UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

IN THE MATTER OF:

INDIAN POINT 2

LOCATION: BETHESDA, MARYLAND PAGES: 1- 97

DATE: OCTOBER 3, 1984

RECEIVED BY:_____ DATE:_____TIME:____

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

7920 NORFOLK AVENUE

BETHESDA, MARYLAND

10.3.84

The Panel met, pursuant to Notice, at 1:00 p.m.

NRC STAFF MEMBERS PRESENT:

STEVE VARGA, Chairman

W. JOHNSTON

J. P. DURR

B. ELLIOT

W. HAZELTON

C. Y. CHENG

ATTENDEES	ORGANIZATION
STEVE VARGA, Chairman	NRC
W. HAZELTON	NRC
W. JOHNSTON	NRC
J. P. DURR	NRC
B. ELLIOTT	NRC
DON NEIGHBORS	NRC
M. R. HUM	NRC
HARRY W. KERCH	NRC
KEN JOHNSTON	NRC
J. B. HENDERSON	NRC
ED LANTZ	NRC
C. Y. CHENG	NRC
WARREN BAMFORD	Westinghouse
B. J. LEFEBVRE	Westinghouse
D. C. ADAMONIS	Westinghouse
J. GASPERIN	Westinghouse
MIMI WEAVER	Westinghouse
JOHN D. O'TOOLE	Con. Edison
DONALD A. DOMEY	Con. Edison
JOHN J. FOY	Con. Edison
S. ROTHSTEIN	Con. Edison
G. WASILENKO	Con. Edison
C. W. JACKSON	Con. Edison
C. W. JACKSON	Con. Edison
BOB SPRING	Con. Edison
LOUIS LIBERATORI	Con. Edison
SAM SINHA	Con. Edison
MANUEL MAINER	Con. Edison
JOHN H. GIESKE	Sandia
WAYNE T. FLACH	SWRI
W. T. CLAYTON	SWRI
K. V. COOK	ORNL

P R O C E E D I N G S

(1:20 p.m.)

MR. VARGA: Well, let's go on the record.

Good afternoon, this is a meeting of Indian Point 2, a continuation of the discussion of the Reactor Vessel Indication that was discovered in the summer. A meeting had originally been held, which was also transcribed, subsequent meetings were held, as well as visits to the test facility; questions were discussed and the questions were sent to Indian Point as a result of these discussions. And we are here now to continue our discussion and evaluation of the indication discovered on the Indian Point 2 Reactor Vessel, and we will listen to what Indian Point 2 has to offer in response to the questions we sent.

Dr. Johnston, with the Division of Engineering, will have some comments now. I might add that since we are on the record, please identify yourself before you ask a question, or comment, and speak loudly and clearly.

Thank you.

DR. JOHNSTON: Thank you.

I am Bill Johnston. I think you pretty well gave a capsule of what has been going on in the last two months. What I would like to do in just about 30 seconds is tell you where we are in the review. After we did the visit to the site there at Westinghouse we, with our consultants, put out

report which I am not sure whether you have seen, or not, but essentially accounting what we did up there.

And we had some additional questions which we gave you at that time, and then some additional questions which I believe were telephoned in subsequent to our visit up there, things that we saw on the visit, that we thought we would like to know a little bit more about.

We received your document, I think -- I don't know whether it was the 25th or the 29th of September, we just recently received it, in other words. It was sent out immediately to our consultants for them to look at in parallel with what we are doing here. We recognize that we are working on a fairly short schedule to get this review done and get it right, so we can make the decisions.

The purpose of this meeting -- well, I guess, in addition we had some questions which we had already seen from the paper that you sent to us. And I think there was a phone call on Monday, with some clarification questions and other things were asked.

I think what we would like to do this afternoon is let you respond to those kind of questions, see if that will help us to facilitate our review. And then we and the consultants will ask you questions, if that is acceptable to you.

MR. O'TOOLE: John O'Toole, Con Edison. I would

like to propose a format which I think will be responsive to your proposal. And that is to rely largely on our consultants, Combustion Engineering and Westinghouse, and about three speakers, to give what will appear to be overviews of the areas of interest from your questions the other day.

But I think in those overviews you will find the answers coming out, rather than go down a stiff question and answer session. Now, if you have a strong feeling on that, we will be glad to accommodate, but I think it would be best if you heard our story from the experts who did the work, on what their impressions are and reactions to your questions. I think it might be useful.

Is that all right? Okay.

Then what I would propose is I will ask Don Adamonis of Westinghouse to give a capsule summary of where we are, after doing this experimental program and after having a visit from your people and your experts, and having gotten additional questions and answered them -- where we think we are.

And then following his brief presentation, we will ask Warren Bamford to cover the concern about the L-top accident at Turkey Point, and some of the work and analysis that we have done, and some of the conclusions we have come to. Then we switch back to Don, and Don will cover, in an

overview fashion, those areas relative to delta technique and the pitch-catch and the other aspects of the UT, and try to cover all of your concerns.

And then for an independent viewpoint, we will switch to Combustion Engineering, to John Fox, and have him give his overview of the same territory. I don't know whether you want to cover them both togetner, or not, but I would rather see our separate consultant give his separate views on these things, if that isn't too confusing.

DR. JOHNSTON: That's fine.

MR. ADAMONIS: Since we met last, I guess it was the 14th through the 17th of August when you were in our facility in Pittsburgh, we have gone ahead with a structured program to address many of the questions that were identified in your request for additional information. This consisted of fabricating another mach up, other than the one you had seen initially, the one you had seen initially we referred to as IPP-1-T block. We fabricated a 2-T block in which we put in many other reflectors, we went ahead and simulated, reconstructed the delta arrangement, using that block, making measurements, numerous measurements on those reflectors and that block; these included notches in the opposite surface, reflectors in the form of side drilled holes and side drilled slots that were just bearing out near the outside surface; compiled the delta information in a format

that we could look at and compare with our mathematical model that we had put together.

We also reconstructed the pitch-catch arrangement using the same equipment. In both cases we used the same equipment, and collected data off of notches that were on the order of one and a half and two-inches deep. We put that information together in a package, went back to the detection transducers and the detection arrangement in a pulse echo mode for transducers and made the detection, we made numerous measurements on notches of known size, in order to establish some kind of factor with respect to what we could expect to see in the way of exaggeration in the sensitivities.

We also made the attenuation checks that you requested, and those results are summarized in the report.

As a result of all of this work, we have identified that using two different approaches -- actually three different approaches; the approach of the exaggeration factor that one could expect to see using Section 11 techniques; the delta arrangement measuring total time of flight and a delta arrangement looking at two signals, apparently two signals very close together on the tapes of the delta work.

We have essentially confirmed and more clearly identified, we feel the size of that reflector in the vessel.

All of the work that we have done at this point only serves to clarify and substantiate, and more finely tune the information that we have presented in our discussion here in August.

And we will go into those results. I understand there are some questions and we are prepared to address those. And we can move in that --

DR. JOHNSTON: Yes. Would you prefer -- we have questions we would like to ask, now we would like to facilitate you having an orderly presentation, without a lot of interruptions. But I think also, periodically, perhaps we ought to stop and take some questions, and then let you proceed uninterrupted for another period of time.

MR. O'TOOLE: I have no problem with that. We had kind of hoped to get an overview of all of the areas that we covered first, but if you want to do it that way, we can.

DR. JOHNSTON: If they are all as short as the first group, then Warren would be speaking next. Yes, we could go through that series.

MR. ADAMONIS: That's the first series. I do want to clarify that all of this work we was done with the identical equipment, transducers, pulser pre-amp, receiver, all of the equipment was identical with that that was used during the investigation on Indian Point.

MR. BAMFORD: I want to show some slides to show the fracture analysis work that has been done. And most of this is an overview of the work that was done.

The analysis work was done early in August, when the indication was first discovered. The analysis work that is contained in the report that we sent you assumes a worse case sizing for the indication a 1.2 inch through-wall and two inches long. That was the original characterized size of the indication. I think Don has just said that the recent work that has been done over the last two months has led to the conclusion that the indication is not anywhere near the original characterized size.

But I want to point out what I am using in my calculations is the original characterized size, and anything smaller than that would clearly be acceptable, too.

To summarize work that was done earlier and reported in the report that we sent in, the hydro test -- the two governing conditions for the vessel are the hydro test condition for the normal upset in test condition, and the small steam line break for the bulk condition.

The hydro test condition, we analyzed minimum temperature of 310 degrees, pressure 3105. The flaw, even though it is 1.25 inches through-wall, is near to the outside surface, so it has to be characterized as a surface indication by the rules of Section 11. So, it ends up being 1.45 inches

in through-wall to two inches long. The applied K-value for that, for the hydro test condition was 46. Constrast that with the allowable K-value for either of the materials in the region of the indication, either the weld where we think the indication is, the allowable K-value is 63, for the adjacent plate material the allowable K-value is also 63.

So, clearly the indication is acceptable by the criteria for normal upset test conditions.

If you look at the governing emergency fault condition, you reach the same conclusion. Looking at all of the conditions that could exist, including all of the pressurized thermal shock events, we concluded that the most serious challenge to an indication in this region would be a small steam line break. The lowest temperature in a small steam line break in the vessel wall is 240 degrees F.

Again, the applied K for the characterized flaw indication of 1.45 inches depth is 38, which I might add is lower than the maximum applied value for the hydro test. The allowable K-values come from the criteria for faulty conditions, K-1C over the square root of two; the allowable Ks are 141 for the weld; 118 for the plate. If you make the calculation with the Reg Guide draft radiation damage method; it is 114.5 if you use the Guthrie methodology.

But, again, 38 compared with either 118 or 114.5 is

clearly the indication is acceptable by those criteria. And those are the criteria which we used in the report, and we feel those are the criteria that need to be used to justify the acceptability of the indication, because they are the governing design criteria.

Now, if you ask a question about whether the indication is acceptable by -- if you postulate that the low temperature over pressure which occurred at Turkey Point, I think in 1981, if that were to occur at Indian Point, would the results still be acceptable.

We have analyzed that condition, there we had a temperature of 110 degrees F., pressure of 1100 psi. Again, the 1.45 inch deep indication gives an applied K of 16.

The results for the allowable K, we have assumed that this is an emergency or fault condition, so we used K-1-A over the square root of two, the allowable K-value for the weld where we think the indication is is 32.9,if you use the Reg Guide draft; or 50.7 if you use the Guthrie method of determining a radiation damage.

Looking at the adjacent plate, the allowables are very similar, whether you use the Reg Guide draft, or the Guthrie method, both about 30, a little more than 30 KSI per inch. So, clearly the indication is acceptable, if such an event were to occur at Indian Point.

Now, there are a number of reasons why this kind

of an event is very unlikely to occur at Indian Point. The main reason is that at Turkey Point they have an automatic system which can close off the letdown. That is it can make the system go solid; such a system doesn't exist at Indian Point. That is there is no automatic letdown cutoff, so therefore, there is very low probability that the system could ever go solid. In fact, they operate with a bubble in the pressurizer and there are many different levels of protection to insure that the system won't go solid and such an event can't occur.

In fact, we have calculated with our risk assessment people what the probability of such an event would be for Indian Point. And I want to review that just briefly here. Our best estimate of the event -- the probability of an over-pressure event occurring at Indian Point is on the order of 10 to the -7th per reactor year, which puts it in a very low probability category. And I can go through the details of the calculations, if you would like.

But the main reason for such a low probability -- well, there are two main reasons, one is the operations of the system at Indian Point are different, it makes it very difficult to have a solid system. And the other reason is there is an over-pressure protection system built in to Indian Point, which is in many ways redundant, it has two trains, two redundant trains and there are many redundant

features in that. It is very good protection system, so there is a very low probability of such an event occurring anyway.

We also made a probability analysis of the probability of such an event occurring, assuming worse case assumptions, assuming that the over-pressure protection system doesn't operate and making several other worse case assumptions that we feel no one could argue with. And we come up with 10 to the -5th per reactor year, even for that particular case.

So, it clearly puts the probability, or puts an over-pressure event, such as happened at Turkey Point, in a very low probability realm for Indian Point. It classifies it in the category of a faulted condition at Indian Point. And, in fact, if you look at the best estimate, you could categorize it as being such a low probability event, that it may not even be worth considering as a possible occurrence.

So we concluded that there is really no large possibility that such an over-pressure event could occur at Indian Point, but even if it did, and even if the indication were as large as originally characterized, it is still acceptable. So, from that standpoint the indication is acceptable and there is nothing that should stand in the way of the plant returning to power.

Now, some of the detailed questions that were transmitted to Westinghouse and Indian Point earlier in the

week are going to be covered by Don Adamcnis, who is going to talk in a more detailed overview of the UT work.

MR. VARGA: I was just going to ask John -- and you may not know -- how did this assessment of the probability compare with the Indian Point PRA that was originally done, was it the same methodology, the same kind of availability figures on the equipment, and that sort of thing?

MR. O'TOOLE: We brought Lou along, Lou worked on our PRA study and he is aware of what Warren used here.

Do you want to comment?

MR. LIBERATORI: Lou Liberatori. The work done by Warren's people was based on generic Westinghouse OPS system and the standard numbers. His people talked with our people and we knew that the generic Westinghouse system analysis system bounded ours, we feel that our OPS system is more reliable to these results. And we concurred on that, that their numbers are probably conservative.

As far as the PRA is concerned, we did not address the cold shutdown type accident in the PRA.

MR. CHENG: I have one question. On the Turkey Point air top event, you assume the pressure reaches 1100 psi, suppose you did not stop, that you go all the way up to 2500. Is that exceeding the fault conditions?

MR. BAMFORD: Yes, we checked that.

MR. CHENG: Then you have a 16 KSI over there,

that is the same factor, but I think we go beyond 30 or 32.

MR. BAMFORD: It is right around -- if you want to make that worse case type of an assumption, you get very close to the allowable limits. But we don't feel that that is even a consideration that needs to be made here, because of the differences in the two systems, it is just so remote a possibility that something like that can happen, it is just not worth considering.

Now, if we had a different system here, you could argue that question. But I think because of the distinct differences between the Indian Point system and the Turkey Point system, that kind of thing is unlikely. And, also, the Turkey Point system, you know, that over-pressurization at Turkey Point occurred when the over-pressure protection system was not operational. Even that wouldn't occur at Turkey Point, because of their over-pressure protection system.

MR. HAZELTON: Warren Hazelton. I just have a question. This information --

MR. VARGA: I thought we weren't going to ask the questions until they got through, and we have already broken our own rule.

MR. O'TOOLE: Well, as far as I am concerned -- and I don't know -- but as far as I am concerned this subject is probably one that should be questioned now.

MR. BAMFORD: This is probably the right time to do it.

MR. HAZELTON: These responses to our questions, are these going to be formally submitted to us, like this discussion he is giving us? This is very good and very useful, we would like to have it on paper.

MR. O'TOOLE: I thought that was what this meeting was for. Isn't this going to be right on the record, too?

MR. VARGA: Yes, John, but in accordance with our normal practice, if there are pieces of information that we rely upon for our evaluation, the transcript is certainly helpful, but it is not what we say is a legally regulatory enforceable document. So, what we would like to do is where there are significant, at least in the view of the staff, and my view, where there are significant responses that are not contained in the formal submittal, we ought to supplement them.

MR. O'TOOLE: I have no problem with that. I take it the objective of the meeting, however, is that you want to make a decision today, is that correct?

MR. VARGA: Well, it will depend upon what the concerted and collegial view is of what you have given us, and we certainly will respond to you after we see where we come out. Whether or not there is a decision made today, I think is probably unlikely, but we may be able to give you

some indications.

MR. O'TOOLE: We brought all of these people here to try to answer all of your questions.

MR. VARGA: And I think it is going to be very useful. There is no problem in submitting a written confirmation -- these people have been doing it for the past two months. There is no problem in adding a few more, I guess.

MR. BAMFORD: I guess we would prefer that the written confirmation, or the written discussion, to repeat what has already been said here, be used as an information item, rather than being something that would be submitted before you make a decision. I think that is what we are feeling for, it is no problem to document this at all. But we prefer that you document it in support of your decision, rather than before your decision. But that is for you to decide.

MR. VARGA: Let's see how that comes out.

MR. CHENG: Since we are talking about the L-top event, I would like to see our system people who have any questions regarding this --

DR. JOHNSTON: Let's take it the next round.

MR. O'TOOLE: This is the only thing on L-top, if you want to do it now.

This ground was L-top fracture mechanics.

MR. HAZELTON: And the probability. It is very useful to us and I don't know that I can remember --

MR. BAMFORD: I can give you copies of the slides, that is no problem at all. The probability numbers are on the slides.

MR. VARGA: I think we ought to go through it. If we have questions on L-top, we ought to conclude it and then go through with the detail we need. And I think John seems to be amenable to that, so let's go.

But I would just like to say one thing, that the sensitivity of this issue is probably such that a decision from the staff, from NRC, in terms of re-start will have to be accompanied by a rather detailed safety evaluation, which itself is backed up with documented submittals from the licensee. And so while I appreciate the concern, and believe me, we are all sensitive to your re-start concerns, but at the same time, we are very sensitive to the safety implications and the fact that all of the bases upon which our decision is made are clearly visible and clearly available.

So, let's continue.

MR. ELLIOTT: Barry Elliott. You spoke about a faltry analysis, in your faltry analysis where there was a chance of filing an Appendix G type of curve, did you look and see how long it would take before it could be violated?

MR. BAMFORD: Yes, the answer to that is because the system is not solid, it is a much longer time. And I can't give you minutes, or hours, but it is a much longer time than if the system were solid. And we built that kind of a possibility into our faltry analysis.

MR. ELLIOTT: Would it be on the order of greater than 10 minutes?

MR. BAMFORD: I would think much longer, perhaps the systems guy from Con Ed could answer that better than myself, but I would think much longer than 10 minutes.

MR. LIBERATORI: Lou Liberatori, Con Ed.

Yes, greater than 10 minutes. I don't know if I could really put a number on the record, since we haven't done the calculation, but on the order of 20-30 minutes, probably. But certainly greater than 10; the original calculation for the water solid case was based on 10 minutes.

MR. BAMFORD: Let me try to put this in a little perspective. The first event in our faltry analysis is the probability that an over-pressure condition occur, this OPC is the probability of an over-pressure condition at a temperature below 250 degrees F, because it is the low temperature ones that we are concerned about.

We have assumed a best estimate there for one in 10, something like that occurring. That is a very conservative number, considering the way Indian Point's system

is designed.

There is another thing that makes this number even smaller than that, is that we assumed that this was the probability that you could have a violation, you could have a pressure of over 1,000 psi at a temperature below 250. In turns out in their operating criteria, when the temperature of the main coolant system is below 310 degrees, they adopt a different philosophy on the size of the bubble they keep in the pressurizer, they increase the size of that bubble, which increases the margin and increases the time that it would take to get to an over-pressure condition.

So the numbers that I have here that lead to this 10 to -7 number, this number here is probably much more conservative than it needs to be, considering the specifics of their operating plan, and the guidelines that they have that they operate to.

MR. VARGA: Has there ever been a low temperature over-pressure event at Indian Point?

MR. BAMFORD: There has never been one since the over-pressure system was put on, which is '77 or '78. In fact, looking at the operating history, you are all familiar with the fact that there is almost no over-pressure events since these systems were installed nation-wide. The only one I know of that was of any significance occurred at Turkey Point, and that was when they were

actually working on the over-pressure protection system. You know, if that had been operational even that one would not have occurred.

So, we are talking about very low probability event with today's level of protection.

MR. HAZELTON: And at Indian Point, you wouldn't be operating when the thing isn't working?

MR. JACKSON: I am Charles Jackson, I am vice president of Con Ed, Indian Point station, I am located there, I was also the chairman of the Westinghouse over-pressure protection analysis group, the analysis that ended up being the bounding cases that Westinghouse is now using.

We designed our system at Indian Point to be a two out of three, 1-E full safeguard system. I will describe it as the Cadillac system. We also have mentioned in our specs some pretty stringent limits on it, so that when we are in these conditions, if we do not have the OPS, over-pressure protection system operable, we substitute either a very large nitrogen bubble in the pressurizer, or we provide an opening in the system, locking open the power operating relief valve to give us sufficient opening size to be greater than the source of water, that would be, let's say, the equivalent of the letdown path.

We have operated in a very conservative manner, we have had to make significant modifications in the facility to be able to accommodate the nitrogen use, as opposed to cladding models. We installed a nitrogen system, we changed the valves on our pressurizer and their design, so that we were assured that we would have operability to meet the 10- minute criteria for those valves. We changed from air operation to nitrogen operation, so that we have not had an event since the changes were made.

MR. BAMFORD: I might add the faltry analysis that we did, based on the worst case scenario, came up with a number of 10 to the -5 per reactor year, assumed that the over-pressure system didn't operate at all, that both trains were inoperable.

So, even under that circumstance, you are still at 10 to the -5 and the event is a very low probability.

DR. JOHNSTON: Did you --

MR. CHENG: He says he doesn't have any questions.

MR. O'TOOLE: Then we will move on to Don. We are going to move into the ultra-sonic area, and some of your concerns. We are going to go over the program that led to your concerns, and do it in such a manner that we hope to address the things along the way, if we don't, you are going to be free to question.

Don.

MR. ADAMONIS: Don Adamonis, Westinghouse.

The initial part of our effort was involved with looking at the delta technique on a series of notches, and reflectors in the form of drilled holes and drilled slots near the outside surface of our mach up. Most of those efforts were concentrated on the -- what I will refer to as the IPP 2-T mach up, and that was not the one that was available when you visited in mid-August, as we were structuring our program -- we needed to get more notches in the block. There was some question as to whether that -- we identified that that particular dropout had come out of the three-loop vessel, or a four-loop vessel, cladding even more typical of the cladding on the Indian Point vessel.

We made multiple delta measurements in both transducer arrangements, that is with what I will refer to as the Transducers 22 and 24, which were the two opposing 45 degree transducers, with 22 as the transmitter, 20 as the receiver, and 24 as a transmitter, and 20 as a receiver.

In total, and we can say that we made at least 70 to 80 measurements on somewhere on the order of 12 to 14 reflectors, combining the total number of transducer arrangements. Those notches and drilled reflectors that we looked at varied in depth from one-tenth of an inch to two inches deep, from the outside surface.

We took that data with, again, all the same equipment that was used on the vessel investigation, and we took the data -- there was another consideration -- with three different operators, the time constraint was such that we manned shifts around the clock, we had three different operators taking data.

When one plotted the results on a plot of the mathematical model that we came up with and one bounded all of the data points, we found that our bounding lines were on the order of plus or minus two microseconds in transit time measurement, which translated into a plus or minus two-tenth of an inch in the measurement.

We also looked at information relative to measuring peaks, double peaks, the time distance between double peaks that we saw from notches. And we found that that gave us a better, a more close approximation than the total time of flight of the depth of those particular notches. We looked at that type of analysis and identified in those cases where we saw two peaks, we were able to identify the depth within plus or minus one and a half microseconds.

We used that type of information in viewing the results from the vessel, and in fact one can see two peaks on those results, on the videotaped results, separated by about -- by less than two microseconds, on the order of 1.8 microseconds. And we identified that as indicative

of a depth from the tip of that reflector of .18 inches.

Now, we went on to the pitch-catch assembly, the 45 degree pitch-catch assembly, and when you were in Pittsburgh we showed you the results off of notches that were on the order of one inch deep. In the second test block we put in notches that included depths of two inches, and one and a half inches.

When we repeat the pitch-catch measurement on those notches, with the same sensitivity that was used on the vessel, one sees the same types of variations in areas that are un-notched, as we saw on the vessel away from the reflector. When one gets over those one and a half and two inch deep reflectors, there is essentially a total loss, a total loss of signal.

MR. CHENG: That is one and a half to two inches?

MR. ADAMONIS: That's right.

I should clarify that that two inch deep notch -- we had some difficulty machining it, it is actually stepped, the minimum depth of that notch is 1.8 inches and out at the ends it is two inches.

DR. JOHNSTON: A question for clarification. Did you repeat a one-inch notch again?

MR. ADAMONIS: Well, we did not put a separate one-inch notch in this new block.

I should clarify even further that the block that

I am referring to, the second block, we fabricated, is more similar in terms of time of fabrication to the Indian Point 2 vessel, than was the mach up that we looked at during your visit in August.

Now, in terms of -- going back to the delta measurement, we were able to, on the notch at varying depth, from 1.85 to two inches, able to make measurements and actually discriminate that difference in depth. If we were looking at the center of the notch, we got one set of time of flight data indicative of that particular depth; when we moved off to the edges, we could see the time of flight getting shorter, indicating a deeper depth.

At no time during the investigation did we make multiple passes across each particular notch, but in repeating the examinations in sequence, when we got back to the various notches, and in that fashion I would say that we sampled along various lengths of each particular notch. And the results correlated quite well.

Are there any questions up to that point?

MR. GIESKE: I am John Gieske.

You say that the double peaks that you measured are 1.8 microseconds and in the report you said it preceded the primary indication. The primary indication, I take it in the report, is 131 microseconds?

MR. ADAMONIS: 131 to 136.

MR. GIESKE: Okay, so you would then be able to subtract 1.8 microseconds from that to get the minimum time possible, if you are going to consider full path lengths now, to get the full depth, is that true? Do you have any objection to that?

MR. ADAMONIS: The point is that if one considers that you do see double peaks off of notches, that that could very well be what we are seeing in that particular instance. There is a secondary --

MR. GIESKE: It is also true that you can consider the full path lengths to be a legitimate way of looking at the --

MR. ADAMONIS: Well, you have to recognize that whenever those measurements were made on the vessel, they were measured to the front of that pulse, because the separation in them, between them is rather short, two microseconds, or 1.8 microseconds.

MR. GIESKE: I am not arguing that. In the report you said 131 microseconds.

MR. ADAMONIS: That's correct.

MR. GIESKE: And in the report you said that the double peak has a preceding pulse in front of the primary pulse. I am asking you the question do I now subtract the 1.8 or do I add it, to get the difference, that's all I am asking?

MR. FOX: I understand his question, do you add the information of the 1.8 to the primary pulse, or do you subtract it? The answer to that question is off of the reactor vessel, to come to the 131 point whatever, the first time of flight information was used, so the first signal that we saw in time was used, so that is the number that is 131 point, and the other number would then have to be added to get to the other -- I guess you would call the primary pulse.

MR. ADAMONIS: The pulse with the largest amplitude.

MR. GIESKE: In the report, even though you said preceding, leads you to believe that you would subtract it.

MR. ADAMONIS: I understand the confusion, yes.

DR. JOHNSTON: I had a question about the attenuation that you found on your I-T 2-T block, compared to your I-T 1-T. Do you remember when we were looking at the flat block, and then looked at the 1-T block, your first one, there was a whale of a difference in the attenuation. What did you find when you made up your new block?

MR. ADAMONIS: The new block was more like our calibration block and from our pitch-catch data, and more like the vessel. The 1-T block appeared to be less attenuative, we put drill holes in the 1-T block, we had drill holes in the 2-T block, obviously we had drill holes

in the original calibration block. And we found the original calibration block to be like the 2-T block, and using the variations in amplitude that we saw from the vessel on the pitch-catch, we were able to identify that those two blocks more closely represent the vessel.

And I would have to go back and look at the exact numbers, but I recall numbers on the order of the 1-T being less attenuative somewhere on the order of 8 db, eight to 10 -- I'm sorry, more like 18 to 20.

Well, if one considers only the amplitudes from the quarter T holes, at 45 degrees, we are talking somewhere on the order of 8 db.

You are just talking about the notch responses, and we feel as though the notch responses --

DR. JOHNSTON: They were so far off scale you couldn't even bring it back on.

MR. ADAMONIS: That's right. And those amplitudes were somewhere on the order of 8 db. But we are saying that at least half of that 30 db is accountable to the geometry, the effect of curvature on the angle of attack in that particular notch, because when we go back and take direct measurements on drilled holes in the block --

DR. JOHNSTON: IP 2-T is a curved block just like IP-1, isn't it, and I think the degrees of curvature and so forth there, they are the same.

MR. ADAMONIS: They are not identical. 1-T was 78.5 inches -- 86.5 inches.

But what we are saying -- when I talk in terms of the attenuation differences between 1-T and 2-T, for now let's just confine our discussion to those data that we got off of the side drilled holes. And you are correct in your statement, when we went to the notches in the 1-T block, the amplitudes were extremely high.

DR. JOHNSTON: Were the notches identical?

MR. ADAMONIS: Yes, in terms of our range, we had a range of depths in the 1-T block and I guess some of the notches were 2 percent of the wall thickness in both blocks. But, again, we were trying to establish what the attenuation difference is.

Let's just talk about the drilled holes, and when we set up on the drilled holes on the 2-T block, using the calibration that was established on the calibration block that was used for the vessel, the one we refer to as RV-70, the distance amplitude curves were essentially the same, in terms of their amplitude.

When we took that same calibration and went over to the 1-T block, where we may have had a Dax curve shape that was 80-40-25, 80-50-30, and when we looked at the 1-T block side holes with the same set up, the amplitude off the quarter T hole was 100 percent screen height, plus 6 db.

We are talking in terms of an 8 db difference there. The three-quarter T hole, this was 83 percent, instead of somewhere on the order of between 20 and 30 percent, somewhere around a three-to-one difference.

MR. HUM: Martin Hum.

Could you tell us how many data points on the vessel you took with the delta technique?

MR. ADAMONIS: Data points on the vessel -- we have on tape at least eight, there were numerous other measurements made during the set up, I would say at least another eight that were recorded. And we saw transit time variations anywhere from 131 to 133.4, or 133.6.

MR. HAZELTON: Is the difference in transit time, is that a function of the vertical location of your transducers? That is, sort of like if you are scanning over the length of the reflector?

MR. ADAMONIS: That's correct, along the length of the reflector.

MR. HAZELTON: So, you could infer that the depth of the reflector varied over its length, is that what you are telling me?

MR. LEFEBVRE: I think what you are talking about, it varied with respect that one determined the peak to be and then taking it to the 50 percent points, it varies downward from that point. But the difference of microseconds

you are talking about is the difference between two pulses, one of which is identified as a flat surface, and the other one as being called what we call the end of the reflector of interest. It is that time between those two pulses that he is talking about, on the order of 1.8 to two microseconds. That's the vertical plane.

MR. HAZELTON: I wasn't talking about the difference between the two pulses -- the shortest time of flight, that varied, depending on where you were, vertical location, or did you take all measurements at the same vertical position of your transducer set up?

MR. ADAMONIS: On the tapes we stepped across and made multiple measurements.

MR. LEFEBVRE: Every half inch across the whole area of interest.

MR. HAZELTON: Are you talking across -- it sounds horizontal, I am talking about up and down. This thing you are saying is about .85 inches long and that length is in the vertical direction.

MR. LEFEVRE: No, it is vertical on the vessel.

MR. HAZELTON: That's what I am talking about. So, I was just wondering, when you are talking about slight differences in the shortest time of flight, whether these were determined at the same exact position as the transducers, or as a function of vertical position of the transducers?

MR. FOX: I understand the question, and I think there was as much -- the correlation that you are looking for in length, or the disparity between the numbers was attacking the reflector in one direction versus attacking the reflector in another direction, but at the same exact transducer location, speaking vertically.

DR. JOHNSTON: Bill Johnston.

As I understood you went across this way (indicating) and what Warren is asking, did you then go back across, or did you go across, go back, then step down and go across?

MR. LEFEBVRE: Stepped down in zig-zag fashion.

MR. ADAMONIS: There w --

MR. LEFEBVRE: -- so yo at the same height.

DR. JOHNSTON: So, you never a went over the same place twice because you stepped down and went across, and stepped down and went across, right?

MR. ADAMONIS: Right.

MR. HAZELTON: The number changed as you stepped down and went across, and then went down and went across. The shortest transit time --

MR. FOX: This information was taken with two sets of transducers, one set of transducers was shooting in this direction (indicating), with zero degree above it, one set was shooting in this direction (indicating) with zero degree

above it. The information was taken as the raster down it, and then raster down it again with the other side. Both of those sets, if you will, were thrown in a bag, shaken up and the low number was this and the high number was this.

I just want to make the point that your calculations on the maximum depth of the reflector is based on the assumption that you are getting -- that the defraction point is the deepest point of the reflector.

MR. ADAMONIS: That's correct.

MR. VARGA: How do you know that?

MR. LEFEBVRE: It is partly that, and it is also because we have no confirmation by other means in which we have looked at it that it would be deeper. It is the absence of evidence by other means that show that it is not deeper than that.

MR. HUN: What are the other means?

MR. HAZELTON: Excuse our curiosity.

MR. LEFEBVRE: Well, some of which we looked at with straight beam transducers, some of which we looked at with the false echo mode, 45s and 60s in both direction, and they tend to indicate that it would not be much greater than that. If it were considerably higher than that, as an example, if it were the heighth that it was originally predicted, we expect that we would get significantly different results from the lower transducers than what we

did get.

MR. HUN: If it were three-quarter inch deep would you expect to see significant differences with those techniques that you used?

MR. LEFEBVRE: Yes, I would.

MR. DURR: Jack Durr.

If using the 60 degree sheer there is an amplitude value which it will discriminate anymore, I mean, no matter how large the flaw gets you will always have the same amplitude reflection, it will essentially saturate, is there is some value at which 60 degrees is capable of discriminating amplitude-wise, do we know what the value is for this 60 degree?

MR. LEFEBVRE: I think we started to see that point with the reflector that we had planned to be two-inches deep, and it turned out to be 1.85. I can't answer your question with absolute certainty, I think we are in that neighborhood with that dimension.

MR. DURR: That 60 degree will discriminate amplitude-wise up through 1.8 inches?

MR. LEFEBVRE: It tends to indicate that, but I haven't looked at that one in-depth. I would suspect it is in that neighborhood, but I can't say that with certainty.

MR. ADAMONIS: I think that is a very difficult one

to answer, because only slight changes in the geometry of the reflector, with respect to being perpendicular can give you changes in amplitude. If you are just looking at an amplitude consideration --

MR. FOX: I think we should also point out that there has been a lot of discussion on the delta and the pitch-catch technique, there is a lot of information on pulse echo technique with the 60 degrees transducers that are in question. And essentially, when plotted up there is no predictor you could tag on to amplitude. By that I mean, you couldn't use amplitude as a basis of predicting -- there is no prediction capability of the amplitude.

MR. FLACH: Wayne Flach.

If I understand the data properly, Don, the notch at three-tenths of an inch deep by one inch long would be approximately the same amplitude as the vessel?

MR. ADAMONIS: That's correct.

MR. FLACH: Would you please address how a flaw could be as efficient reflector as a machined notch? How a flaw which is slightly smaller in both direction could give the amplitude that the machined notch did?

MR. ADAMONIS: I think that one can see that some of the angle notches give similar types of reflections, and depends in large measure on what -- again, what the geometry of that reflector is, what its relationship is,

its proximity to the surface, and what its attitude is with respect to the sound beam that is incident on it. I think that in the case of notch A, which is a one-tenth of an inch deep notch, that one had an amplitude of somewhere between 100 percent DAC plus 10 db.

We are talking about rather small notches. Oh, I'm sorry, between 100 percent DAC plus 10 db and 100 percent DAC plus 20 db, indicates the one-tenth of an inch notch in the 2-T block.

MR. O'TOOLE: Has anybody made an interpretation what the origin of the reflector is?

MR. ADAMONIS: We've looked at the data and considered from the standpoint of what it really isn't. I think in terms of a characterization, that one can say absolutely, with absolute certainty, this is what it is. I don't think we have reached that point, and I am not sure that one can.

MR. O'TOOLE: The reason I ask is there has been a lot of discussion -- you know, depending on what the nature of the reflector is, and --

MR. ADAMONIS: I would say if you look at the difference in amplitude response from the two sides of the reflector, one could make an argument for the buttress notches might have been left in there; one can argue that there was a repair in the area, that a local repair, and

there might have been an attachment very close where they left in some slack, or undercut. You know, I have seen indications on bottom head to lower shelf seams where it is just a fact that one didn't completely grind out the wall dripple that was left there, we have indications in excess of 100 percent DAC.

MR. FLACH: Does it look as though it was machined on the OD?

MR. ADAMONIS: I really can't answer that question. I am really not sure.

MR. FLACH: I know in those days they did machine some of those individually. This one looks as though it is, but it is not obvious.

MR. FOX: I think what could be apparently looking like it is is actually some -- there is some protection coating put on the outside of the vessel, and we haven't seen -- we have seen some areas that looked like cosmetic depressions, you know, when you survey all of the photographs. So, I can't say categorically whether it is, or isn't.

MR. O'TOOLE: I would like to ask John Fox to answer Martin Hun's question, as well as Don, because I think he has a little different response. Would you like to field that?

MR. FOX: Sure. In all of the information that

was thrown at these two mach ups, and all of the information that was essentially processed by the reflector in the vessel, the reflector in the vessel most behaves like a 2 percent notch. The amplitudes that we are getting back very much reflect like a flat bottom reflector, meaning that it is flat bottom to the incident sound beams, so therefore, a V-type notch, or buttress notch would behave very much like this.

We have seen no information that discounts that from being a candidate.

MR. KERCH: Perry Kerch.

Your radiographs would show that, wouldn't they?

MR. O'TOOLE: No, and the answer is that the end process radiography would have been done as the welds were being fabricated, not necessarily when the subsequent ultrasonic inspection was done.

MR. CHENG: Okay, how about the PSI UT, they should have picked that one up.

MR. O'TOOLE: The post hydro?

MR. CHENG: Yes.

MR. O'TOOLE: The post hydro would have been the -- the inspection that we are referring to as having put that notch in there.

MR. ELLIOTT: Barry Elliott.

Do any of the techniques -- are any of the techniques

capable of finding branches in cracks?

MR. ADAMONIS: I don't know that we have gone far enough with the technique to be able to identify that and make a category statement.

MR. HAZELTON: I would like to ask a question, but perhaps -- in looking at the reflectivity of this particular reflector, how would that compare with the reflectivity of the type of cracks that we have been referring to as "hatch" type cracks, that Bill discussed in relation to the Pilgrim problem, and so forth, the heat effect at zone type thing? Do you have any feeling for the reflectivity similarities or anything?

MR. ADAMONIS: I have not, no. I can't address that.

MR. KERCH: Can we assume now that we are saying that this indication is not in the weld, but in the plate now?

MR. ADAMONIS: No, that's not correct. The delta results at the location that we find the delta results, the peak amplitudes from the delta indicate that it is 345.45 degrees, it is in the report, but that would indicate that it is at the weld, in the weld.

MR. HUN: If there are no acoustical differences between the weld and the base weld, how can you determine it is in the weld?

MR. ADAMONIS: Martin, all we can do is use positional information, where we have our peak amplitudes, positional information and information from the drawings on the vessel.

MR. DURR: What is the positional tolerances on your fixture?

MR. ADAMONIS: Positional tolerances --

MR. DURR: You can measure within plus or minus, because a degree is about an inch and a half, or so?

MR. ADAMONIS: That's right. And our result for that particular access is 100 counts per degree. So, that is .15.

MR. DURR: If the drawings that you are going by are correct, you are sure you know where you are, so the only possible error is in the positional information given you, as to where the weld is, in relation to the other locator that you are using, whatever it is?

MR. ADAMONIS: That's correct.

MR. CHENG: If I hear what you people are saying is correct, you may be thinking that the indication in the vessel is a notch, or something like that. I think in one of the questions that we asked earlier to Con Ed, to back and search the fabrication and its history, do you have any answer --

MR. ADAMONIS: I think we answered that in our

report. But I would be glad to summarize that for you. That search was not clear cut with the time that has elapsed since those were compiled, it is not unusual I think. The fabrication records at Combustion could not confirm a trip report by a Westinghouse person that there was a notch used in the shell course for calibration of UT equipment.

So, we have the one document that says there was a notch in it, we have no record on the manufacturer's part of having put a notch in it. I think there is an acknowledgement that that practice was used at one time, but it is no longer used. So that leaves that kind of anomalous.

The X-ray results, as we told you last time, X-ray film to some calibrated eyeballs show what may be some indication at this location. And we said we were going to enhance the X-rays and we gave you a response in the report. I think the conclusion -- and I think Don Domey might be the best one to answer that -- the conclusion of the X-ray enhancement is also an anomaly, and best ought to be ruled out as an element in this consideration.

MR. DOMEY: The image enhancement was done by Shonberg Corporation, and looked at by Mel LeFettis (phonetic) in California. The image enhancement showed us an irregularity of the film. The irregularity was not visible precisely with the naked eye, it wasn't visible, especially, when compared to a 2 percent penetrometer, and so the conclusion

that they drew that it was an insignificant indication, and that the image enhancement was probably not valid in making any measurements.

The initial report we got was that it was significantly less than 10 millimeters, which is 400,000th of an inch, .400 inches.

MR. HAZELTON: In length?

MR. DOMEY: In depth -- significantly less than.

MR. HAZELTON: -- on the basis of density differences?

MR. DOMEY: No, not on the basis of any density difference, but based on the knowledge of the capability of the image enhancement as a measuring tool. He just bounded it by the capability of the tool.

When questioned further, subsequently to receiving the report, the expert said it is significantly less than -- since you have asked me how much significantly, he says "The best I can do is it is significantly less than the three millimeters", which is about 120,000ths.

We asked the question can you make any definitive statement from the image enhancer that would correlate to any of the ultrasonic data that we have told you about; he was not privy to ultrasonic when he did the image enhancement. And as early as this morning, or as late as this morning, he informed me that since he does not know the precise method of how the X-ray was taken, the angle of

incident of the source, the location of the source, that he can make no statements relative to location.

And we told him the characteristics of the ultrasonic data, and asked from what he knew of the image enhancement, could he make any statements relative to the two. And he said there is no way to correlate those either.

So we left it that the image enhancement was essentially non-relevant information, which showed something which couldn't be seen precisely with visual, it could not be compared to a 2 percent penetrometer, so therefore, a size could not be put on it visually, and the image enhancement essentially didn't give us anymore measurement data.

MR. HAZELTON: You keep talking about the depth of it, what about the length of it?

MR. DOMEY: Well, we weren't specifically trying to measure length, and I have to go on my memory, because I also saw the films.

MR. HAZELTON: Regardless, it would appear if you see something about an inch long in that location, and specifically in that location, it would seem to be a tremendous coincidence to say that it wasn't somehow related.

MR. O'TOOLE: I am not sure, because the method is so marginal in this case, that I am not sure what you are seeing. And I am not sure what you are seeing is valid.

MR. HAZELTON: Not what I am seeing, what somebody

is seeing.

MR. O'TOOLE: What they are seeing. But that is what I concluded, I looked at the results and I just shrugged my shoulders.

There is one last thing to complete the answer to your question, and that is ultrasonic data. And there was ultrasonic data from the original vessel, and not only was there not any correlation between this indication, the present indication and the original data, but there was no correlation between anything and anything.

In other words, the original ultrasonic data was not correlatable with anything that we found during the ISI.

MR. HAZELTON: There was a good bit of discussion about one UT indication that was found that was significantly less than reflection from three-tenths deep notch by one inch long, in the belt line of the vessel and that would be monitored on the ISI program, and we don't know exactly where that was, but you are telling me, I think, that you have taken a look at the detailed pre-service that was done with the rubber wheel, and all that sort of stuff, and you see no correlation.

MR. WASILENKO: George Wasilenko, from Con Edison.

We particularly tried to correlate this indication with those results, and we could not locate that. We did a gross correlation, and you couldn't see any obvious pattern.

In one case we tried to locate with the tool an indication that was on the early map, and we could not do that.

So the conclusion was that in general there is no correlation. We haven't made a specific dimensional check, point by point, we were primarily concerned with this indication.

MR. O'TOOLE: Excuse me, maybe just to close this part out, one other item was the photograph, and I think you have seen the photograph, and I think the photograph showed something with a good correlation to position. There was a patch, opitcally that showed as a shiny -- what appeared to be a shiny patch, it could have been a repainted area, after having local dressing or grinding, or something.

There was no obvious depth to that, that any trained eye could find, but it is a coincidence that --

MR. HAZELTON: And you have definitely been able to show that this UT reflector is in the area of that light colored patch.

MR. O'TOOLE: Yes, that is a fairly decent correlation, just by looking at the position of the nozzle and looking at the bottom penetration of the vessel, you can get a very good correlation of that.

MR. ADAMONIS: I need to make a clarification,

on the show UP results. Subsequent to having gone through the quick exercise of trying to locate reflectors that were identified in that report, we got a hold of the procedures that were used. This was going on while the record search was going on at CE. And it turns out that the entire vessel belt line region was examined using a wheel transducer, and the wheel transducer is 45 degrees sheer wave and straight beam.

The circumferencial scan with the angle beam search unit was in the clockwise direction when the vessel was viewed from the top. And the axial scan was done from top to bottom.

There was some question when we had the post UT whether they recorded the position of the indication, the position of the reflector, so that is the kind of information that we were armed with. We had a map, or a large blow-up of the vessel laid out and just some identification of the number of reflectors that were found with a little mark in each particular area, not a great deal of meat with respect to the location.

MR. O'TOOLE: One other possibility was when they weld the lower shell course to the intermediate shell course together -- there are tie bars that used in fabrication -- and tie bars were used on this vessel. They were used at several locations, which combustion in their records apparently has the general location was. None was specified to have been located at this position.

However, as I understand it, George, is that still a valid statement that 180 degrees from this position there was one. Is that a correct statement?

MR. JACKSON: Yes.

MR. O'TOOLE: And that seems like a weak correlation. There is a 180-degree symmetry at that stage of fabrication, and so it could be that kind of thing.

MR. CHENG: I have one more question regarding the trip report. The Westinghouse trip report indicates that perhaps the notch was put in over there -- and I have raised this question when I was in Pittsburgh, and I think, either from you or somebody told me that there is another internal report or trip report which has a completely opposite indication, that there was no notch put over there.

MR. ADAMONIS: There were two visits made in the period of a week, I believe, a very short time frame. The report that described the examination of the vessel lower shell course described calibration with 45-degree shear wave on a

.240 or a .250 inch deep buttress notch machined on the outside surface of the shell.

There was a trip report from days later -- and I can't remember the exact time frame -- that described examination of vessel nozzle-to-shell welds, with a different set of examinations.

MR. FLACH: Don, I think part of that -- if I remember the old '65 Section 3, I think it talked about those buttress notches being placed in the production piece when they were talking about forgings, not in the examination of plates. I may be wrong, but -- and I believe combustion used those buttress notches in their shell blocks as late as '71 or so in there, before they went away from the buttress type notches, in addition to the side drill holes.

It could be that they used the buttress notch for the nozzle-to-shell weld since that's in the range of forging.

MR. ADAMONIS: No, I'm saying there are two pieces of paper, two different trip reports. One clearly states that the notch was machined in the OD of the shell, and they did it in --

MR. HAZELTON: These reports were written by the same person, or different people?

MR. ADAMONIS: The second report I don't have the cover sheet for.

MR. GIESKE: If that was a machined notch, you

wouldn't expect a real sharp corner on that notch. Wouldn't it be true that you would expect to see that on a zero-degree 5 megahertz transducer, straight down?

MR. ADAMONIS: We can't see the buttress notches --

MR. GIESKE: That's right, but if it was a notch, wouldn't you expect to see that tip -- that quarter of an inch --

MR. ADAMONIS: You're saying a buttress notch, at an angle, with a straight side and an angle, and I don't see buttress notches in my mockups.

MR. GIESKE: That's right, and how would you machine it so that you didn't get a little bit of a curvature right at that point in that vessel that you can't see that with a 5-megahertz transducer, and you'd look with plenty of gain, and you looked with a 5-megahertz zero-degree --

MR. ADAMONIS: Is it a correct statement that I'm making, that we did not see the buttress notches in the mockups?

MR. HAZELTON: You did put buttress mockups and you didn't see them with straight beam?

MR. LEFEBVRE: We did put them in. And I'm not so sure -- I believe we did see them, but I can't see I actually remember them, but I can't subscribe to your theoretical bit that if that was a machined notch in that vessel and it had the sharp corner, that it would have been left there.

MR. FOX: Bernie, let me respond to that. When we

were putting the results together for the final report, the question and the data was gone over for the zero-degree information, and that question was asked at that time, and we could not see the V-notch at that point in time. So, the buttress notch at that point in time could not be seen.

MR. ADAMONIS: Nor could the 90-degree reflector, 45 degrees on the side.

MR. JOHNSTON: You mean a couple of weeks ago, John, when you were putting the final report together?

MR. FOX: When this final report was being put together, yes.

MR. JOHNSTON: What you are saying is, I presume, that somebody looked in the notebook that the people were using, and the sheets of paper, when they were going through the process, and they had something on their piece of paper that said in this particular location look for an indication, and they got something that says nothing seen, or something of that sort?

MR. FOX: I don't know that that information was put down on a data sheet. That was asked of the operators and that was asked of the people who observed the test.

MR. DURR: That seems a simple thing to confirm.

MR. HUM: You did see, with the straight beam examination, the rather shallow notch that I think is something like 1/8th or something like that?

MR. ADAMONIS: The flat bottom?

MR. HUM: Or the flat notches.

MR. ADAMONIS: Yes, we did. The one that was 180,000 speed we saw.

MR. HUM: How wide?

MR. FOX: All the information says an eighth of an inch.

MR. HUM: So an eighth of an inch wide.

MR. FOX: Three-sixteenths.

MR. ADAMONIS: It had to go to three-sixteenths on the deeper ones because of difficulty in maching, but all of the slots -- 1/10th of an inch, 3/10ths, 5/10ths -- the 2 percent deep were 1/8th of an inch wide. When we went to the 1 1/2 inch deep and 2 inch deep, they were 3/16ths of an inch wide.

MR. DURR: Was there a rationale for not using a 60-degree pitch-catch, seeing as how that was the one that gave you the strongest reflection, back reflection? Is there some rationale for not using a 60-degree?

MR. ADAMONIS: From the standpoint of how we would have to locate them on an array and put them pretty far apart, and also considering the amount of spread we see with a 60-degree.

We are subjecting ourselves, when we go to the 60-degree, a lot more of the effects of cladding, problems with positioning. Again, now I've got to make an array that is quite

a bit larger, to get those transducers out-board far enough in order to effect the examination. That was the rationale for going 45.

MR. DURR: Do you think you would get any more information from a 60 than a 45, assuming that you could meet the geometry?

MR. ADAMONIS: No, I don't believe so.

MR. GIESKE: Well, weren't you curious to see if that signal did occur at 60 degrees in the delta technique? Didn't you look for the signal with the 60-degree delta at all?

MR. ADAMONIS: No.

MR. GIESKE: You didn't even consider it as being a possibility --

MR. LEFEBVRE: We weren't prepared for that, no, but a special plate was made for 45s. There were no 60s on that plate.

MR. CHENG: I guess he was asking when you made the inspection, did you try to use the 60-degree --

MR. ADAMONIS: The arrangement of the transducers during the detection mode is such that it won't allow that. This is a special array plate that allowed us to do the delta measurement.

MR. HUM: On the actual vessel, on your eight data-points, did you see an indication from both the tip and the base, or are you only seeing the tip?

MR. ADAMONIS: On at least four, perhaps six, we do see two signals.

MR. HUM: What is the wall thickness measurement at the location of the indication?

MR. ADAMONIS: 8.9.

MR. CHENG: By zero-degree measurement?

MR. ADAMONIS: By straight beam.

MR. HUM: Were there any measurements above that, I mean, greater than that?

MR. ADAMONIS: When we made the scans with the 5-megahertz straight beam, we took that information and plotted it up, and we saw variations that -- and this covered the area that bounded the indication, perhaps a couple degrees on either side and 3 inches along the axis -- and when we looked at that information, plotted the position of the back wall, 8.9 to 9 inches is the only variation that we could see, and that covers the area where the indication was located.

MR. GIESKE: Are you saying that a 9-inch depth is possible right where the indication is?

MR. ADAMONIS: A 9-inch thickness?

MR. GIESKE: Thickness, from what you just said. I mean, in other words, we could say plus-or-minus .1 --

MR. ADAMONIS: I'm going to have to ask Dave Kurek. Right at the indication location -- we made numerous measurements. Those measurements were made up and videotaped. We

made numerous measurements in the area of interest, especially when we were setting up the delta, and we came up with the 8.9 at that particular location.

The range that I'm talking about is a volume or a surface area that might be 4 degrees by 4 inches that would show that kind of variation.

MR. GIESKE: What variation would you put at the exact position where this indication is?

MR. ADAMONIS: I have to say, at the indication, all the measurements we made, we came up with 8.9 inches, and we looked at that a number of times.

MR. HUM: Are you now assuming that you are establishing the depth from a subtraction from the tip from the actual measured wall thickness -- are you saying that the depth was based on the difference in transit time between the tip and the base of the flaw?

MR. ADAMONIS: No, I think we are saying you can look at it both ways and, if you look at it one way you come up with .24, based on total transit time you come up with .24, it you base it on the difference between the two pulses, you come up with .18.

MR. FOX: Martin, let me answer that question in a slightly different way. There was a question posed by NRC in the meeting at Pittsburgh. The meeting -- in the meeting at Pittsburgh, they asked us to establish some uncertainty data

on the delta technique.

Numerous measurements were made with numerous operators, with the equipment run in two directions along the length of the indication, and they were all essentially shaken out, and the uncertainty bound that came out of that was .2 microseconds on the delta technique, using what we call the first or total transit time.

That technique was considered to be that uncertainty was considered to encompass variations in the angle of the sound beam, the clad thickness, geometry, all of those things which could influence that.

We found that the numbers that we got off would be, if you will, the subtraction of the total time applied to the tip versus the total time applied to whatever is giving us the second indication, and we considered that to be the bottom of the reflector or the intercept between the reflector and the OD surface.

We found that difference essentially subtracted throughout the majority of those variables because it came out consistently to be representative of the number that we got off of the .18 inch notch.

While we saw variations along the length of the reflector on specific indexes, the reflector and the reactor vessel, we found that those two stayed and essentially correlated. As you traveled across the reflector, we found that those two

stayed that distance apart. So we found that to be a better correlation, or a more exact correlation that was not subject to the uncertainty bound of the total transit time.

So one of the questions regarding -- that are really coming out about the thickness variations and that type of situation are thrown into that, if you will, uncertainty bound that they put .2 microseconds on the total transit time, however, most of that gets factored out when you start talking about the differences between the two peaks.

MR. JOHNSTON: Let me interpose just a second. I've tried to track the questions as we've been going. At one point, we were starting to talk about the calibration work that was done at the Westinghouse place, and then we've drifted now, I think, into discussing the vessel inspection itself.

It might be more fruitful if we kept the two separated if we can. We have a line of questions going on now that relate to the inspection that took place at Indian Point itself and of details of it.

Would it be useful if we asked all those kinds of questions and then remember to go back to the questions that are going to have to do with the data that's been taken since that time, as part of our questions that we asked you. I don't want them to get lost.

MR. CHENG: One of the questions we were asking was the notch -- the region has concerns, you know, translated

back to the vessel. That's what the whole interest has been.

I have a question on this uncertainty band. We're talking here about 2 microseconds?

MR. ADAMONIS: Yes.

MR. CHENG: Which, I guess, translated to be 2/10ths of an inch, or something like that. That was not added to this final number you people --

MR. ADAMONIS: No, nor was that statistically determined. That was just based on putting all the dots on the plot, the plot of actual depth versus predicted depth, with our ideal model line drawn on it, and just filling in all the points and saying, okay, those are the upper modes. There was no statistical approach at all.

MR. HAZELTON: You don't have a thickness measurement on that same weld, down about a foot or so from where this reflector is? Is it a different thickness number?

MR. ADAMONIS: I would have to say no.

MR. HAZELTON: The obvious purpose is to see whether this light streak on the photograph might be a blend-out and, below that, you might have a thicker wall, but you don't have that data that would give us a better handle on --

MR. CHENG: Excuse me, Don. I thought that you mentioned earlier, within the 4-inch square assumption that you made, it would go up to 9 inch.

MR. ADAMONIS: That's correct.

12

MR. HAZELTON: But the mark on the vessel is bigger than 4 inches, so that's why I was asking the question. You may not have an answer.

MR. LEFEBVRE: We went outside of that on the arc.

MR. CLAYTON: Don, on your scans with the delta technique, where you picked up the defracted weight, did you always have the secondary pumps? On every one of the scans that you have what you considered the tip pulse, did you always have the secondary or base pulse?

MR. ADAMONIS: I would say on the last one scan, there wasn't any apparent -- but at the lower extreme or the upper extreme, there wasn't any apparent second signal.

MR. CLAYTON: On every other scan, you did have those two signals.

MR. ADAMONIS: Yes.

MR. CLAYTON: And on every one of those scans, they maintained the same proximity to each other in time?

MR. ADAMONIS: Spacing, essentially. The best you can tell is from reading the tape.

MR. DURR: If we discount the delta technique and we look at Section 11 and the requirements of Section 11 for the examination and what you do when you find an indication, do you meet today's Section 11 fracture mechanics? Have you satisfied all the requirements of Section 11?

MR. BAMFORD: No question about it. Even considering

13

that the indication is as large as originally reported, which we no longer believe, it still meets all of the criteria of Section 11.

MR. VARGA: Let me interrupt for a moment. We've been about two hours, and I would like to give the transcriber at least a little break, and anyone else that might need one. Would this be an appropriate time to take about a ten-minute break?

MR. JOHNSTON: Yes.

MR. VARGA: All right. Let's meet back here in about ten minutes.

(Whereupon, a short recess was taken.)

MR. VARGA: I guess we were still in the question and answer stage, and I guess we are going to try to focus attentions on meaningful areas and meaningful responses and move ahead. So now let's get back on the record.

MR. HUM: Could we perhaps discuss how the length of the indication was adjusted?

MR. ADAMONIS: If you recall, there was a 1-T block. The spacing between those -- and there's some data in here that indicates the amplitudes and the sizes of the notches -- we found that on that particular block, even the spacing between notches of 1 1/2 inches wasn't discernible if we tried to make length measurements, so attempts at length measurements were aborted.

We fabricated a 2-T block with more length in between -- 3 inches between notches on the 2-T block -- and took information from the notches that were in the amplitude range that we saw from the reflector in the vessel, with TR-27, and determined how much those lengths were exaggerated, looked at the mean and the standard deviation, and applied that kind of analysis to the indication -- actually a correction factor, a conservative correction factor -- and applied that to the information that we had off the length of the vessel, the reflector in the vessel.

MR. JOHNSTON: I forgot the detail. What the magnification ratio that you were talking about in this last operation. You recall when we were up there, we made some recalculations on what it was from 7-to-1 down to about 3-to-1.

MR. FOX: 7.79.

MR. JOHNSTON: Are you still using 7.79 on this correction?

MR. FOX: No. 7.79 was the correction factor for depth. That's the oversizing or the exaggeration in depth, and that's a percentage. By that I mean that it's 779 percent oversized, based on the numbers that we're seeing of the amplitude range that we're talking about for the reflector in the vessel.

For the correction factor in length, since that number

essentially agreed with the results of the delta information from the reflector in the vessel, it likened that we could also come up with a correction factor for an amplitude for essentially what consists of the size of the sound beam for those amplitudes, for those reflectors which showed an amplitude the same as the indication in the vessel, and correct by what we will call the constant K. Constant K was arrived at as taking all the numbers, determining what the oversize was in inches, and taking the mean of that, the standard deviation of that, subtracting the standard deviation and the mean to arrive at a number of inches that could be subtracted.

MR. CHENG: Is that a good way to do it because in this you are including all this measurements from the different depths of the notches. And you know the different depths, you know, have different types of modifications.

MR. FOX: It showed that it was not dependent on depth. It was dependent on length. The numbers we threw in were from a smorgasbord, and it didn't matter --

MR. CHENG: No, no. I'm sorry. I'm talking about length. We are discussing the length here.

MR. FOX: That's what I'm saying. It did not matter on the depth of the indication.

MR. CLAYTON: John, on that table of measured lengths versus actual lengths on notches, we find that the number you used was derived based upon a statistical average and then

subtracting the standard deviation from that. How do you support that as opposed to taking the most conservative number on those pages, or the most conservative number on, say, a notch of what you feel is the same depth as the indication?

MR. FOX: If you went with the worst case analysis, the number that would have been used would have been somewhat around, I believe, 8/10ths of an inch whereas the number used was 1.1. That number was way out in left field as well, which means that essentially it was way outside of a standard deviation away. So, just from a statistical standpoint, it says exactly what is coming back, is that that number is not part of the data set, from that standpoint.

MR. CLAYTON: Can you really say that when you know the conditions under which you're examining, which is through cladding? You can hardly throw away anything because deviations like that can occur and can be real, and can be giving you real numbers. It's not like you did not have other variables. That was my question.

We know that there is going to be a significant variation in how a notch or an indication appears relative to what part of the cladding you happen to be going through at that time, and so forth. And so those variations, instead of being errors, might just be how the sound beam happens to be performing at that particular point. I'm wondering how you can just throw that out as an error.

17

MR. FOX: Bill, let's arrive at the objective of the report, as stated in the report, again. The objective of the report was to try to come up with as accurate a projection of the size of the indication of the vessel as possible, with some degree of conservatism.

It was felt that taking a number that was way outside of a standard deviation away from a length projection, when the number that had already been shown by the depty projection using the same type of approximation, turned out to be a good predictor of what the delta said.

So, using that analogy, you can say that that is a very good approximation, and also a very conservative approximation, of what the length is. It would have been an error for us to use the mean, even though that turned out to be the best predictor of the depth, was the mean.

So that would not be conservative, so instead we used the standard deviation.

MR. CLAYTON: One last question. On the depth correction that you did, that came out with the 7.79 number, did you also throw out that 13 times number that was on the .3 notch that was way out from a liberal standpoint, when you did that one? Do you know which one I'm talking about?

MR. FOX: I'm not exactly --

MR. CLAYTON: On the .3 inch deep notch in the plus-10 to plus-20 dB range, it was a 13 times number.

18

MR. FOX: No, because in that situation we were also -- that number isn't being relied on essentially by itself.

MR. CLAYTON: But you used that to determine the 7.79 number?

MR. FOX: I think so. I'd have to go back to my notes. I can't say exactly because I don't remember exactly which one -- do you remember exactly which one he is talking about?

MR. ADAMONIS: It must be this one here.

MR. FOX: So that number was used.

MR. CHENG: Yes, you used the number. And all together, you have four points only, only four datapoints. In this measurement, you used ten datapoints?

MR. FOX: Yes. If you use essentially all of the datapoints, the number came out 5.74.

MR. CHENG: And you only used the four datapoints to come out at 7.79?

MR. FOX: Yes, only the information with TR-27.

MR. JOHNSTON: I'd like to see if we can come to some kind of closure. Are there other questions regarding that length? No more questions on the length?

MR. O'TOOLE: You may have some questions after John gets through with his summary. You know, there is some overlap because they are doing the same job separately. So maybe we could have John take over.

MR. FOX: Well, a lot of the information I would normally give in a summary, in an overview of where we're at and the types of information that have been processed has already come out in the question and answer period, so I think what I will do is to drop back slightly, not belabor the point, and just kind of summarize.

I will state the objective of the program, again, was to come out with the most accurate projection that we could possibly give, with some conservatism. The results were taken with the delta technique. Results were taken on two mockups with the delta technique, pitch-catch technique and pulse echo.

You've already heard the number of datapoints that we're talking about. You've seen the tables of the datapoints that we're essentially talking about.

The conclusion should be restated. Essentially, the conclusion was that we arrived at a reflector that was .26 inches by .85 inches, that's depth to length.

That was the information that came out of the amplitude case criteria, and that was essentially the worst case of the numbers that we ran. That would assume that of the methods that we processed, that's the information that came out.

The pitch-catch information essentially supported the fact that it could not be as large as the 1.5 inch or some number that was much bigger than that. The delta technique --

we essentially found that there were two techniques inherent in the delta technique, or two methods of sizing based on the delta technique. One of them, which is the through-transmission time, or essentially the first point on the time of flight, showed that the indication could best be described as something in the order of .24 inches in depth, as the best predictive value. However, we felt that that was subject to some irregularity, some uncertainty due to irregularities in thickness, clad, so on and so forth, angle, and that those irregularities did not occur as greatly occur in the differential, the difference between the two peaks, and that number could best be described at the worst case as the same as the 2 percent notch, or .18 inches. That's the best way we could characterize the difference between those two reflectors on the reactor vessel.

The majority of the data that we took, or the statistical information that we took off of the amplitude based information was based on all of the information that we could get. It was processed many times, and we gave you the information that we feel is the most valid for that.

I think as an overview, what we are really saying is that our feeling right now is that the reflector can be no deeper than .26 inches and we are still presenting you with a very conservative number based on the backup that's been given by the delta information from both of the measurement methods.

So that would be what I characterize as an overview.

MR. JOHNSTON: You focused most heavily on a depth measurement?

MR. FOX: Yes.

MR. JOHNSTON: And a second question, among the dat that you gave us in your report, was this all pitch-catch -- I'm sorry -- delta technique, I mean, of the successively deeper notches and drill indications that we asked you questions on, had asked you to make measurements on?

What I've heard from the staff, I think, was that most of what you gave us is amplitude data, and not delta data. Am I right? In other words, the measurements that you redid --

MR. CHENG: There is no data on the delta technique measurements on the notches. One of the questions we asked on this past Monday, we asked you people to provide measurements. All you have here is depth measurement on how deep is the indication, but the data measurement does not provide that.

MR. JOHNSTON: You've been talking about some 80 points or something. Was that included in what you sent us previously?

MR. ADAMONIS: No, I don't believe it was.

MR. JOHNSTON: I guess then there is some information you haven't supplied us yet, which was the specific information that we asked. We asked you to make measurements on what became plate 2-T, by the delta technique, looking at indicators

with depths wherever we said. We gave you a list up to 2 inches deep. We don't have that data yet, officially, but you've got it over there?

MR. ADAMONIS: That's right.

MR. JOHNSTON: How's come? How's come we didn't get it along with the input that came in a few days' ago, since that was a key piece of information we asked you to get us to help us make our decision. Why the delay?

MR. WASILENKO: It was never my personal understanding that you asked specifically for specific types of data on a delta. You asked us to do measurements, and we summarized these measurements in our report. It was not clear to me that you wanted in-depth data comparable to the type that you did submit in support of our length and amplitude exaggerations.

So, I'm kind of surprised that you're saying you asked for something we didn't give you. I don't recall that being --

MR. O'TOOLE: Can you point out the question?

MR. CHENG: I think in Monday's --

MR. O'TOOLE: Yes, in Monday's questions, I understand you asked for --

MR. JOHNSTON: I'm talking about question 1, I thought, of what we transmitted back in August.

MR. HUM: It was question 1-A, B and C. And the thrust of this was that the licensee supply the basis of the

delta measurements and the pitch-catch measurements and try to simulate the conditions that he observed in the reactor vessel. I would have assumed that this would have included the table of the actual sizes into the notches, the measured values that you derived from your '80 measurements and any deviations from these measurements that kind of indicate what the accuracy of the measurements would be, and the validity of the technique.

MR. WASILENKO: I'd like to comment on that. Most of the question 1 were statements. And our answer was we did basically have all this stuff, and we described that.

MR. JOHNSTON: Question 1, confirmatory testing with basic calibration block and reactor vessel mockup. Additional artificial reflectors should be introduced into the vessel mockup to simulate crack degrees, depth of the OD of the vessel, should determine the maximum size crack that would produce an ultrasonic response similar to that observed during reactor vessel exam, and normal scanning and the evaluation exams with pitch-catch and delta techniques. They should have a length and orientation that is the same as the actual flaw indication. The depth of the artificial reflector should include the allowable flaw signs based upon item 3,000, Section 11, of 1/2, 1 inch, 1 1/2 inch, 2 inches.

That's a request to make a series of measurements using the various techniques, particularly the delta. What the staff told me is they didn't get an answer to this, and

that's the reason for my question of how's come because that was the most -- in one sense, one of the most important pieces that we discussed with you when we gave you the closeout discussion up there. We said that's the data that we really want you to get so we'd know --

MR. O'TOOLE: We refer to Attachment A as the table, Results and Conclusion in Attachment A. Where is Attachment A? Are we saying -- I don't know what we are saying at this point -- but are we saying, in answer to Bill Johnston's question, that Table I and Table II, all those tables, don't include anything with delta technique?

MR. WASILENKO: It's not obvious from your question that you needed that information.

MR. JOHNSTON: I thought it was pretty obvious from the question what we needed, particularly since we even emphasized it when we closed out up there at Pittsburgh. We said we tentatively felt that the data that you presented us there when we went through the calculation on that block that day looked pretty good, but we wouldn't be able to draw any conclusions until you had made the additional measurements with the additional indicators, using this technique, which was precisely this question. We said that very clearly, I think.

MR. O'TOOLE: Do we have the data? Do we have it here, now?

MR. ADAMONIS: Yes.

MR. O'TOOLE: Why don't we give it to them.

MR. ADAMONIS: Why don't we go over the plots. As we initially discussed, we had put together a mathematical model on a delta technique, and we put together the model based on the reflector depth versus transit time. This line represents what we would predict.

What I have here are three plots that were made up which summarize all of the measurements from which we started to talk in terms of a plus-or-minus 2 microseconds uncertainty limit.

We had a notch at 3/10ths, at 5/10ths, an inch, 1 1/2 inches -- actually this one was about .875, this was a slot cut in, it was a subsurface slot near the outside on the 1-T block, so the maximum depth from the outside surface was .875 -- 1 1/2 inch deed and a 2 inch deep.

And you will recall that this 2 inch deep actually stepped between 1.85 to 2 inches deep.

The different figures which represent the actual measurements made on the block represent the scan direction and the different transducer arrangements, thus, this dot would indicate TR 22-20 in a counterclockwise direction looking at naught, and I've broken these up as far as the various shifts went, but if you took all of this data and put it on one plot, those would represent the datapoints that we were talking about previously.

MR. JOHNSTON: Don, when you say mathematical model, isn't that just a 45-degree plot? No, I guess that's not a 45-degree plot, it was just a plot of depth versus time.

MR. ADAMONIS: That's right. Well, you calculate based on the angle and the velocity at shear, to get to the reflector, and then calculate the path back to the transducer based on the longitudinal wave velocity, and you come up with, at this point, at point zero would represent a zero-inch depth on a 9-inch thick standard. This number would change, for an 8.9 inch block.

We made all these measurements on a block which was 9 inches thick, our 2-T block was 9 inches thick in the area of interest, and if you took all that and put it on one figure, you could bound it by essentially 2 microseconds, and that's all the data -- no statistical approach or anything, as far as handling this spread.

MR. JOHNSTON: I was going to ask if you took the reproducability of data to give depth and treated that statistically -- I think you have enough datapoints in some cases to do that --

MR. ADAMONIS: I have not done that.

MR. JOHNSTON: -- and get an idea of what the uncertainty is in measurement of a given depth.

MR. ADAMONIS: I have not done that.

MR. O'TOOLE: It's not that we didn't -- we tried to

27

evade anything here. We thought we were answering the question. When the question came up when George was making the report, how much data should we put in here, you know? You're asking a question that doesn't need data to answer, but we answered your question with the thing determined from all the tests, and when we did put the data in, we decided to be selective, and we have it. I guess it's a matter of how you want it.

MR. JOHNSTON: I think that should be supplemented for the record.

MR. FOX: That is the data strictly from the --

MR. JOHNSTON: 2-T?

MR. ADAMONIS: Total transit time measurement.

MR. FOX: -- total transit time measurement.

MR. JOHNSTON: How about the delta --

MR. ADAMONIS: That's it. That's the delta based on total transit time.

MR. JOHNSTON: I guess that question had another part in it, about pitch-catch information. That would be the data that, I guess, would show whether the signal disappears or not as you get deeper and deeper in the notch. You mentioned it this morning, but is that in the record for us, too?

MR. ADAMONIS: Only to the extent that we make the statement that for the 1 1/2 inch deep and 2 inch deep notches, you effectively lose the signal for a range as you move across it, and it's very clear.

28

MR. JOHNSTON: Is it still there for the 1 inch?

MR. ADAMONIS: We really didn't repeat any measurements for the purposes of this particular investigation on the 1 inch when we recognized that the radius of curvature was different.

MR. FOX: We essentially reviewed -- rereviewed the videotapes from the original data that we ran on 1-T and looked at that again, and renoted the fact that there was a definite drop in the reflector when it ran across the 1 inch, but it was still within, or it could not be huge compared to the variations over the -- the large variations we'd seen over another portion of the vessel, which was a 50 to 90 percent fluctuation.

MR. JOHNSTON: The radius of curvature on the 1-T block, was that closer to that of the Indian Point vessel and the 2-T?

MR. ADAMONIS: The 2-T is identical.

MR. JOHNSTON: Oh, the 2-T is the closest. We did this in geometry while we were there, and concluded that you were able to transfer from flat to curved blocks pretty well anyway. There shouldn't be any problem converting back and forth between these two different radii, should there?

MR. ADAMONIS: That's correct, but we also had noted the difference in attenuation, at least in the angle of the mode between the two different blocks and the vessel, and we

-- when we went back and made the pitch-catch measurements on the 2-T block, we could use the same gain settings we had on the vessel and see the same kinds of variation. If you use those exact same gain settings on the 1-T block, the amplitude, the through transmission amplitude is higher.

MR. JOHNSTON: And your 2-T block did have a 1 inch notch or something in it, didn't it? You said you had everything from 1/10th to 2 inches?

MR. ADAMONIS: Well, I didn't mean everything, in the range of tenth of an inch. I don't believe there was -- there was a 1/2 inch, there was a 1 1/2 inch and a 1.85.

MR. JOHNSTON: Nothing in between a half inch and 1 1/2?

MR. ADAMONIS: No.

MR. FOX: The reason being we already had two notches that were 1 inch in depth, and we didn't want to overpopulate that block, too.

MR. CHENG: We asked you people to docket important information. You say you have a 70 or 80 point on this one. I'd like to see some table submitted on the depth measurement, and including some of this stuff you have -- you know, what kind of notch size, what kind of numbers you produced from that.

MR. DURR: When you did the delta technique in the vessel, when you initially did the scan with the 60 and the 45

and the array and you ended up with 500 plus percent signals on the 60 and 200 percent plus on the 45, when you did the delta technique in the vessel, did you also end up with a comparable pulse echo signal on the 45 that would compare with the 200 percent DAC signal?

MR. FOX: Yes, that's correct. When we were in position to see the delta signal from the reflector, say, with the transducer 22-20 combination, we could go back and look at the -- switch channels and I could program TR 22 in the pulse echo mode, yes, you could see it.

MR. DURR: It was essentially a 200 percent signal, so you were sure you had the same --

MR. ADAMONIS: Well, when we did the delta work, we did have to increase the gain to get the delta signal over and above that that was used for the examination, but if you re-established all the calibration parameters with TR 22, which was the 45 degree in the counterclockwise direction, yes, that amplitude and location was verified.

MR. CHENG: I would like to follow up on one of the questions we asked this past Monday about this crack, the fatigue crack. I don't know if anybody has followed up on that. I was just curious. I think you people responded there are no such signs of crack under the variables. I talked to Bob Spring this past Monday. Have you people looked into everything?

MR. ADAMONIS: The ones that you are discussing are the ones that were made up -- are essentially flat blocks and have implanted defects within one-half the wall thickness of the inside surface of the vessel.

In at least two of the cases, those blocks are 11 inches thick, and may be a case where, in fact, it's 9 inches thick, but all the indications are up near the inside surface. There are also some unintentional reflectors in there, in the form of slag and when they were making that particular weld.

There is nothing where we could take the same arrangement and go over and utilize that type of thing. I am aware of the ones that you are talking about.

MR. CHENG: In this case, I'm interested -- you don't have to shoot from inside. I'm interested in the delta technique from all the surface. I guess one of the questions is trying to see how good it can measure the fatigue crack because the concern here is, how do you know that the vessel will come from the deepest spot of the indication. That was quite commonly of interest to many people here. There may be one way you measure, say, you have a real fatigue crack over there with a known depth, and you always have a flat block, you can use the delta technique, indeed, you can size the fatigue crack very well, but it may be additional support for the argument that, yes, you did see the tip at the deepest spot of indication, not necessarily from somewhere in the middle.

MR. O'TOOLE: This is a meritorious R&D program that should be done. The problem I have with that is that using the best techniques we've got, we sized the indication and we say it's within code allowable.

Now, I don't know whether this would be a separate program, or whether you are looking to that to justify what we've given you. I don't know if anybody else has done this to justify an indication in a reactor vessel. I don't know, maybe they have, but the question in my mind is, is this a generic question that we'd all like to know the answer to, or is it something that we need to know to get on with this one.

MR. CHENG: I feel this is in support of your argument. You are asking staff to accept the size you come up with based on the delta technique.

MR. O'TOOLE: There's a lot of merit in that argument, but I think still my argument holds that we used techniques probably state of the art techniques that many others haven't even used in justifying indications that they had on vessels that were acceptable or unacceptable, I don't know which, but they used state of the art type of thing. We did beyond that.

And I guess the question is, do we move the state of the art even further? Do any of you gentlemen have any thought on that?

MR. CHENG: Let me comment on that. One of the reasons we are asking so many questions about this, you just

mentioned this. You are using the non-code technique, the state of the art technique, based on the Section 11 code obviously is not acceptable. When you try to use this non-code procedure to justify is within the code allowable, and there is nothing wrong with that. Obviously, you know that I will do that, I will try to consider how people are going to accept this non-code procedure, and one way to do it, what I would consider a prudent way to do it is to do a couple of similar techniques to support. Although this is a non-code procedure, however, we have looked on this technique, looked on this angle, and this should all support this number. We didn't accept the number, otherwise, you only come in with one technique and say, this is the number and it is within the code allowable, so everything is okay. I can see that we would have some difficulty for people to accept this kind of argument.

MR. O'TOOLE: It's hard to argue with 100 percent assurety. You know, I think that there are two things that can happen, I guess, if you look at fatigue cracks in a block you can prove that, indeed, we can detect them adequately with our method, and that what we are seeing is what we are seeing, and maybe it is a fatigue crack that we have that is .24. The answer then is still acceptable under the code and we should go.

I guess the only answer that you are suspecting might

come out is that there's some fraction or whatever effect that might take place in a fatigue type crack that would give you an underestimate by the methods that we're using. Is that the concern, that we are underestimating what we have?

And I kind of thought that all that we've done has exhausted all of the known methods of determining whether we're underestimating or overestimating, and we've determined we've overestimated by factors of 7. And it's kind of hard for me to see how we've got to go back down and do this fundamental work.

MR. FOX: The only thing I'd like to say is that most of -- or all of the work that has been done has been on -- most of the work that has been done has been done on plane reflectors that have been put in there, which are supposed to be simulating the worst case event in the vessel.

In the event -- you can always -- I think that the results off of the T-crack study may be conclusive or non-conclusive because if you -- let's hypothesize that it didn't work, which the literature doesn't support that that will be the answer, but let's say it didn't, then could you categorically say then that if it didn't work, that what you have in the reactor vessel is not a crack since it does behave like the notches in the mockup? That is obviously a tangent that could be -- an approach that could be taken. So I don't know that the results of that would be conclusive.

I think what has been embarked on here is a reasonable program within the time constraints and with the size of the program, to prove that the technique has merit and is, if you will, conservative. And the rest of it we have to essentially rely on what is state of the art literature says defraction information tells you, and that information is fairly common, fairly public domain, and is being used.

MR. O'TOOLE: I guess the concern I also have is the geometric concern. You know, we found that geometry has an awful lot to do with the results we're getting in here. We are talking about two blocks that, according to Don's description, don't even simulate their thickness. They are flat and there are cracks in them that I don't know whether they are fully characterized and actually known, but maybe they are, but when you get that result, you know what you do with it. That's what concerned me more than anything.

MR. ADAMONIS: They were designed to do work from the inside, looking at reflectors essentially within the inner core or the T. There are some reflectors at what you would call a half-T, but they are in the inner core of the T. I don't even know what the condition of the outside surface is.

MR. JOHNSTON: My only concern in listening to this conversation is that it didn't take place about three or four weeks ago when we first asked that you make these kinds of measurements.

What we got back at that time was a lot of "We don't know what you're talking about, never heard of it, we don't think there are any such samples", and so forth. I would have liked to have had this kind or this quality of discussion back then instead of the quality that we had.

I think we could talk techniquely about this kind of thing. It's a little bit late now.

MR. O'TOOLE: There was a conversation on the telephone, Bill. I wasn't part of this, but on that question, and--

MR. DOMEY: It was a conference call we had with George and Bob and myself.

MR. O'TOOLE: But I don't know what the -- they talked about that question, and I think we reacted consistent with our reaction today, at that time, at least that was our intention.

MR. CHENG: No, no. The conference call was to try to clarify the question. I remember we talked two or three times in a conference call. You people came back saying we don't understand the question, what do you want, and we explained what we wanted. We never came to the discussion we are talking about here.

MR. O'TOOLE: This couldn't come out. We didn't have that information at that time. We have all been working on this thing full-time, trying to get it resolved. When this information came out, I don't think that's an applicable --

in my judgment, I don't think taking those blocks and doing anything with them is going to shed any light on this problem. I think it's going to confuse us more than help us. That's my judgment. Now, it's one more bit of data that maybe we will all learn something from, but I'm not sure it will focus on what we are trying to get at here.

MR. JOHNSTON: Well, I guess what I'm trying to say is that I think you have raised some legitimate technical questions. I think we have some legitimate technical comebacks that I think we would like to have with you, and I would have liked to have this quality of conversation sooner so that we could, if we decided to do it, be able to do it and have the information available to help us make our decision.

There's always a risk on getting more information on any subject. You might an answer you like or you might get an answer you don't like, that's quite true, that's a risk, and that's something to negotiate.

It's a little bit late now for us to be doing much negotiation with you in terms of helping us to get the plant started up right now. I don't think we can get the data information in a short time period. I think we probably could have gotten some of this possibly, if we'd agreed to do it, we probably could have had some indications as to whether this technique can see the cracks or not. If you'd done it sooner, I think we think it would have been helpful to us to have had

that kind of information, to be able to say, if it came out that way, that we could answer people's concerns that the artificial notches are one thing and real things are something else, and how do you make the bridge, how do you bridge the gap, and if there was any way to do it, I would have like to have had that information available. Apparently the communication process, it seems like it takes us a long time to find out what we are trying to get.

MR. O'TOOLE: Once the question came in, we addressed it just like we addressed all the rest of them. We had people working on the problem. What we determined was that it was not feasible within any reasonable time frame, within the outage time frame, to take a nozzle drop-out type of block, with the curvature of the vessel, and put a real crack in it.

MR. JOHNSTON: We didn't ask for that.

MR. O'TOOLE: We determined that. We spent some time determining that because that, in our minds, would have been a technical confirmation that would have been very useful for this, an outside diameter crack on a curved block 9 inches thick. We looked at that. That, in our mind, would be a valid thing to do, if you had the time and money to do it.

Then this other question of are there any blocks with cracks in them came up, and it took time to determine that. So, I'm sorry if we didn't respond.

MR. DURR: On your .26 dimension, does that include

error? Or if I apply your error band, does that get bigger?

MR. ADAMONIS: It includes the first standard deviation, one standard deviation of the exaggeration factor. The .24 from the delta does not include any error band because there has not been one statistically developed for it.

MR. CHENG: Are you sure that's the correct answer because I asked that same question earlier. I thougnt that you indicated you did not include it in the error band over there at .26 inch.

MR. FOX: Let me clarify that. The .26 number was determined from the mean of the exaggeration factor and depth, the amplitude. The .24 was determined from the mean or is not a mean because we didn't use a statistical number, but essentially the model that was used for the delta so, therefore, it does not consider the uncertainty.

The .18 came from the best approximation of what we could get off of the delta differential technique.

MR. HUM: There's still one outstanding question on question number 4, about the evaluation of data. It says that we requested that you reassess the relevant and non-relevant indications because you wanted to get some confidence that there were no indications, relevant and non-relevant indications because the staff wanted to get some confidence that there were no flat line indications in the relevant and non-relevant indications.

I'm not sure that response really addressed that subject in the sense that it does define certain laminar type reflectors and certain spot type reflectors which I think staff understands. The other one, at least the development ones, indicated that they were acceptable in the code.

I still would like to know whether in your interpretation of the data, when you re-evaluated, whether these other reflector lenses, whether there were any cracks.

MR. ADAMONIS: When we went back through the report, Martin, we went through the entire report, reviewed all the information that was presented, including the 49 indications that are noted in your report.

Of that 49, 29 were mid-plate laminations; 5 had -- no, 29 were either mid-plate laminations or spot, essentially no measurable length amplitude dropped off as soon as you moved off the indication; 5 we found in nozzle-to-shell welds; 5 were reflectors in nozzle-to-shell welds; and there were 15 indications that were left.

We went back through, checked all of our calculations. In all cases, these were indication reflectors that were found in one direction all very small, most of them well within the acceptance criteria. Nothing -- number one, nothing that was suggestive of a condition that we're looking at here, nothing associated with the outside of the vessel, at or near the outside of the vessel, and nothing that was of any great

concern in that 15 indications for a vessel examination of that scope is not unusual.

MR. HUM: I recognize that. I just wanted to make sure that there were no other evidences of cracking in any of these indications from your reassessment of the data.

MR. ADAMONIS: Our review of all the data doesn't indicate that there is anything in there that would suggest a crack.

MR. VARGA: Well, I sense a kind of a winding down at least on the frequency and intensity of the questions, so perhaps it might be a good time now for us to caucus and then reflect upon what you have presented to us, and then get back with you in like maybe 15 or 20 minutes.

Anybody have anything that they would like to say or add or ask before we break?

MR. GIESKE: I have one question. As far as Table III is concerned, you have exaggeration of depth and length versus receiver gain. Would you be able to tell me which one of those gains correspond in Table I?

MR. ADAMONIS: 20dB, it's from receiver gain 20 dB.

MR. ELLIOT: I have one question, a regulatory question. Information was given here on the fracture mechanics and a probability study. Do we get that information?

MR. VARGA: We will discuss that at the end,as to copies.

MR. ELLIOT: Could we also include the table that describes the headings on the probability study? Is that included?

MR. BAMFORD: I put that in. Even though I didn't show that as a handout, I put that in with the handout figures.

MR. VARGA: Okay. Well, you can all stay here for a while, and we will go up to Bill Johnston's -- or 412, if it's open.

(Whereupon, the panel left the room to caucus.)

MR. VARGA: Well, first, we want to thank you all for coming, it was rather short notice, and for the depth of the presentation. And based upon what we heard and the documentation of three pieces of information that were discussed here in the meeting, first, we would like to have documented that fracture mechanics discussion, including the probability analysis that we heard today; second, that delta information that Don was presenting here as a result of that misunderstanding, or perhaps misconception of that question 1, the data; third, we would like to have on the record whatever evaluation you used to locate the flaw. You discussed what information you used to locate your best estimate of where that flaw is. We would like to have that on the record.

Based on that and what we have done so far with the information that you submitted to us on the 25th of September and which has in it, I would guess, concluding stages of review

but not yet finished, and our evaluation of what you have said today, it appears that we are coming out to something like this, that we will find it acceptable probably for restart, however, there will probably be some additional requirement other than the standard ISI inspection interval for additional inspections of the reactor pressure vessel, the exact timing or the frequency of that we have not yet concluded.

Now, as far as when we will finish our evaluation, I can assure you that everyone is working diligently and extra hours on that evaluation. We had expected to get that somewhat earlier. We recognize the constraints and the resource that you all have put on it, but we had expected to get it a few weeks earlier. Consequently, our time is somewhat impacted, but we can assure you that we are going to do everything we can to complete that.

I would, at this point, not want to put a date on it, but as rapidly as we can, we are going to conclude that, but I would like to get your submittal of the three pieces of information that we requested as soon as possible.

Bill, do you have any comments?

MR. JOHNSTON: No.

MR. VARGA: John?

MR. O'TOOLE: Well, I'd just like to thank you for your prompt response. I think that your conclusions are

reasonable. I see no problem. I don't see anybody else wringing their hands here. It appears like the ISI, additional ISI, while I understand that approach, I think that's something we ought to have a little bit of interchange on, if you would permit us to. I think, not here, but along the way I think we ought to talk about that because I do think that it's over and beyond code, and obviously we do have a feeling that if you accept our evaluation as being acceptable under the code, and you propose to give us additional inspection beyond the code, we'd like to participate with you in the rationale for that because we'd all like to know why we're doing it.

MR. VARGA: Without speaking for the staff, but speaking more as a knowledgeable layman, if I may say, it appears to me that based upon the weight of the evidence and looking at it with some expertise, based upon the weight of the evidence, I think that it's highly unlikely that a reasonable regulator will find acceptable the -- your evaluation of the crack depth and the crack length. That's my impression, that the likel hood of us finding it acceptable within code is unlikely.

MR. O'TOOLE: That's the missing link that I didn't have. Anybody else got anything here on our side? Charlie?

MR. JACKSON: I have one clarification on schedule and where we are with the outage. We are now at about 170

degrees in rapid cooling system, and around 400 pounds of pressure, and we are estimating a couple more shifts of testing work before we are able or ready to proceed. We are going out of cold shutdown to our hydrostatic tests on the rapid cooling system.

I don't mean to put words in your mouth, but if I interpret your position of your budget management, that it is acceptable for us to proceed with that testing phase of the outage.

MR. VARGA: Well, is there anything in the tech spec that would prohibit you from proceeding to some point beyond cold shutdown?

MR. JACKSON: Not anything that I'm aware of other than -- you know, we have certain prerequisite tests that we must pass, which is the work that we are concluding right now.

MR. VARGA: But all of it within a Mode 5 or Mode 6?

MR. JACKSON: Yes. We would be proceeding -- our next step would be to proceed out of cold shutdown to the approximately 340 degree temperature --

MR. VARGA: On-pump heat?

MR. JACKSON: On-pump heat, all sub-critical, and we would be several days of additional testing before we could clear our next position, which is 350 degrees.

MR. VARGA: What psi would you be at?

MR. JACKSON: For the hydrostatic test, we go up to

approximately operating just over --

MR. VARGA: Well, that would be a problem, my guess would be, and I'd have to rely upon some of the staff for that. You're talking about going to operating pressure?

MR. JACKSON: Operating pressure for the integrity test.

MR. VARGA: Well, I couldn't give you a definitive answer at this point. My suggestion would be to inform us of what your plans are, send us a letter as quickly as you can about where your steps are and what the dates are that you plan to get up to those steps, but you must recognize that this is a very sensitive area that we're in. Consequently, any stress on the reactor vessel, no matter how insignificant it may appear, will require some evaluation from us simply because we requested that before you start up we approve it.

Now, startup has a definition that is interpretable. My interpretation would be, I think, that you could go to some pump heat, to some pressure, some fraction of operating pressure. Perhaps the technical staff would have a perception that it might be acceptable, but that I would have to document.

MR. O'TOOLE: Charlie, you are within a day --

MR. JACKSON: Our current estimate -- of course, that depends upon additional test results, but we are within about two shifts of current estimate before we're ready to come

out of the cold shutdown.

MR. JOHNSTON: The stress on the vessel on those conditions is real small in fracture mechanics.

MR. ELLIOT: You are going over 310 degrees Fahrenheit on your hydro test?

MR. JACKSON: We have to. It would give us a window in our tech specs, which is just between 340 and 350.

MR. VARGA: It's a technical question. Except for our letter which we had sent, it said that NRC approval is required before startup. It is our interpretation of what we mean by startup.

MR. JOHNSTON: We should have a consistent. It's not any different for this reactor than any other one.

MR. CHENG: I think your approach, you probably might want to have them come in with this schedule, when you are going to do, when you are going to reach what kind of pressure?

MR. VARGA: I would telecopy something in tomorrow, or at least in the next week --

MR. JACKSON: Why I'm asking the question was that we had proceeded to a phase of documenting our commitment that we would not proceed without concurrence of your staff, and I don't want to do that if there are any misunderstandings on what that means.

MR. VARGA: I appreciate the cooperation and the

sensitivity. I think that is well placed, and we will try to respond in kind.

MR. JACKSON: But we are close.

MR. VARGA: Anything else anyone would like?

MR. HAZELTON: When would you expect to go critical?

MR. ADAMONIS: Approximately a week. I'm giving you an approximate -- it could be sooner. We are proceeding in stages -- cold shutdown, 200 degrees retrocooling system, up to zero, our next full point is when we've administratively put on with an agreement of staff, at 350 degrees, where we must do another series of tests to verify all the remaining safeguards equipment are operable, which includes a considerable amount of closer maintenance testing, some of which you can't do until you have the indications on temperature and pressure.

The next stage would be to move to the -- heat up to the hot shutdown condition wherein we will hold for a series of tests such as the control rods, we have to do at-temperature tests, then we would go critical, and we would stay at low power for approximately three to four days for physics testing before we begin our power escalation, so the if in there is the time for testing and if there is any leakage that is unacceptable we find during this testing, of course, we stop and go tighten up.

MR. VARGA: In your letter, why don't you just list

chronologically all the things you are going to be doing and the reactor vessel conditions up to criticality.

MR. SPRING: This has been given to the project manager over the phone, by the way, in detail.

MR. VARGA: Well, put it in a letter and then --

MR. JOHNSTON: Steve, there is a tech spec definition that determines when the startup is.

MR. VARGA: Right, except that this has a pressure vessel with an indication in it. And you're right, and all I need is for the technical staff to say there is no problem up to this point, and then I have no problem.

MR. JOHNSTON: Well, we'll do that officially in a few minutes.

MR. VARGA: Very good. Thank you all for coming in.

(Whereupon, at 4:45 p.m., the meeting was adjourned.)

CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings,

IN THE MATTER OF:

INDIAN POINT 2

DATE: OCTOBER 3, 1984

PLACE: BETHESDA, MARYLAND

were held as herein appears and that this is the original transcript for the files of the Commission.

REPORTER: PHYLLIS YOUNG

SIGNED: Phyllis Young

TRANSCRIBER: NEAL R. GROSS

SIGNED: Neal R Gross