highest yeild strength. If you take that CMTR data and convert it into a distribution for the ID apex stress -- again, this is the circumferential stress at the apex ID, which is the one that -- the ID tension is the one that's going to initiate cracking due to primary water stress cracking.

You can see that the distribution of stresses ranges from approximately 52,000 KSI up to 94 KSI. What's interesting, I guess I've looked at this and asked why are we seeing only seven tubes that have cracks in row two, and, in fact, if you look at this data, this would indicate that these tubes, that have the highest stress, are the most susceptible tubes. These would be the next susceptible, and then this collection of approximately 18 tubes, this is about either three or four tubes, and this is two or three tubes, but then 18 tubes susceptible.

Again, if you took the ratio of this stress to this stress, which is approximately 1.2 or so, and the time to initiate that cracking is a fourth power relationship. So you would expect these tubes, wherever they are in the generator, to be most susceptible and they would be most likely to crack first, followed by these tubes.

There's a lot of tubes in row two that just aren't susceptible because their stresses in the apex are much, much lower. If you take the relationships, and I've done this for the row three tubes and I'll tell you the results, but this is the stress of the fourth relationship. This two down here would be expected to crack more than ten times or probably ten times in the amount of time it took for the first row two tube to crack.

So having seven flaws is not inconsistent with the way you would expect the row two behavior to progress.

MR. STROSNIDER: Just a question on this. This plot is -- this is applied stress.

MR. ESSELMAN: This is the stress -- this is the stress in the tube.

MR. STROSNIDER: Right. So this would give some indication of susceptibility based on stress in the tube. All right. Applied, residual, all the stresses in the tube.

But isn't the susceptibility also related to the yield strength of the tube itself and couldn't there be some sort of normalized relationship here that might encompass more of those factors?

MR. ESSELMAN: Unique to the threshold. Below a certain threshold, related to yield stress, you will not get cracking -- this power to the fourth relationship applies to tubes that are all in or around the yield stress of the material.

This tube is higher by a percentage value than the yield stress than are these tubes. So this tube would be more susceptible, because it also is a higher percentage over the yeild stress. These tubes down here are a lower percentage.

MR. MURPHY: This data assumes all tubes see the same amount of hourglassing.

MR. ESSELMAN: Correct.

MR. MURPHY: When, of course, I mean, different tubes will -- the hourglassing probably varies from zero to 4,800ths of an inch.

The data you have would suggest that there may be a huge time interval between the occurrence of cracks in the first seven tubes or whatever versus the rest of the population. But if one were to go through a Monte Carlo process, where one considers the likelihood that a given tube will be associated with a given amount of hourglassing, one may have a distribution of flaws that perhaps doesn't have such an extreme tail and one might have a totally different picture of how quickly one encounters new cracks.

MR. ESSELMAN: Correct. Two points are important. Number one, again, across the stresses in the tubes across a flow slot, that's hourglassed approximately half an inch in the center, is relatively constant because of this threshold where the stress of the tube plateaus out.

So that the tubes in the hourglassing region are approximately -- have approximately equal stress. You are absolutely correct in that the tubes in the hard spots, and that's in between the flow slots, really have a very low leg displacement and there are about two tubes in each flow slot, and there are six flow slots, so that's about 12 tubes per generator that won't be seeing the U-bend displacement.

But I believe the rest of the tubes are similarly stressed.

MR. MURPHY: One thing we haven't talked about yet, and I hope to get there, is the differences in hourglassing from flow slot to flow slot, whether all flow slots are hourglassed and how much.

But it's not constant from flow slot to flow slot. Some may have zero.

MR. ESSELMAN: That's correct, and that would change this distribution and it would change the -- we've treated every tube as having the maximum U-bend or the same leg displacement. So from that point of view, we've skewed this to have more cracks, more tubes susceptible than might actually be the case.

MR. MURPHY: But at the same time, you have to reckon with the fact -- I don't know what the current count is on U-bend indications, maybe seven or eight indications, so the statistics have to allow for the fact that --

MR. ESSELMAN: I offer as an indicator the behavior driven mostly by the wide variation in yield stress, which is also a somewhat unexpected variation in yield stress. I was surprised that it was so large in that variation in yield stress. There are other factors that we have to take in account. Variation in yield stress is very significant, though, in driving the behavior of the tubes.

We do the same thing in the row three tubes, the CMTR data that is shown here, which is roughly the -- it is roughly the same as the row two. Similarly, for the row three tubes, the same stress conditions are shown here and these are the ID apex stresses in the tubes that vary from 42 to 66 KSI.

What's interesting is to take the row three tubes and overlay it on the row two tubes, which are shown here, and you can see the row two tubes have a large number of row two tubes that are more susceptible than row three tubes.

If you use the stress of the relationship, which is this tube is right around, at or near the yield stress, the strain or yield stress for that material is generally applicable to the stress of the fourth relationship.

The initiation time between this tube and this tube will be a factor of four. That is, the fourth power, this tube would be expected to initiate cracking four times longer than it took this tube to initiate cracking.

I think that that's a widespread and significant in terms of understanding the behavior of the row two tube and understanding why, in row three, there were zero defects which could very easily be because there are no defects because those cracks in row two have not initiated.

In similar fashion, and as I look at the data and analyze it, you have -- if you add up all these tubes, there's approximately -- the tubes that are more susceptible or as or more susceptible than the worst row three tubes, I did that, add them up, and I don't remember the number, but it's approximately 240 tubes, again, presuming a uniform susceptibility, uniform loading, I guess I think that having 240 tubes initiate cracking in row two is inconsistent with having only seven defects, and I think that really, in the process, in row two, we're down in this region, and we -- at initiation, what we're seeing or detecting are cracks that are out in this region, and that, in fact, this would indicate that the row three tubes would be would be lagging by quite a bit.

MR. MURPHY: Still, the assumption here is that you're seeing a uniform loading on all the tubes in terms of displacement. The conclusions one might draw from a chart like this would seem to be more clear if one had gone through, say, a Monte Carlo process in generating this curve, which accounts for the fact that not all flow slots are hourglassed, that different tubes will see different amounts of leg displacement, and you rack that all up and --

MR. ESSELMAN: But the effects -- but let me say, again, the effects, though, of those differences are secondary. The threshold for hourglassing that puts you on this plateau is around 100 mils, so that if you have a flow slot that had zero hourglassing impact, those tubes would be much less susceptible.

And in doing it in a Monte Carlo type analysis would be significant, if they were flow slots that we felt had zero. We are presuming that we are above the threshold in all the flow slots in this analysis, because if we had any flow slots at zero, that would be much, much lower stresses than ten or 15,000 psi off the chart.

I understand the point that you're making really clearly, but you have to look at the fact that we have relative independence across the flow slot and once you pass about 100 mils, you're on a plateau and you're not increasing the stress incrementally.

That said, I will go back and look in more detail at particularly the higher strength tubes that might behave differently.

The conclusions that we've drawn clearly -- the work continues in process and is starting to understand all the subtleties that we're dealing with -- is that the ovality does appear by both test and analysis, but not by a significant role in the ID apex stress.

The cause of the cracking that we've seen in row two is linked to hourglassing in the top tube support plate. It provides a stress state that is very compatible with the occurrence of stress corrosion cracking. We've also seen that row three is much less susceptible than row two tubes, because of the stress state inside.

Thank you very much.

MR. ALLAN: It's now approaching 1:20. It's a good time to break for lunch. We will meet back about 2:15. For the visitors here, we have a cafeteria on the first floor of this building and there's locations across the street and next door for food.

If you do leave the building, report to security as far as your badge and you have to be escorted back up.

[Whereupon, at 1:20 p.m., the meeting was recessed, to reconvene this same day at 2:15 p.m.]

AFTERNOON SESSION

[2:20 p.m.]

MR. ALLAN: We have one more presentation and then we'll open it up to question and answer from the NRC staff regarding the review of the IP-2 event with Con Ed.

We recognize that this may go beyond 4:00, but we plan on still having an opportunity to answer some questions. With that, I'll turn it over.

MR. PITTERLE: I'm going to address the planning and a little bit of the input data for the U-Bends on the condition monitoring and operational assessment. This is not the conclusions of the operations assessment, but rather more of a plan type approach we had taken.

Just some definitions that are pretty standard. Condition monitoring is looking at the last cycle evaluation of the indications found in the inspection against the performance criteria. The operational assessment is looking at the end of the next cycle and evaluating tube integrity against the performance criteria at the end of the next cycle.

A burst, because we'll be talking about ligament tearing and burst a little bit, burst is defined as a gross structural failure of the tube wall, unstable opening, crack extension, and is distinguished as different from just ligament tearing.

MR. MURPHY: Tom, you were just making the point that the failed tube configuration was not a burst.

MR. PITTERLE: I'll get into each of the specific tubes that might be in question.

MR. MURPHY: I was speaking of R2C5.

MR. PITTERLE: We don't believe that that was a burst. It was ligament tearing. But from the fundamentals, it doesn't really make much difference. It failed condition monitoring by leakage, but there are no indications that that reflected a burst of the tube, as contrasted with ligament tearing.

Arguably, if they're at that point in time, with two inches opening, would there have been any more burst pressure, we're not taking any position on.

It did fail condition monitoring just because of the leakage. The issue of that particular tube, whether it's a burst or ligament tearing is not particularly relevant.

The performance criterion, tube shall maintain structural integrity over the full range of operating conditions. The dominant criteria treating the U-bends are sludge pile is free span and for the Indian Point-2, in the margin of three against burst under normal steady-state full power operation is the dominant criteria. The margin of 1.4 against steam line break is less restrictive than the three delta P.

Then the primary to secondary accident induced leakage shall not exceed one gpm under steam line break conditions. That represents the sum of all leakage in the generator, not to exceed one gpm.

Those are the overall performance criteria that will be used.

The basic issues to be addressed show that the structurally significant degradation has been detected. That's, of course, with the 800 kilohertz probe. And that which is undetected will not grow to be structurally significant during the next operating cycle.

The three key inputs to that type of an analysis, probability of detection, growth rate, and, to a lesser extent, sizing of the indications in the outage that's been completed.

Complicating factors, we've had lots of discussion before between the 400 and 800 kilohertz probes, on the influence of the secondary side, deposits that resulted in low signal-to-noise ratios, and that does complicate some of the interpretation of the data.

We think we're covering that and I'll try to highlight a few things we're doing to address that.

The overall tube integrity considerations, the CMOA, condition monitoring operational assessment, must address all degradation. Here I have highlighted what we believe to be the two, although we do need to look at all the degradation, but the two key elements are the low row U-bends PWSCC, the cracking mechanism for row two, column five, that led to the leakage, PWSCC at dented support plates is -- well, no indications, confirmed indications found in this particular outage.

The sludge pile, a lot of effort went into the inspection to look at, in considerable depth, the sludge pile assessment will be performed as a part of this effort. And there is the area above, if we call the sludge pile ten inches, above the sludge pile, those two, sludge pile and free span above the sludge pile are OD degradation, and then areas that have to be addressed in the tube sheet.

There is denting at the top of the tube sheet, can cause some indication in those dents. Crevice region ODSCC, the tubes in the Indian Point units are fully expanded for the first two and a half inches or so at the bottom of the tube sheet. They're not expanded the rest of the way in the tube sheet and you get deposits on the tubes and you can get OD cracking inside.

And then in the role transition, a couple inches from above the top of the tube -- the bottom of the tube sheet, there is a PWSCC indication. These will all be addressed in the final CMOA.

We focus more of this discussion today on the low row U-bends and then come back and more briefly address the other degradation mechanisms again.

We've, I think, pretty much covered these first couple topics, 2-5 would not have been called in 1997. We believe and very strongly believe that the improvements in going to the 800 kilohertz high frequency probe, we would not be leaving any indications of that size in service as a result of the 2000 inspection.

So the POD has been improved by probe, improvements in the analysis, guidelines and training. The high frequency probe, which improved the signal-to-noise ratio. Clear indication is one of the 2-4 type, row 2, column 4 indication, shown earlier. We can really begin to see a lot of the very small indications relatively clearly with the high frequency probe.

And a very important element, no indications found in rows three and four and all the indications have been in row two. We will discuss the PODs that we are planning to apply in the assessment for the U-bend and compare basically based on the extreme case of industry experience.

MR. MURPHY: I take it you're going to -- go ahead, go ahead.

MR. PITTERLE: This represents, in searching back through PODs that have been developed for PWSCC, this represents kind of the range of PODs that have been found for a +Point coil. Now, applying this here specifically to the 800 kilohertz probe, I'm not trying to draw conclusions on the 400 kilohertz, where the issues of bad data come into play.

But as far as looking forward from this inspection on, showing what has been one of the better PODs found for +Point for PWSCC has been for denting at tube support plates. In this condition, the dents are relatively symmetric. The +Point probe tends to cancel out a lot of the denting effects and detection with the +Point probe is very good.

And we see that developed through a combination of pulled tubes and extensive laboratory testing is shown the +Point POD here, lost call rates for that particular exam were very, very low, a few percent, and it represents considerable effort in training, procedures, went into that program.

MR. MURPHY: Tom, you're not suggesting that these PODs here on this chart are representative of those in the U-bend, even with the +Point.

MR. PITTERLE: I believe that this POD is representative, the lower one, representing the lower band of indications of +Point detection is typical of what we can expect for the 800 kilohertz probe, not for the 400 kilohertz.

MR. MURPHY: I guess based on the information on the table, we wouldn't agree with that. It would seem to us that one is not going to detect a significant fraction of these until the max depth is on the order of 70 percent thruwall.

Be that as it may, there's no -- you don't have it with the surface deposits.

MR. PITTERLE: I agree we do not have a performance demonstration set, but I think I disagree with the first statement that if you look at the depths that we found, the average depth, this is average depth, the max depth shifts approximately 30 percent to the right, we found an indication in this range, two indications in this range. Let me get the numbers straight.

I don't have a full set of numbers, but there's two indications, one around 14, one about 20. A large group of indications found in this range of average depth.

MR. MURPHY: Those are square with the experience. The seven or eight U-bend indications found at Indian Point.

MR. PITTERLE: This is Indian Point.

MR. MURPHY: I'm talking about the U-bend indications.

MR. PITTERLE: Yes.

MR. MURPHY: Our assessment is these indications don't become visible to the +Point until you have about a one volt response from the flaw and taking the +Point depth measurements at face value, the depths have to penetrate on the order of 60 to 70 percent thruwall before you get anything approaching a one volt response from the law.

So that would seem to be roughly the detection threshold for cracks in the U-bend.

MR. PITTERLE: I don't know the basis for that statement. We sized these indications and as I will go through a little bit, the sizing of the indications, when we do benchmark calculations, demonstrates that we are being pretty conservative with these indications.

And when we evaluate the data, 400 kilohertz data, which is the basis for sizing, because that's the one that's been developed, and the techniques that we're applying were developed for 400 kilohertz, there is a 14.4 average percent depth indication, a 23 percent average depth indication, large group between 38.9 to 45, 55, the 64.

MR. MURPHY: This is for axial dents, though.

MR. PITTERLE: The technique is derived from axial dents, but the sizes of these particular indications found in the U-bend.

MR. MURPHY: But I guess the question that arises is where is the evidence that this kind of information applies to cracks in the U-bend.

MR. PITTERLE: Don't have any direct indication, I concede, because there has not been an equivalent demonstration program. But the coils from the indications are riding the surface quite well, and certainly at the apex, where these indications are located, that the basic technique should be reasonable accurate.

We've increased the uncertainty by 25 percent to cover the differences between the support plates, the standard deviation. When we do the tube integrity, we increase the uncertainty by 25 percent.

But there's no good reason to expect that it's going to be radically different in the U-bend. We've looked at it for the same techniques, for example, against pulled tubes and role transitions. They work quite well. I agree there's going to be and we felt it also necessary to increase the uncertainty treatment for it, but I believe we're certainly of the right magnitude.

But increase in deviation, we're looking at maybe plus or minus ten percent average depth, a little bit more, 12 percent. And there is nothing to indicate that these tubes are all -- that we found are all up here.

MR. MURPHY: Well, I explained how we came to our conclusion and I think to support a different conclusion is going to take a heck of a lot more supporting information than has thus far been presented.

MR. PITTERLE: Well, I don't think I understood how you came to a conclusion that all these indications are 70 percent.

MR. MURPHY: It appears that that is the threshold at which flaws are being detected, that's correct.

MR. PITTERLE: But I don't see any data that says that.

MR. MURPHY: The decision was made not to pull tubes, so we don't have any supporting tube information, pulled tube information to help resolve this issue.

Certainly pulled tubes from Indian Point would be very helpful, but it was decided not to do that.

MR. PITTERLE: I think we'll need to certainly cover that one in more detail and I do believe that the sizing is reasonable, if I'm looking at 12 percentage average depth, that's a lot of difference in an average depth of the flaw, size in error, the large error that we've encompassed, and I cannot believe that something more sizing that 20 percent is 70 percent average depth. It just --

MR. MURPHY: Well, you're not sizing the U-bend flaws at 20 percent.

MR. PITTERLE: Yes.

MR. MURPHY: Okay. I see the source of our discrepancy then, because our profiling analysis, by our consultants, indicates that these depths, when they're being detected, are on the order of 70 percent thruwall.

MR. PITTERLE: I think we'll need to sit down very carefully with the consultants and see how --

MR. MURPHY: I understand. Once you have a 70 percent thruwall penetration that produces at least a one volt response, making it detectable, now, with the ability of hindsight, you can go in and you can pick out lower percentage thruwall components of the crack below the noise. You can dig those out.

But unless you have a segment of the crack that's producing at least a one volt signal, by which time it's about 70 percent thruwall, unless you have that situation, you're just not going to pick out a lesser crack from the noise. It's just not going to have the voltage amplitude.

MR. PITTERLE: I think we're different already at the voltage dependence on depth. These flaws that we're sizing at 23 percent, for example, is .86 volts. No reason, on an ID flaw, we believe that a one volt ID flaw is 70 percent deep. It doesn't jive. The ID voltages are much higher than you can anticipate seeing an OD flaw.

But it's something I think we're going to have to come across the table in detail to --

MR. MURPHY: We've done a lot of profiling analyses on our end of the table and it may be that there are some -- a conciliation process that needs to be gone through here. But we seem to be arriving at far different numbers.

MR. PITTERLE: I think the only way to reconcile that is some of the ways we've had to do it, sit down across the table and look at the data, because we seem to be quite a ways apart in sizing these indications and, agree, if you would, somehow conclude that these flaws are 70 percent average depth means every one was structurally unacceptable, they would have failed the in situ test.

MR. MURPHY: First, I was referring to max depth. We needed max depth penetrations on the order of 60 to 70 percent thruwall to get a one volt response.

MR. PITTERLE: The max depth one is still not as bad as what you're addressing, but it's shifted about a factor of 30 percent to the right when you look at max depth. So max depth is about 50 percent detection at 40 percent depth. But the average depth is far more important for these types of indications, where we've got a few, we're not going to get a lot of cumulative leakage, if we're going to have any problems, it's going to be a result of ligament tearing or burst.

Well, that is the max depth and indeed that does shift approximately 30 percent to the right compared to an average depth POD.

I'd like to address the other issue and one of the questions previously given is in terms of growth rates and, again, that we have to establish the period that the largest undetected flaw will not grow to be structurally unacceptable, we need growth rate information as well as POD.

A determination of the growth rate, we are able, through the sizing techniques, to take nine indications found in five tubes back to the '97 data and evaluate at 400 kilohertz, since the '97 data is limited to 400 kilohertz. Nine indications to work with between '97 and 2000.

We're using these, as I mentioned before, the +Point and sizing techniques developed under the PWSCC dented support plate program and believe that with the increase in the uncertainty, that we're applying, the 25 percent, that they give us reasonable sizing and believe that, we don't have the details here, but doing a lot of benchmark testing by testing these against the in situ test, we cannot be very far off on the sizing.

Then from the cumulative distribution standpoint, it's a small sample. We've just adjusted the cumulative probability to reconcile the small sample.

And looking at what we derive from the Indian Point data, for example, the dented tube support plate growth rates, as a test of reasonableness.

This represents the database that we have. I don't want to get into all the numbers. One point to make, and I'm not going to cover this in a whole lot of detail, in 2-5, the one that led to the leakage, given that tube, at its end of life or at the time of the leakage and calculating what depth, average depth it had to be at that point in time to tear through, actually it's about 2.2 inches long, would have been about 90 percent depth at the time of the leakage event, on an average depth basis. Locally, it may have been thruwall, but the average depth would be -- to tear through the rest of the uncorroded ligaments would have been about 90 percent.

Sizing of this, one has some range of estimates that we have on -- and I use -- just a point of explanation. I'm going to use the term burst effective link. What that means is in the flaw, there can be relatively long and shallow tails of the flaw that don't really have an effect on the structural character. So what is done in the tube integrity space is to search that flaw for the lowest burst pressure contribution of the flaw, which tends to be the deepest area of the section.

So in order to not let the tails influence average depths and so on in an inordinate or unacceptable manner, we're using the burst effective depths.

And to compare it with ligament tearing, the burst effect is in ligament tearing, lengths are about the same.

So we had two depths estimates for this tube in '97. Then using the lower of the two, the 73.8 average, on a growth per effective full power year, gives us about 11 percent growth in that indication.

Then for the other indications that we sized, shown here, key elements, the average of that growth and average depth, order of three percent, with about the 11 percent maximum growth rate.

These data have been used, as I will describe, to what we believe to be a conservative growth distribution.

This shows the data points or the cumulative distribution of those data points that I just showed you on the table. Then typical with growth data, there are some that will be negative due to size and uncertainty, and our intent here is to fit a lognormal distribution, which is one of the best shape functions for representing growth distributions, we wanted to get rid of the negative tail.

But yet what we're trying to do here, and I'll come back in a later viewgraph to discuss what sometimes is called tube growth, which has a more rigorous correction for the uncertainty. What we're trying to do here is just take out the negative tail, but bias it to the deeper part of the database.

So all we look for is to define an uncertainty for the NDE uncertainty that will take out the negative tail, while forcing the distribution bias to the upper side of the database.

When you do that and you take, for example, this lognormal distribution, combine it with this uncertainty, we would predict this dotted line as a series of measurements that says, yes, this combination of uncertainties, this biased curve to the right, maximize the growth, makes a reasonable combination for the expected series of measurements.

So this is the way we've defined average depth growth, max depth growth and the length depth growth. I'm not going to go through all of the details of the other ones at this point in time.

This is basically the stars are the data we showed before and the curve. Just to compare with some other data and one of the more extensively developed sets of data for -- and most accurately sized, the PWSCC dented tube support plates, which is shown by the dotted line curve here.

The way we've bounded the Indian Point, we are predicting the growth to be larger and significantly larger when you consider this degree of the tail out here, and basically trying to force the data through the largest one that we've found to make sure that we are in this process biasing the data towards the larger side of the growth rate.

If this process for correcting for NDE

uncertainties is done more rigorously, what the general studies show is that the average growth rate, with or without uncertainties, basically should not be changing by the uncertainties.

But if you do this correction for ND uncertainties, forcing the generated curve to maintain the same average depth, then you end up with an uncertainty distribution, shown by the dotted line, combine it with this particular curve, which should be, say, so to speak, the best estimate of the true growth, and combine it with -- so we take this best estimate, combine it with the uncertainty, we get this prediction, which matches quite well, again, the measurement distribution.

The key difference here between what I showed in the first one and this assessment and the traditional way of trying to define tube growth is the average growth rate is maintained.

The way we biased it was to let the average growth rate increase so that we kept biasing the growth to the larger side of the database. And given the small set of data, I think that appropriate conservatism would apply.

But it's pretty clear, this X curve here is what we're using, the best estimate of the tube growth would be a curve like this. We think we've built in conservatism into the growth distribution.

MR. STROSNIDER: Tom, what sort of assumption are you making on NDE uncertainty in those calculations?

MR. PITTERLE: The NDE uncertainty ends up to be pulled out. If you look at these kinds of studies in detail, they -- and the way these growths are defined, it's the same analysts doing both years at the same time.

When you do that, your uncertainties are a lot lower than they are in any absolute estimate. So I forget the magnitude that this curve entailed. Probably about, I'm guessing, because I can't remember the specific numbers, but maybe about a six percent standard deviation, that order of magnitude, but I don't want to be held to the number, because I don't have it here to check it.

But this is very frequently estimates of the true growth are used in a number of integrity assessments. We are not using that to maintain and mechanize the limited database. We're using the one biased to the larger side of the growth rates.

As I mentioned, just to show a couple examples and what was shown early on in sizing techniques is if you do go to a higher frequency, and this shows a comparison of the 400 and 800 kilohertz sizing, or 269, as an example, and I'll show another example.

But the 800 kilohertz data would give you about 18 percent, on an average, higher depth than obtained with the

400 kilohertz.

Since we are using correlations for the uncertainty developed on the 400, we believe that the combined use of the 400 and I will show, not today, but in the full operational assessment, that the benchmarking seems to be better with the 400 kilohertz when we compare it with predictions of, say, in situ tests, that the data for the 400, given the uncertainties in the correlation we're applying, is the appropriate way to do the assessment.

We will still do the operational assessment both ways and both of them as a demonstration that we can still meet a recommended operating cycle, but the most reliable combination of sizing is the 400 kilohertz, based on expectations from the way that the techniques were defined, as well as some of the benchmarking that we will discuss in more detail at a later date.

This is one example. You see it's pretty much a general shift up rather than getting a strong difference.

This is another one showing the differences and later on in the presentation, I will compare these two. This is 271, which I will discuss later in the discussion of in situ testing, and we'll show the post-in situ profile and compare it with these two.

But, again, the differences show up typically on the average depth. For example, there is, in this case, an

11 percent difference in the average depth.

It doesn't say -- I'm not trying to say that if you went through an 800 kilohertz program, develop the correlation with truth, that, in the end, that one might end up at a slightly smaller standard deviation. That may happen, but the fact is we're using 400 kilohertz correlations and consistently then, when we apply those with the 400 kilohertz, the integral checks also show it to be better.

As you saw in Tom Esselman's presentation a little earlier, ovality is not a -- in fact, can be bigger in row three than row two, because the ball mandrill was pulled through to reduce ovality in the row two tubes and not in the row three.

Ovality is not a big issue between two and three.

Some of the conclusions from Tom's presentation, pull together, relevant to the operational assessment. I would like to go through in just a couple minutes.

As Tom described, the site measurements of displacements have been input to the plate model. You develop displacements by row and tubing fact. Determine most stresses at the apex due to the plate stresses. And a key part of that is if you notice this presentation, that at a displacement of about a tenth of an inch, you basically have saturated or leveled off the -- induced the stress from the pinching or collapsed legs, whatever term we want to call the hourglass effect.

So the row three stresses are less than row two, as shown in Tom's presentation. The other key element is that the stress effects from these leg displacements, they are present in history, the denting is not changing significantly. The stress effects of the denting have been there, they're going to continue to be there without a significant change, and where the displacements are significant, that stress has already been built into whatever may be affecting the initiation rates, as well as growth rates.

There is not a significant change in denting anticipated and if, for example, as Tom's presentation said, a couple of tubes may have significantly different stresses. If those are driving it, we probably have seen some of the larger growth rates.

But the main point is that there is not going to be a big change in the future as a result, since denting is basically arrested. These effects have been in there for years. So our growth rates, the initiation rates we're seeing reflect those.

But again, a point relative to initiation and the operational assessment, is the number of new sites is not going to have any effect on the operational assessment.

We are going to be limited by the largest undetected indication, the new ones do not grow fast enough to be an issue in the operational cycle length that we're going to be talking about.

Whether or not we have exactly the same initiation rate is not important to the operating cycle just because they are not going to grow from initiation to a structural challenge in one cycle.

MR. STROSNIDER: Tom, I'd just like to make one comment. We didn't bring this up earlier, but I think it's a question that will come up, and that has to do with the impact of strain rate on the susceptibility.

You're making a point here that you think you've basically got a static situation and because I think there is a strain rate sensitivity, you're going to have to be looking for some information to support that.

MR. PITTERLE: It can be addressed in the details.

MR. STROSNIDER: Or assessing the strain rate effects, maybe some combination of the two.

MR. PITTERLE: Right. The industry experience, to our knowledge, and we have not made a complete review of all data, but we do not know of any indications in a row three tube anywhere in the industry. Row two is now plugged, as has been presented.

MR. MURPHY: I think you're right, industry data is of interest with respect to row three and we could do a fairly --

MR. PITTERLE: Yes, that is --

MR. MURPHY: I think you guys at Westinghouse are probably in a better position to --

MR. PITTERLE: Well, we looked through our database. There are some we may not have done the inspections, and that's my only, at this point, reservation to not saying absolutely.

MR. MURPHY: I think certainly one might think that row three experience had the most heavily dented units in the past, like Surry, Turkey Point. San Onofre might be of particular interest.

But I noticed, I looked at one plugging map I think that was current at the time, the Surry Unit 2 generator that was autopsied at PNL. That plugging map showed that all row ones, virtually all row twos, and the vast majority of row threes had been plugged at the time of the replacement activity around 1980.

And I think almost all that plugging in the inner three rows occurred within either the time the tube ruptured in 1976 at Surry to a year afterward. So there wouldn't have been much, at least for that particular generator, I don't know about others, but there wouldn't have been a whole lot of operating time accumulated for rows two or three in an unplugged condition.

And if you plug it, perhaps it's less susceptible to PWSCC assessment that point.

MR. PITTERLE: Yes, I certainly would agree. At Surry, we did not look back at. We've been going backwards from time so far and the more recent inspection, the operating plants have not found. We did look at all their tube exam reports and nothing was found in row three in the tube exam reports.

What we've not done is the step that you've gone, and we'll do that to look at causes of plugging in row three before they replaced. That piece we have not gotten that far back in time yet in searching. But I agree that's something we will have to take a look at and we'll do that.

MR. SHARRON: Tom, for the ones you have looked at, though, have you ascertained that the hourglassing, if there was any, was as severe or more severe than what was seen at --

MR. PITTERLE: Most of the operating plants that we're looking at do not have any significant hourglassing.

MR. SHARRON: I'm having difficulty then trying to understand how you conclude that row three tubes are not a problem if they do not have the hourglassing that was seen at Indian Point 2. From a historical perspective, I mean, I understand the stress analysis that was done, but to sit there and make the conclusion that says, gee, we haven't seen it at other plants, but other plants don't have the same problem that Indian Point does.

MR. PITTERLE: There is a tradeoff that, admittedly, I can't quantify between denting effects or displacement of the legs and temperature, but most of the other plants are operating at about 20 degree higher temperatures and no one has heat treated the row three tube yet.

So there is a tradeoff between the temperature effect driving, which is a strong effect on PWSCC, and the stresses from denting, and those two are unquantified compensating effects.

I'm not trying to make an absolute, as I'm going to show in a minute that really to establish an operating cycle, since there is nothing in row three, we're really going to establish the operating cycle extremely conservative by assuming that we will have row two in service. I don't know how to do it any more conservative than that. We'll go through that in a minute.

All right. Because of the issues of deposits and sizing, uncertainties in sizing, there's an extensive amount of benchmarking and the analysis methods. It's really a test of the methods and the data to support the adequacy of the data and the methods.

Trying to demonstrate that when we apply these techniques at the specified confidence level, at which we're using for an operational assessment the 95 percent probability, 95 percent confidence, that we will demonstrate that the methods are giving us conservative predictions of structural and leakage integrity.

And these provide a combination, dependent on which set we're looking at, of the sizing technique, the NDE uncertainty, material properties, and then we compare '97 projections to 2000, also includes the growth rates.

We specifically tested the in situ tubes as high as we could, but the equipment to 5,500 trying to drive them to some kind of a leakage, if we could, and then that helps us a little bit in this comparison and I'm not prepared to go into extreme detail on this, but just to emphasize that this is what's being done.

We can take our '97 sizing and project forward to 2000, as a check on the methods, how well did we predict that R2C5 should have a large leakage event, and the standard methods we would normally predict it's steam line break and, in fact, when we predict this tube at steam line break, we would have predicted something in excess of 100 gpm, with the methods that we are projecting at our 95-95 confidence level.

The other things we can do is compare projections of the '97 data to what we have predicted any of the other ones that are -- they all satisfied the three delta P, but would we have predicted the weaker of the tubes, like those that had some leakage. And in general, we are predicting, in fact, more conservatively, the burst pressures of those tubes and leakage, than we found in the in situ test.

And we can compare, as another means, a little bit more indirect, take the '97 projections, compare it with burst pressures and leakage from the 2000 profiles, which, since the indications are a little bit larger, would be a little more reliable, and, again, we find that these projections are conservative to the 2000 profile.

Check the 2000 data, we have the in situ test and we compare for the 2000 profiles did we predict the weaker tubes of the in situ test and we'll show that we have done those rather conservatively.

MR. SHARRON: Is that based on -- when you do these look forward things, that's based on the growth rate data?

MR. PITTERLE: Yes. The growth rates that --

MR. SHARRON: Maybe I'm missing something here. Isn't there a circular argument here? Because you're --

MR. PITTERLE: There is a little bit in --

MR. SHARRON: You're predicting your growth rates based on comparing the 2000 data to the '97 data and then you bias it a little bit conservatively to account for the fact you had some negative tails here, and then you apply it here and then you say, gee, it's conservative.

Well, of course, it would be. I'm trying to --

MR. PITTERLE: You're totally right in that element of the '97 to the 2000. It combines everything together, but the growth rates are going to be consistent with the data. However, we're using a distribution of the growth rates, not, for example, to predict the 2-5-2, not the 2-5 growth rate by itself.

So it does give an integral check of the methodology and the use of distributions and statistical, but I agree with you that it's not a completely independent check, because the growth rates are derived from that cycle. The difference is distribution versus tube specific growth rates and demonstration of the rest of the techniques combined.

MR. STROSNIDER: One thing, I think you said, though, Tom, I just want to understand, is that you're going to make this comparison at a 95-95 value, and I'm wondering if, for benchmarking, it shouldn't be best estimate. The 95-95 seems to me that would be a non-conservative benchmark.

MR. PITTERLE: We are providing the nominal and the nominals will be, in general, on the slightly non-conservative side. Now, too bad that we do predict the weakest tubes nominally, but it may not be totally right on or anything like that.

We do agree that these benchmarking tells us, for this analysis, we should use the 95-95 confidence, and we want to incorporate that.

MR. STROSNIDER: I need to think about that a little bit, because if you're telling me that you can go back and take the '97 data, run it through the model and predict the 2000 condition at 95-95, I mean, that's something that the -- that condition shouldn't be happening very often at 95-95, yet there it is, and it seems to me that that's probably not the best demonstration.

MR. PITTERLE: The more --

MR. STROSNIDER: If we're talking the upper 95.

MR. PITTERLE: We're talking the lower conventional 95 confidence. And I agree with you that there is, as Brian has addressed, a little bit of a circular part in the use of the growth rates derived from that data set. What it is basically saying is when you put it all together in the methods, the methods will give you the right projections, but it's not perhaps as strong as saying, all right, let's take the profiles from 2000.

MR. STROSNIDER: Okay.

MR. PITTERLE: And do we predict the weak links of the tubes from the in situ. And we've even gone to calculating the ligament tearing pressures of each of the tubes individually and the probability distribution of that to support it.

That is basically independent of the more circular loops, which this part is demonstrating an integral methodology, use of distributions, and that provides it. But we will be providing both the nominal predictions and the lower 95, so that element of it can be evaluated, to see how we are doing on a nominal prediction versus the 95.

We'll show that the nominals are predicting the weakest links, but the nominals may give, say, a little higher burst pressure than we actually attain. It is doing a pretty good job of identifying the weaker element of the tube.

A lot of detail here that's not appropriate for today and I'm sure we'll have many discussions over it, but it is an important element given such questions as Emmett raised about the fact that the sizing techniques are not directly U-end at all.

FROM THE AUDIENCE: Can I ask a question for clarification? When you started the discussion, you mentioned that you took '97 data and projected the 2000.

MR. PITTERLE: Yes. I'll give the normal operating projection. I don't remember what that is, but the reason I cited that is that's the normal calculation. The fact that it predicted it to be a big leakage event would say --

FROM THE AUDIENCE: It would be higher pressure.

MR. PITTERLE: Yes, it is. But we will show, I believe, and I don't remember the number exactly, but at the nominal, we have that projection done. I think we're predicting maybe ten, 20 gpm at nominal conditions. I don't remember the exact number.

The breakthrough is -- in the other calculation, the breakthrough is calculated at the nominal delta P. My main point was that's the standard techniques and are we predicting that this tube would not have made it? Yes, it would be clearly failing.

The nominal delta P calculation is included in the benchmark with its predicted leak rate, and I don't recall the exact number, but it's in the multiple gpm range.

The low row U-bend in situ tests, in situ testing, the total was ten tubes to as high as the equipment could go to support benchmarking. Tested all indications.

The three delta P criteria for condition monitoring is 4617 psi under hot temperature conditions. The tests are generally done at room temperature, I've converted them all to the hot, just because typically that's the condition we work with in most of the tube integrity assessments.

The only one that did not directly exceed the three delta P criteria of the in situ tests was the row two C71 indication. This test was limited and the testing to hot condition was about 4206 psi by a progressively increasing leak rate. This was not like some of the other ones that failed above three delta P or leaked above three delta P, that had a sort of like an instantaneous change.

This is a rather progressive increase in leak rate from simulations of normal operating conditions on up to the point at which it reached two gpm at 4206 psi.

MR. SHARRON: Did you do any predictions for these tubes before you ran these tests?

MR. PITTERLE: Did we do any predictions?

MR. SHARRON: You said you had a predictive model that says I know growth rate and stuff and you obviously know where these things were in 1997. So if you applied that and then said, gee, if I run this pressure test, would I expect this tube to fail.

MR. PITTERLE: Yes. As I was saying, that if I take the '97 and project it forward, yes, we predict this tube to fail. We take the 271 2000 profile, we predict it to fail, and we have about a 95-95 it's predicted to fail and there's about a 50 percent probability of having significant leakage and ligament tearing at this condition.

So I think this test supports the profile of the 271 tube fairly specifically. Again, these indications from '97 to 2000 are single indication projections using distributions, so it does give a growth check on the profile back in '97. There's a lot of detail to go into this in the benchmarking, but just to complete this discussion.

The two ways to now assess this tube -- well, three ways are actually applied to evaluate this tube after the test. We've done post-test NDE, which I'll show in the next viewgraph. There's also a leak rate measurement made after it reached peak pressure, come back down and measure the leak rate at a given pressure differential, because that's something that's relatively easy to calculate and back out a thruwall length and look at the plastic opening of that crack, for example, back at the peak pressure.

When we backed out the thruwall length based on the post-in situ, and that's about a .39 inch thruwall NDE would imply that it's less than .39 or there could be, as I'll show, from the NDE, two indications that may be thruwall at the time or two elements of the crack, not two indications, the parts of the crack that may be thruwall, contributing to this leakage.

But it's pretty clear from the NDE and this data that at the time this test was terminated, had not reached even the full ligament tearing elements of the deep section, which is the order of a half an inch long, and the burst pressure, by all the number of different ways of evaluating it, would be more than 300 psi above the ligament tearing pressure.

We do conclude, in a lot more detail than this, that this indication would have met the 4617 burst pressure.

MR. MURPHY: 5173 for cold.

MR. PITTERLE: Cold, I remember the -- I'm not sure where that number is. The correction from hot to cold -- oh, that's greater than three delta P, the 5173. The U-bend, the hot to cold ratio is about 1.07. This test is -- I don't remember the exact number, I think 1.07 times that and that would be the room temperature three delta P.

This is a room temperature number and they were tested to that number, which is just somewhat greater than three delta P. That's the three NDE tubes that were tested to really look is there something that -- the purpose of this basically is to get the pressure up high, and this is sometimes used, for example, in tube exams, to open up indications, to make them more visible to NDE, and that was one of the purposes of trying to do this, to see if there are a broad degree of cracking in the U-bends, you'd expect that this pressurization would open it up to be able to see it within the NDE after you've expanded the tube, in fact, very close to plastic deformation at that point.

MR. MURPHY: This tube is an interesting benchmark for your leakage model, but the test for row two C69 I think was also interesting. I think that one appeared to be at a point of incipient burst at the three delta P number, just making it.

But you do -- you're treating that particular thing as a burst at three delta P.

MR. PITTERLE: It's above three delta P.

MR. MURPHY: When you're establishing the burst point -- what burst model are you using?

MR. PITTERLE: We're using the Westinghouse WCAP-1528 burst pressure. All the -- although there will be two tube integrity analyses, the WCAP-128 methods that you've reviewed and are familiar with, and also a multi-cycle analysis as a backup will be done.

Now, there was data years ago indicating that burst pressures for U-bends might be as much as 40 percent higher than for straight links of tubing, everything else being equal.

Are you factoring that somehow into your comparisons between the test data and the analytical predictions?

MR. PITTERLE: No. Only -- what we've done is to define an effective flow stress, the U-bend tube, that accounts for the strain hardening and it's based on fitting burst pressure on the apex. On the apex, the -- or on the extrados, the burst pressure increase is not as high as, say, on the flanks. The flanks have the highest increase relative to a straight leg tube.

So the flow stress that we use has been based on a couple U-bend tests, the type of analysis Tom talked about, like the 50 percent increase in yield, and then fitting to burst tests of U-bend tubes, and trying to use that as basically a lower bound that fits all those combinations.

But indirectly, yes, but the effect of that burst increase on the extrados of the tube is small. The big increase is on the flanks and I think on the introdos, I'd have to go back and look at the data again, but the extrados does not have a large increase just because it's a U-bend.

I'm trying to remember the data in detail and it's been a while, but if that's a question, we can certainly get that data out. There's extensive --

MR. MURPHY: Yes. I think we ought to somehow reconcile this whole Westinghouse data. I think it dates back probably 20 years.

MR. PITTERLE: Well, not quite, but close to it. You're right, and I'm trying to remember the same data you are and I'd have to go back and refresh my memory, but certainly it's something we can bring along and discuss what the differences are around the tube.

But, again, the main point here is, not a pure statistically based test, to give an established confidence that there is not general cracking. The general objective here was to at least establish that it's not widely cracking in the tubes and this test and the following post-test NDE after the pressurization would open up the cracks, found no indications in the U-bend.

This is a little bit complicated, because there's three profiles given here. We cover them one at a time. The red here is the depth profile derived from 271 after the in situ test. That is shown basically implying that two sections of the tube had torn to thruwall. These depths, from an NDE sizing technique, are in the range that we would not conclude that those couple points in that area could be within uncertainties be thruwall.

But the main point of this to conclude from just the post in situ is that it clearly has not torn the deep length of the crack, thruwall, there's lots of remaining ligaments, so that the full ligamentary pressure or burst pressure of this indication has not been achieved at the time of the peak pressure test.

The other thing that's shown here are the 400 kilohertz profiles and I showed you this one, where it's just the 400 and 800, I showed you that alone. And basically you can shift this alignment maybe a tenth of an inch any way to line it up, because in the U-bend, there is a little bit of slippage, plus difficulty of locating the reference point for measurements.

I did some semi-arbitrary alignment within a tenth of an inch or so of these profiles, and I won't dispute somebody shifting them another tenth of an inch.

But the point is that where the indications seem to show some kind of spiking, this one or this for the 800 kilohertz, this for the 400, appears to be where the ligaments tore thruwall from the result of the testing.

And even the predictions by both the -- there is another 800 kilohertz point right behind there and both profiles predicted a spike where the post-in situ also indicates that there is a deeper section of the indication.

And in general principals, the fact that the cracks are opened up after the in situ, signal-to-noise is better, at least the general ability to size, shape and so on may be expected to be somewhat better than the base data.

But the key point here is that it really tends to support the general shape of the profiles that we've developed from the sizing of the indication. The predictions from these two profiles to probability of leaking by ligament tearing we will show as part of the benchmarking and it is somewhere about 50 percent probability of ligament tearing of a large ligament at the point of this test termination.

But a general support that the sizing is of a reasonable magnitude and shape from the profile.

Just to wrap up the discussion on the U-bends and then briefly address some of the other degradation mechanisms again. The low row U-bend operational assessment, clearly row two has been plugged, row three is now the limiting row, no indications found. As we talked about before, we know of no cracking in row three. We are continuing that review.

But although they're not dented, and we already covered this point, that they are operating at higher temperatures, maybe 20 degrees higher, which, for PWSCC, is - you'd generally expect, for example, growth to go up by 70 percent or more with the 20 percent change.

These -- and none of the row three tubes have been heat treated. At least to some degree provide, without the denting effect, but a general consensus that maybe there is a distinct difference between row two and row three.

But the problems that we encountered to try and predict row three, with no indications, you can postulate a single indication, you can postulate some missed indications, we felt that the best way to treat the operational assessment is to be extremely conservative and let's evaluate how long we could have operated with row two.

With row two, we can make corrections for undetected indications, we can do it as though this was a postulated distribution of indications in row three, even use the row three flow strip, even though we're really evaluating row two.

So it's an extremely conservative way to approach the operational assessment to use row two rather than row three. Our best judgment, we don't really expect to find any. We could be arguing for a long period of time relative to not whether something in row three, but I think hopefully everybody would agree that row three is no where going to be as bad as row two.

MR. MURPHY: Let me understand something. You're proposing to assume that the condition of row three is identical to the condition of row two at the beginning, upon restart from the '97 outage. Is that the premise?

MR. PITTERLE: Row three we believe probably may not even be correct. All we're trying to do by this is to say that a very conservative way to evaluate row three is bound it by row two.

MR. MURPHY: Starting when, in 1997 or the current condition?

MR. PITTERLE: No, this is the --

MR. MURPHY: The current condition of row two.

MR. PITTERLE: We're taking the current condition of the indications found in this inspection in row two, doing an operational assessment forward and see how long we could run if it was row two, as the clear bound of row three. I would hope that everybody would agree that that is about as conservative assumptions as you could do and it's relatively -- we expect to find nothing and that's -- we'd argue forever whether or not there was nothing.

But the most conservative way to do the assessment is to assume that basically we left row two in service, how long could we operate if we had left row two as a clear upper bound of row three.

MR. MURPHY: Row two minus the tubes with indications that you found.

MR. PITTERLE: They're plugged, yes. The plugging goal of row two is basically the form of the evaluation. That way, there's well established techniques to account for the undetected indications. Methodology is extremely well established and I hope everybody agrees bounds are anything you can do on row three. That's the intended purpose.

Take out the arguments about whether or not there's some possible cracking in row three or not and let's see how long we could have run with row two tubes left in service. That's the approach we're proposing to take.

I think it avoids a lot of debate.

MR. MURPHY: It doesn't totally avoid debate, because now your detection threshold and POD and that kind of thing becomes extremely important.

MR. PITTERLE: The problem is you'd raise the same issue if you're at row three and there would be arguing about more qualitative manner what the potential missed indication was in row three. It doesn't totally get by the issues of POD, but it certainly says it's a -- well, I believe, by any measure of doing tube integrity analysis, a worse case bounding condition to assess growth rate.

If we took growth rate from everything we know, we'd say, yes, I can go easily through full cycle of operation, but there is nothing to indicate that we would expect indications. There's nothing in history to say that there would be indications.

We would sit there and debate forever whether that was a correct judgment. We're just trying to avoid that, if at all possible, by saying let's treat it as an upper bound and worst case scenario that we did not plug out row two and how long could we operate.

I don't know how to do this any more conservative than that. So we are taking credit, clearly, the high frequency probe POD is an essential element.

MR. SHARRON: You're assuming that any indication found in row two would have been plugged.

MR. PITTERLE: Right. It's plug and go, plug and go of row two, yes, and certainly not leaving any or projecting to consider any indications left in service. The plug and go analysis of row two.

The POD corrections, we use the POD, we propose what we believe to be a lower bound POD for the 800 kilohertz probe, corrected that in a generic letter approach to one over POD, minus one type indications left in service, to account for the potentially undetected indications.

The methods here are those of the WCAP that we reviewed as part of the PWSCC alternate repair criteria.

That's what we're calling the reference single cycle profile analysis, the WCAP methodology.

As an independent and backup and, to some extent, insight into the problem, we've done a multi-cycle analysis using EMECA technology and support to evaluate, as an independent check on this evaluation, and also one of the advantages of this technique or insight it gives us that it looks at the initiation prior to cycle detection, and we do find that the only way to fit with this methodology, the inspection results require that there be only a few indications initiated over time years back and growing to what we found in the current inspection.

These are obviously in much more detail in the operational assessment. Just to quickly try to wrap this up and address some of the other degradation mechanisms.

Dented tube support plates, CECCO results, number of calls, none were confirmed by +Point, but we do believe that, from everything we can see here, that no confirmed indications by +Point. They are repaired off the CECCO calls and this basically inspection has been previously reviewed by the NRC.

There's no indications confirmed. This does not look to be an issue for the next cycle.

MS. KAUFMANN: Tom, I sort of missed that second bullet. What is the basis for that statement, the CECCO qualification?

MR. PITTERLE: There were a number of NRC meetings before the CECCO was used for that inspection, describing what was going to be done and I believe there was a consensus to go ahead. The agreement was that rather than using +Point for confirmation, we would repair off of CECCO, because it wasn't exactly clear which probe might be more sensitive at the time.

The CECCO calls are repaired based on those prior discussions.

MS. KAUFMANN: I would just say qualification accepted by the NRC is something of an overstatement.

MR. PITTERLE: I agree. I'm sorry. I forgot all the ramifications of the word qualification in the statement. Reviewing at that point. You're right, that's a strong statement.

The area above the sludge pile, basically inspected the CECCO, the bobbin probe, and the more extensive testing was done to look at 20 percent of the tubes in each steam generator, with the +Point up to just below the first support plate, and basically this indicated that basically the lack of finding any significant indications confirmed that CECCO was indeed over-calling in this region.

Then to further look at this region, to give a technique that had less influence of copper, 23 tubes were inspected with UT to reduce that copper effect and there is one tube that couldn't get the probe through the first support plate, but other than that, the test went through the first plate.

Two tubes were inspected after the in situ test and, again, basically found agreement with the CECCO call, with no additional indications identified.

In the area above the sludge pile, there were, again, five in situ tubes -- tubes in situ tested, all of which met the structural and leakage criteria.

At this point, I would like to divert just a little bit to clarify one point on in situ test requirements. I understand there has been some discussion and I need to straighten that out.

It has been stated that none of the tubes met the requirements for in situ testing and let me emphasize that the decision before we even made up the criteria was we were going to in situ test above all sludge pile indications. But in the first issue -- let me start at the beginning.

In the degradation assessment, the in situ test requirements are defined by a statistically developed curve of effective burst pressure -- burst effective length versus depth.

The first sets of data that we put out, and partially because of haste in review, did not use burst effective length and depth. By that criteria, one we'll talk about a little bit later, 3451, would not have required in situ tests. However, when burst effective length depths are put in as in the degradation assessment to be done, is the only tube that would have required in situ testing.

When the in situ test requirements are used with the data that the requirement was specified, the curve was specified, predicted the only tube that even came close to challenging the in situ test requirements.

MR. MURPHY: Was there a burst test for that, too?

MR. PITTERLE: I'm going to come into that one, I think, in the next viewgraph for some of the 3451. This is the area above the sludge pile and five tubes tested there with no leakage whatsoever.

MR. MURPHY: This one leaked.

MR. PITTERLE: Yes. The 3451, which we will get to here and we'll discuss in more detail. Basically, in this area, the original effort was detect with CECCO, confirm with +Point, found that we were getting basically a lower confirmation rate than the qualification, some sort of an implication maybe we were over-calling, but basically that was the base inspection.

We looked at this again in the extension of the overall inspection, 100 percent of the hot leg tubes were inspected with +Point from the tube hot, the 24 inches above the top of the tube. Out of this, only a total of about six small indications were identified that had not been found by CECCO, and that's quite good to take two different probes and all you see with one or the other is about six small indications.

MS. KAUFMANN: Tom, do you mean the production CECCO?

MR. PITTERLE: Yes. This is evaluated against the original CECCO call. If we went back and looked back, I don't know if you could say whether there was a signal. I don't know what that answer is.

But basically that's a small population when we're looking at 100 percent of all hot leg tubes to find one probe or the other. There is always a sensitivity of one probe versus the other that's going to detect some of these small indications and the other one won't.

This is really -- I would argue it actually supports CECCO, but I'm not trying to make any point of one probe or the other, other than this certainly is a good over-check on the inspection, the detection in the sludge pile region. That's the main point.

Again, as I talked about before, we did the U-tech inspection of 23 tubes. That confirmed the CECCO call.

All of this was basically done, this additional testing was done to make sure that we had done everything practical, possible, debate between those two adjectives, to identify any of the indications in the first pass above the top of the tube sheet.

In addition, since we basically made the decision, we're going to basically in situ test everything in the sludge pile region just to make sure that we did not have sizing problems. Thirty-one tubes were in situ tested. All met the 9706 or the burst criteria, the leakage criteria, basically none leaked, steam line break.

The only one that leaked at all in the full test pressure of 5500 psi cold was 3451. And this, the peak test pressure reached was 4985 and I'm just going to -- I'm used to working with hot, it's 4591 psi into a hot condition.

Well, you talked about how close together one was, it's at three delta P. You take the 4591, divide it by the 4985, 4591 psi hot, divide it by the 1539 normal operating delta P, the burst margin is 2.98, bounds to three.

MR. MURPHY: Perhaps you can clarify something. Am I correct in recalling that this tube was detected by one of two analysts?

MR. ADANONIS: That's correct.

MR. MURPHY: Here we have a tube that, at this point in time, is marginal with respect to meeting NRC performance criteria and was only detected by one of two analysts? I believe that five -- only five of the eight actual indications were detected by both analysts and the review conducted by our consultant indicated that this indication certainly was one that should have been detected by both analysts.

I think as we indicated to you last week, I guess this is a source of concern for us and certainly I think we need to understand a little bit better why this type of indication, which includes one that was quite marginal in terms of its burst pressure capability, why there isn't better performance in the field in picking up this kind of indication.

MR. ADANONIS: And we are in the process of going back and pulling together those statistics and looking at these particular indications in detail to be in a position to address that question. We have the same question.

MR. PITTERLE: The bottom line is that the tube and looking at that, it didn't even open up the length of the crack, it probably did not burst either, it's a ligament tear, but in either case, just met the three delta P criteria.

In the cold leg program, the initial inspection with CECCO and bobbin probe, then followed by 20 percent of one steam generator with +Point from the cold to just above the first tube support plate.

Inspected 20 percent of each of the other three steam generators to the tube end cold to 24 inches above the tube sheet, but, again, an over-check with +Point to assure that we've picked up any significant indications.

No crack-like indications were found in any of these inspections, were found to varying degrees of pits, and as a result of finding some pit-like indications, 23 and 24 were expanded to 40 percent of the tubes.

And that, in fact, is all pit-like indications have been plugged and no cracking was found by any of this extended inspection on the cold leg.

The cold leg is basically pits and tend to be small, from all indications, negligible growth, but as a conservative element, they have been repaired. See really no anticipation of this being an issue in the operational assessment.

MS. KAUFMANN: Tom, when you talked about the comparison between CECCO and +Point for the sludge pile, did you do a similar comparison for the cold leg inspection between +Point calls and CECCO calls?

MR. PITTERLE: I think the only difference was a couple pit calls.

MR. ADANONIS: There were a number of pits. I don't have the statistics with me, but there were pits identified with the bobbin or identified with the +Point.

MR. PITTERLE: The point is when you take a +Point to something like the pits, even if you inspect, say, new steam generator tubing, you get what's sometimes called a lap indication, you can see those with +Point, down to two or three percent depth. You see in some of these it may well be a manufacturing defect, there is no way of separating them in this vintage of a plant.

Tube sheet region, then the tube sheet burst is not an issue, the restraint of the tube sheet prevents the tube from bursting, so it's really just the -- not just, but is a leakage related question.

Again, CECCO and +Point correlation as far as confirmation with CECCO was similar to that of the qualification. There appears to be less of an influence of copper within the tube sheet crevice.

We found, after post-in situ testing, one indication that the pressurization above 5000 psi had opened up a crack, probably including some tearing, and identify the crack that had not been originally called.

Now, it may have gone from 40 to 70 percent or something as a result of the pressurization. We really don't clearly can't distinguish from that. But on the basis of that, the inspection was further expanded to include the tube sheet region, including the 100 percent of the hot leg with the +Point inspection and 20 percent of the cold.

Now, we have the CECCO and the +Point as, again, independent inspection techniques.

In situ tested five of these indications. They all met the leakage criteria, none of them leaked.

We don't anticipate that, given the combined inspection, that this is really an operational assessment issue.

Finally, to wrap this up, a couple general consideration comments, as I said, all pit indications are being plugged. You may be aware I didn't discuss -- I don't even recall if there was a pluggable indication, I don't think so, but AVB Wear has been around in these units for many, many years, it's not an operational assessment issue.

What growth we've evaluated has been consistent with the rest of the industry experience and Tom Esselman described previously the evaluation being conducted for the support plates that indicates the plates will maintain adequate tube integrity over the next operating cycle, and then there's some additional loose part that is being closed out, but that's not really a tube integrity issue.

That summarizes, details, whichever way you look at it, of this presentation.

MR. SULLIVAN: Tom, I have a question about the pit indications.

MR. PITTERLE: Yes.

MR. SULLIVAN: The slide indicates that the sample in 2-3 and 2-4 was expanded to 40 percent due to pit indications.

MR. PITTERLE: Yes. Found a couple pits in the first part of the 20 percent sample and that led to following the EPRI guidelines to an expansion of the inspection.

MR. SULLIVAN: So the expansion was done based on EPRI guidelines, not based on tech specs.

MR. PITTERLE: I think that's correct.

MR. SULLIVAN: And what was it about the EPRI guidelines that allowed the sample to stop at 40 percent.

MR. PITTERLE: For volumetric type indications, it's different than, say, for example, cracks or plugging -- I don't remember exactly. Do you remember, Don?

MR. ADANONIS: Yes. If you find -- well, any volumetric or any non-crack type indication would have been beyond the repair limit would have thrown us into a further expansion.

So if you -- if we would have had something beyond a pit beyond 28 percent depth, which is what we had in the first sample, threw us into the expansion. In the second expansion or in the expansion, we found no pits deeper than 28 percent, which was our calculated repair limit.

MR. SULLIVAN: So you've got a qualified sizing technique.

MR. ADANONIS: There is an EPRI-qualified sizing technique, with a bobbin probe.

MR. PITTERLE: The reason that limit is 28, we didn't plug at that limit. The 28 percent is the result of the uncertainty in the EPRI sizing technique is very, very low. It would not support -- the growth rates are very small. The uncertainty is so large that if you back off from the structural limit at that NDE size and uncertainty, we would have repaired, if we had not decided to plug them all, at about 28 percent.

It's just a huge size in uncertainty.

MR. SULLIVAN: But you're relying on some sort of qualification for your decision on expansion.

MR. PITTERLE: Yes, the EPRI databank.

MR. SULLIVAN: How does that fit Indian Point?

MR. PITTERLE: From a pit standpoint, volumetric standpoint, should fit. Pitting should not be very --

MR. SULLIVAN: Were there ever any pulled tubes?

MR. HENRY: Yes. There are pulled tubes included in that data set.

MR. PITTERLE: As we said, all decision was made to repair all of them, even though the growth rates are close to nil, just to avoid the issue of size and accuracy for plugging. It was only used as a consideration in the expansion of the inspection.

Are there any other questions?

MR. BAUMSTARK: We can get into the question and answer.

MR. MURPHY: Last week, we sent you a set of very detailed questions and presumably we will be getting a detailed response to these written questions. The presentation we heard today, of course, was fairly high level compared to the detail involved in some of these questions.

However, there were a couple of, I think some of the most important of these questions have not really been addressed at all.

One, the thrust of the -- I think the most significant contributor that you've identified from the standpoint of the cracking mechanism, the most significant contributor you've identified is leading to the U-bend cracks, the flow slot hourglassing phenomenon.

And of course, you have identified that the flow slot adjacent to the tube which failed, R2C5 in SG24, that that flow slot was hourglassed, 4800ths of an inch.

Of course, a number of other U-bend indications were found in the steam generators, and to what extent have you been able to confirm that flow slot hourglassing is also associated with these other U-bend cracks?

This would be pretty central, I would think, to support the notion that hourglassing is a necessary condition for these cracks.

MR. PITTERLE: Can I try to separate that from the operational assessment? It may be an influence on initiation.

MR. MURPHY: I think we heard that we can't get cracks, ID initiated cracks without the influence of -- at the apex without the existence of hourglassing. I think that was identified as a necessary condition for the kinds of cracks that we're seeing. So the question is have we, in fact, confirmed hourglassing associated with each of the U-bend cracks that have been identified.

MR. PITTERLE: I'm trying to understand if there is any concern relative to doing the operational assessment.

I understand that from a technical standpoint, of understanding what may have caused the cracking, but the influence of the --

MR. MURPHY: I think it does, Tom. I think it does. Of course, question was made in the context of a root cause analysis and the earlier discussions related to root cause or causal factors.

But I think that an operational assessment, in order to be credible, certainly needs to be consistent with what is known about the causal factors involved with the cracking.

So I think we need to understand the root cause of these cracks in order to have confidence in the operational assessment.

ESSELMAN: The cracks that were identified, the indications that were identified were all identified in the outer-most flow slots. That is, at the periphery or the second flow slot in. Only a single flow slot has been measured, though, and the measurement was made in steam generator 24 and it was made in a flow slot that, as we said, was visually difficult to determine, both because of the image that you get from the video cameras and because of the rough cut, flame cut.

But yet the location that the cracks occurred are where we expect the hourglassing to be most severe, which is the outer most near the periphery. The indications appear in the center of the flow slots.

None of the indications have been at the regions away from where the hourglassing has occurred, and even though we haven't gone to the other flow slots and performed a measurement, the location of the other indications are consistent with the presence of hourglassing.

The other thing that we've learned is that hourglassing that exceeds approximately two-tenths of an inch in total leg to leg hourglassing, less than half of what was depicted.

Also would be sufficient to cause the level of stress that we saw in the tube that was analyzed with the flow slot that was measured.

So I believe what we have absent measurements of the other flow slots that would allow us to draw hard conclusions is consistency of indication locations and a low threshold that is very plausible relative to the extent of denting.

MR. MURPHY: You've performed visual inspections from each of the hillside ports in each of the four steam generators and apart from the flow slot we've been discussing in SG24, and I think one additional flow slot in SG23, has there been hourglassing identified in some of the other flow slots and are we able to confirm the existence of hourglassing as being associated with each of the row two cracks that have been found?

ESSELMAN: The direct link, short of going in and measuring the other flow slots to say that there is sufficient hourglassing, is only through the consistent appearance of locations.

We also know that absent that mechanism, absent hourglassing, in that we've looked at the pressure stress and the effects of ovalization in those tubes, the other consistent path has also led us through the other potential contributors and none others have appeared as likely contributors.

So I think that we have identified a low threshold. We have, for intense, in the outer flow slot, measured hourglassing. I don't know that we know a mechanism why the second flow slot in would not also be hourglassed.

The occurrence of denting is a relatively uniform event in that we're seeing flow slot deformation in the lower tube support plates across the entire bundle.

I don't know that we know a mechanism why we would get hourglassing in the flow slot that was measured, for instance, and would not see it in the flow slot that's adjacent to it.

But I think that there is a -- and I think we could develop this also, but the consistency of location of flaws within the flow slot, in the outer flow slots, it's consistent. Also, the mechanism that's caused the denting, what we know about -- in detail about the lower support plate denting, and that is that the denting effect clearly is uniformly occurring across the bundle.

I could lead you to not have a mechanism, not have an alternate mechanism nor have an alternate mechanism why you wouldn't have denting.

MR. PITTERLE: Could I just add that I don't believe we can show with absolute certainty that denting is 100 percent requirement. There have been one or two other apex indications reported in other plants without substantial denting, most of whom are at the tangent points, which are a different causative stress. But it's not 100 percent dependent on denting.

MR. MURPHY: I think before moving on to ovality, I guess I'm a little concerned about the lack of corroborating evidence of hourglassing as being associated with the occurrence of the known cracks in row two.

This is the operative theory about the main causal factor for the state of stress at the apex of the U-bends, at the ID, that basically drive the initiation and growth of the cracks.

There are a number of potential mechanisms to explain cracking at the apex and the next one I'd like to explore a little bit is ovality and excessive ovality introduced by fabrication.

But before getting into that, given that there's more than one way to explain, hypothetically more than one way to explain the occurrence of ID cracks at the apex, I would think that there would be more of an effort to corroborate the particular theory that you've come up with as being the primary cause.

In the late 1970s, the Dole unit in Belgium experienced a rupture at the apex of a small radius U-bend. That plant did not have any significant denting reported or any reported hourglass effects.

The ultimate cause of the failure was determined to be the result of excessive ovality introduced during fabrication and this degree of excessive ovality was confirmed after the fact by running ball gauges through the U-bends to establish the diameters.

Also, in the past, at Indian Point, back in 1997, in your 1977 inspection report, dated July 29, you reported quite a number of restrictions in the U-bend, U-bends that reportedly restricted a six-ten-inch diameter bobbin quell.

In the root cause report, you also note that restrictions existed in the U-bend at and above the sixth support plate.

So I guess the general question is how -- what has been done to rule out fabrication induced ovality or excessive ovality relative to the allowed -- the drawing specifications on a lot of degrees of ovality during the fabrication?

MR. PITTERLE: I'd just comment that I think that Dole has to be put into almost a separate category. The bending process did not use the ball mandrill. As a consequence, those tubes were small radius tube bent without the ball mandrill to help reduce the ovality, much different animal than anything we're looking at.

MR. ESSELMAN: The other comment that I would make, and I guess I would like to not provide a final answer, and I'd like to go through it, but ovality, manufactured ovality was specified as limited to ten percent on the drawings. One of the features of ovality is that from an ID cracking point of view, that as you pressurize the tube, you do induce a compressive stress state into the ID surface. So that there's two things that you need to look at, and that is what may the residual stresses be, which I guess is -- I try never to talk about residual stresses that I hadn't thought about before without having some time to think about them, because they're very complex, but yet the operating stresses, absent any other driving force like U-bend cracking, would tend to give you a compressive stress on the ID.

So in general, we've looked at ovality, we've considered ovality in the work that we've done, and we have not been able to attribute a feature to the initial manufacturing ovalization of a tube, especially absent the leg closing that would give you stresses that would be sufficient to induce cracking.

MR. MURPHY: Wasn't the Dole failure attributed to primary water cracking?

MR. ESSELMAN: Yes.

MR. MURPHY: There must have been tensile stress there on that. Tom, you referred to the ball mandrill process used for rows one and two at Indian Point. Until the root cause report was issued a couple of weeks ago, we had talked about, in earlier discussions with Con Ed, the fabrication methods that were employed on these U-bends.

And until the root cause report came out, the information consistently had been that at least that the Huntington tubes had not been fabricated with a ball mandrill.

I think early on in our discussions, in the initial weeks, I think the initial information we had was that perhaps there were two suppliers involved for the tubing in rows one, two and three, and that perhaps some had been fabricated with a ball mandrill and some hadn't.

But the story had been, right up until the root cause report, that a ball mandrill hadn't been used and when the root cause report did say that a ball mandrill had been used.

So my question is what was done and how confident are we that, in fact, a ball mandrill process was used during fabrication?

MR. PITTERLE: Part of preparing the root cause report was a rather detailed review. We've had many, many telecons with the Huntington Alloy people with regard to how those tubes were bent, because some details of the procedure, even to the point of whether they would make additional tubes the same way.

As a result of those discussions, it's been clear that they did use the ball mandrill, slightly different technique than, say, the tubes made by Westinghouse, where most of the cracking may have been at the tangent points, differences in how the ball mandrill is used in the process, but they were very explicit in the use of the ball mandrill to reduce the ovality of row one and two, and it was not used in row three.

MR. MURPHY: Was there a post-process inspection of these U-bends in terms of ovality measurements before they were installed?

MR. PITTERLE: I can't say that I've reviewed the inspection records. That part we did not do. I would expect that there is a drawing requirement, but I cannot verify anything that was specifically done, because we did not review that. There generally is an ovality limit on the tubing that's supposed to be passed.

We have not tried to dig out those manufacturing records to confirm that at this point.

MR. MURPHY: Okay. I think I also alluded to your 1997 inspection reports, talking about reported instances of U-bend restrictions. I know you've taken a look at that issue and I know you have some thoughts on that and perhaps before going on, you might briefly summarize your conclusions as to what those reported U-bend restrictions were all about.

MR. NEFF: The U-bend restrictions, when we investigated, we found that they were not intended to be tested through the U-bends. The reason why those low radius U-bends were tested was to get the straight legs inspected.

The probe, when it tries to jam up into the bend, naturally stops. The operators made messages on the tape that said that these tubes were restricted in the U-bend.

There were other cases where those tubes were restricted at other elevations, lower elevations. And that is the reason why they were plugged. But I didn't find any reference that stated or that showed that that probe was caught in the U-bend proper, and I so stated in the letter that I wrote. But I also couldn't say where it stopped in the U-bend. It was just that the probe is too long, physically too long to negotiate the U-bend.

MR. MURPHY: Go ahead, Stephanie.

MS. KAUFMANN: I just want to make the general observation, start off by making the general observation that in the letter that we sent to you last Friday with the 17 points on it, I realize those are very detailed questions and that you couldn't address all of them today.

So I'll be looking very much forward to seeing the written responses to these questions. But I just want to bring out probably in terms of my interactions and input on this whole event with IP-2 is that I noticed in your root cause analysis, I thought there could have been a much more candid and objective evaluation of the root cause and your overall steam generator management program and how the industry guidelines factor into that.

You know the staff has to look beyond just what's happened at IP-2 and consider the generic implications and we can do that, but it's best if we both do that. That's something I would really much like to see in your cause evaluation, specifically reliance on generically qualified techniques without perhaps a very close critical questioning attitude about whether those are applicable at one's plant.

That's an example.

MR. STROSNIDER: I'd like to just follow up on that and I think Stephanie characterized that issue very well. Basically, the root cause evaluation says there is an inability to detect because of noise and secondary contributing factor, the hourglassing and stress aspect. But I guess there's a lot more that could be said about that.

Why did this inability exist, why wasn't it corrected, what needs to be done to correct it, which is getting into corrective actions, but certainly, from your perspective, you need to take probably a deeper look to make sure that your corrective actions and your ongoing efforts are appropriate.

And from our perspective, we need to understand this and it's very important because we'll be looking at this not only for Indian Point, but for generic implications for the industry, and it's important that we focus on the right thing and that's one of the reasons for going into the detail that we're going into and trying to understand the a very complex area.

So I think in general, I'm glad Stephanie brought this up, there's perhaps a deeper, more probing look that could be taken at this and I think there are probably going to have to be more discussions in that regard.

So that's just a general comment. I think some of the specifics we've asked are going to help in that regard, some of the discussion today will certainly help. We've talked about what was the status of generic requirements in terms of qualifying the inspection. We talked a little bit about how you apply them at Indian Point. But I think we need to have a very good understanding of that to make sure that we really focus on the right corrective action for you and for the industry.

So I'm glad Stephanie brought that up and that's an important sort of general comment.

MR. ADANONIS: That is a good point. One of the facts is in 1997 was the first time that this plant, that these generators had been looked at with a +Point probe. So there wasn't even any basis for any kind of comparison as you see now going into the different requirements.

MR. BAUMSTARK: At the same time, we appreciate your comment on the rigor with which we approach this whole issue.

MR. PITTERLE: And I think we all agree that we need to proceed and follow this on a generic basis. I do hope, though, that we can separate the timing of the review of the operational assessment from the generic issues, because we do want to get everything we know about it on the table and done. We, speaking for Westinghouse and I'm sure everybody else, will look at this and what should be done on a broader generic basis.

But we get the operational assessments and everything else pulled together, it gives us much better technical knowledge to then, on a timely basis, but extended basis, look at the generic issues. But I hope that they can be separated in the short term from the review of the operational assessment.

MR. STROSNIDER: I'd just comment. NRC is experienced in looking at plant-specific issues in parallel with their generic implications.

MR. PITTERLE: I understand.

MR. STROSNIDER: And that's what we'll be doing. But clearly there is a close coupling and we don't want to lose sight of that. We'll be looking at issues as we understand them along the way from both plant-specific and the generic perspective.

MR. SHARRON: I marked down here, from earlier, I just would just like to get a clarification. I'm not sure. You started, when you talked about the EPRI guidelines for changes in leak rate, I think you said that the latest guidelines or something basically reduced the -- they reduce, I guess, the threshold at which you would start increased monitoring or something.

MR. PARRY: Yes.

MR. SHARRON: The rest of my question is I didn't understand, when I look at the leak rates that you experienced, when you went from like I think it was one to 1.5 to three gpm or something like that, how does that fit, in other words, from a lessons learned standpoint or a root cause? Does this change have any affect on how you would have managed your steam generator or what you would have done?

And, again, the question is hwy is that okay if there was no change?

MR. BAUMSTARK: The answer to your question, I think, is, would we take different action now if we had a 3.5 gallon per day leak, and the basic answer to that is probably not, because you can't find a leak or it's been demonstrated because of utilities that have had leaks of ten gallons and less that they were never able to locate the locations of those leaks by shutting down and investigating the steam generators for a potential leak.

The industry standards, and correct me if I'm wrong, Gary, the industry standard is basically that there's a leak of less than ten gallons per day, you are probably not going to be able to find it.

In the history of our plant, we existed, I think, between 1983 and 1994, with a recorded leakage, I think it was 5.9 gallons per day, and we were never able to find that leak.

Cycle after cycle after cycle, going into those generators.

MR. SHARRON: One of the questions that was raised was do we even know that this increase in leakage that occurred just prior to the failure was actually coming from the tube that failed or was it from something else.

MR. BAUMSTARK: And we don't know the answer to that question. We know, based on all the examinations that we've done, that we are plugging a number of tubes and we may well, in plugging those tubes, disguise what the real source of that 3.5 gallons per day was, as we believe we did in the 1993 timeframe because that leak, after one outage, was not there in the succeeding operational cycle, yet we could never find a distinct leak.

MR. PITTERLE: Of all the tubes tested, only row 2, column 71 leaked under normal operating conditions. A lot less than the -- I think it was more like .1 gpm. But I think the answer is no, we cannot really assure that that is the contributor to the leakage. I think it's recognized in all the leakage monitoring documents, the EPRI, NRC, DG-1074, that leakage monitoring gives you defense-in-depth, it protects you against high percentage of potential flaws, but there are always going to be some flaw that can tear without preceding leakage. That may or may not have been this case, but clearly it wasn't very much leakage, zero or very small leakage.

MR. MURPHY: There are many detailed questions here that I'm not -- time doesn't permit going through in detail, but a lot of them deal with the general issue of how it came to pass that the symptoms and precursors of the kind of failure that occurred at Indian Point in February this year, how these went unnoticed or no red flags got raised, years ago, that this was a problem that was building up on you from the early evidence of the hourglassing, to the finding of the an apex indication that the row two U-bend in 1997, the fact that you clearly had very noisy data in the U-bend, and flaw detection was clearly a big challenge, whether there shouldn't have been much more careful attention and perhaps further action to ensure the integrity of the inner row U-bends.

So this will certainly be a subject of continuing discussion with you and among ourselves and with the regions.

MR. STROSNIDER: And I think most of those issues that Emmett sort of listed there are captured in the letter that we sent you last week and we'll be looking for your perspective on those issues when we provide that answer.

Today was helpful, but I think there's probably some more detail, some more information that you might be able to provide to help us understand that.

MR. SHARRON: Let me just close, if I could. You presented us with a lot of information, some of it's new. We obviously have to go back and review it. I think you've heard there's a number of areas where I guess we feel that in terms of the 17 questions we asked under root cause, we would need some more information. And so obviously we'll look forward to getting the answers to those questions and basically I would encourage you that when you go through these questions and reflect on what we've heard here, there's a transcript, so obviously you can go back and get some more detail on that.

But to the extent you can address those in the written response, I think that would help us and we're always available for clarifications or anything with a phone, if you need them, with regard to the questions.

With that, I guess I'll turn it back over to Jeff. I'm sorry.

MR. BAUMSTARK: Can I say something?

MR. SHARRON: Sure.

MR. BAUMSTARK: Unless you just want to close out. Again, Con Edison and specifically those of us at Indian Point, we appreciate the opportunity to come down here and discuss our steam generators in this particular forum. As you know, we're working very hard right now on our condition monitoring operational assessment report.

One of the reasons we built that into the presentation today was we believe it answers a number of the questions that were not answered as part of our root cause analysis.

This assessment, in our minds and clearly in your minds, it needs to be the same way, will determine the readiness of our generators for safe operation and I certainly want to emphasize safe operation. We need to determine, first ourselves, and then have you concur that these generators are safe to operate, and then based on the operational assessment, what type of operational cycle that assessment will define.

Again, we appreciate the opportunity to come down here today and present this.

MR. ALLAN: I just want to make one other comment. We had stated in the April 28 about the topics for discussion for this meeting, that we were kind of expecting that all the information that we heard today would be -- we'd also receive in the form of writing, and also everybody is concerned about timing of when we'll receive responses.

Would you just keep us up to date on when you expect to submit documents to us, so that we can have an idea, as well as inform the public, because I get calls all the time of when is this going to be submitted.

With that, that concludes the technical meeting between Con Ed and NRC and we're going to go now to question and answer session with the public. We appreciate your time.

[Whereupon, at 4:30 p.m., the meeting was concluded.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

NAME OF PROCEEDING: NRC AND CONSOLIDATED EDISON TECHNICAL MEETING REGARDING IP2 STEAM GENERATOR

PLACE OF PROCEEDING: ROCKVILLE, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

B. Charles Hopchas

B. Charles Hopchas
Official Reporter
Ann Riley & Associates, Ltd.

Indian Point Unit 2
Low Row U-Bend Examinations

Jimmy Mark and Andy Neff
Con Edison

May 3, 2000

Technique Qualification for 1997 Row 2 and 3 U-Bend Examinations

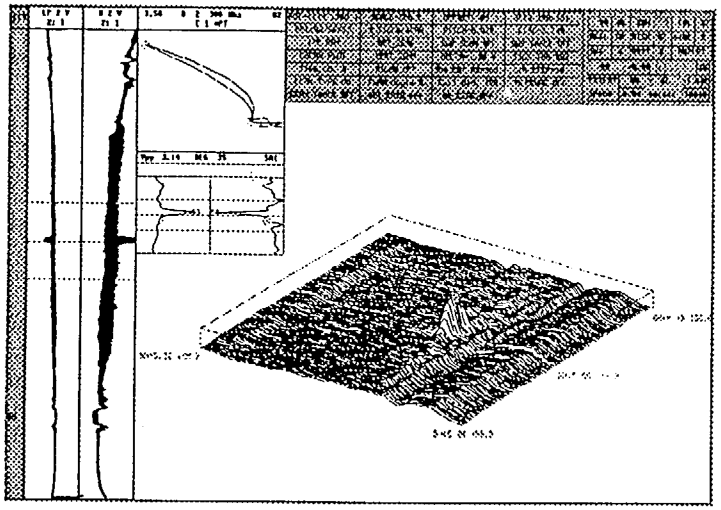
- Performed in accordance with the then current Rev. 4 of the EPRI PWR SG NDE Guidelines
- Midrange +Point probe
  - Qualification documentation ETSS 96511 - 150, 300 & 400 kHz
  - Twenty-six sample data set
    - Two pulled tubes (~40% TW)
    - Twenty-four EDM samples (27% to 100% TW)
    - All 26 flaws detected
    - 91.5% POD at a 90% CL
- No requirement for site-specific technique qualification
- First utilization of midrange +Point at Indian Point Unit 2

1997 Row 2 and 3 U-Bend Examinations

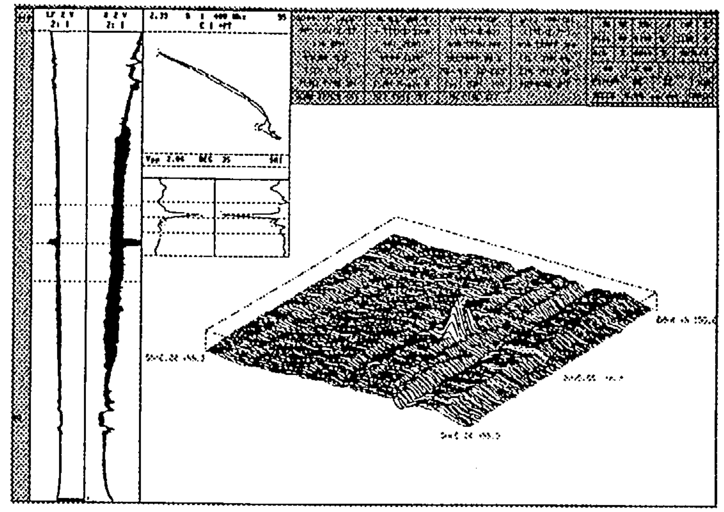
- Site Specific Performance Demonstration (SSPD) in accordance with EPRI PWR SG NDE Guidelines, Rev. 4
  - Practical examination utilized industry degraded U-bend +Point data
  - U-bend cracking not observed previously at Indian Point 2
- Calibration setup within industry variance
- Data quality requirements
  - No quantitative industry standards exist
  - Being incorporated into Rev. 6, EPRI Guidelines, March 2001
- +Point noise level similar to other MA Alloy 600 SGs
- PWSCC identified in the U-bend region of one tube; SG24, R2C67
  - Provided confidence in detection capability

SG 24, R2C67 - 1997 Midrange +Point Data

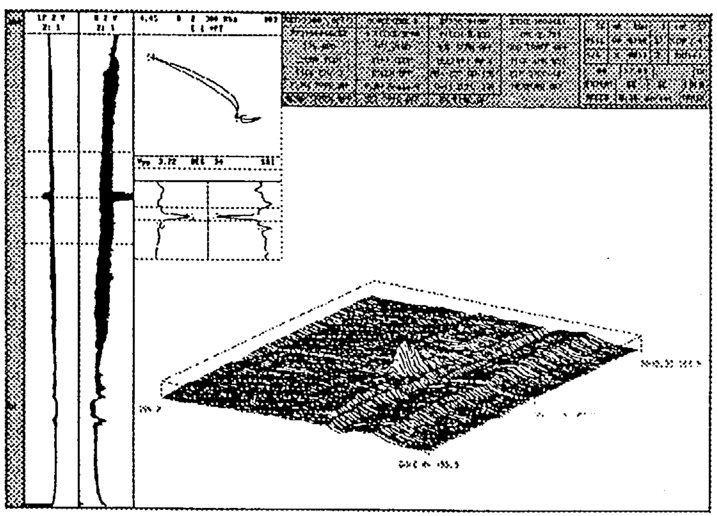
300 kHz 1997 Setup



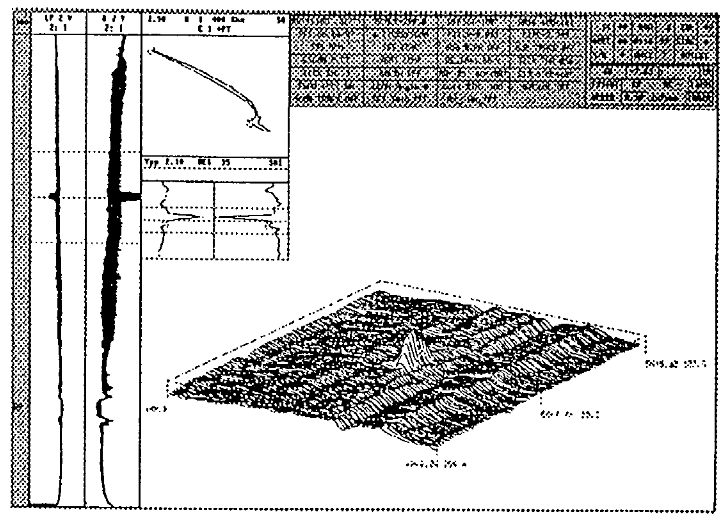
400 kHz 1997 Setup



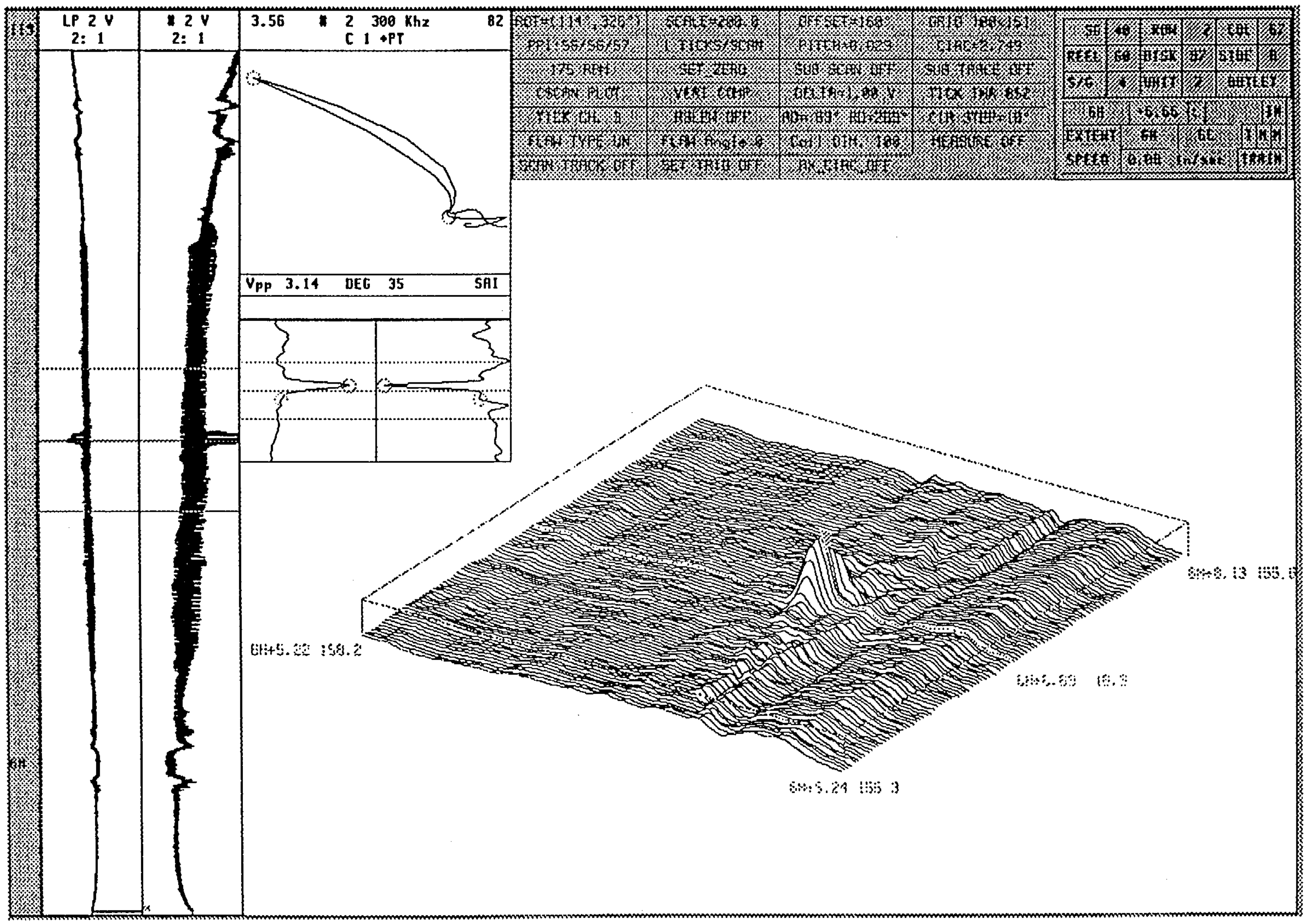
300 kHz 2000 Setup



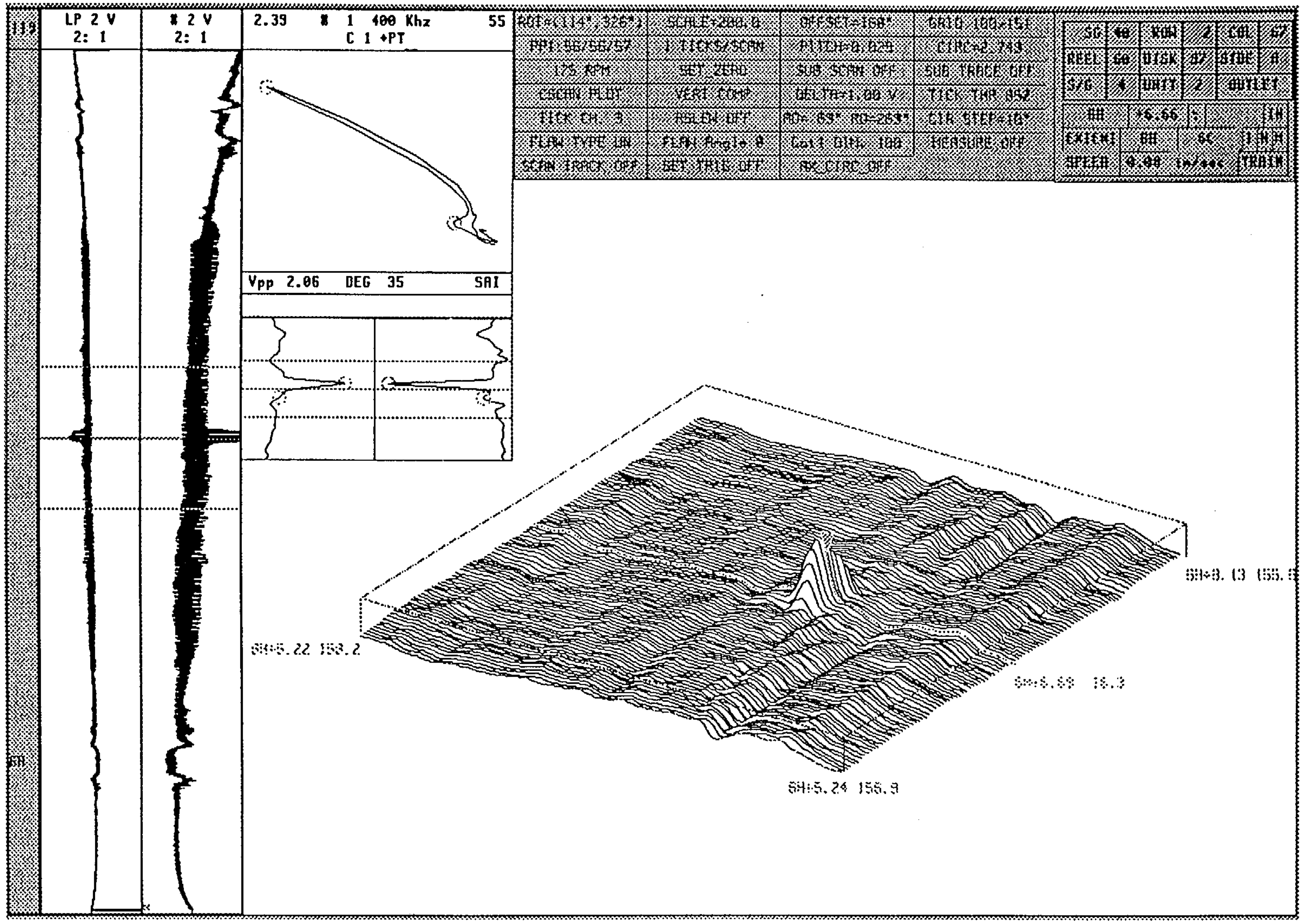
400 kHz 2000 Setup



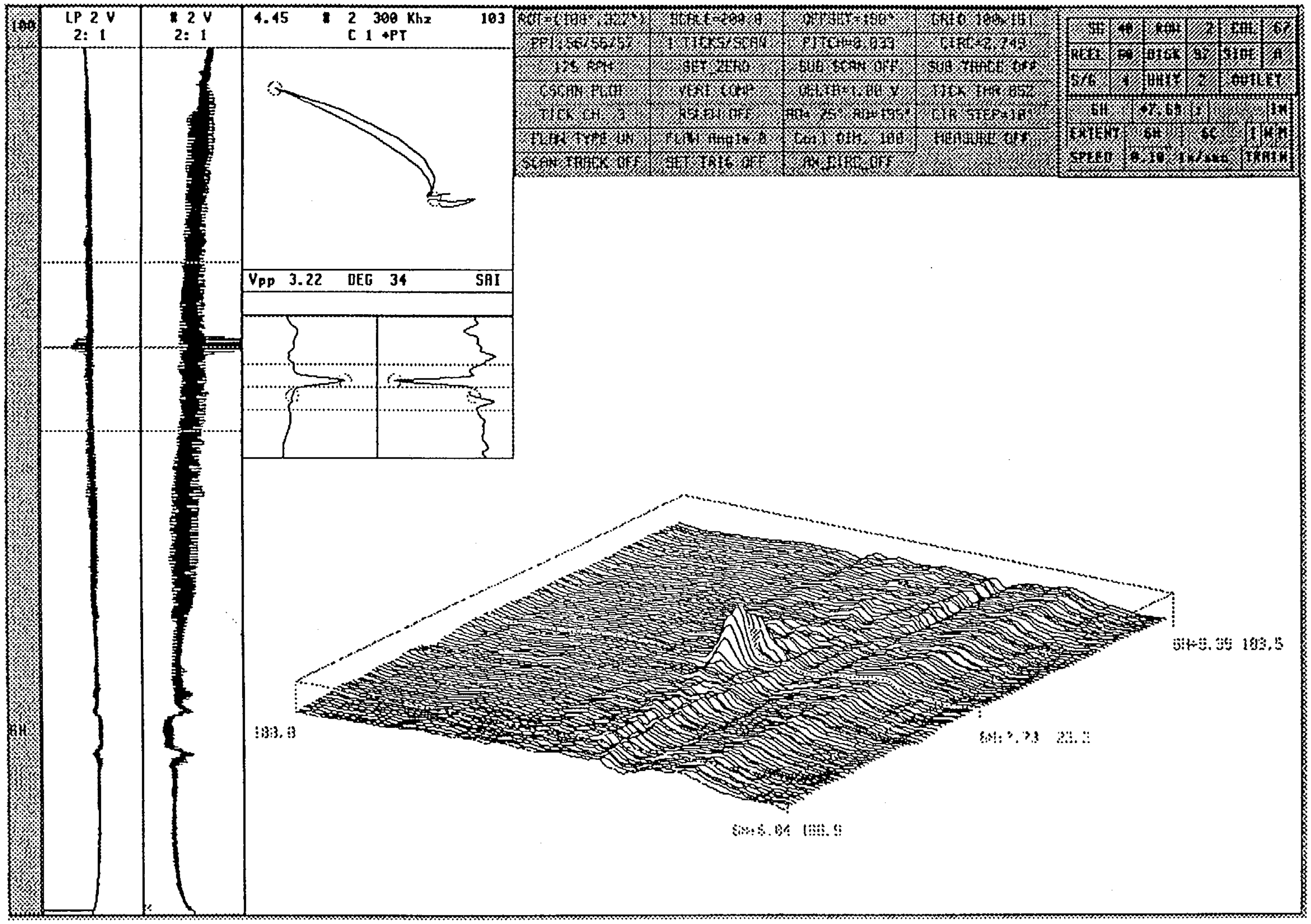
SG 24, R2C67 - 1997
300 kHz Midrange +Point 1997 Setup



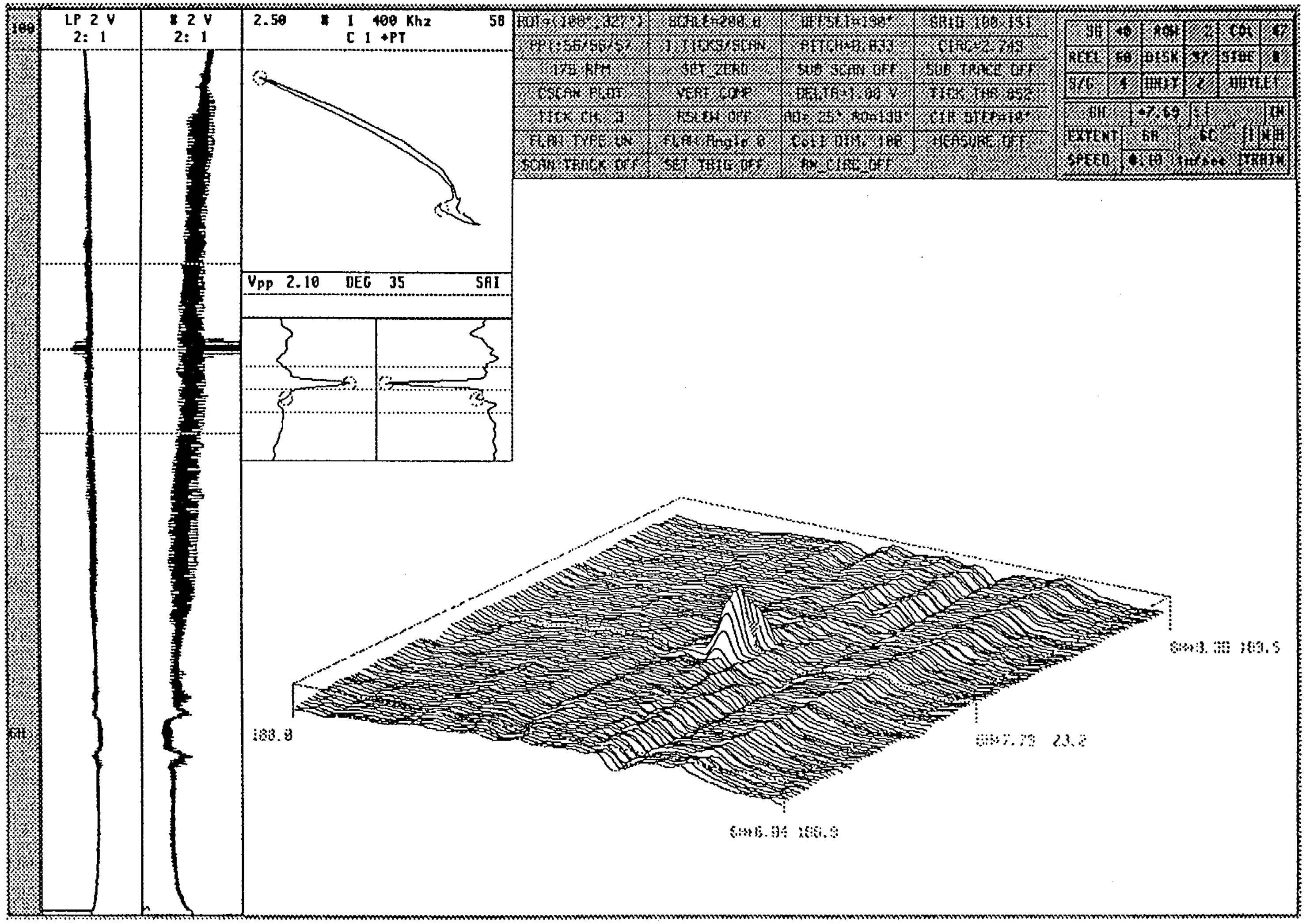
SG 24, R2C67 - 1997
400 kHz Midrange +Point 1997 Setup



SG 24, R2C67 - 1997
300 kHz Midrange +Point 2000 Setup

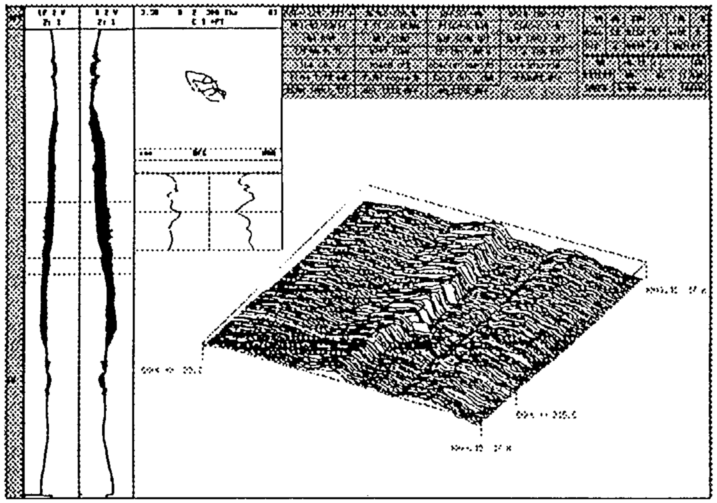


SG 24, R2C67 - 1997
400 kHz Midrange +Point 2000 Setup

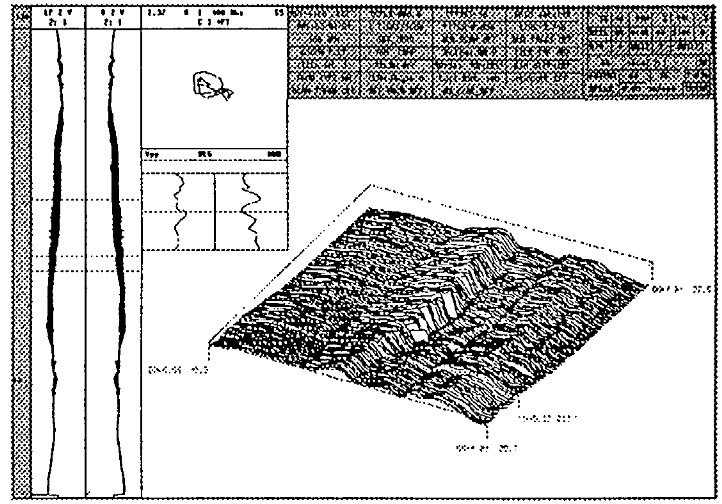


SG 24, R2C5 - 1997 Midrange +Point Data

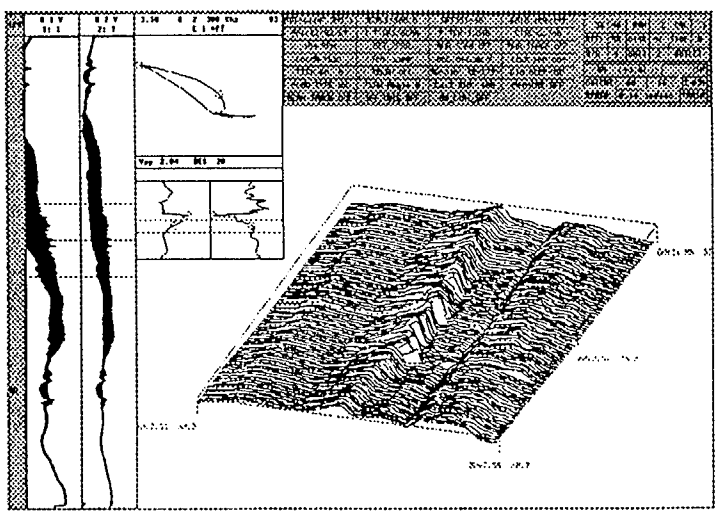
300 kHz 1997 Setup



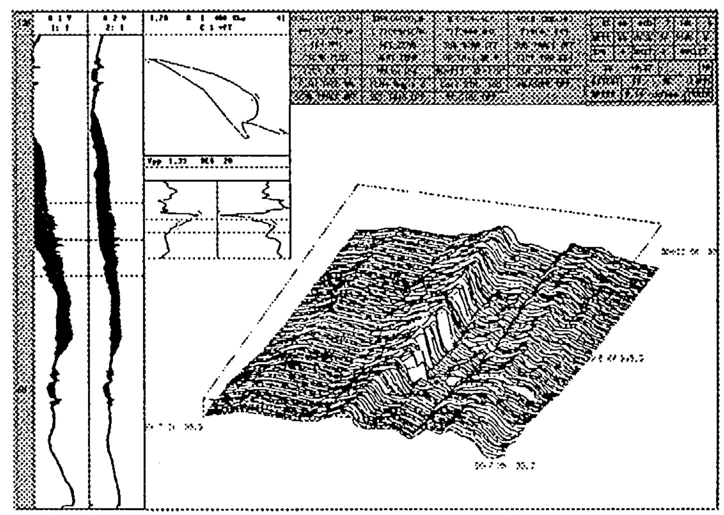
400 kHz 1997 Setup



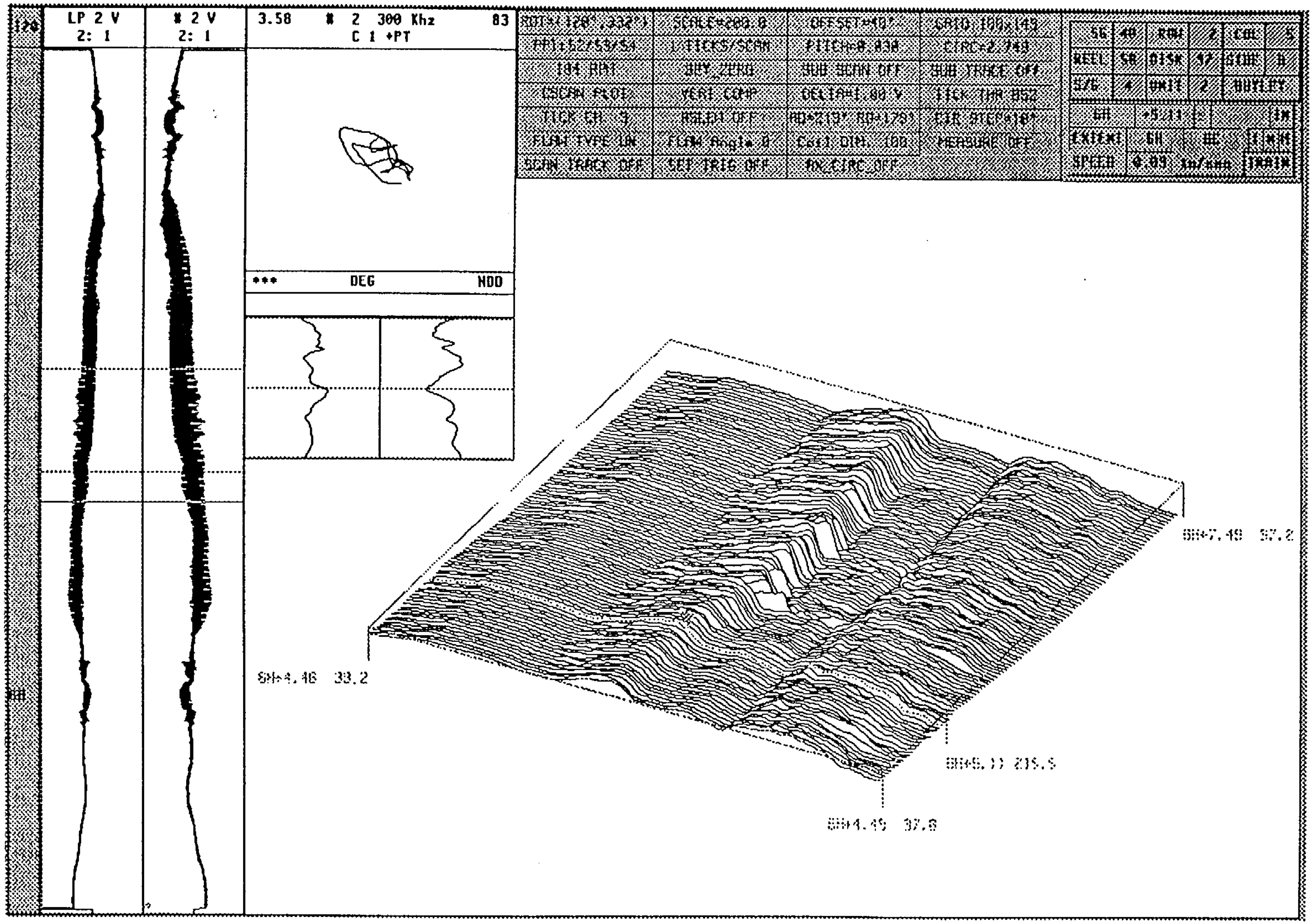
300 kHz 2000 Setup



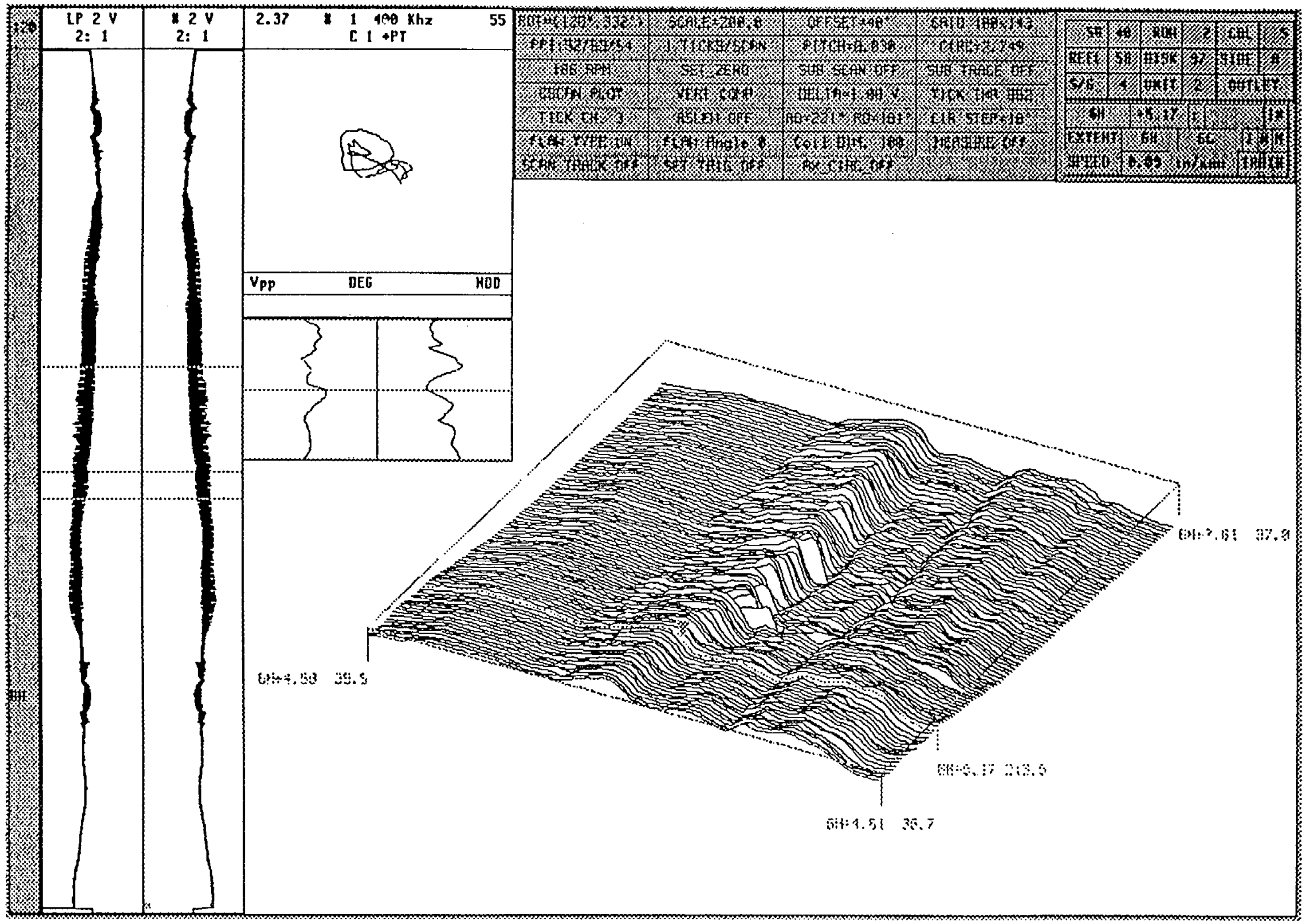
400 kHz 2000 Setup



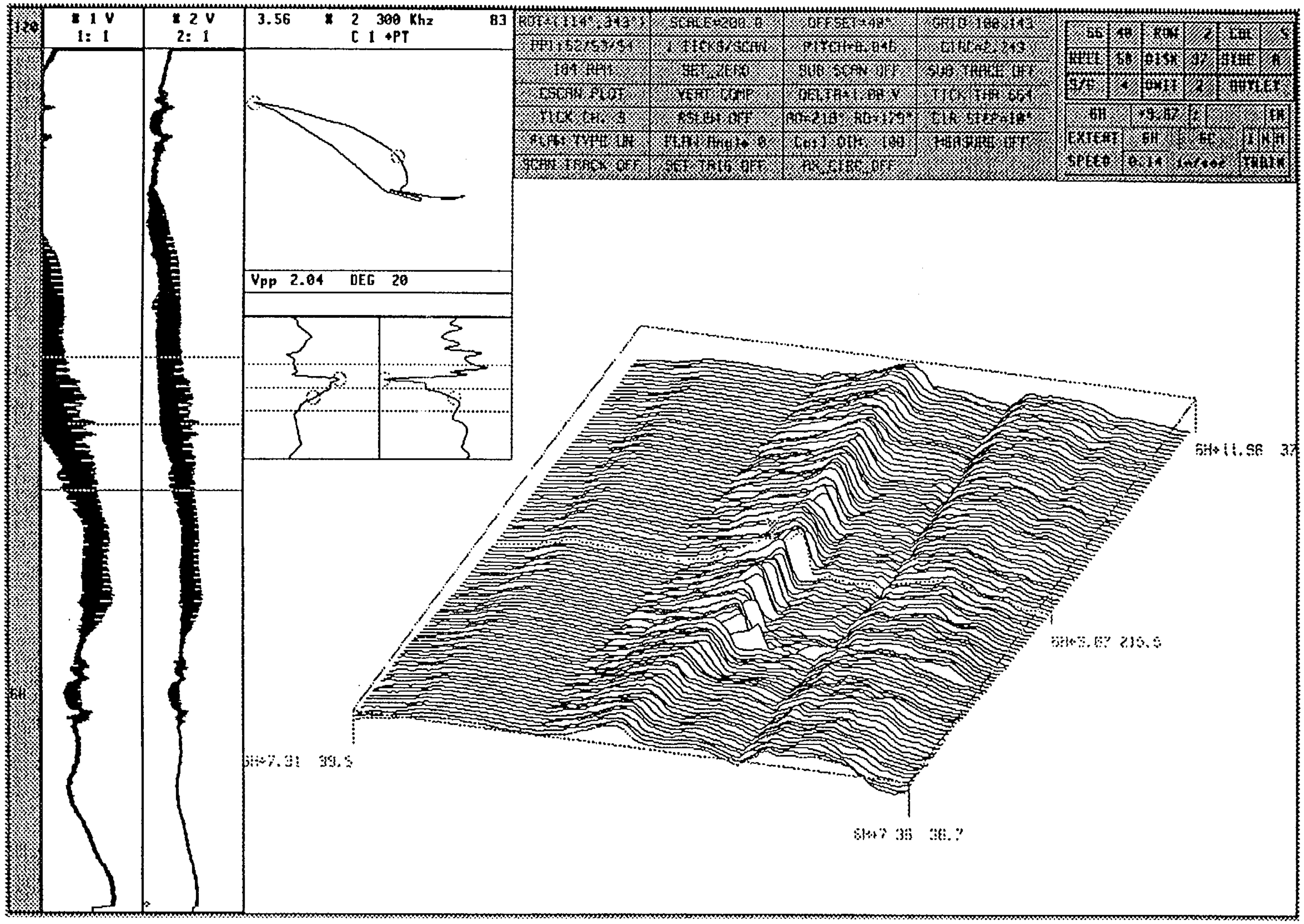
SG 24, R2C5 - 1997
300 kHz Midrange +Point 1997 Setup



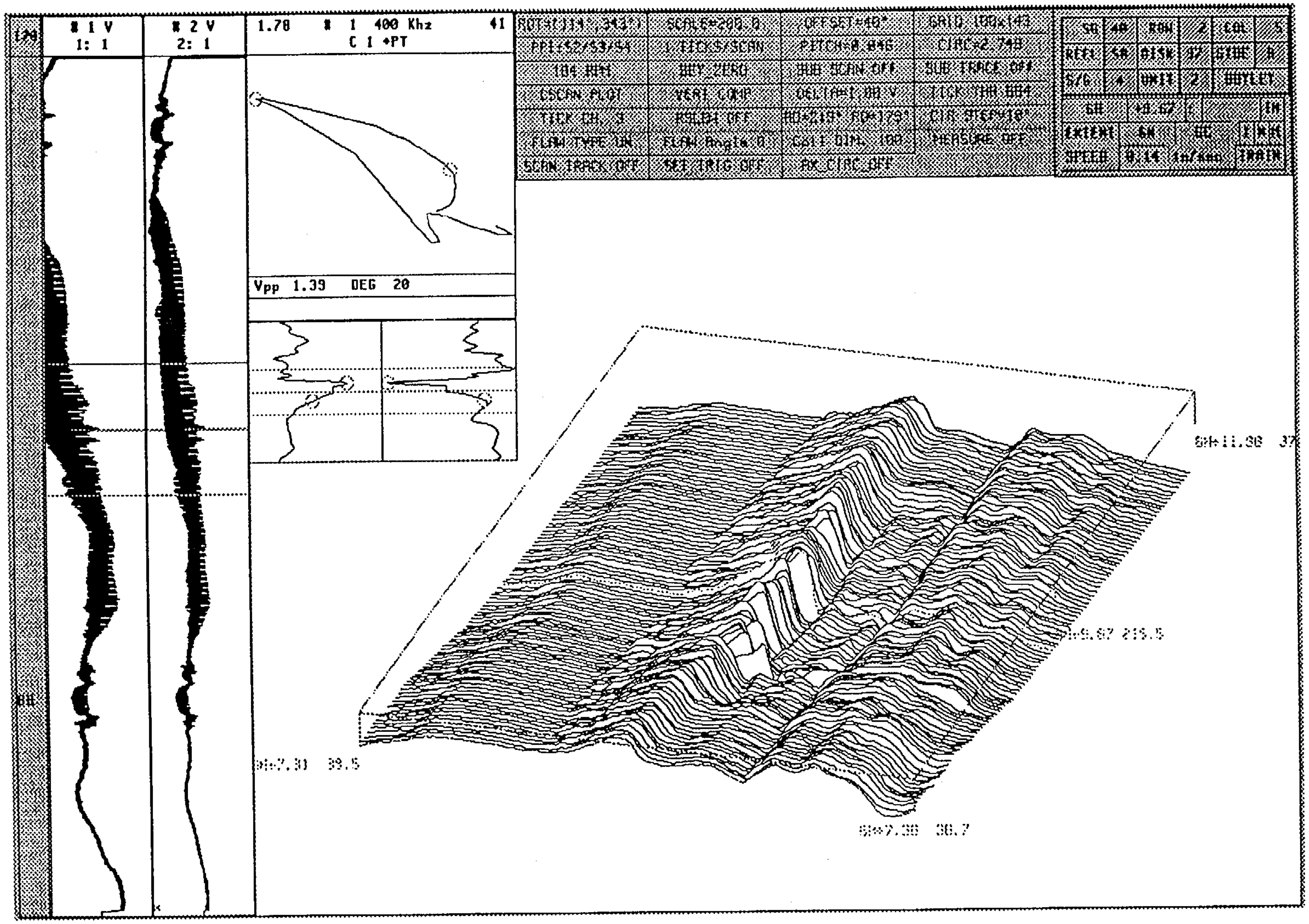
SG 24, R2C5 - 1997
400 kHz Midrange +Point 1997 Setup



SG 24, R2C5 - 1997
300 kHz Midrange +Point 2000 Setup



SG 24, R2C5 - 1997
400 kHz Midrange +Point 2000 Setup



2000 Row 2, 3, and 4 U-Bend Examinations Midrange +Point

- SSPD in accordance with Rev. 5 of the EPRI NDE Guidelines
  - Written training supplement developed
    - "IP2 Spring 2000 Outage U-bend +Point Analysis Training"
    - Setup with 20% ID EDM notch visible at 6 to 10°
    - Requirements for data quality
- All row 2, 3 and 4 U-bends inspected with midrange +Point
  - PWSCC identified in three U-bends
    - SG 21; R2C87
    - SG 24; R2C69 and R2C72
- Data quality criteria evolved as inspections progressed
- Independent review of all low row U-bend data (Tertiary review)
  - Senior analysts performed primary, secondary and resolution
  - Specific training administered
  - Revealed no new indications
  - 457 of 863 U-bends classified as BDA due to low S/N ratios

Assessment of High Frequency +Point Options

- Site test of midrange +Point at 750 kHz and 800 kHz high frequency +Point
  - Prototype 800 kHz +Point probe manufactured
  - Retested two tubes with PWSCC identified by midrange +Point
    - SG 24; R2C69 and R2C72
  - High frequency probe showed better S/N ratios
- Decision to reinspect with the 800 kHz high frequency probe

High Frequency Probe Qualification

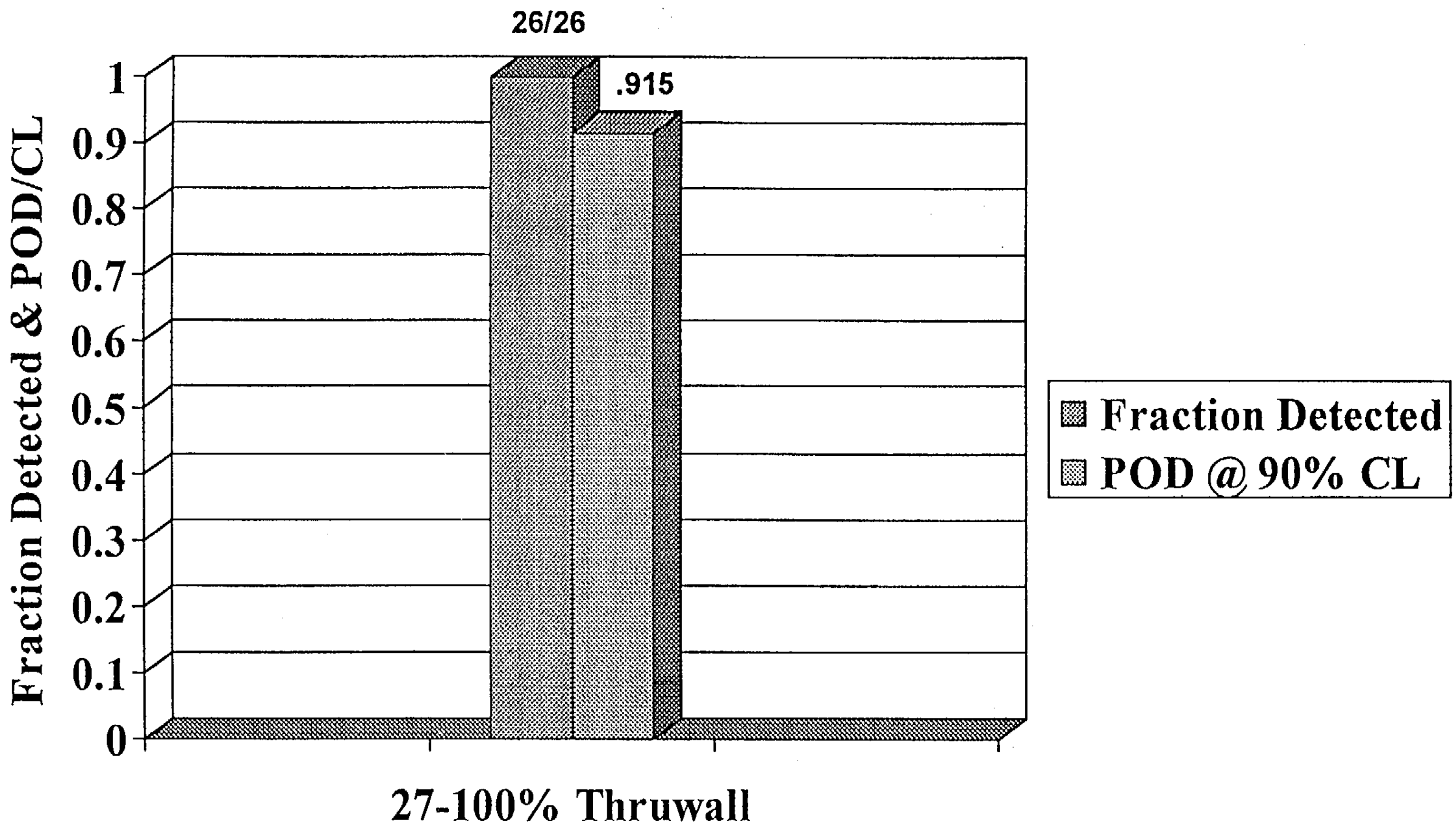
- 800 kHz high frequency +Point qualified per EPRI Rev. 5
- Qualification documentation
  - EPRI ETSSs 99997.1 (800 kHz) and 99997.2 (1000 kHz)
  - Twenty-six sample data set
    - Two pulled tubes with service-related degradation (~40% TW)
    - Twenty-four EDM samples (27% to 100% TW)
    - All 26 ID flaws detected
    - 91.5% POD at a 90% CL
    - Deposit simulation with Cu foil had no effect on detectability
- High frequency probe was site qualified

High Frequency +Point
ETSS 99997.1 Qualification Data Set
for U-Bend PWSCC

Type	Depth (% Thruwall)
Pulled Tube	40
Pulled Tube	40
Lab EDM	62
Lab EDM	62
Lab EDM	42
Lab EDM	42
Lab EDM	40
Lab EDM	100
Lab EDM	100
Lab EDM	44
Lab EDM	60
Lab EDM	60
Lab EDM	50

Type	Depth (% Thruwall)
Lab EDM	54
Lab EDM	40
Lab EDM	58
Lab EDM	55
Lab EDM	55
Lab EDM	55
Lab EDM	55
Lab EDM	44
Lab EDM	44
Lab EDM	45
Lab EDM	32
Lab EDM	27
Lab EDM	41

High Frequency +Point
ETSS 99997.1 Technique Performance



2000 U-Bend Examinations
800 kHz High Frequency +Point

- First industry application of the 800 kHz high frequency +Point
  - All row 2 and 3 U-bends
  - Row 4 U-bends classified as BDA with midrange +Point
- Applied data quality requirements developed for midrange +Point
- High frequency +Point identified PWSCC in four U-bends classified as BDA or RST with the midrange +Point:
  - SG 23; R2C85
  - SG 24; R2C4, R2C71 and R2C74
- Five tubes remained classified as BDA with 800 kHz +Point
  - All BDA tubes plugged

Data Quality Results
Midrange Vs High Frequency +Point

Row	Midrange Independent Review Results (Number of Tubes)		High Frequency Results (Number of Tubes)	
	Acceptable Data	Low S/N Data	Acceptable Data	Low S/N Data
SG 21				
2	40	32	71	1
3	32	57	88	1
4	41	46	46	0
Totals	113	135	205	2
SG 22				
2	16	19	35	0
3	31	43	74	0
4	38	36	36	0
Totals	85	98	145	0
SG 23				
2	7	38	44	1
3	23	49	72	0
4	30	60	60	0
Totals	60	147	176	1
SG 24				
2	28	28	54	2
3	37	48	85	0
4	83	1	1	0
Totals	148	77	140	2
Overall Totals	406	457	666	5
Percent Bad Data		(53%)		(0.8%)

2000 U-Bend Examination Results
Midrange Vs High Frequency

Steam Generator	Tube	Midrange Coil	High Frequency Coil
21	R2C87	SAI	SAI
23	R2C85	BDA	SAI
24	R2C4	BDA	SAI
24	R2C69	SAI	SAI
24	R2C71	RST*	SAI
24	R2C72	SAI	SAI
24	R2C74	BDA	SAI

*** Reported restricted at 6H and UB**

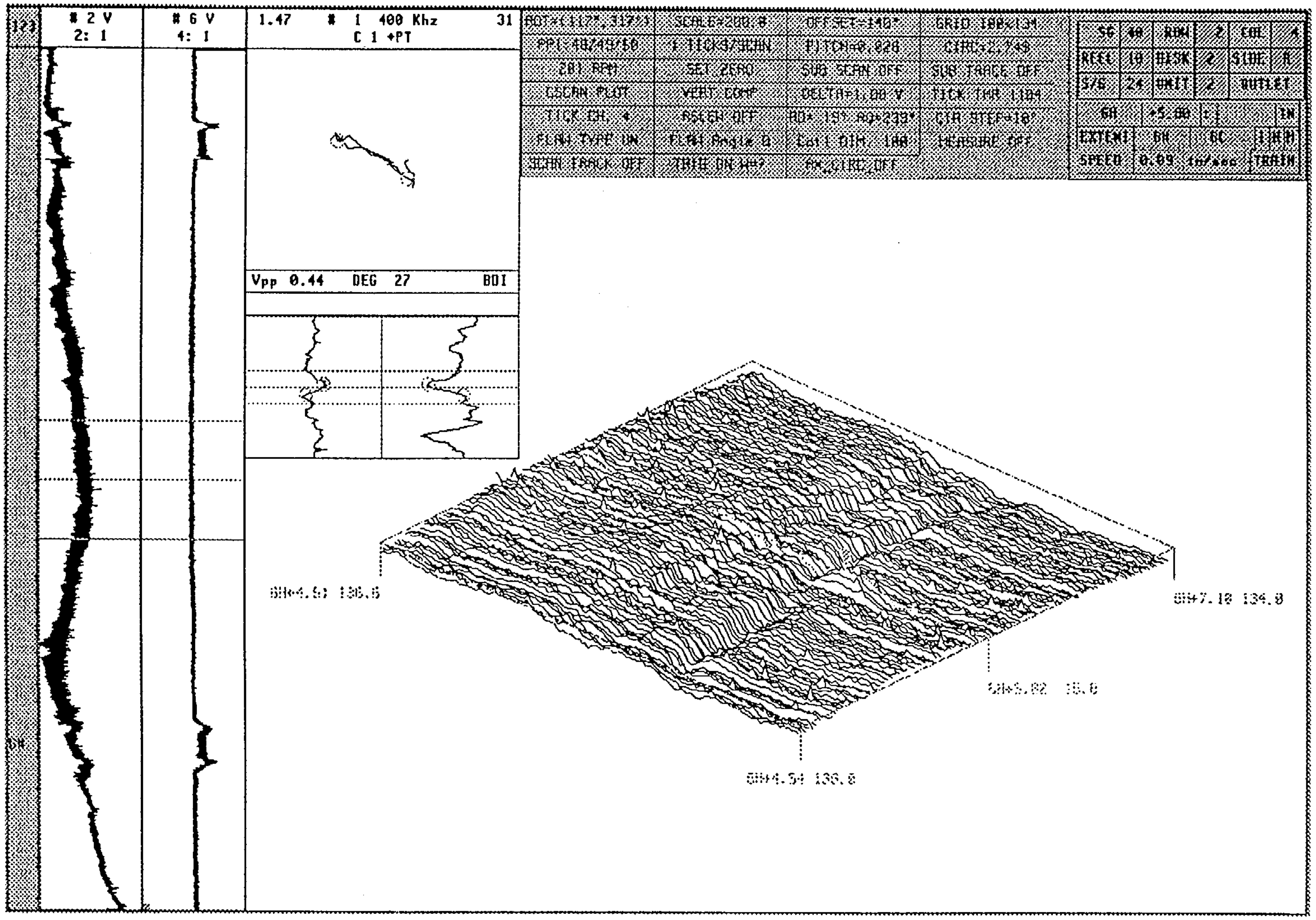
Conclusions
Low Row U-Bend Examination Programs

- 1997, 2R13 examination program met industry guidelines
  - Revision 4 of the EPRI PWR Steam Generator NDE Guidelines
    - Industry qualified technique
    - Site-specific performance demonstration
    - Calibration setups within industry variance
    - U-bend +Point data quality similar to industry (Alloy 600 MA)
- 2000, 2R14 examination program met industry requirements
  - Revision 5 of the EPRI PWR Steam Generator NDE Guidelines
    - Site qualified technique
    - Site-specific performance demonstration
    - Calibration setups within industry variance
    - New data quality requirements

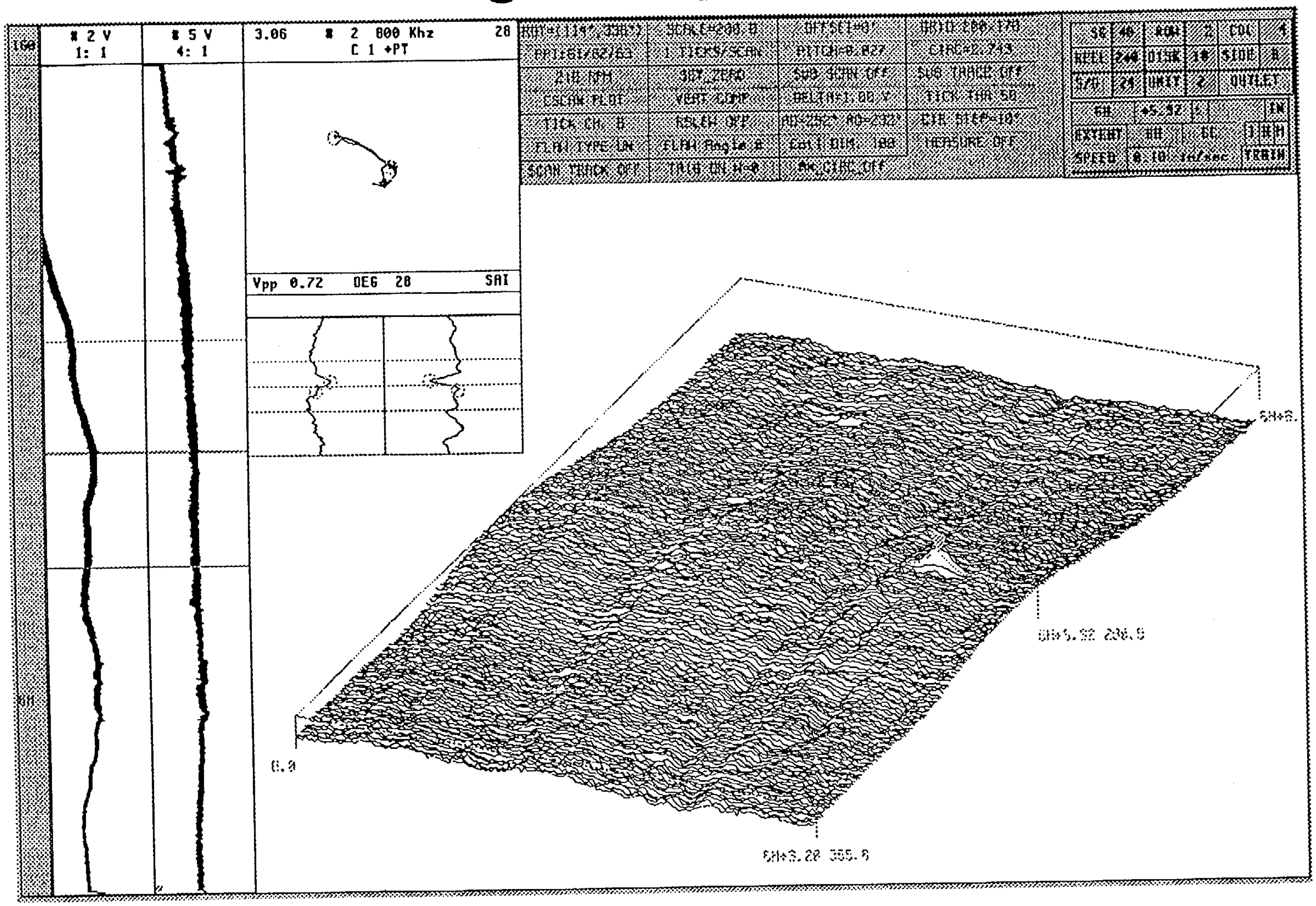
Conclusions (Cont'd)
Low Row U-Bend Examination Programs

- When lessons-learned are considered, use of high frequency +Point provided the most significant improvement to POD
  - Setup rotations and 300 kHz versus 400 kHz midrange data provided minimal improvement
- The high frequency probe is site validated for use at Indian Point Unit 2
- Technology being transferred to other utilities and vendors

SG 24, R2C4 - 2000
400 kHz Midrange +Point



SG 24, R2C4 - 2000
800 kHz High Frequency +Point



ROOT CAUSE ANALYSIS REPORT OVERVIEW

J. O. PARRY
PROJECT MANAGER

May 3, 2000

AGENDA

- OVERVIEW OF EVENT
- PRIMARY TO SECONDARY LEAKAGE
- TUBE RESTRICTION HISTORY
- OTHER ISSUES
- ROW 2 - 1997 VIEWPOINT
- DOMINION ENGINEERING REVIEW

OVERVIEW

February 15, 2000

- Primary/Secondary leakage @ 1915 hrs. 3.5 GPD
- 1929 hrs. 75-100 gpm, plant shutdown
- R2C5 in SG24 leaking at U-bend (2/27/00)
- Welch-Allyn video probe used, shows large axial indication in the U-bend (2/28/00)
- Attempted +Point analysis of R2C5, probe caught in U-bend, obtained partial data from crack tip, location on extrados of tube

OVERVIEW

February 15, 2000 (cont.)

- R2C5 bobbin tested, 1.7-1.8" long located at apex (3/1/00)
- +Point obtained by gluing coil "shoe" down
  - Located on Extrados
  - Axial indication
  - Length between 2 to 2-1/2"
- Secondary side inspections and analysis begin concurrently

OVERVIEW

Analysis of leaking tube begins 2/28/2000

- Reviewed 1997 data for R2C5, 20/20 hindsight, Identified possible indication at U-bend apex
  - Concluded indication Axial PWSCC based on phase angle
  - Analyzed the program to determine any weaknesses
  - Analysis identified low signal-to-noise (S/N) ratio
  - Sources of noise identified
    - OD deposits mask flaw signal
    - Probe rotational speed variations possibly due to ovalization
  - Examined data analysis setup and techniques used in 1997
    - Noise rejection criteria developed
    - Determined that 400kHz has slightly better Signal/Noise ratio
    - 1997 data would be classified as BDA with 2000 criteria

OVERVIEW

Training initiated incorporating lessons learned

- 2000 utilized EPRI rev 5 guidelines for ID phase setup
- Instructions for poor S/N data rejection
- Program of retesting noisy data until acceptable or plugged is instituted
- Training administered to analysts
  - Separate training sessions
- Lessons learned evolved during U-bend inspection

OVERVIEW

Initial Inspection starts

- Inspection scope - 100% of U-bend in rows 2, 3 and 4
- Examined initial scope
- 3 PWSCC axial cracks located on the extrados of the U-bend apex found in row 2
- No indications found in any row 3 or row 4
- Extensive retesting conducted on noisy tubes, many still unacceptable
- First analysis of U-bends complete

OVERVIEW

Independent analysis begins

- Updated training program - second phase
- Senior analysts used, new to project
- Primary/Secondary and Resolution teams
- Analyzed all 2000 U-bend midrange +Point data
- No new indications reported; 3 row 2 indications reconfirmed
- Result - 53% of inspected tubes still unacceptable due to low S/N at this point

OVERVIEW

Higher inspection frequencies to enhance PWSCC detection are investigated

---

- Mid range (MR) +Point at 750kHz
- New high frequency (HF) plus point: 0.075" diameter @ 800kHz - 1MHz
- Used both techniques to examine known PWSCC
- HF +point is chosen best
- Qualification is formalized (3/20/00)

OVERVIEW

Production testing with HF +pt begins

- Retest rows 2 and 3, and low S/N tubes in row 4 with HF probe
- 4 new PWSCC flaws discovered in previously noisy tubes
- No new indications found in Initial Program NDD tubes
- No indications in rows 3 or 4 U-bends

OVERVIEW

Production testing with HF +pt begins (cont.)

- 452 tubes recovered from original BDA calls
  - Data quality much better on HF
  - 5 tubes remain bad data, are plugged
- Insitu pressure tests all SAI Ubends
  - 7 tubes tested
  - Exceeded requirement for Insitu test
  - 3 Ubends leaked - met burst criteria
- All row 2 tubes are plugged at the conclusion of U-bend inspections

OVERVIEW

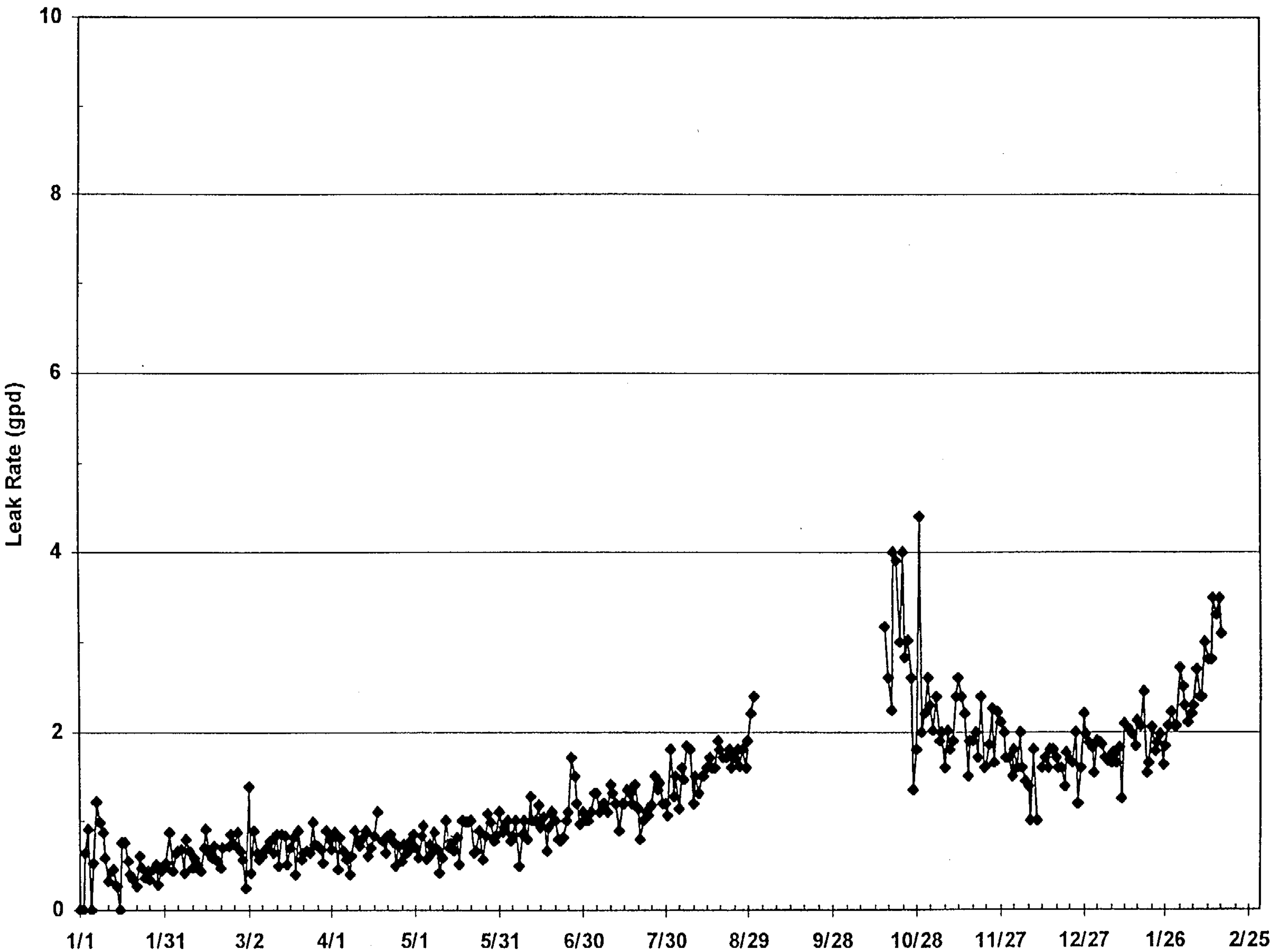
Concurrent Activities

- Secondary side pressure tests on each SG
- Secondary side component inspection and evaluation
- Install hillside ports
- Investigate and measure hour-glassing
- Sludge Lancing/Fosar

PRIMARY TO SECONDARY LEAKAGE

- Prior to February 15, 2000
  - Nitrogen 16 radiation monitor in service
  - Alarm points
    - 10 gallons/day
    - 25 gallons/day
    - 150 gallons/day
    - Recorder out of service
  - Common alarm in Control Room
    - Accident assessment panel
    - Operator responds locally for common alarms
  - Local alarm also
  - Leak rate trend for 1999-2000 is attached

Indian Point 2- Primary to Secondary Leak Rate Calculated from Condenser Off Gas



PRIMARY TO SECONDARY LEAKAGE

- Chemistry routine checks N-16 radiation monitor 1/shift
- Air ejector radiation monitor R-45
  - Rad monitor provides overall trending
  - Chemistry samples pathway – calculates overall leak rate
    - IPCA 110, primary to secondary leak rate calculation
  - 7:15 PM on 2/15/00 chemistry rounds, leak rate = 3.4 GPD per N-16

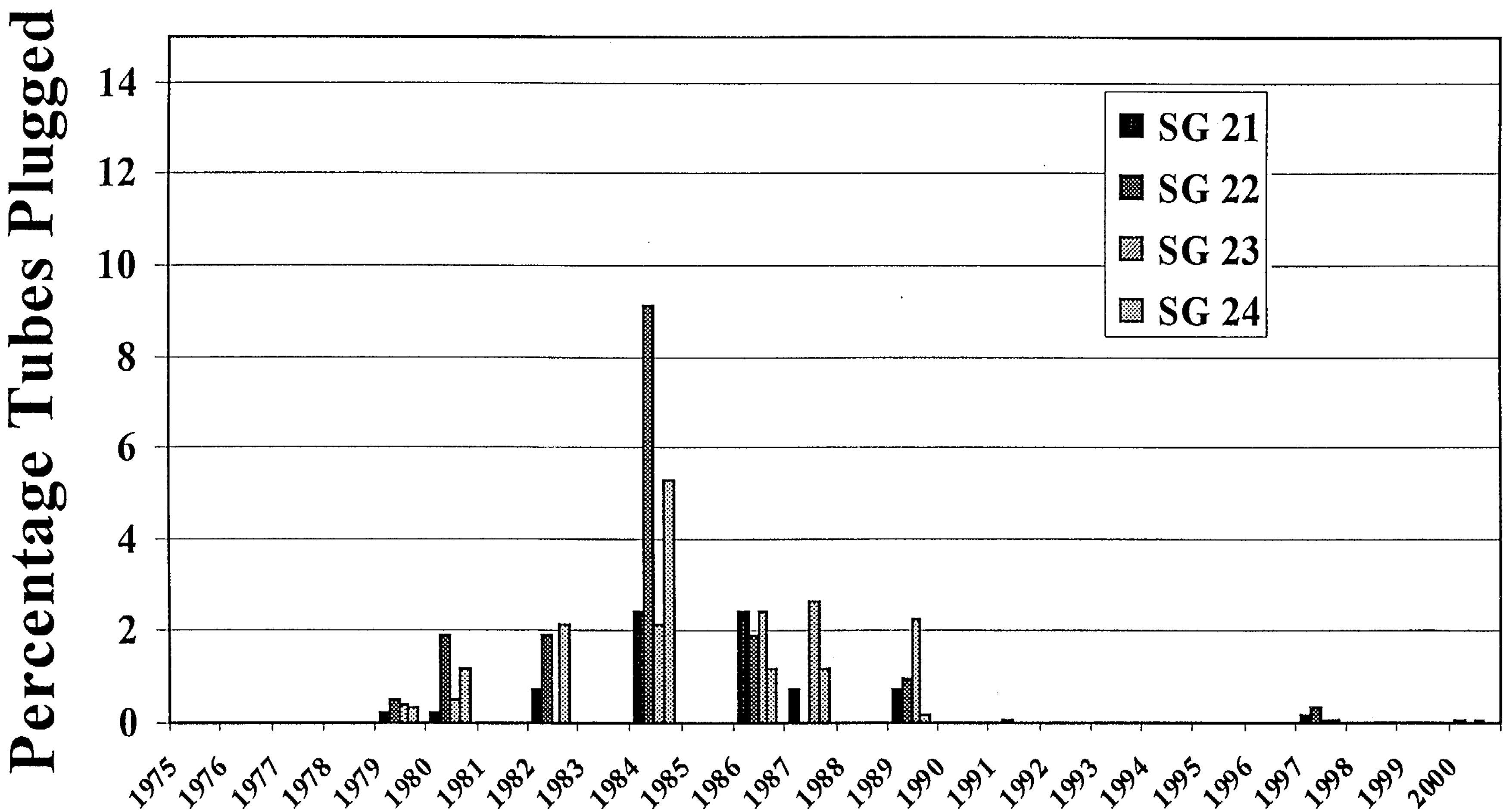
PRIMARY TO SECONDARY LEAKAGE

- Proposed 2000 Primary to Secondary Leak limits
  - Base administrative limits on new EPRI guidelines, February 2000

TUBE RESTRICTION HISTORY

- Frequency of plugging due to 610 probe restrictions has not significantly increased
- 2000 - 2 tubes plugged due to 610 probe restriction
- Data from 1997 & 2000 allows improved tracking

Tubes Plugged For 0.610" Probe Dent Restrictions



COMPARISON OF TUBE/TUBE SUPPORT PLATE RESTRICTIONS FROM 1997 - 2000

STEAM GENERATOR 23

Location	Restriction Size		
TSP	Smaller	No Change	Larger
6H	6	8	4
6C	11	17	5
5H	0	7	0
5C	1	7	3
4H	2	2	2
4C	1	11	2
3H	0	17	0
3C	2	16	8
2H	2	14	1
2C	0	13	2
1H	4	20	4
1C	0	14	2
Total	29	146	33
%	13.94%	70.19%	15.87%

- **13.94% of the intersections became more restricted**
- **70.19% did not change**
- **15.87% of the intersections became less restricted**
- **No history to compare**

OTHER ISSUES

- Susceptibility of ALLOY 600
  - PWSCC is a function of material, stress, environment
- Material Anneal Temps - 1850 F
- Environment, T-hot - 590 F
- Primary Water Chemistry
- Stress - Evaluated separately by ALTRAN & Westinghouse

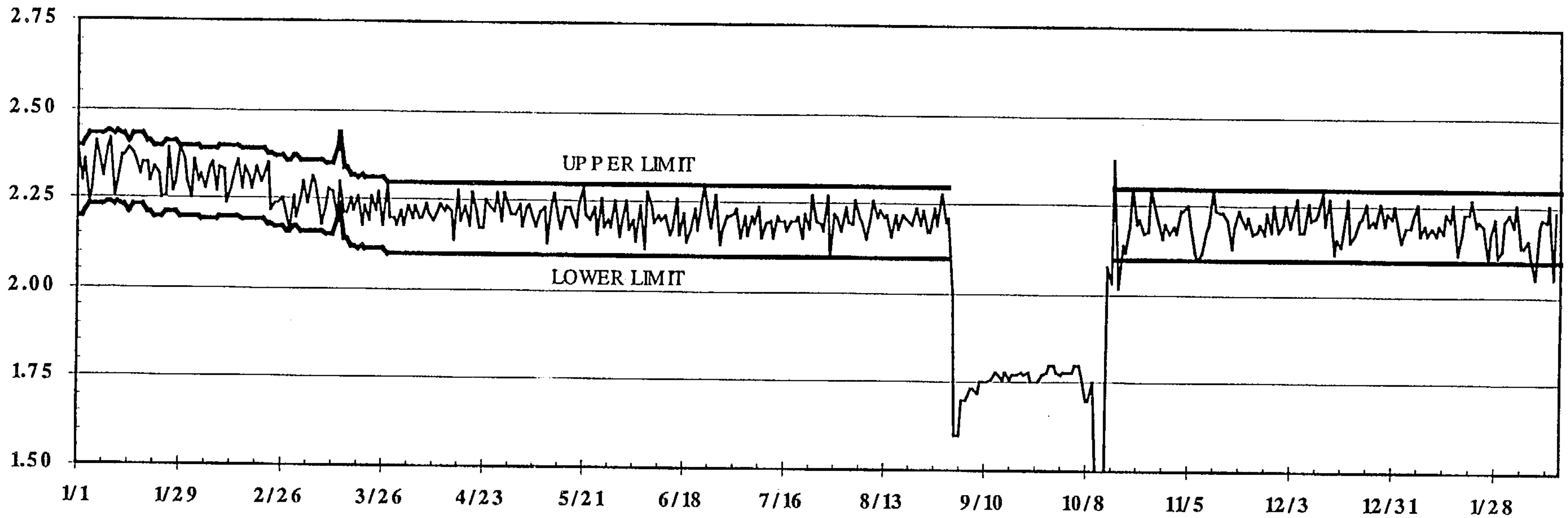
OTHER ISSUES

- Primary Water Chemistry
  - Followed industry guidelines
  - Boron/Lithium curve
  - Hydrogen concentrations
  - Assessed as not an issue

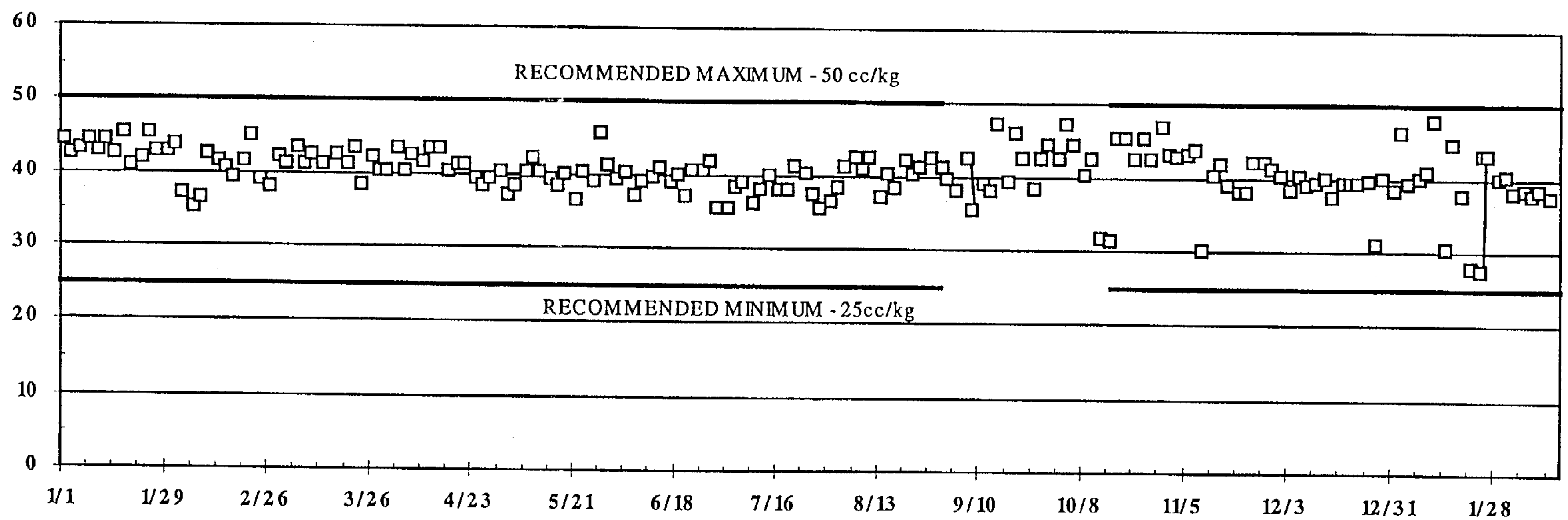
OTHER ISSUES

- ODSCC potential for ROW 2 U-bend apex consideration
  - This was a consideration to plug ROW 2 and examine ROW 3 & 4
  - High frequency detection verified for ODSCC at top of tube sheet
  - Verified OD flaw detection on U-bend samples

REACTOR COOLANT LITHIUM



REACTOR COOLANT DISSOLVED HYDROGEN



ROW 2 - 1997 VIEWPOINT

- 1997 Visual inspection of top support plate
  - Visual inspection performed on 22 & 23 steam generators
    - No cracking in 6TH support plate flowslot in 22 or 23 SG
    - No observable hourglassing

ROW 2 - 1997 VIEWPOINT

- Inspection program consistent with industry standards
- Quality of data issue in EDDY CURRENT
  - +Point probe used for ROW 2 & 3 in 1997
    - Best probe available for U-bend
  - Program detected one PWSCC indication in R2/C67 in 24 SG
  - No indication detected in ROW 3 in 1997

DEI REVIEW

U-BEND PWSCC PREDICTIONS

- DEI was tasked to prepare tube degradation predictions for IP2 in 1995 and 1997.
  - Included U-bend PWSCC
- In 2000, DEI was asked to update 1997 predictions based on re-analysis of 1997 data.

DEI REVIEW

1995 U-BEND PWSCC PREDICTIONS

- No U-bend cracks were detected through 1995.
- Predictions performed using industry data for time of first detection for Huntington tubes, and assumed rate of increase based on industry experience with PWSCC (Weibull slope of 3).
- No Cracks Predicted for 1997 or 2000.
- Main factors for low rate of PWSCC:
  - Low temperature
  - Huntington tubes

DEI REVIEW

1997 U-BEND PWSCC PREDICTIONS

- Single U-Bend PWSCC flaw detected in 1997.
- Predictions developed based on plant specific starting point (1 flaw) and rate of increase of roll transition PWSCC in Huntington tubes (Weibull slope of 4) (known to be consistent with U-bend slopes).
- Predicted 1 new flaw during next operating cycle.

DEI REVIEW

EVALUATION of IP 2 U-BEND PWSCC EXPERIENCE

- Rate of increase of U-Bend PWSCC at IP2 assessed using re-evaluation of 1997 data and mid range +Point results from 2000 (i.e. consistent inspection sensitivity)
- Assumes 3 flaws in 1997
  - 5 cumulative flaws in 2000
- Above data give Weibull slope of 4.7

DEI REVIEW

EVALUATION of IP 2 U-BEND PWSCC EXPERIENCE (Cont.)

- Weibull slope of 4.7 is consistent with industry experience, and indicates that IP2 would experience about 8 new Row 2 flaws in the next fuel cycle (624 EFPD) if not preventively plugged, using high frequency probe
- Industry data:
  - EPRI report NP-7493 (1991) Figures 3-6, 7 & 8: Weibull slopes of 4.5, 4.4, and 4.2 for row 1 and 2 U-bends.
  - EPRI report TR-104030 (1994) Table 5-1: PWSCC median slope for row 1 and 2 U-bends about 4.4.

DEI REVIEW

EVALUATION of IP 2 U-BEND PWSCC EXPERIENCE (Cont.)

- Observed Weibull slope of 4.7 is within bounds used in analysis in 1997 (Weibull slopes assumed to range between 2 and 6, median of 4).

DEI REVIEW

Comment Regarding SCC Statistics

- SCC typically exhibits large scatter, with small numbers of early failures, and increasing numbers as time progresses.
- This pattern often modeled using Weibull statistics, with Weibull slopes in range of 2 to 6 commonly seen.
- Applying these slopes to IP2 indicates that moderate increases in numbers of new PWSCC flaws at Row 2 would be seen in future cycles, if tubes were not preventively plugged.

DEI REVIEW

Potential for Row 3 U-Bend PWSCC

- No known cases of row 3 U-bend PWSCC leaks in industry.
- No detected PWSCC in IP2 row 3 U-bends.
- Crack initiation in row 3 expected to be significantly later than in row 2 based on lower stresses and lower cold work.
- Crack growth rate expected to be lower than in row 2 based on lower stresses and lower cold work.

DEI REVIEW

Potential for Row 3 U-Bend PWSCC (cont.)

- Conclusion: Very low likelihood of large flaws developing during next inspection interval based on:
  - Low potential for crack initiation
  - Low growth rate
  - Modified operating interval

Indian Point 2 Condition Monitoring Operational Assessment Plan

Tom Pitterle

Westinghouse Electric Company

Definitions

- Condition Monitoring (Backwards Looking)
  - Evaluation of indications found this inspection against performance criteria
- Operational Assessment
  - Evaluation against performance criteria at the end of the next operating period
- Burst
  - Gross structural failure of tube wall---unstable opening displacement
  - Not ligament tearing

Performance Criteria

- Steam generator tubing shall retain structural integrity over full range of operating conditions
  - Margin of 3 against burst under normal steady state full power operation
  - Margin of 1.4 against burst under SLB
- Primary to secondary accident induced leakage not to exceed 1 gpm. under SLB

Issues

- Show that all structurally significant degradation has been detected and that which is undetected will not grow to be structurally significant during the next operating cycle
  - Probability of Detection (POD)
  - Growth Rate
  - NDE Sizing
- Complicating Factors
  - Secondary side scale deposits (copper) result in low signal to noise ratios making NDE data more difficult to interpret

Tube Integrity Considerations

- ***Low row U-bends (PWSCC)***
- Dented tube support plate intersections (primarily PWSCC but also potential for ODSCC)
- ***Sludge pile (within 10" TTS)--(ODSCC)***
- Area just above sludge pile (ODSCC)
- Tubesheet region
  - Dents at TTS (primarily PWSCC but also potential for ODSCC)
  - Crevice region (ODSCC)
  - Roll transition (PWSCC)

Low Row U-Bends

- R2C5 leaked in service
  - Would not have been "called"
  - Improvements in NDE would not leave an indication of this size in service in 2000 inspection
- POD
  - Improvements in analyst guidelines & training
  - High frequency probe has improved data quality & detection
    - Enhanced S/N ratio
    - Found additional smaller indications
    - No indications in rows 3 & 4
  - POD compared to other industry experience
    - PWSCC in symmetrical and axial dents

Figure 5-1. Comparisons of +Point Average Depth PODs

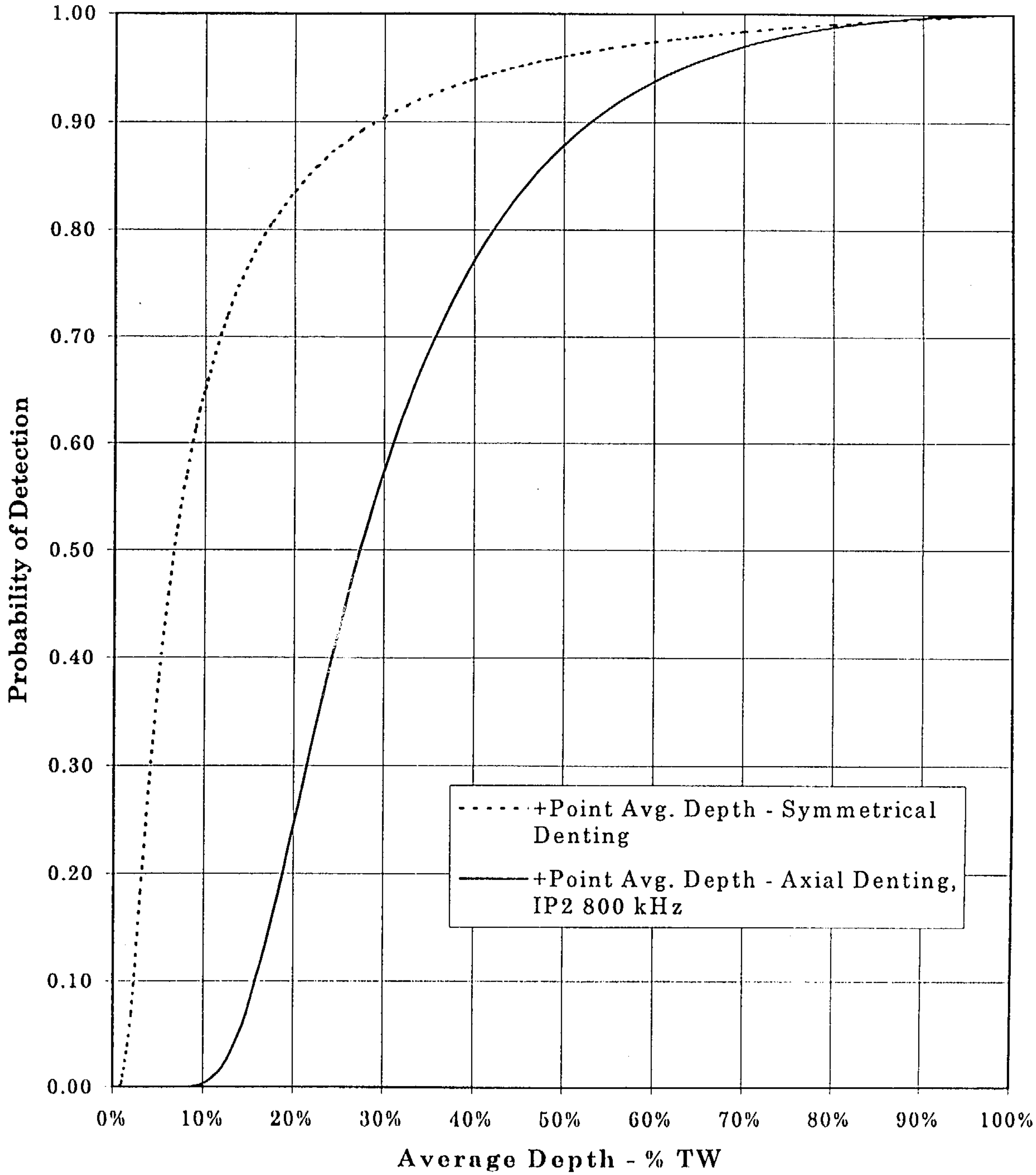
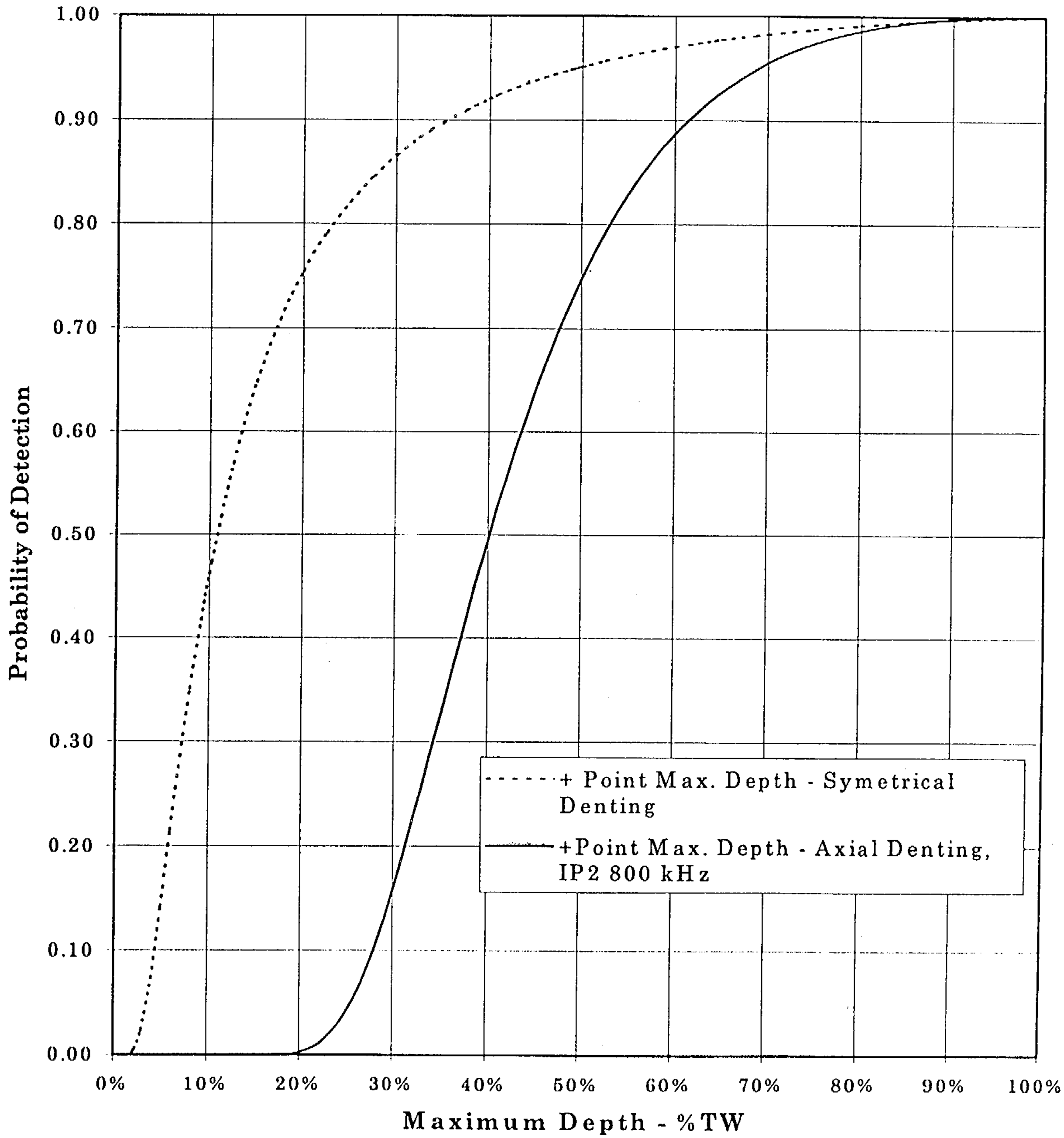


Figure 5-2. Comparisons of + Point Maximum Depth PODs



Low Row U-Bends

- Growth Rate
  - Establish operating period such that the largest "undetected" flaw will not grow to be structurally significant during next operating period
  - Combination of POD and growth rate determines operating period
- Determination of growth rate
  - Derive estimate from 9 indications in 5 tubes
    - Comparison of 400 KHz data
      - Must compare like data between 1997 & 2000
      - +Point sizing techniques based on 300-400 kHz techniques and uncertainties (std. dev. increased by 25%) from PWSCC at dented TSP intersections as used for PWSCC ARC (WCAP-15128)
      - 400 KHz sizing data more consistent with in-situ test results
    - Data adjusted for small sample size
  - Comparison to historical data from dented TSPs and other industry data

Table 5-1. Indian Point-2 U-Bend Axial PWSCC Growth Rates (400 kHz Data)

			+Point - 2000 inspection Midrange Coil						+Point - 1997 Inspection Midrange Coil						Growth per EFPY 2000-1997 = 1.48 EFPY					
SG	Tube	Crack No.	Max. Volts	Max. Depth	Avg. Depth	Length	Burst Avg. Depth	Burst Length	Max. Volts	Max. Depth	Avg. Depth	Length	Burst Avg. Depth	Burst Length	Max. Volts	Max. Depth	Avg. Depth	Length	Burst Avg. Depth	Burst Length
	R2C5 Note 1	1		100			90		2.27 2.31	92 92	63.2 70.6	2.43 2.43	73.8 80.2	1.26 1.87		8.00	10.95		10.95	
4	R2C69	1	2.71	74	55.2	0.91	58.3	0.79	1.33	84	57.0	0.9	62.8	0.7	0.93	-6.76	-1.22	0.01	-3.04	0.06
			1.03	74	44.5	0.11	44.5	0.11	0.54	50	31.5	0.25	39.0	0.16	0.33	16.22	8.78	-0.09	3.72	-0.03
			0.94	54	38.2	0.23	42.0	0.20	0.61	50	33.8	0.16	37.7	0.13	0.22	2.70	2.97	0.05	2.91	0.05
4	R2C72	11	3.17	82	59.8	0.54	66.6	0.44	1.3	79	61.8	0.39	66.4	0.35	1.26	2.03	-1.35	0.10	0.14	0.06
4	R2C71	1	2.43	96	64.0	0.57	69.0	0.48	1.87	87	57.5	0.68	63.1	0.57	0.38	6.08	4.39	-0.07	3.99	-0.06
1	R2C87	1	1.68	55	42.8	0.30	48.0	0.25	1.05	63	40.8	0.15	40.8	0.15	0.43	-5.41	1.35	0.10	4.86	0.07
		1	2.25	61	43.6	0.29	48.0	0.25	0.76	53	36.4	0.19	40.8	0.16	1.01	5.41	4.86	0.07	4.86	0.06
		1	2.28	53	41.6	0.35	44.3	0.31	0.95	63	36.5	0.27	45.2	0.19	0.90	-6.76	3.45	0.05	-0.61	0.08
														Avg.	0.68	2.39	3.80	0.03	3.09	0.04
														Max.	1.26	16.22	10.9	0.10	10.9	0.08

Note 1. R2C5 not sizeable in 2000 by NDE after crack opening resulting in leakage. Maximum depth in 2000 is assumed to be throughwall. For ligament tearing, which is the expected cause for opening the R2C5 crack, the average depth to tear the ligament of a 2.2 to 2.4 inch flaw would be about 90%. The 90% depth value is applied with the smaller burst effective depth estimate for R2C5 in 1997 to assign a conservative growth value to R2C5.

Figure 5-3. Indian Point-2 U-bend Average Depth Growth Data and Distribution

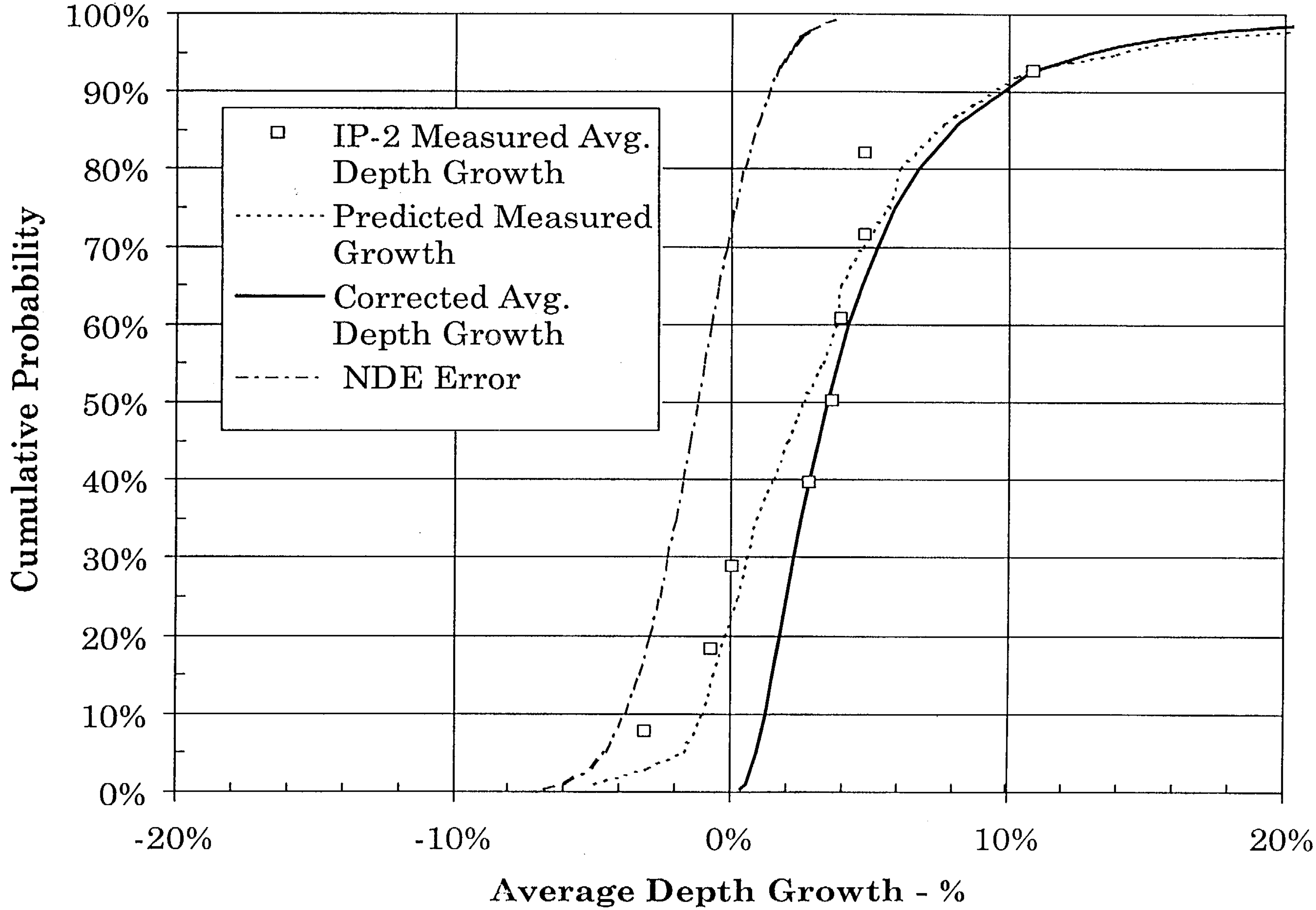


Figure 5-5.
Comparison of Indian Point-2 and Dented TSP PWSCC Growth at 590°F

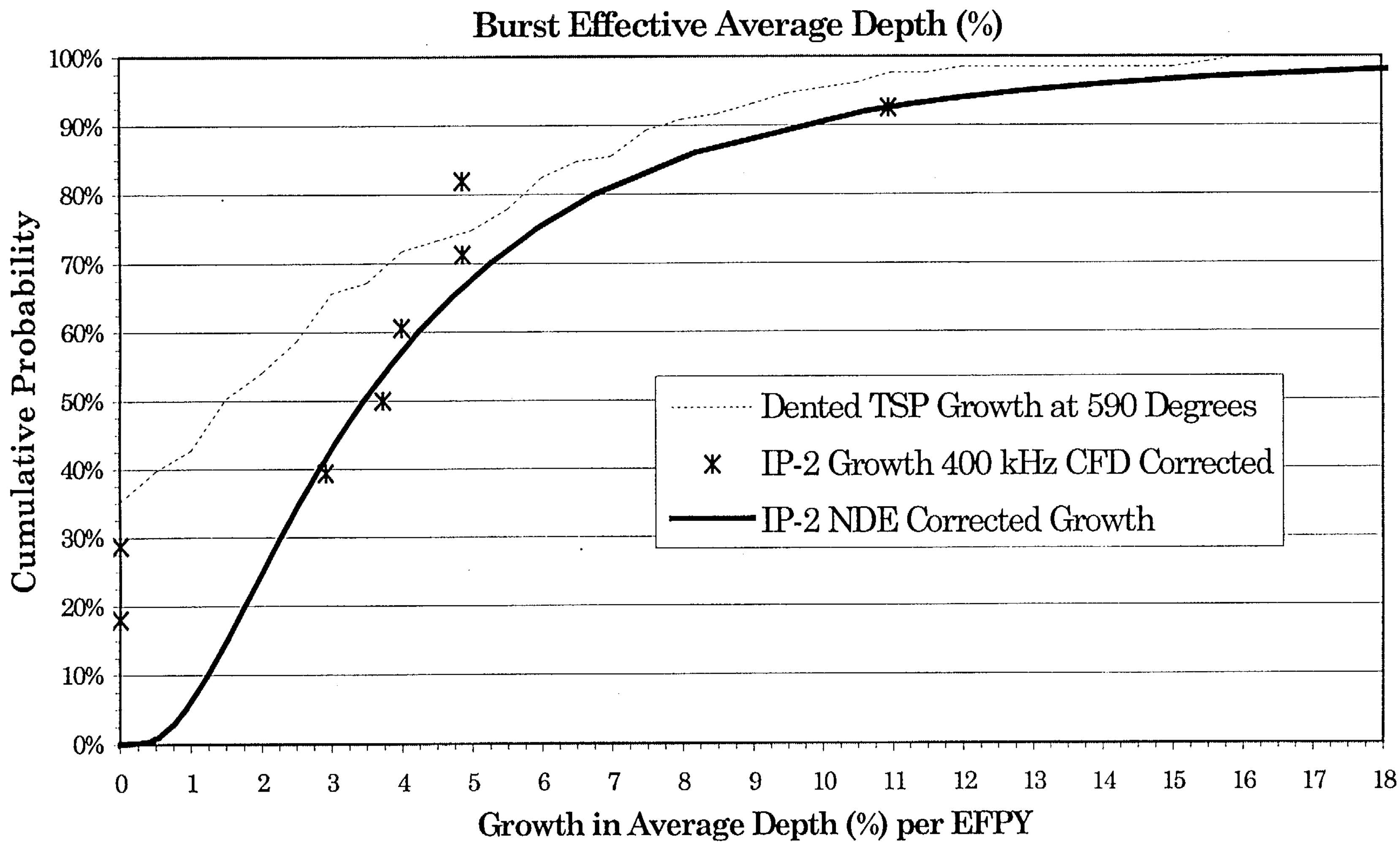


Figure 5-8. Comparison of Indian Point-2 Best Estimate "True" Growth with Distribution for Operational Assessment

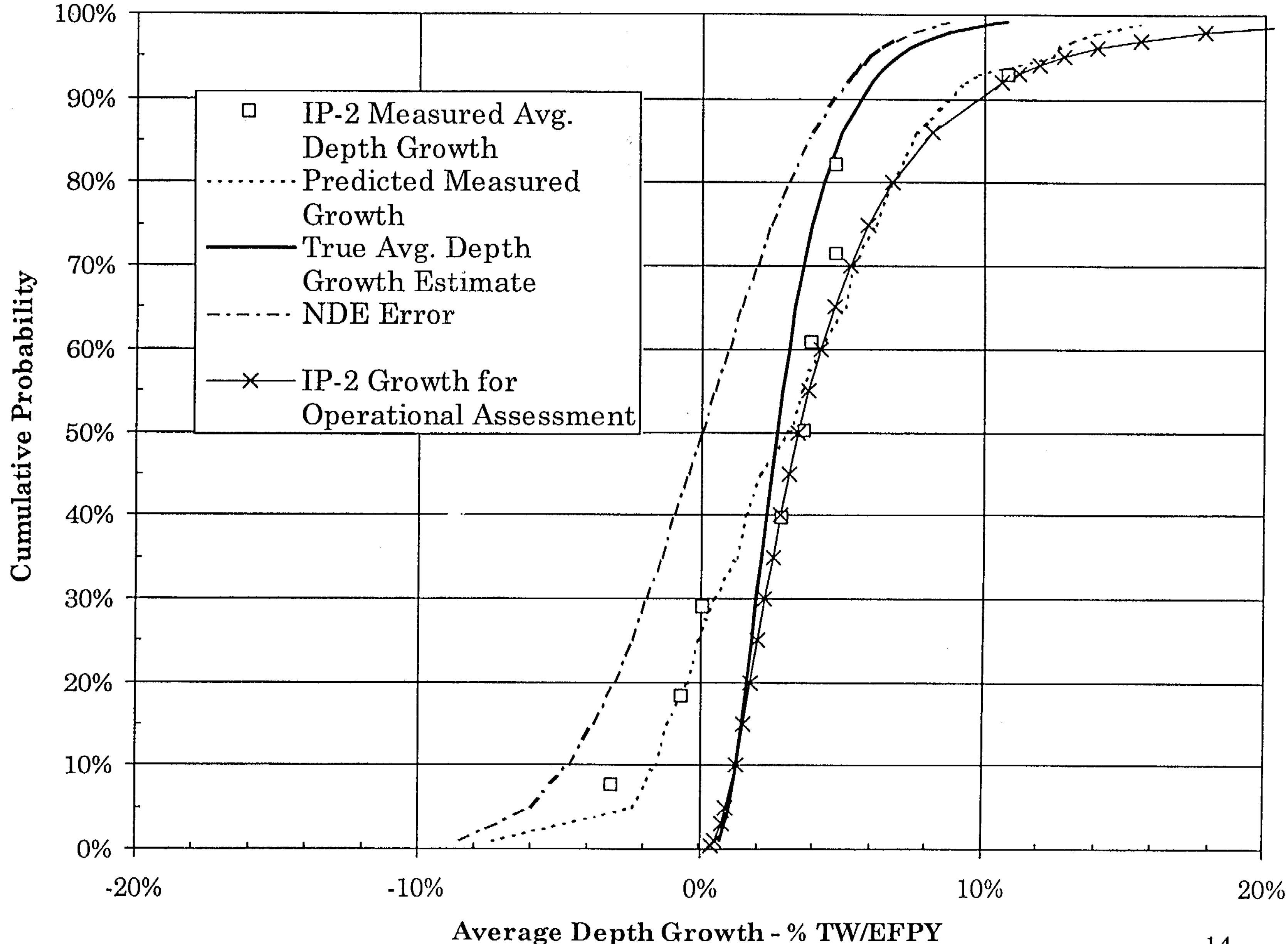


Figure 3-4. Indian Point-2: Comparison of SG 4 R2C69 400 and 800 kHz Depth Profiles

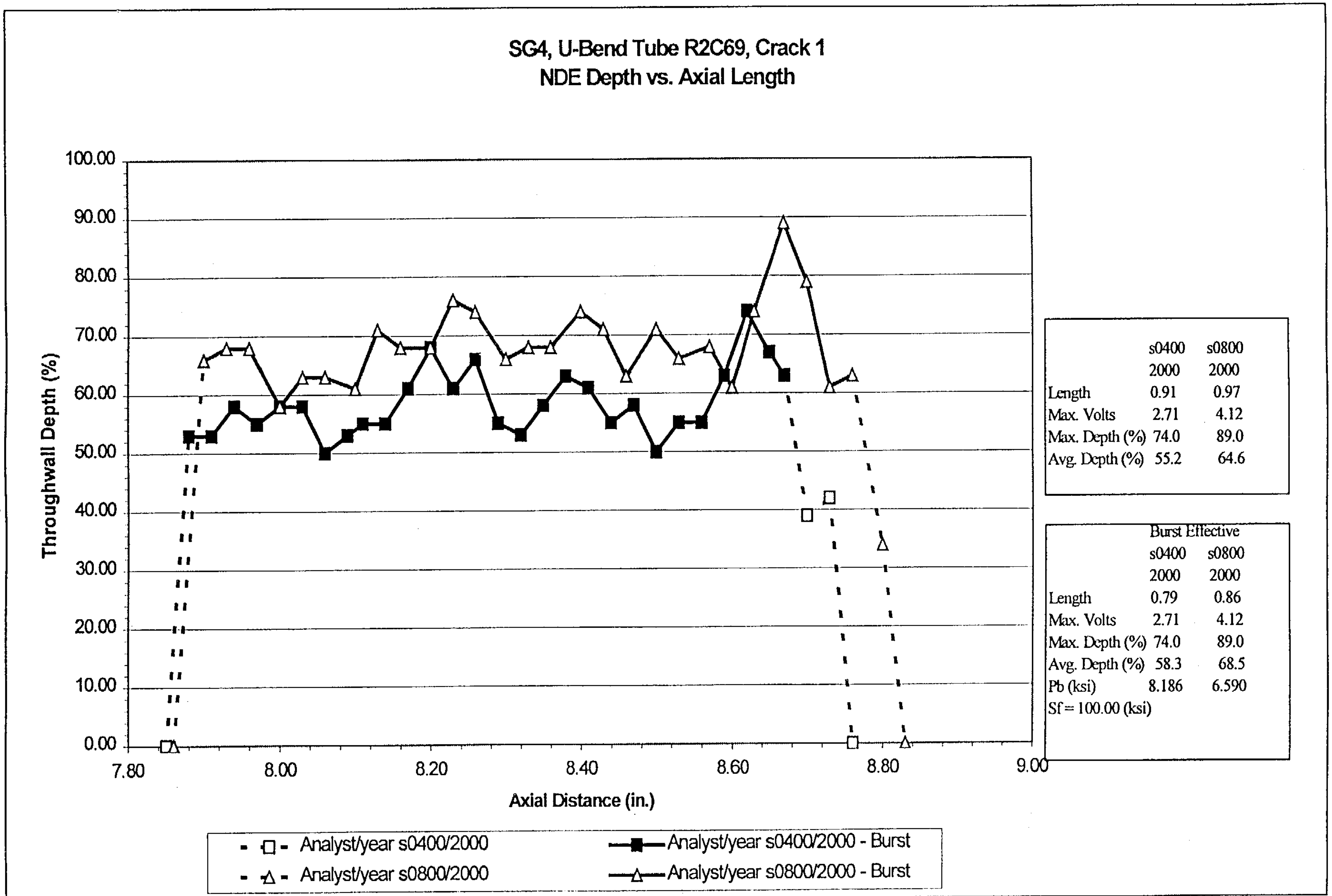
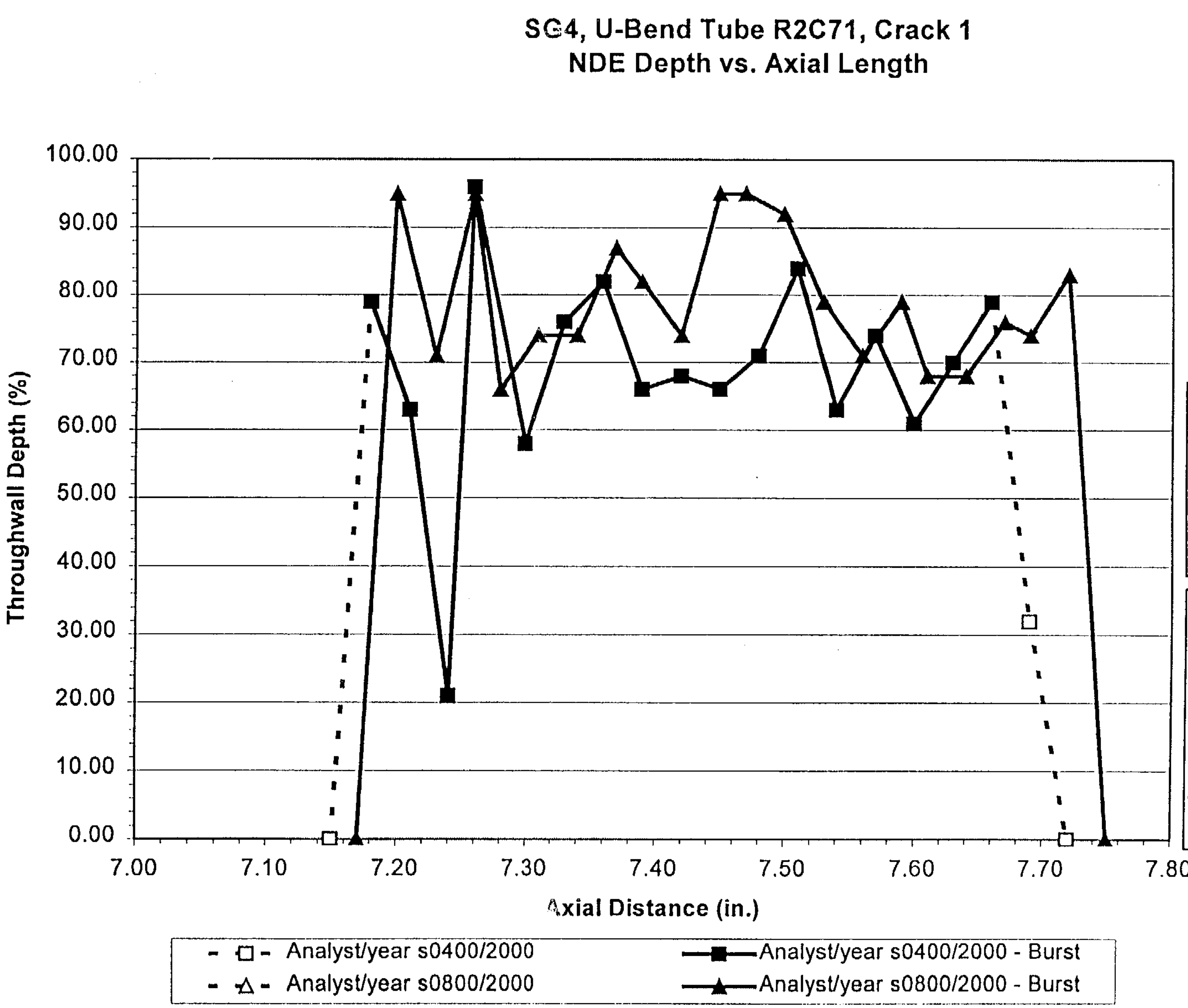


Figure 3-5. Indian Point-2: Comparison of SG 4 R2C71 400 and 800 kHz Depth Profiles



	s0400 2000	s0800 2000
Length	0.57	0.58
Max. Volts	2.43	1.77
Max. Depth (%)	96.0	95.0
Avg. Depth (%)	64.0	75.6

Burst Effective	s0400 2000	s0800 2000
Length	0.48	0.58
Max. Volts	2.43	1.77
Max. Depth (%)	96.0	95.0
Avg. Depth (%)	69.0	75.6
Pb (ksi)	7.300	6.078
Sf = 100.00 (ksi)		

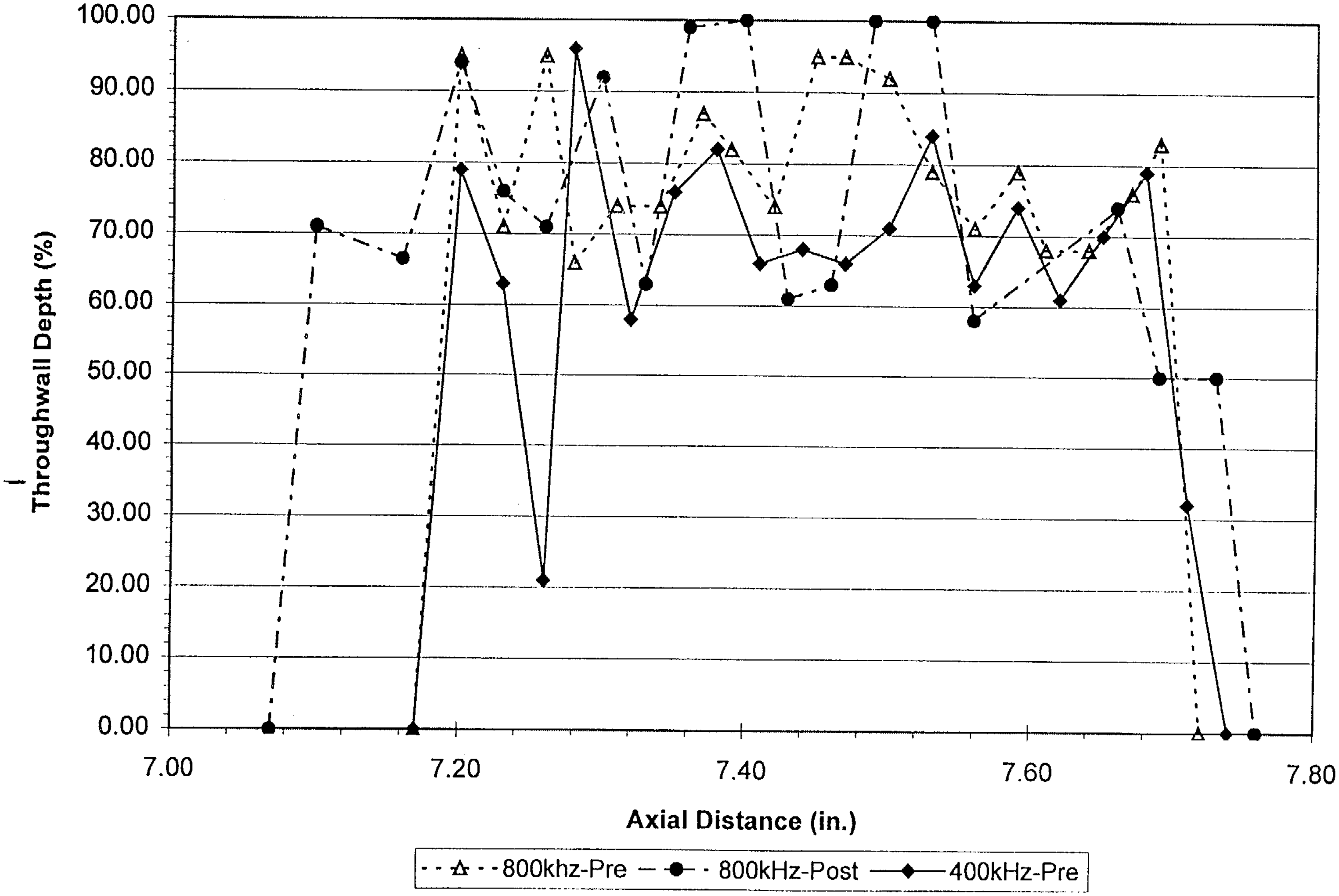
Low Row U-Bends

- Ovality has little or beneficial effect, particularly row 3 compared to row 2
- Effect of leg displacement on operating stresses
  - Site measurements of displacements input to plate model
  - Plate model determines displacements by row
  - Determine stresses at apex due to plate displacement
  - Row 3 stresses less than row 2, etc.
  - Stress effects from leg displacement present in the past and not changing significantly with time
- Industry Experience
  - Industry data suggests row 3 is not a concern
  - Row 2 is now plugged at Indian Point-2

Benchmarking of Analysis Methods and Data

- Benchmarking Analyses performed to support adequacy of data and methods
  - Demonstrate the methods provide conservative predictions of structural and leakage integrity at specified confidence
  - Provide integral test of NDE sizing technique, NDE uncertainty, and material properties
- Compare 1997 projections to 2000 data
  - R2C5 leakage
  - In situ tests
  - Comparisons with burst pressures and leakage from year 2000 profiles
- Compare analyses using year 2000 profiles with in situ test for burst pressure and leakage thresholds

Figure 8-3
SG 4, U-Bend Tube R2C71, Crack Depth Profiles
Comparison of Pre and Post In Situ
NDE Depth vs. Axial Length



Low Row U-Bend In Situ Tests

- In-Situ testing--total 10 tubes
  - Tested to as high a pressure as possible to demonstrate margin (5500 psi.)
  - Test results used to benchmark tube integrity analysis methods
- Tested all indications
  - Test results met NEI-97-06 Criteria ($3\Delta P_{NO}$ = 4617 psi, hot)
- R2C71 in situ test
  - Test limited to 4206 psi (hot) by progressively increasing leakage
  - NDE and post peak pressure leak rates show short TW (<0.39")
  - Indication has not reached full ligament tearing of deep section (≈0.5") and burst pressure would be about 300 psi above tearing
- Tested 3 NDD tubes to >$3\Delta P_{NO}$ (5173psi)
  - 2 Row 2 tubes, 1 Row 3 tube
  - No leaks & no indications in post test NDE

Low Row U-Bend Operational Assessment

- Row 3 is now the limiting row
  - No Indian Point-2 indications found in row 3
  - Industry experience for Model 44/51 is no cracking in row 3
    - **Higher operating temperature plants with no row 3 heat treated tubes**
- Operating period very conservatively calculated by assuming indications found in row 2 were found in row 3
  - High frequency probe POD
  - POD correction per NRC GL 95-05 applied to account for potential undetected indications
- Analysis methods employed
  - Reference analysis is single cycle profile analysis as applied for PWSCC ARC at dented TSP intersections (WCAP-15128 Rev. 2)
  - Multi-cycle analysis methodology as independent check and guide to crack initiation history

Dented TSP Intersections

- Cecco results
- Qualification accepted by NRC
- No Cecco indications confirmed as flaws by +Point inspection

Area Above Sludge Pile

- Inspected with CECCO/bobbin probe
- 20% of tubes in each steam generator inspected with +Pt. to just below 1st TSP
  - Confirmed CECCO overcalls in this region
- 23 tubes in one steam generator inspected through 1st TSP with UTEC
  - UT inspection lessened influence of copper
  - 1 tube could not be inspected through 1st TSP
  - 2 tubes inspected after in-situ test
  - Confirmed CECCO calls
- 5 tubes in-situ tested in this region
  - All met NEI-97-06 structural and leakage criteria

Sludge Pile

- Detect with Cecco/confirm with + Point
  - Confirmation rate lower than qualification data
- Inspected 100% of hot leg tubes with + Point from TEH to 24" above top of tubesheet
  - Found a total of 6 small indications not found by CECCO
- UTEC inspection of 23 tubes in 1 steam generator
  - Confirmed CECCO calls
- 31 tubes in-situ tested in this region
  - All met NEI-97-06 structural and leakage criteria
  - R34C51: peak test pressure = 4985 psi = 4591 psi hot
  - Burst margin = 4591/1539(ΔP_{NO}) = 2.98 = 3ΔP_{NO} burst margin

Cold Leg Program

- Initial inspection with CECCO/bobbin probe
- Inspected 20% of 1 steam generator with +Pt from TEC to just below 1st TSP
- Inspected 20% of each of the other 3 steam generators with +Pt from TEC to 24" above top of tubesheet
- No crack-like indications found
- S/G 23 & 24 expanded to 40% due to pit indications
  - All pit indications plugged

Tubesheet Region

- Within tubesheet, burst prevented by tubesheet constraint
- Cecco/+ Point correlation similar to qualification
- Less of an influence of copper
- However---one indication within crevice did grow larger (crack opening and probable tearing) after an in situ test to 5000 psi for another indication
- Tubesheet region included in 100% H/L +PT. inspection and 20% C/L inspection
- In-situ tested 5 indications
  - All met NEI-97-06 leakage criteria

Other Tube Degradation Considerations

- Plugging all pits
- AVB wear is well understood
  - Growth rate consistent with industry experience
- Support plate condition
  - Analysis shows plates maintain tube integrity support function
- Wear due to loose parts also being evaluated

Indian Point Unit 2

U-Bend PWSCC Susceptibility Investigation

Status Report
May 3, 2000

Altran Corporation
451 D Street
Boston, MA 02210
(617) 204-1000
Fax (617) 204-1011

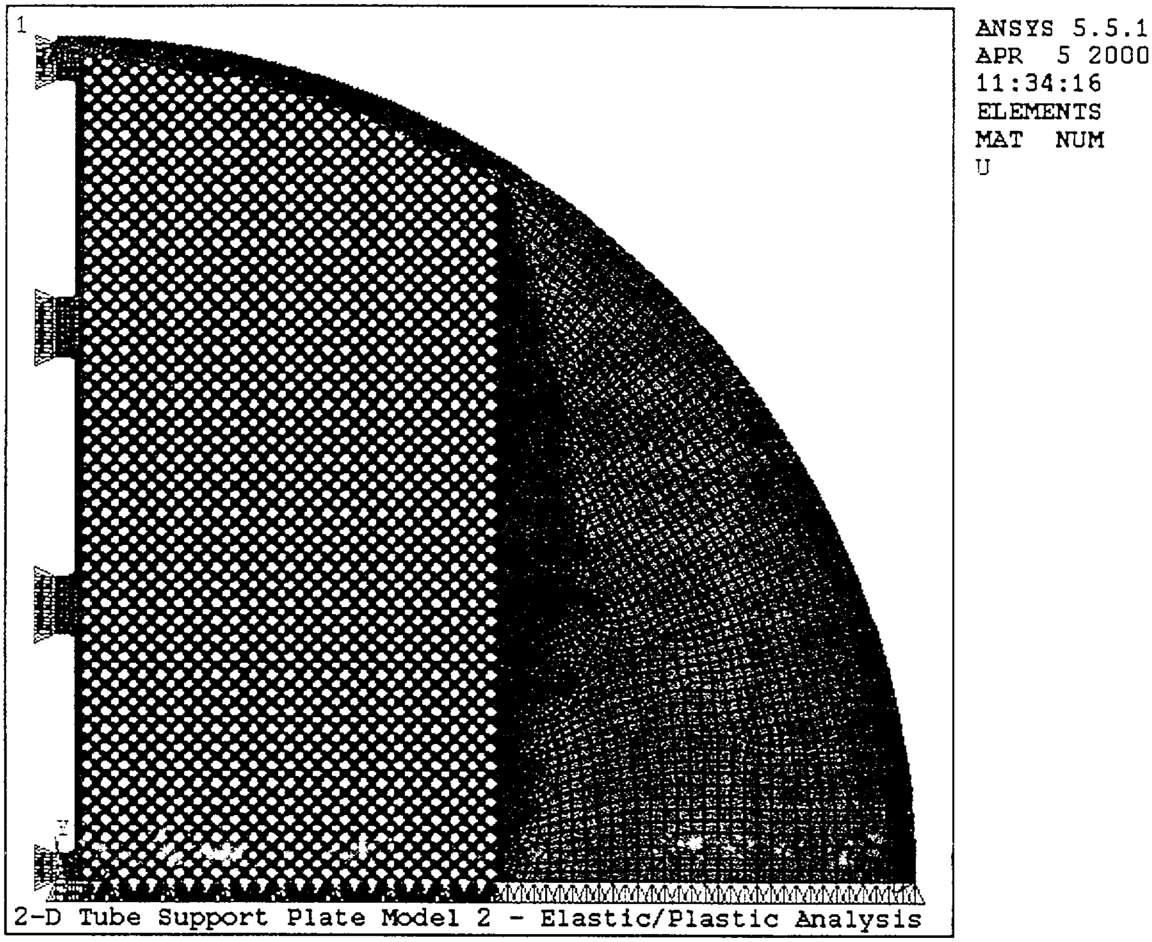
altran

U-Bend Tube Investigations

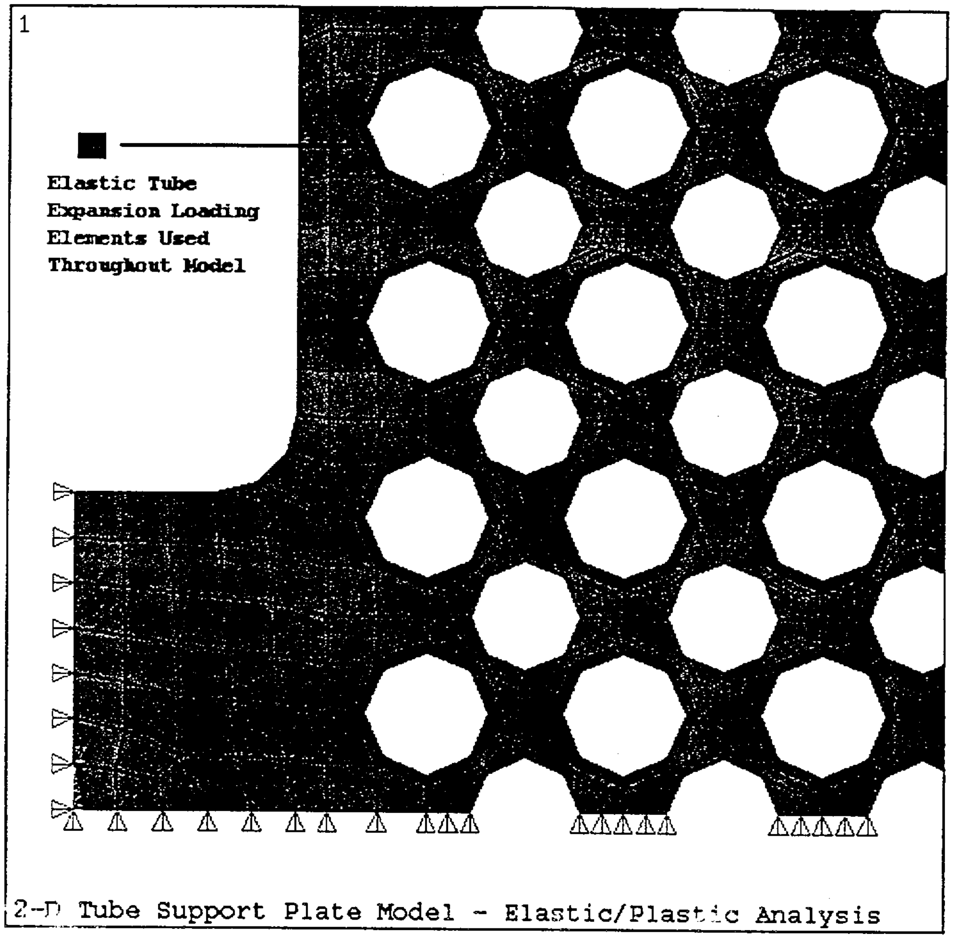
- Purpose: Determine Relative Susceptibility of Small Radius U-Bends to PWSCC
- Approach:
  - Determine tube displacements for specified amount of hour-glassing
  - Determine stresses in U-bends due to TSP deformation and other operating conditions
  - Determine the residual stresses
  - Assess time to initiate cracking due to PWSCC
  - Estimate life expectancy of Row 3 tubes relative to Row 2 tubes
- Analyses and tests performed
  - Tests performed on Row 2 and Row 3 tubes provided by EPRI
  - Stress Analyses performed for Row 2 and higher rows of tubes

Tube Support Plate Motion

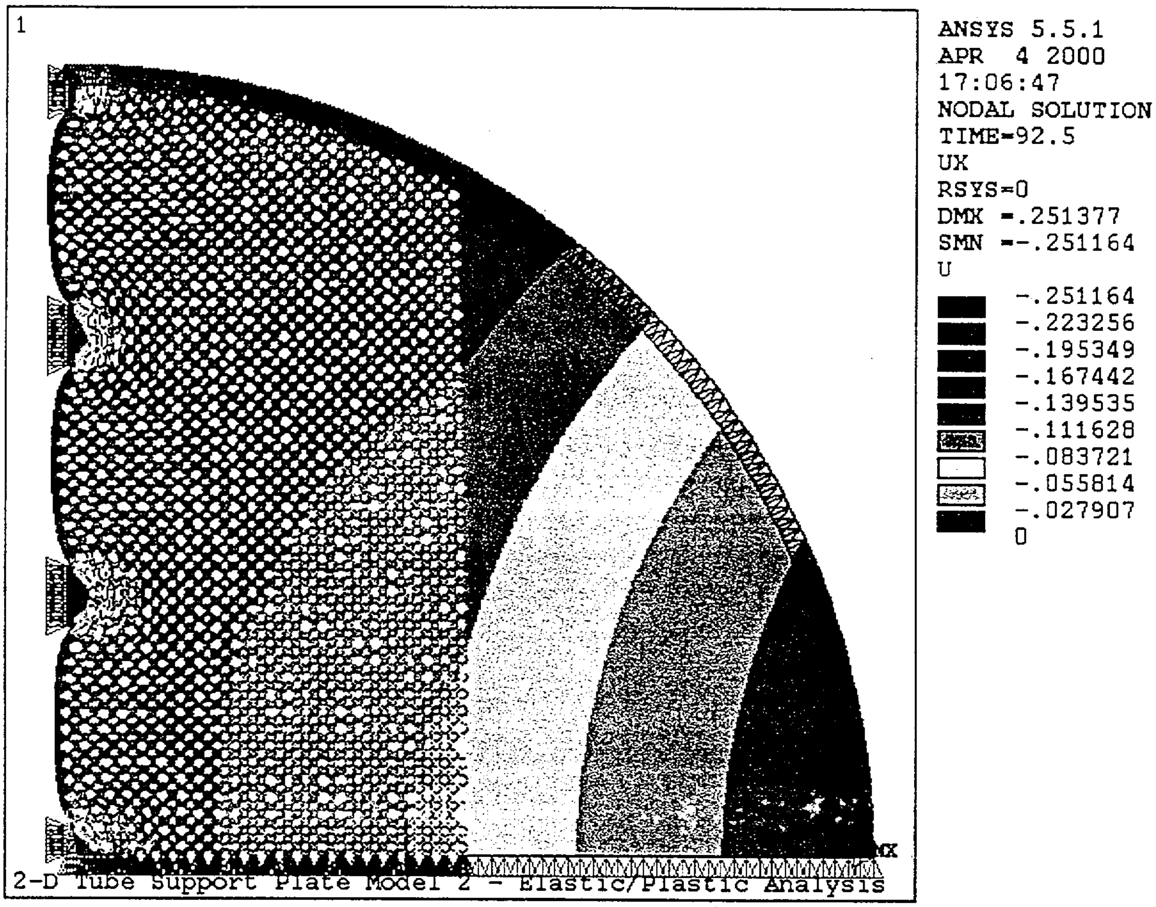
- Analysis Objective: Quantify the movement of Row 2 and 3 tubes for a given amount of hour-glassing
- Analysis Assumptions
  - 3-D Elastic/Plastic Finite Element Model
  - Model consisted of a Quarter Plate
  - Applied corrosion packing loads inside tube holes simulated by thermally expanding elements inside the tube hole
- Corrosion packing load causes in-plane compression in the TSP and hour-glassing at the flow slot
- Analyses performed represent the measured total hour-glassing at flow slot of 476 mils.
  - Row 2 tube displaces approx. 63% to 97% of the flow slot deformation
  - Row 3 tubes displace approx. 63% to 92% of the flow slot deformation



Tube Support Plate
Finite Element Model



Tube Support Plate
Flow Slot/Hole Region Finite Element Plot



Full Quarter TSP Model – Shown with Exaggerated Displacements at Flow Slots

X-Displ. Normalized to R1C7 of Outer Slot				
Column No.	Row 1	Row 2	Row 3	Row 4
3	**67**	**63**	**63**	**63**
4	**85**	**81**	**76**	**73**
5	**93**	**90**	**85**	**80**
6	**98**	**94**	**90**	**85**
7	**100**	**97**	**92**	**87**
8	**100**	**97**	**92**	**87**
9	**99**	**96**	**91**	**86**
10	**96**	**93**	**88**	**83**
11	**91**	**88**	**83**	**78**
12	**83**	**79**	**74**	**71**
13	**67**	**63**	**62**	**63**

X-Displacement for the Tubes at Outer Slot from the Quarter Model; (Values Normalized In Percentage to the Maximum Displacement of Tube R1C7)

U-Bend Tube Investigations

- Stress Analysis Objectives: Quantify Row 2 and 3 tube stresses due to TSP #6 hour glassing
- 3-D Elastic/Plastic Finite Element Model
- Effects include:
  - Temperature and Pressure
  - 0.003" of tube wall thinning and thickening (all rows the same)
  - Residual Stresses (determined from testing)
  - Imposed U-bend leg displacement due to hour glassing
  - Strain hardening in U-bend increases yield strength by approximately 50%
- Analysis performed with 0.238" one side hour glassing
- Range of yield strength data from IP2 Generator tube CMTR, adjusted for strain hardening and operating temperatures.

As-Bent Ovality and Thinning

- Row 2 and 3 U-Tubes were located in the EPRI archives.
- Similar geometry to IP2 tubes – 7/8" tubes with 0.050" walls
- Performed material composition and mechanical property testing
- As-received tubes used for ovality measurements, wall thinning measurements, and measurement of yield stress in bend versus straight run

As-Received U-Tube Samples

Sample Number	Tube Row	Wall Thinning (in.)	Percent Ovality
00603-1	2	0.003	4.99
00603-2	2	0.003	5.48
00603-6	2	0.004	2.28
00603-3	3	0.004	5.30
00603-4	3	0.004	5.65
00603-5	3	0.002	4.99

Ovality Investigation

- Purpose: Investigate effect of as-manufactured ovality and U-tube leg displacements on apex stresses.

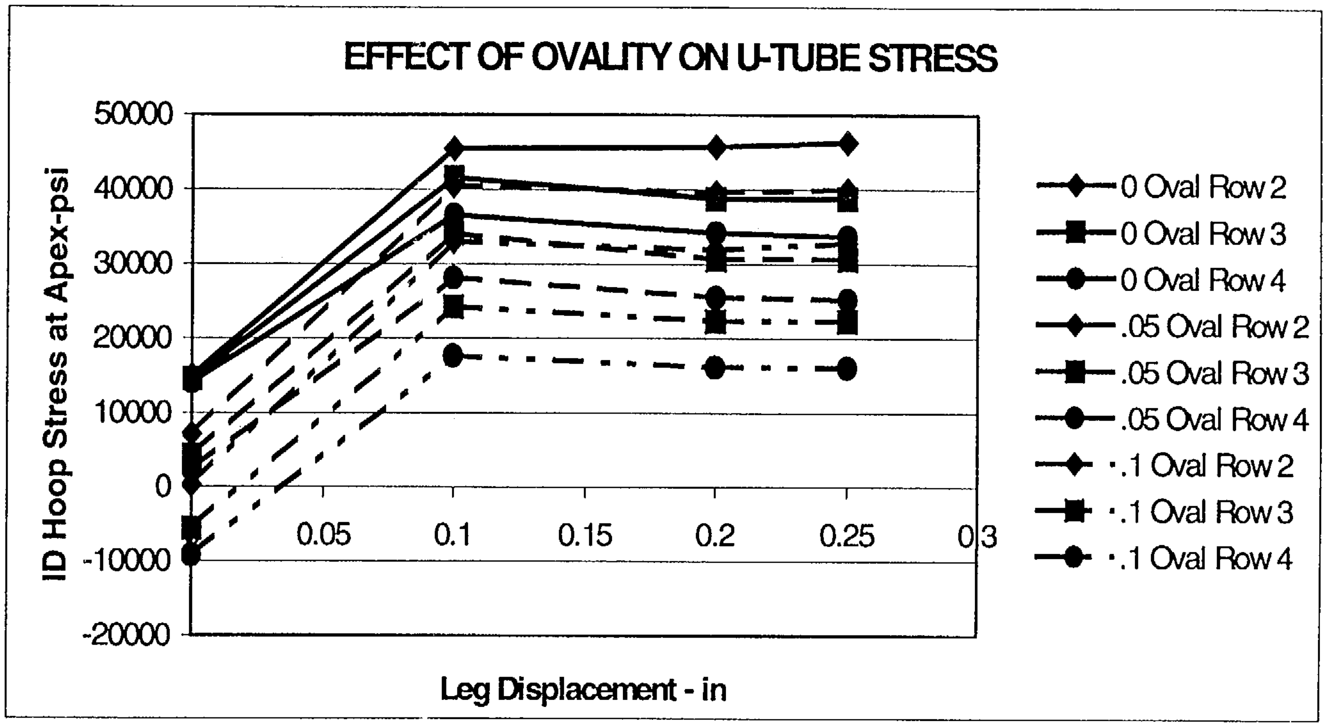
- Elastic-Plastic finite element model with ovality for Rows 2, 3 and 4

- Ovality: 0, 0.05, and 0.10

$$\text{Ovality} = \frac{(\text{Flank Dia.} - \text{Extrados/Intrados Dia.})}{\text{Straight Leg Dia.}}$$

- Lateral displacement of one leg: 0." 0.1", 0.2" and 0.25"

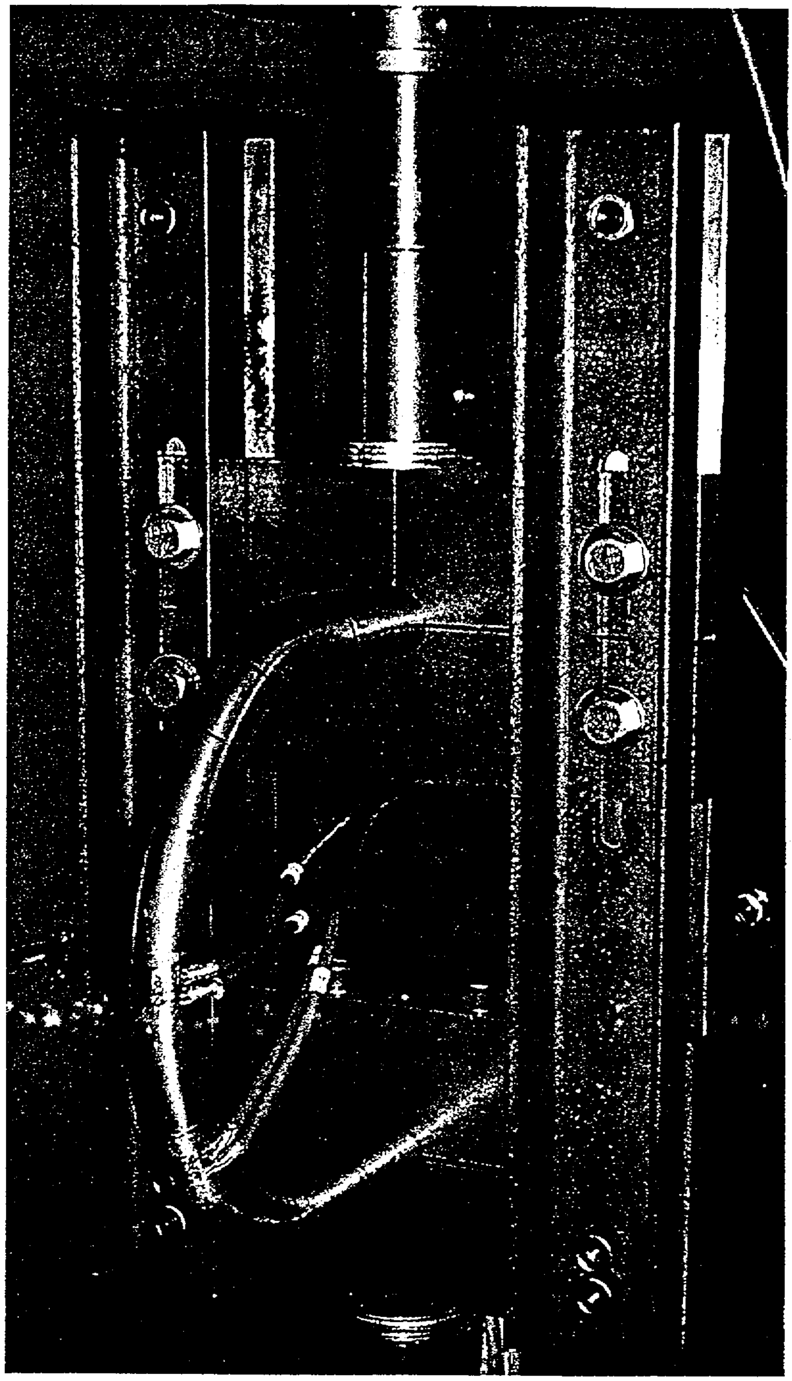
- A U-bend analysis model with a circular cross section will result in conservative ID hoop stress values at the apex.



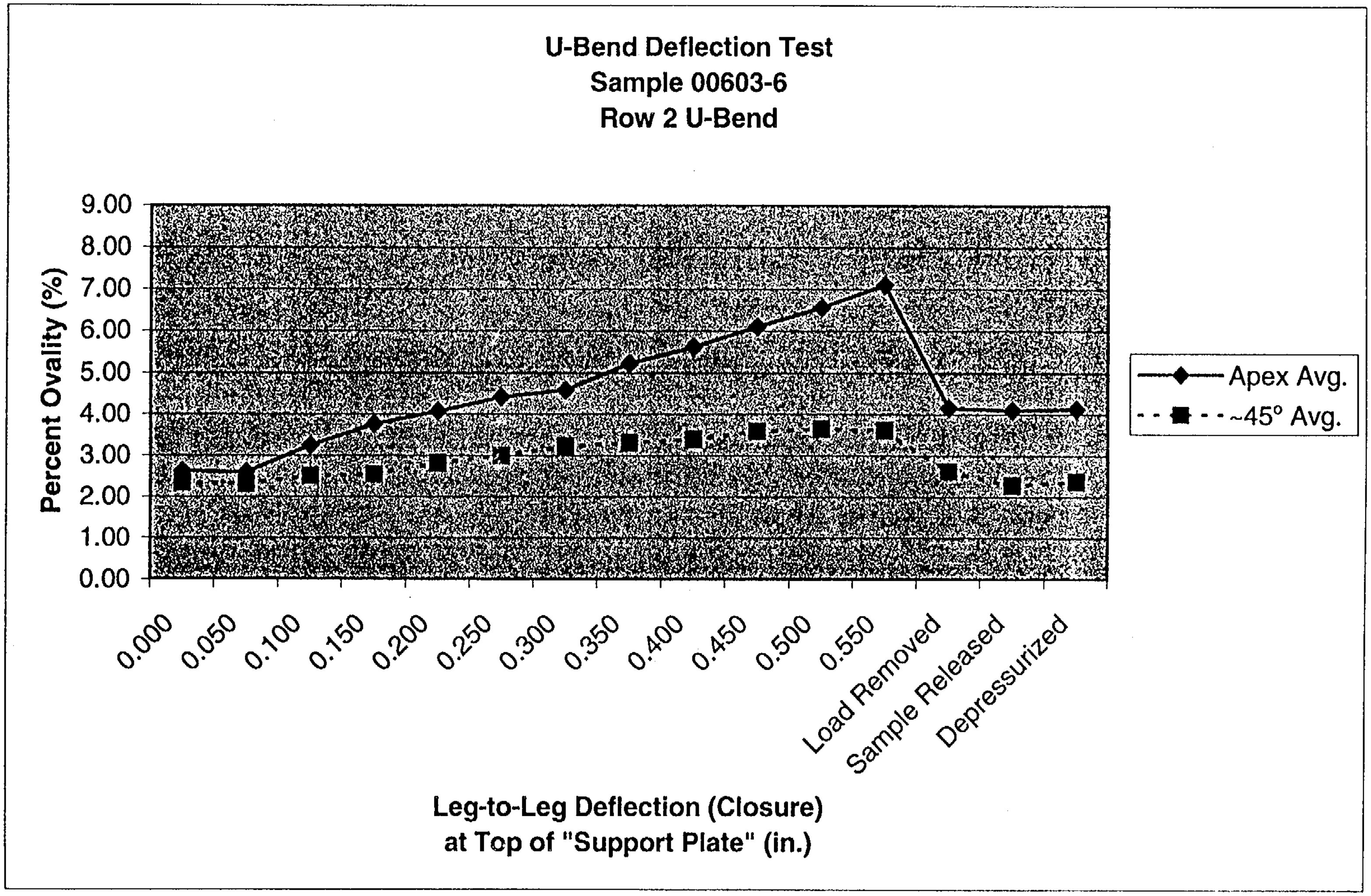
Comparison of ID Hoop Stress of the Extrados at the Apex (Analysis performed with yield stress of 40 ksi)

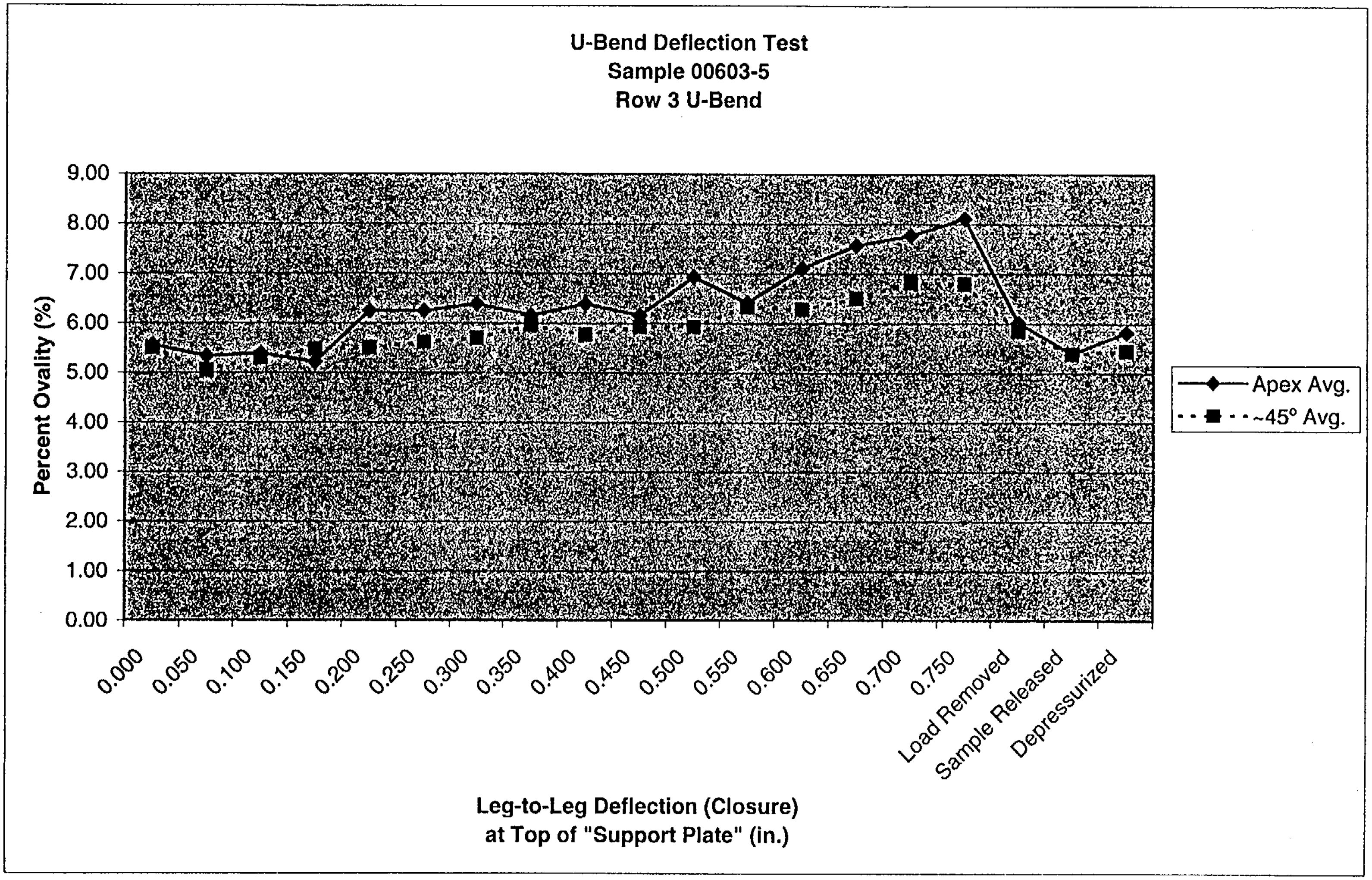
U-Bend Leg Displacement Test

- Fixture designed to apply boundary conditions that allow almost no rotation to simulate the support plate hour-glassing.
- Incremental displacement applied while internally pressurized
- Ovalization, Strain, and Displacement measured



Sample 00603-6 in Test Fixture Prior to Testing


U-Bend Deflection Test
Sample 00603-6
Row 2 U-Bend
Percent Ovality (%)
9.00
8.00
7.00
6.00
5.00
4.00
3.00
2.00
1.00
0.00
0.000
0.050
0.100
0.150
0.200
0.250
0.300
0.350
0.400
0.450
0.500
0.550
Load Removed
Sample Released
Depressurized
Apex Avg.
~45° Avg.
Leg-to-Leg Deflection (Closure)
at Top of "Support Plate" (in.)

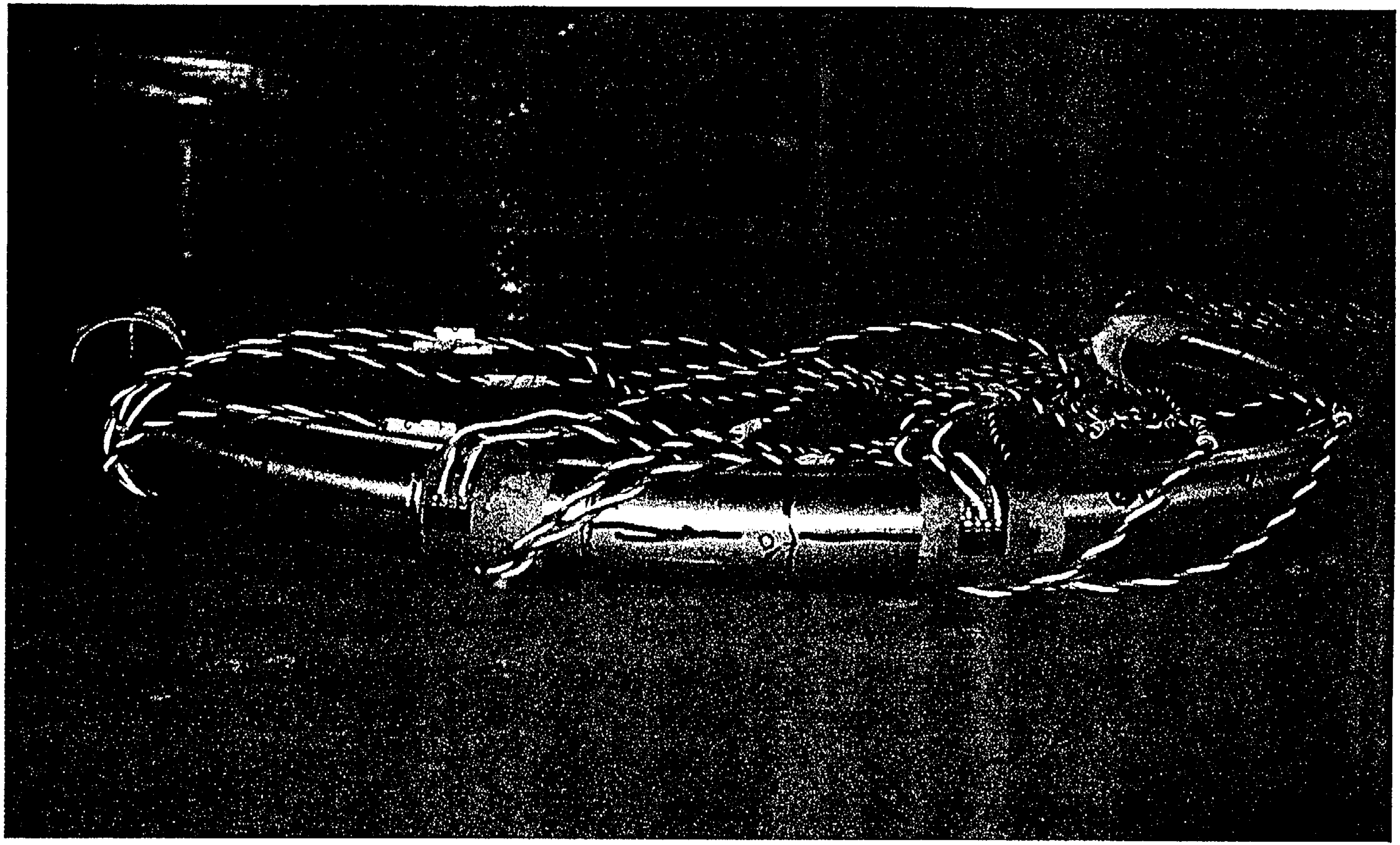

U-Bend Deflection Test
Sample 00603-5
Row 3 U-Bend
Percent Ovality (%)
9.00
8.00
7.00
6.00
5.00
4.00
3.00
2.00
1.00
0.00
0.000
0.050
0.100
0.150
0.200
0.250
0.300
0.350
0.400
0.450
0.500
0.550
0.600
0.650
0.700
0.750
Load Removed
Sample Released
Depressurized
Leg-to-Leg Deflection (Closure)
at Top of "Support Plate" (in.)
Apex Avg.
~45° Avg.

Residual Stresses

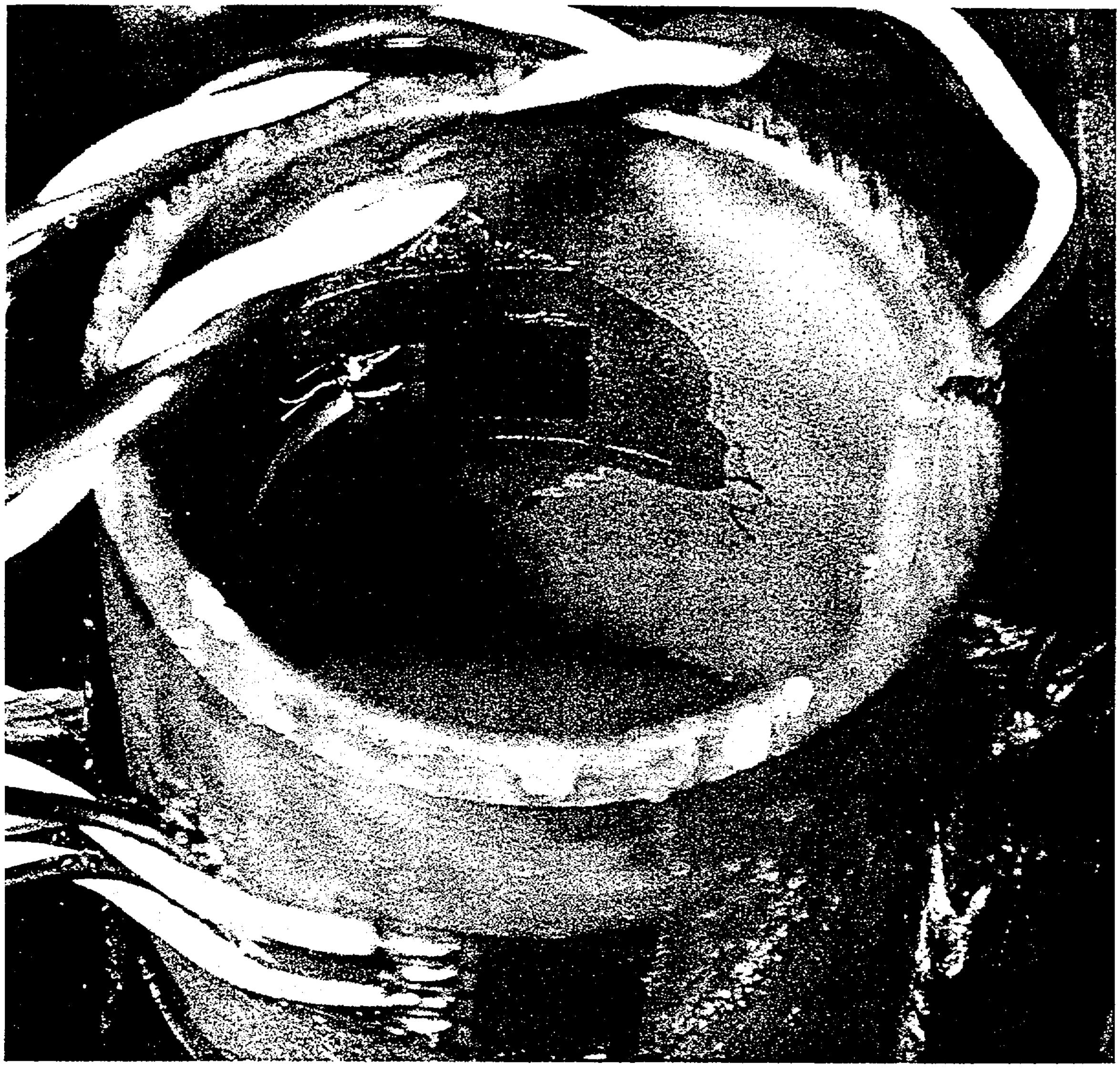
- Tests to determine the residual stress were performed.

- OD strain gages were applied to a Row 2 and a Row 3 tube and the "restraint" initially relieved by cutting the tubes circumferentially.

- ID gages then applied and the tubes were cut axially



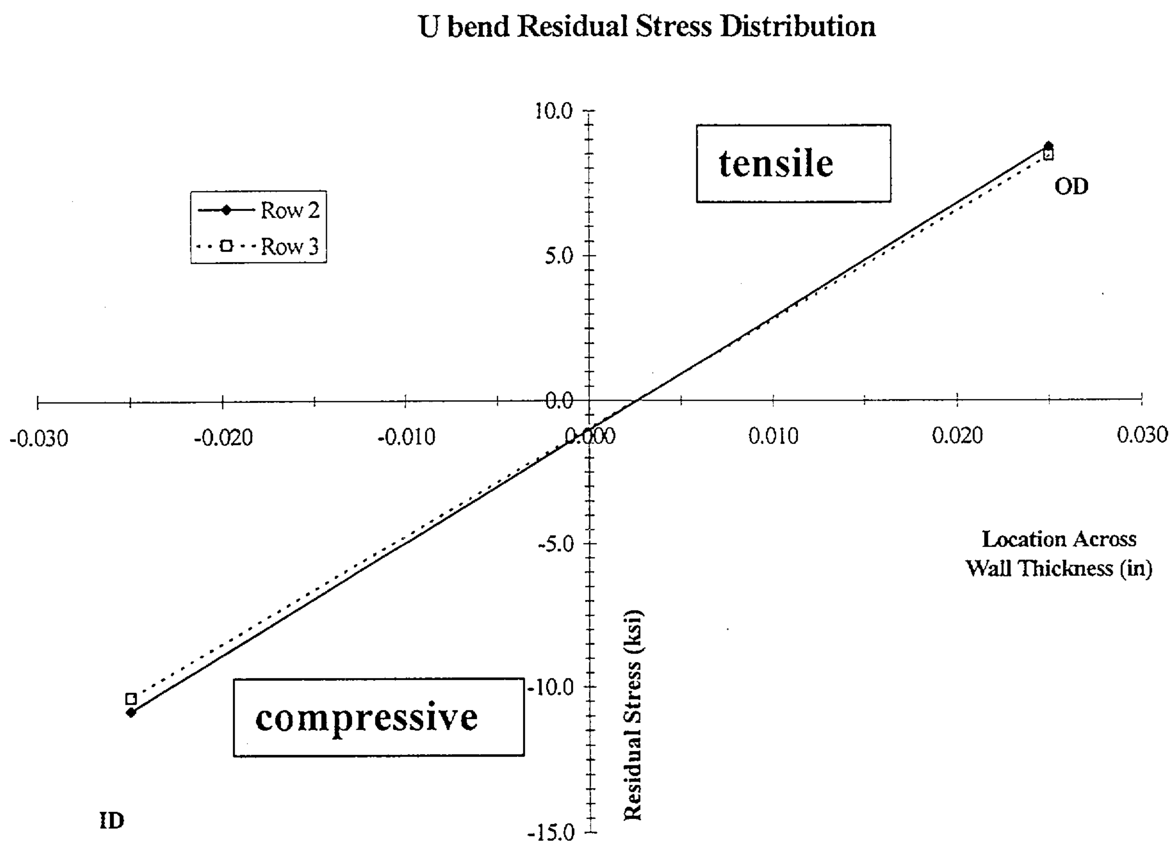
Strain Gages Attached to Extrados
For Residual Stress Measurement
Sample 00603-4



**Strain Gage Attached to I.D.
For Residual Stress Measurement
Sample 00603-4**

Average Released (Measured) Hoop Strain in Rows 2 and 3 Samples at the Apex

Sample	Average Total ID Hoop Strain (in/in)	Average Total OD Hoop Strain (in/in)
Row 2	0.00038	-0.00030
Row 3	0.00036	-0.00029



Equivalent U-Bend Elastic Residual Stress Distribution

Stress Strain Properties of the Tubes

- Row 2 and Row 3 tube yield stress will be higher than nominal due to strain hardening. Yield strength adjustment determined from elongation induced during bending (Row 2 strain hardening is greater than Row 3).
- Testing showed an increase in yield strength at the tube U-bends of approximately 50% due to strain hardening
- CMTR records from IP2 provided a range of yield strengths in the generator for the various rows
- The analysis model incorporated CMTR data adjusted for strain hardening and operating temperature

Finite Element Analysis

- Analysis was performed with ANSYS.
- Pressure, temperature, residual stress, and leg displacement included
- Yield strengths shown below were utilized in the analysis – these are corrected for temperature and strain hardening.

		Row 2	Row 3
0.2% Yield Stress (psi) – Mil. Test Cert. Values Adjusted for Design Temp.	**Lower Yield**	44,100	40,300
	Average Yield	61,000	58,800
	Higher Yield	86,000	82,700

Stress Analysis Results

Loading Condition	SUMMARY OF APEX HOOP STRESSES AT CENTER OF FLOW SLOT			
	Apex Hoop Stress (psi)			
	Row 2		Row 3	
	I.D.	O.D.	I.D.	O.D.
Equivalent Elastic Fabrication Residuals	-10,800	8,700	-10,400	8,400
Differential Pressure Plus Thermal Expansion	15,427	8,929	15,009	9,629
Total Loading – Lower Yield Strength	50,845	-35,169	40,743	-13,934
Total Loading - Average Yield Strength	68,496	-48,952	52,711	-30,172
Total Loading – Higher Yield Strength	92,378	-64,060	65,439	-37,204

Crack Initiation

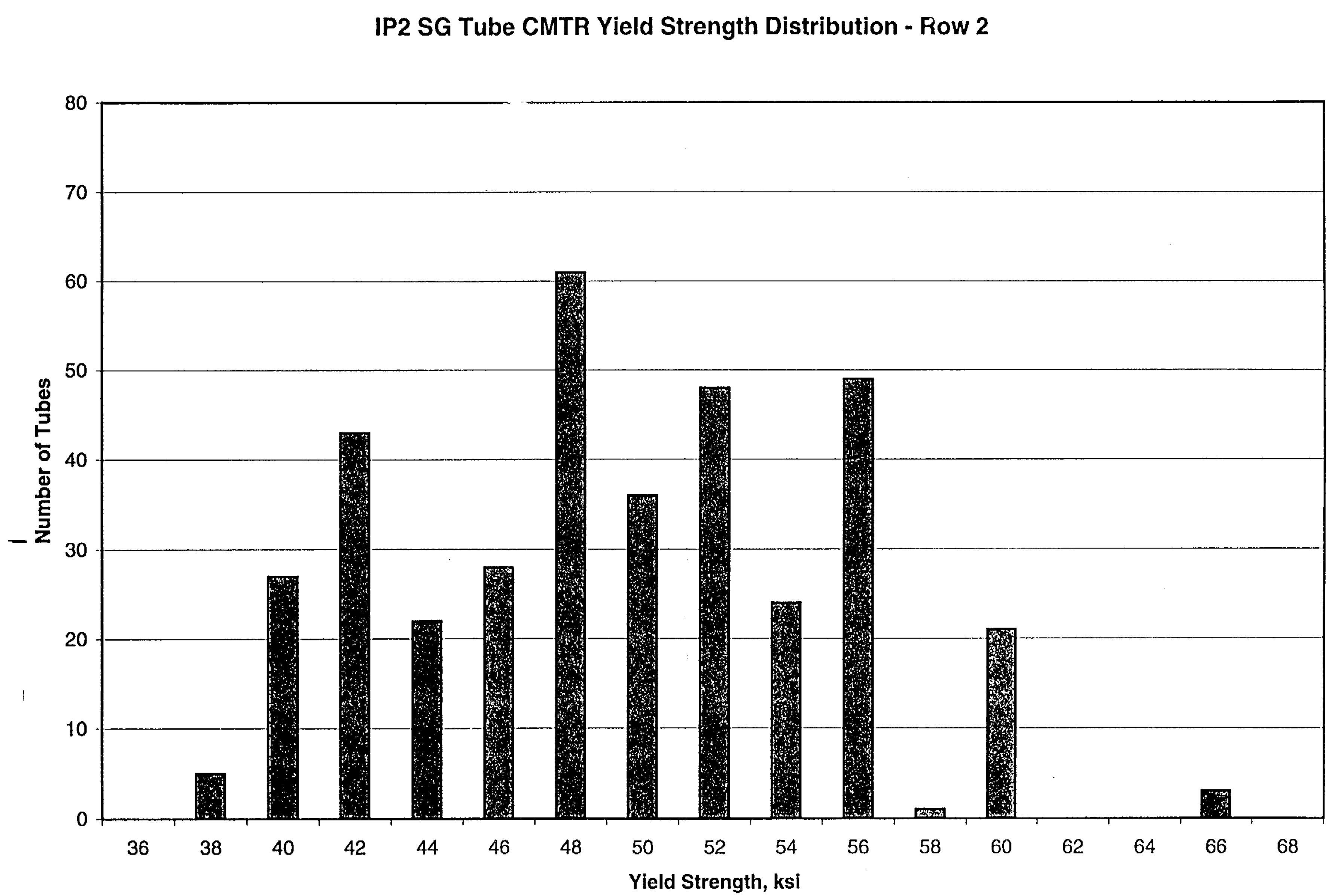
- Time to crack initiation is proportional to the applied stress raised to the 4th power.

- Time to crack initiation in Tube i will be proportionally longer that in Tube j by the following:

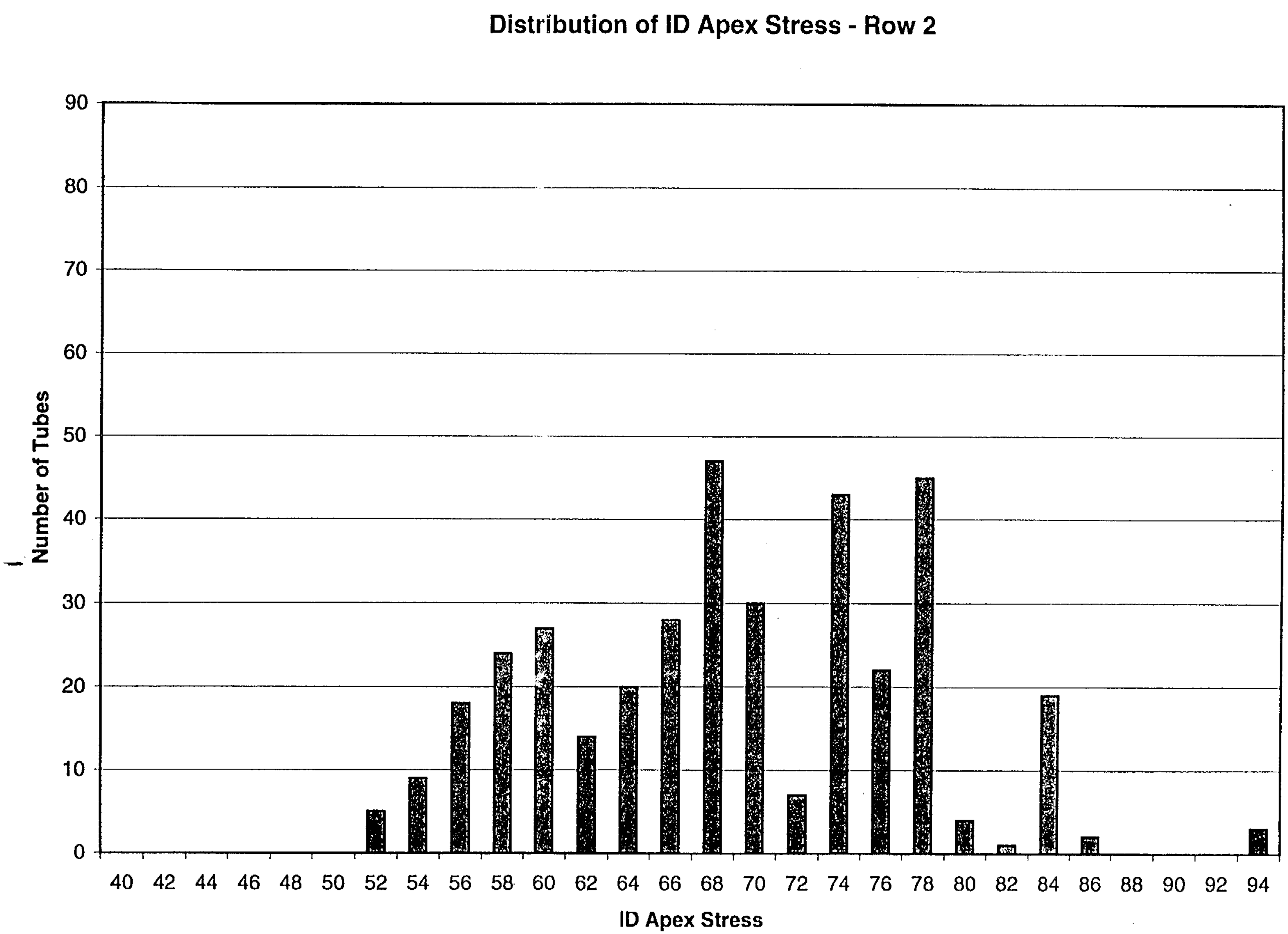
$$t_j = t_i \left[\frac{\sigma_i}{\sigma_j} \right]^4$$

- Cracks will initiate at different times in different Row 2 tubes

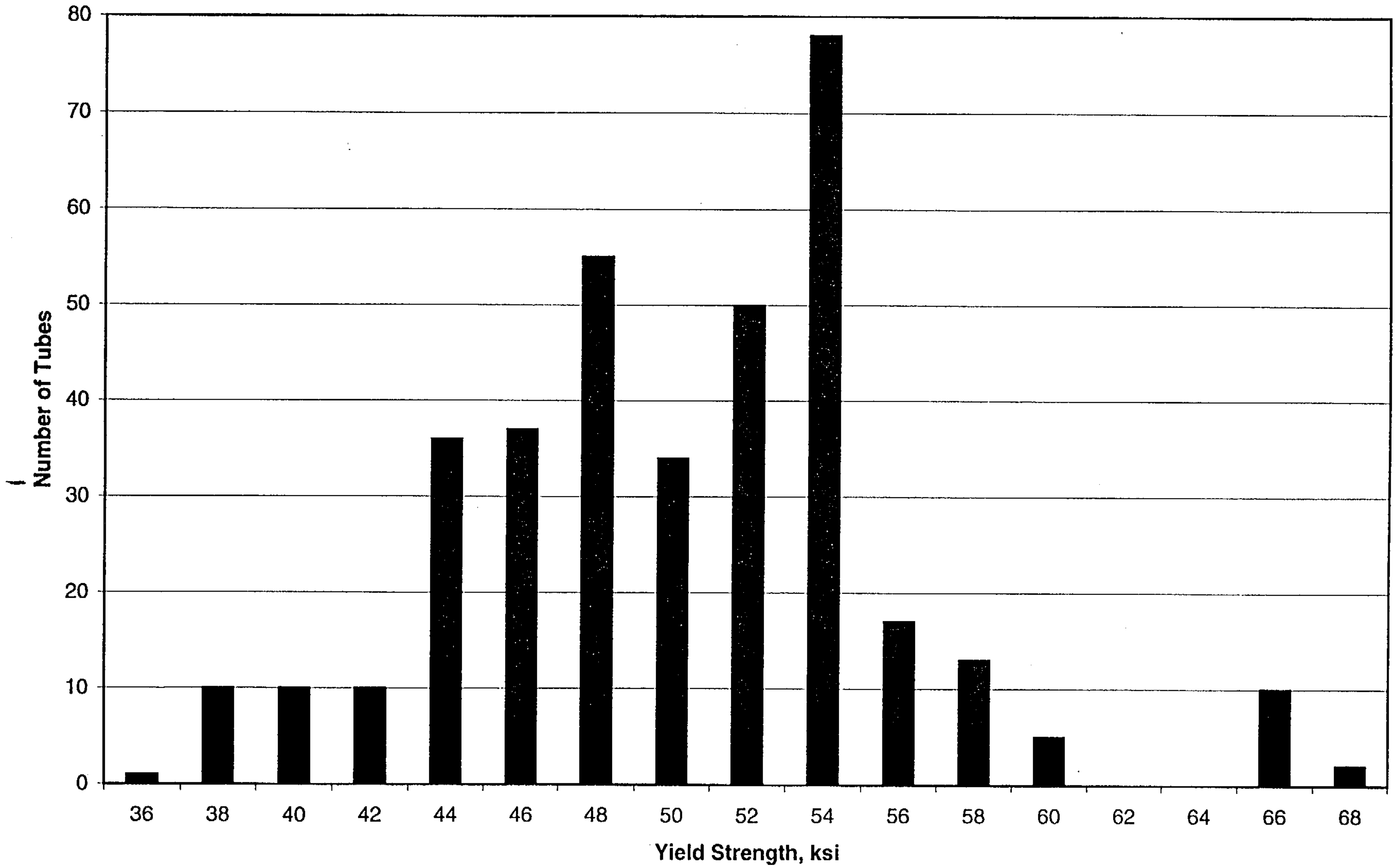
- Cracks will initiate at different time in Row 3 tubes compared to Row 2 and other Row 3 tubes

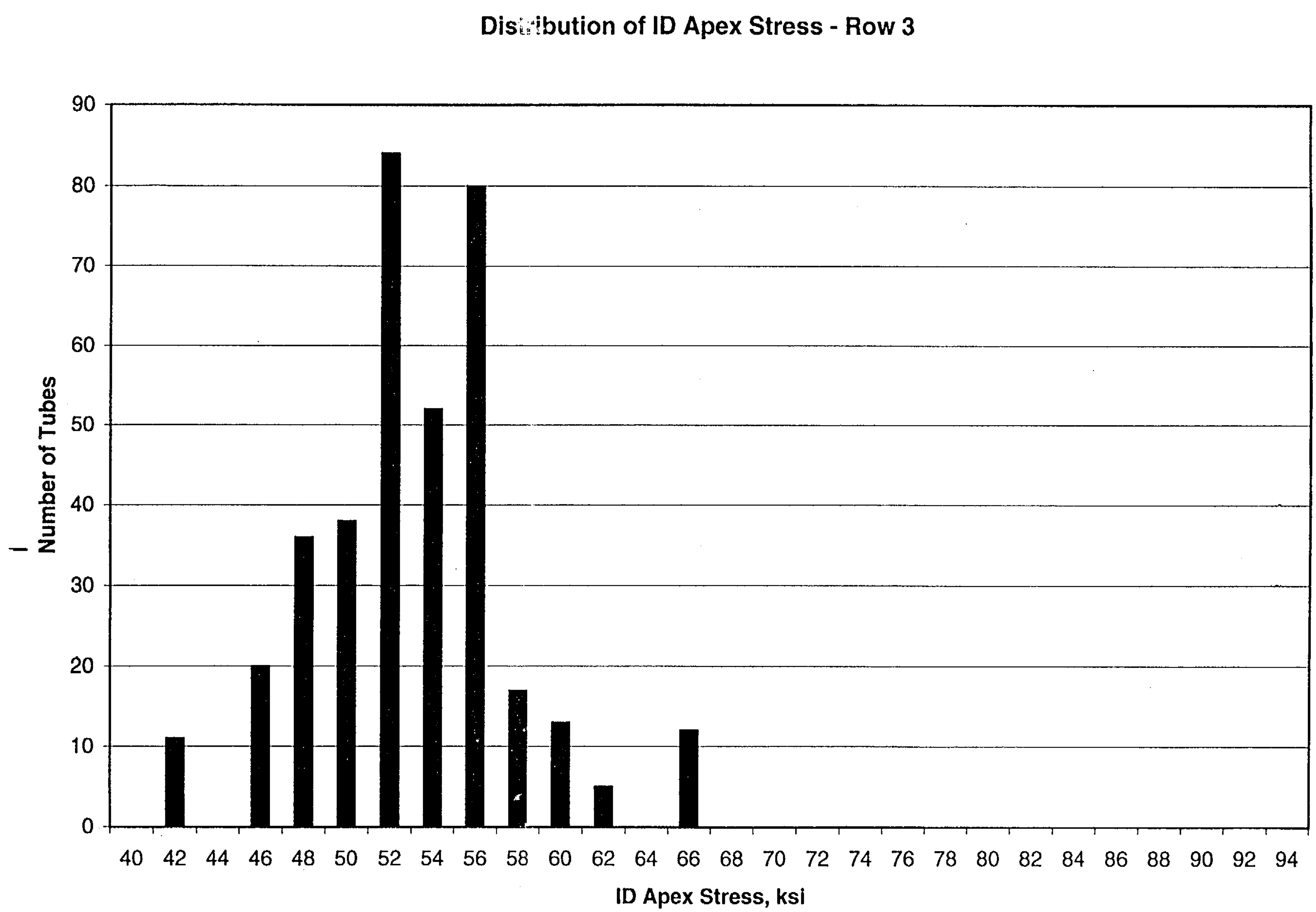


IP2 SG Tube CMTR Yield Strength Distribution - Row 2
Number of Tubes
0
10
20
30
40
50
60
70
80
36
38
40
42
44
46
48
50
52
54
56
58
60
62
64
66
68
Yield Strength, ksi

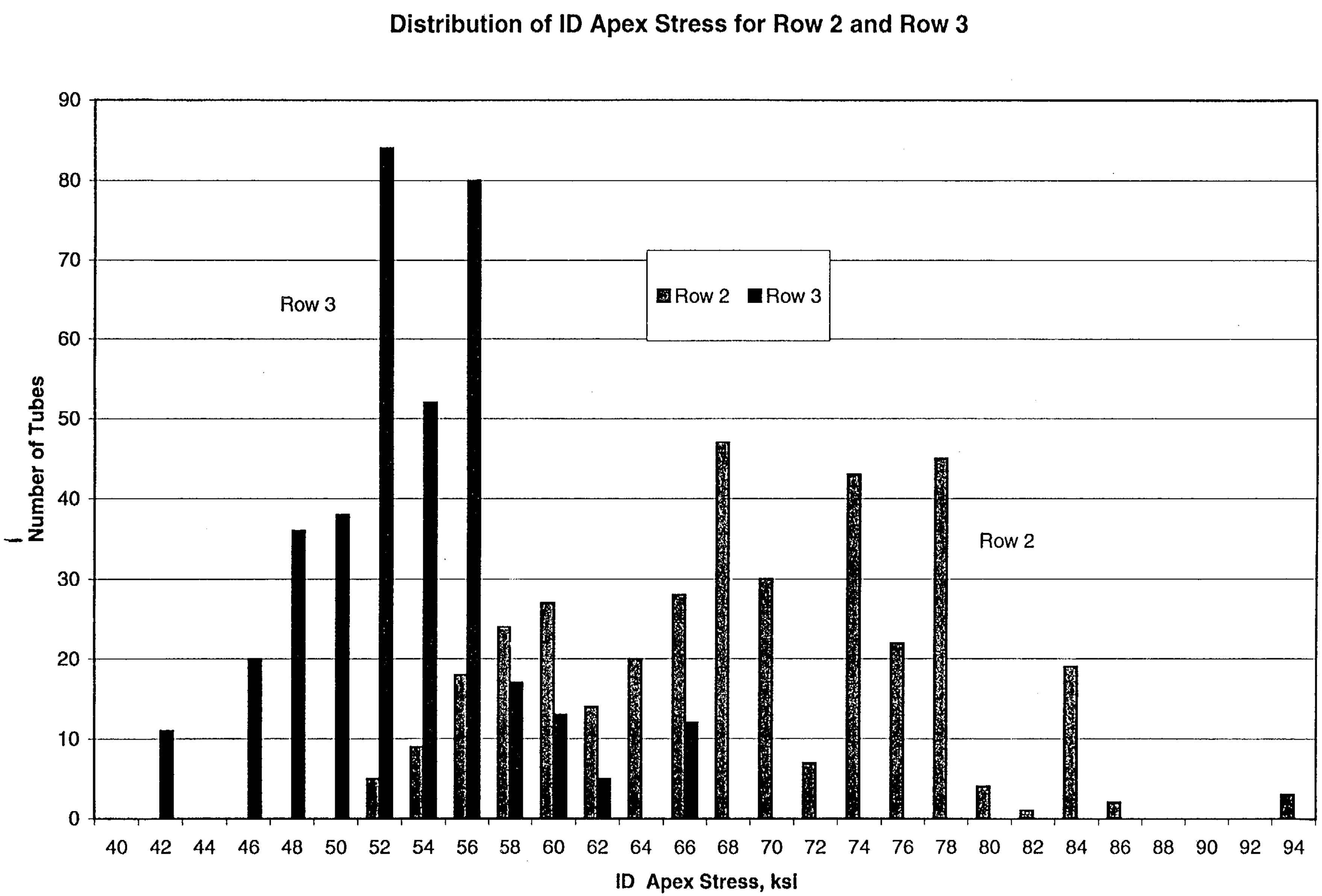

Distribution of ID Apex Stress - Row 2
Number of Tubes
0
10
20
30
40
50
60
70
80
90
40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74 76 78 80 82 84 86 88 90 92 94
ID Apex Stress

IP2 SG Tube CMTR Yield Strength Distribution - Row 3





Distribution of ID Apex Stress - Row 3
Number of Tubes
0
10
20
30
40
50
60
70
80
90
40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74 76 78 80 82 84 86 88 90 92 94
ID Apex Stress, ksi


Distribution of ID Apex Stress for Row 2 and Row 3
Row 2
Row 3
Row 3
Row 2
Number of Tubes
0
10
20
30
40
50
60
70
80
90
40
42
44
46
48
50
52
54
56
58
60
62
64
66
68
70
72
74
76
78
80
82
84
86
88
90
92
94
ID Apex Stress, ksi

Conclusions

- Ovality appears by test and analysis to not play a significant role in the ID apex stresses
- Cause of cracking linked to hour-glassing in the top TSP
- Row 3 much less susceptible to PWSCC than Row 2 tubes