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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ATOMIC SAFETY AND LICENSING BOARD

(ASLB)

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HEARING

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In the Matter of:	
DUKE ENERGY CORPORATION	Docket Nos. 50-413-OLA 50-414-OLA
Catawba Nuclear Station,	ASLBP No. 03-815-03-OLA
Units 1 and 2	

Thursday,
July 15, 2004

The above-entitled matter came on for hearing, pursuant to notice, at 8:00 a.m.

BEFORE:

ANN MARSHALL YOUNG, Chairperson
ANTHONY J. BARATTA, Administrative Law Judge
THOMAS S. ELLEMAN, Administrative Law Judge

APPEARANCES:

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I-N-D-E-X

WITNESSES:	DIRECT	CROSS	REDIRECT	RECROSS
Duke Panel: Kevin McCoy Bert Dunn Steven Nesbit Robert Harvey	2336	2395	2398	
Edwin Lyman		2455		

EXHIBIT NO.	DESCRIPTION	MARK	RECD
51	Rim Structure	2470	2470
52	LOCA Results	2475	2475
53	Nissley email	2504	2504
A	01/31/04 Post Quench Ductility Results	2547	
B	03/23/04 Post Quench Ductility Results	2547	
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P-R-O-C-E-E-D-I-N-G-S

(8:06 a.m.)

CHAIRMAN YOUNG: All right. After a series of mishaps and uncertainties about the airlines, we are all here this morning, after numerous adventures last night and this morning, making sure that we were all going to be here.

I believe we're going to start with cross examination of Duke's witnesses. So if you could come forward to the witness box. And while they're doing that, are there any matters that we need to take up of a procedural or preliminary nature before --

MR. REPKA: Judge Young, I just have one matter.

CHAIRMAN YOUNG: Okay.

MR. REPKA: We can proceed with cross examination as we discussed yesterday. We are prepared to proceed with some limited direct on the rebuttal testimony, if you would think that would be helpful. Otherwise, we can defer on that. If we do, it perhaps might focus some of the issues for later discussion, but we could do it either way.

CHAIRMAN YOUNG: Why don't you go ahead and do that. If it would focus us, that might be helpful.

And, gentlemen --

MS. CURRAN: I need to ask a question. What is it that you are planning to do now?

MR. REPKA: I'm planning to do some direct examination to respond to the rebuttal testimony.

CHAIRMAN YOUNG: Just short. You're talking about five minutes or so.

MS. CURRAN: You're doing surrebuttal first?

CHAIRMAN YOUNG: You're talking about spending about five minutes clarifying a couple of points? Is that what you're talking about?

MR. REPKA: Yes.

CHAIRMAN YOUNG: No more than that?

MR. REPKA: Yes.

CHAIRMAN YOUNG: Okay.

MS. CURRAN: Okay.

MR. REPKA: There may be more discussion of those same issues later. Or if the Board wants to ask questions now, it may take longer than five minutes, but that's up to you.

CHAIRMAN YOUNG: Try to take about five minutes.

And, gentlemen, you remain sworn from yesterday.

WHEREUPON,

KEVIN McCOY, BERT DUNN, STEVEN NESBIT,

AND ROBERT HARVEY

were recalled for examination by Counsel for the Licensee and, having been previously duly sworn, resumed the witness stand, were examined and testified as follows:

DIRECT EXAMINATION

MR. REPKA: Gentlemen, are you ready?

DR. McCOY: Yes.

MR. DUNN: Yes.

MR. NESBIT: Yes.

MR. HARVEY: Yes.

MR. REPKA: Maybe just to reacquaint yourself, why don't you each introduce yourself again, for the sake of the parties and the Board. I'll start at the left with Dr. McCoy.

DR. McCOY: This is Dr. J. Kevin McCoy.

MR. DUNN: Bert M. Dunn.

MR. NESBIT: Steve Nesbit.

MR. HARVEY: Robert Harvey.

MR. REPKA: Gentlemen, do you have in front of you a document that was submitted on July 8th of this year, the rebuttal testimony of Dr. Edwin S. Lyman regarding BREDL Contention 1?

DR. McCOY: Yes.

MR. REPKA: I want to direct your attention first to page 2 of that testimony, paragraph AR-3. Are you with me? In that testimony, Dr. Lyman, about midway down, states that he is looking at Figure 10 and Figure 11 from the Duke Energy testimony. And he states that for high exposure fuel, relocation does occur. The maximum clad temperature at the ruptured balloon location is about 300 degrees F higher than the ruptured balloon location for the no relocation case. Do you see that?

DR. McCOY: Yes, we do.

MR. REPKA: Do you gentlemen have a response to that statement?

MR. DUNN: The 300 degrees higher is valid in comparing the two figures. What happens here is that it's using this to draw a conclusion that that's the change that should be expected in a ruptured node, depending on whether there's fuel relocation or not fuel relocation. And he's comparing two different experiments that are run under two different circumstances.

So we have to allow for the fact that certain things such as the power in the -- or the heat transfer process that is actually taking place in these two experiments was different. If you look at the heatup rate before any action occurred on the -- relative to the relocation or the swelling and rupture of the fuel pin, the heatup rate is different.

One, it reaches its -- the point of rupture at about 45 seconds. The other case it reaches the point of rupture about 35 seconds. Just dividing those two will tell you the approximate power ratio between the two tests, and it's about 20 percent different.

Other data published in this case in the handouts that the Staff included as an exhibit yesterday indicates that there's about a 10 or maybe a 12 percent difference in the power or the heatup rate for the two tests, which would be an indication that the -- at least the power was quite a bit different.

Now, that power directly controls the temperature you reach in these tests, because if the test is a balance between the heating rate of the power and the cooling rates of probably in this case the shroud. So --

MR. NESBIT: And if I could add a little bit to what Mr. Dunn said, we included Figure 10 to show the relative behavior of a ruptured and an unruptured node during the heatup phase of the LOCA. We included -- without fuel relocation. We included Figure 11 to show the relative behavior of a ruptured and an unruptured node in a post-LOCA heatup which includes fuel relocation.

However, the conditions of the tests from which Figure 10 is drawn, and the tests from which Figure 11 are drawn, are significantly different. Therefore, it is not valid to make a conclusion about relocation impacting the peak cladding temperature in the unruptured node by comparing what happens in Figure 11 and Figure 10.

MR. REPKA: The two tests, in other words, are apples and oranges.

MR. NESBIT: That's correct.

MR. REPKA: Now, a second question is, if we go further down in the response AR-3, it's the third paragraph, there's a discussion of some numbers related to peak clad temperatures. It starts with the number -- it says that the MOX LTA peak temperature at the hot pin rupture location is 1,841 degrees F.

I believe you made a clarification yesterday on the record related to that. Would you care to explain that?

MR. NESBIT: We clarified this in our

testimony yesterday. The value of 1,841 F was taken from a case, which was summarized in the license amendment request, which was used in order to demonstrate the difference, or, in this case, actually the very close similarity between the LOCA analysis with MOX fuel and with LEU fuel. It was an apples to apples case.

However, that case was not one of the LOCA analysis cases that ultimately went into the development of our LOCA limits. Those cases were described in a response to a request for additional information, and also are described in our testimony, paragraph 55 I believe.

MR. REPKA: And in those cases, the peak temperature at the hot pin rupture location was what?

MR. NESBIT: The peak temperature of the unruptured node was --

MR. DUNN: The peak temperature was 1,750.

MR. REPKA: At the ruptured node.

MR. NESBIT: Yes. The temperature in the -- the highest temperature in the ruptured node location, the cladding temperature.

MR. REPKA: A bit further down in that discussion, Dr. Lyman adds a 313-degree Fahrenheit factor. I'll let you address that later. But I want to focus for just a second on the next factor he adds is a 20 degrees F factor, which he adds to the maximum temperature at the rupture location that he would expect. Do you have a response to that? Do you agree that that's appropriate, to add that 20 degrees?

MR. NESBIT: We don't agree that it's appropriate to add that 20 degrees. That appears to be an offset that's inferred from, again, Figure 11. And the basic idea there appears to be a theory that there's always a 20-degree difference between the peak temperature at a ruptured node and the peak cladding temperature elsewhere on the rod. We have maintained -- continue to maintain that that effect is not there.

MR. REPKA: That the peak clad temperature is -- your position is the observed peak clad temperature is not at the ruptured location, correct?

MR. NESBIT: That's correct.

MR. REPKA: And, secondarily, that that's independent of the temperature at the ruptured location.

MR. NESBIT: That's right. The ruptured location is not controlling the peak cladding temperature elsewhere on the rod.

MR. REPKA: That's all I have at this time. I think there may be more to be said about this, but we can defer.

CHAIRMAN YOUNG: I'm sure there will be. Go ahead, Ms. Curran.

MS. CURRAN: Okay. I might need a minute.

CHAIRMAN YOUNG: All right.

(Pause.)

CROSS EXAMINATION

MS. CURRAN: Mr. Nesbit, I believe you just said that Duke no longer relies on the peak clad temperature of 1,841 degrees Fahrenheit that's in Table 3-5 and Attachment 3 of the LAR, is that right?

MR. NESBIT: That analysis that was presented in the LAR was an apples to apples comparison. It does not form the operating limits for the MOX fuel lead assemblies.

MS. CURRAN: What do you mean by "apples to apples"?

MR. NESBIT: We did that analysis so we could see -- making the -- after -- to begin with, to start our work on LOCA, the first thing we did is we reviewed the characteristics of MOX fuel relative to LEU fuel and made appropriate changes in the evaluation model to address those characteristics of MOX fuel that might have the potential for impacting the results.

In doing so, we actually did not address everything. There were some things that were obviously conservative for MOX fuel, so we continued to use the LEU fuel properties there.

After we had made those model changes, we ran a comparison analysis between a MOX case and an LEU case, and we looked at the difference in the results. And what we found was that the difference in the results was basically insignificant. It was on the order of 46 degrees or something like that, which is insignificant in LOCA analysis space. So that was the purpose of that analysis.

Subsequently, we exercised that model under a variety of burnup conditions and power conditions, etcetera, in order to define an operating envelope for our plant with respect to the MOX fuel lead assemblies and the LOCA analyses. And as I mentioned, those analyses are summarized in Exhibit 2 to our testimony, which is the response to request for additional information.

MS. CURRAN: Did you provide this new information to the NRC Staff in the course of this licensing review?

MR. NESBIT: Yes, we did. It was a response to an NRC request for additional information.

MS. CURRAN: And did that include the value of 1,750 degrees Fahrenheit in your testimony?

MR. NESBIT: No, it did not.

MS. CURRAN: Because the Staff used the 1,841 degree figure, right, in its safety evaluation?

MR. NESBIT: The 1,841 is clearly higher than the 17- value, so yes. Yes. The key point is that it's not the peak cladding temperature. It's not the figure of comparison to the acceptance criteria in 50.46. It's an ancillary piece of information, the temperature of the ruptured node. The key factor is the peak cladding temperature of the local oxidation, the global oxidation.

MR. HARVEY: May I add, all those sensitivities that were done for power shape and burnup, all the PCTs for those cases were presented to the Staff in response to the request for additional information.

MS. CURRAN: Can I let Dr. Lyman ask a question?

CHAIRMAN YOUNG: Yes, go ahead.

DR. LYMAN: I'm sorry. Just to clarify, the point we want to make in this is that in the Staff safety evaluation, particularly Section 2.4.1, the figures that they refer to are the ones from Table 3-5 in their discussion of what the evaluation of the LTA performance under large break LOCA conditions are.

So the Staff seems to have relied on Table 3-5 figures in their safety evaluation, yet you said that Table 3-5 is not part of the -- I have to get the language precise -- is not one of the cases establishing the LOCA limits for the MOX fuel assembly.

So our question is: if that is the case, why did the Staff rely on the values from Table 3-5 in their safety evaluation?

MR. NESBIT: I think the Staff is well aware of the information which we provided them. And I think the conclusion that they came to in their safety evaluation is valid. Beyond that, I would have to let you address that question to the Staff.

MS. CURRAN: Mr. --

CHAIRMAN YOUNG: When you said the Staff was aware of the information, you're talking about the information you just gave us this morning?

MR. NESBIT: I'm sorry. Could you repeat that?

CHAIRMAN YOUNG: You said the Staff was well aware of the information. You were talking about the information that you just gave us this morning?

MR. NESBIT: I'm referring to the results of the calculations, the LOCA calculations that formed the operating limits of the plant for the MOX fuel lead assemblies, which is the peak cladding temperatures and the conditions at which those are evaluated, showing conformance to 50.46.

CHAIRMAN YOUNG: And where is that found in your testimony?

MR. NESBIT: That's Exhibit 2 to our testimony, and it's also summarized in paragraph 55.

CHAIRMAN YOUNG: But you were also referring to the new information that you gave us this morning?

MR. NESBIT: The temperature of the ruptured node for that PCT case is provided in paragraph 55.

CHAIRMAN YOUNG: What I'm trying to ascertain is just simply -- the new information that you gave us this morning in response to Mr. Repka's questions, is that part of the information you're saying the staff was aware of?

MR. REPKA: We did not provide any new information this morning. This information was in our -- the information about the peak cladding temperature at the ruptured location was in our testimony. It was provided on July 1st.

MR. DUNN: But I might add, Judge, that the information on those cases, the peak cladding temperatures, the results in those cases, was submitted to the Staff in RAIs, requests for additional information, responses to the Staff in support of the license amendment request.

MR. NESBIT: November 3, 2003.

CHAIRMAN YOUNG: Okay.

MS. CURRAN: I guess this is to any of you, and I'd like you to -- you can -- whoever wants to answer the question can go ahead and answer it, but I'd prefer that you not consult before answering the question.

Is the 1,841 degree figure in Table 3-5 the correct result of a valid LOCA analysis?

MR. DUNN: Yes, it is, subject to the fact that the plant will not actually allow the operation of a MOX fuel pin at that power level that was used in that analysis.

CHAIRMAN YOUNG: Ms. Curran, let me ask you a favor. Would you mind, when you refer to the tables, to also, if you have them handy, use the official exhibit numbers?

MS. CURRAN: I'll try.

CHAIRMAN YOUNG: That would be very helpful to us.

MS. CURRAN: I will clarify that Table 3-5 is in Exhibit 1 of Duke's exhibits.

CHAIRMAN YOUNG: Okay. Thank you.

JUDGE BARATTA: While Ms. Curran is consulting, may I ask a question?

The LOCA analysis that was done at 1,841 you said was done at a power level that would not be allowed in operation. What was that power level?

MR. DUNN: It's actually the LOCA power level --

JUDGE BARATTA: Right.

MR. DUNN: -- Judge Baratta. There is an application in the control scheme for McGuire and Catawba in which they allow peaking factors -- a peaking factor as a function of elevation only, so that for the bottom six feet of the core the total FQ is allowed, starting at --

CHAIRMAN YOUNG: The total what?

MR. HARVEY: It's a three-dimensional peaking factor.

MR. DUNN: FQ refers to the total peaking factor --

CHAIRMAN YOUNG: What does it stand for?

MR. NESBIT: It's the --

MR. DUNN: Total, I think.

MR. NESBIT: It is the total peaking factor --

MR. DUNN: Yes.

MR. NESBIT: -- in the core.

CHAIRMAN YOUNG: Is it an acronym for something? That's all I'm asking.

MR. DUNN: I can't come up with that right now, Judge Young.

JUDGE BARATTA: F refers to --

MR. DUNN: I'm not sure. It's a Westinghouse term, and I'm not familiar with where it came from.

CHAIRMAN YOUNG: Okay.

JUDGE BARATTA: F refers to factor, and Q is the heat.

CHAIRMAN YOUNG: Thank you.

MR. NESBIT: And it refers to the fact that it includes the axial and the radial components.

MR. DUNN: Let me just use a word that makes sense. Let's just use total -- total peaking.

CHAIRMAN YOUNG: Thank you.

MR. DUNN: Okay. Up to six feet, the total peaking for the MOX fuel assembly will be 2.4.

Above six feet, it starts to schedule down. We drop it a little bit. And it drops by a 10-foot elevation, which is where this case was analyzed at. Approximately the 10-foot elevation is what -- the peak in the core, because typically the cladding temperature response is worse the higher you go in the core.

The power has dropped to about .97 percent of 2.4. Someone would have to run that number for me right now, but it's around three or four percent. 2.3? 2.32 or something? 2.3.

MR. NESBIT: That's for the cases, the ultimate cases. I believe that the case that was in the license amendment request was 2.4.

MR. DUNN: Yes. At the time we did the work for the license amendment request, we weren't sure whether we were going to use that, and we were just trying to create, as we said, a comparison between LEU and MOX fuel. And we did that calculation with a peaking factor of 2.4.

In fact, the plant will not allow that -- a peak at that elevation higher than 2.3 -- three I guess is as accurate as we should get. And that's the reason for the two different power levels for that -- those two analyses.

JUDGE BARATTA: Well, am I correct in saying that the information that I guess is in Table 3-5 -- is that the correct reference?

MR. NESBIT: Yes.

JUDGE BARATTA: Is the true envelope or --

MR. NESBIT: No, it's not.

JUDGE BARATTA: I'm sorry. It's this number that appears here.

MR. NESBIT: Yes. I would point out that Table 3-5 in the license amendment request, which we were just talking about, Exhibit 1 to Duke's testimony, is clearly labeled, large break LOCA, sample calculations, and tariffs. It's for that purpose. It's not a limits case. The limits are provided in Table Q14-1 of Exhibit 2.

MR. DUNN: Exhibit 2 being the RAI response from November 2, 2003.

MR. NESBIT: Page 31 of that response.

JUDGE BARATTA: And the limits that were used to determine those are in the tech specs?

MR. NESBIT: Do you want to take that, Bob?

MR. HARVEY: It's essentially the tech specs. These limits have been moved to the cooperating limits report, which is essentially the same vehicle as the tech specs. It's a document that's controlled by the utility, and this can be changed without having prior NRC approval.

MR. NESBIT: Bob, is it not correct that those limits are manifested as allowable axial offsets as a function of power level?

MR. HARVEY: That's correct. If I could add one more point related to this topic. In our license amendment request, which is Exhibit 1 -- and I guess it's page 3-26 -- we stated that whatever calculations that were going to be performed to address sensitivities of time and life steam generator design and power distribution -- that's what Mr. Dunn is referring to as far as the allowed peaking values.

So it was noted in our license amendment request that we will do the sensitivity calculations subsequent to the submittal of the license -- original license amendment request. And those calculations that were reported in our response to additional information included those sensitivity -- the results from those sensitivity --

CHAIRMAN YOUNG: What page did you say?

MR. HARVEY: It was page -- the bottom of 3-28.

CHAIRMAN YOUNG: 28. All right.

MR. HARVEY: I'm sorry. I'm sorry. I lost my páge now. 3-26.

CHAIRMAN YOUNG: Okay. Thank you.

MR. REPKA: That's in Exhibit 1.

MR. HARVEY: Correct.

CHAIRMAN YOUNG: Okay. Go ahead, Ms. Curran.

JUDGE BARATTA: Whenever she's ready.

MS. CURRAN: Okay. Can you say that the maximum peak clad temperature for the rupture location is never going to exceed 1,750 degrees Fahrenheit for any design basis accident LOCA?

MR. DUNN: Yes, I believe we can say that.

MR. NESBIT: That was for peak.

MR. DUNN: That's the worst one in the evaluations that were performed, the worst result from the technical specification. You do an evaluation of the core for peaks at roughly five locations. I think in this case we used actually four, and you're looking for a span of whether the peak is at the bottom of the core, the middle of the core, or the top of the core.

And that case is the -- that case is the highest -- has the highest ruptured node temperature of that set.

MR. NESBIT: I would add that our

expectation would be that the highest ruptured node temperature in the event of a large break LOCA would be significantly lower because of all the inherent conservatisms in 10 CFR 50, Appendix K.

MS. CURRAN: Mr. Dunn, in paragraph 154 of your direct testimony, I believe you stated that even --

CHAIRMAN YOUNG: I'm sorry. It's paragraph --

MS. CURRAN: Paragraph 154.

CHAIRMAN YOUNG: Thank you.

MS. CURRAN: I believe you stated that even if the pessimistic IRSN predictions are simply added to the current Catawba MOX fuel location evaluations, the results remain well below the acceptance criteria of 10 CFR 50.46. Is that correct?

MR. DUNN: Yes, ma'am.

MS. CURRAN: In your opinion, is it scientifically valid to simply add the IRSN predictions for the change in peak clad oxidation due to relocation to the Catawba MOX fuel peak maximum clad -- maximum clad oxidation predictions, in order to determine whether the 10 CFR Section 50.46 acceptance criteria for peak cladding oxidation are met?

MR. DUNN: In answering this, I've got to put one caveat on here, and that's because you used the term "scientifically valid." The addition of the differential from one person's calculations to another person's calculations has -- would have to be done carefully.

In this case, we know, for example, that the IRSN calculations were conservative. They are, in my opinion, a substantial overprediction of the results of a fuel relocation differential, such that I'm satisfied that taking this procedure creates a relatively conservative expectation for the results of a fuel relocation in combination with our evaluation model.

I would conservative by, you know, 150, 200 degrees perhaps, that much. And that's based on knowing what is not in the IRSN calculation, which are some additional cooling mechanisms that result, as the IRSN has admitted in their presentations -- and, actually, they have plans to upgrade their models to include these additional cooling mechanisms, which is -- I hope that was responsive to you.

MS. CURRAN: All right. Thank you.

In the sensitivity studies that you were discussing just a few minutes ago, did you seek to maximize the peak clad -- peak burst clad temperature?

MR. DUNN: You're talking about the sensitivity studies that Framatome performed to determine where or how to carry out the -- what we would call LOCA limits calculation?

MS. CURRAN: Yes.

MR. DUNN: No, ma'am. We thought to maximize the peak cladding temperature. We evaluated -- I wouldn't say we sought to maximize peak cladding temperature. That's incorrect. We select the distribution of power axially, and under other conditions that the plant is likely to find acceptable from the standpoint of operations, and then we analyze the LOCA cases at that distribution to make sure that they conform to the criteria 10 CFR 50.46. That, in fact, I did.

MS. CURRAN: So how can you be so confident that those cases actually show -- accomplish the maximum -- encompass the maximum burst clad temperature?

MR. DUNN: They were run at the -- on the basis of a sensitivity study that looked at a range of exposures, the range of burnups during the -- that the fuel assembly might be at. I looked at I think a beginning of life case, a 20,000 gigawatt stage per metric ton. That's about the end of the first cycle of the operation of the fuel assembly.

We looked into the second cycle of the operation of the fuel assembly, and we looked into the end of the operation of the fuel assembly. I'm guessing there's about 12 to 15 cases that we ran, and of those 12 to 15 cases this was the highest in the -- highest ruptured node temperature that we achieved.

As I described, we're actually looking for the peak cladding temperature, not the ruptured node temperature.

MR. NESBIT: It's correct to say that there was a prediction of cladding rupture in each one of those cases that were evaluated, or most of them. Right, Bert?

MR. DUNN: Yes. When you're looking at -- a rupture occurs, or the large ballooning of the cladding occurs at a temperature between 1,500 and generally 16 or maybe 1,650 degrees Fahrenheit. So when one location of the fuel pin is going to go up to 2,000 degrees Fahrenheit, you will have ruptured that fuel pin in some location.

MR. NESBIT: So we looked at quite a few cases, and each one of those cases involved a ruptured node. And the peak among those was 1,750 degrees.

CHAIRMAN YOUNG: Can I just clarify something for myself? It may be more from a lay level. The difference between maximizing the peak clad temperature and having a distribution, where does the distribution -- the top level of the distribution lie in comparison to what you would consider maximizing it? Does that make sense?

MR. NESBIT: A distribution of what?

MR. DUNN: Yes, I don't -- it didn't actually make sense to me. I'm sorry.

CHAIRMAN YOUNG: Okay. The question as I understand it was: did you seek to maximize the peak clad temperature? And you said -- first, you said no, and then you -- first you said yes --

MR. DUNN: First, I said yes.

CHAIRMAN YOUNG: -- and then you said no --

MR. DUNN: Right.

CHAIRMAN YOUNG: -- we used a distribution instead, and then you talked about using a number of cases. Within that -- as I understood -- maybe I understood incorrectly, but within the distribution of numbers that you got from those cases, what would be the difference between the highest number you got among those -- that distribution, and what would be the maximum?

MR. DUNN: Steve, you can --

MR. NESBIT: Yes, let me -- in Table Q14-1 from our Exhibit 2, we list the peak --

CHAIRMAN YOUNG: In Q14, did you say?

MR. NESBIT: Yes, it's page 31 of Exhibit -- our Exhibit 2, Table Q14-1. We list peak cladding temperature results for a variety of the cases that we analyze there to define the LOCA limits. And as you can see, they range from a lowest value of 1,788 degrees up to the highest value of 2,023.

CHAIRMAN YOUNG: What I'm trying to get at is, the question asked if you sought to maximize the peak clad temperature. The 2,019.5 degrees, when you say you didn't seek to maximize it, if you had, how much higher would -- can you estimate how much higher it would have been?

MR. NESBIT: Well, ma'am --

CHAIRMAN YOUNG: Or does that -- I mean --

MR. NESBIT: Judge Young, maybe if I start over, then, a little bit. The plant is going to try to determine what power level -- what power level distribution axially, from the bottom of the core to the top of the core, it wants to operate under.

The plants can control the power and the fuel assembly typically through rod motion, small rod motions, to push the power up or down in the core. So it has some control of that, and they will establish something that they believe is comfortable for them to operate at for a given cycle. Okay?

Then, what we do is we look at that power distribution. We say, okay, this is the maximum power this plant is going to allow to happen at this position. And then we do a calculation, a LOCA calculation, to determine whether that -- operation at that power meets the criteria of 10 CFR 50.46.

If the plant is happy with that power level, and the cladding temperature is under 2,200 degrees, it's acceptable, and we probably won't do anything further. So that would be the highest we would go.

If the plant wanted more peaking than should have been allowed, and the calculation came out above 2,200 degrees, we would reduce the allowed peaking, the plant would be forced to operate with lower peaking, and we would stop there.

CHAIRMAN YOUNG: So let me -- just so I can see if I understand. This 2,019 figure arises from your calculations based on the maximum power that the plant would be operating under, based on what they tell you.

MR. NESBIT: Well, Mr. Harvey maybe can clarify that a little bit.

MR. HARVEY: That's an assumed peaking level that the plant would be allowed to run at. Once these LOCA limits -- we call these LOCA limits -- once they are established, the core designer, the person who designs the fuel design, he sets the enrichment and the location of these assemblies to stay within those constraints.

So the more peaking you're allowed in the LOCA analysis, the more flexibility you give to your core design. So Mr. Dunn can do calculations at any peaking level you'd like. He can run them to 3,000 degrees Fahrenheit. He can run them to 1,000 degrees Fahrenheit. The higher he goes in his allowed peaking, the more flexibility we have in core design space. So --

CHAIRMAN YOUNG: But his allowed peaking -- these figures -- is based on the information he got from the plant in the first place. Am I --

MR. HARVEY: No, he makes the assumption first.

CHAIRMAN YOUNG: I thought that's what you said.

MR. DUNN: I'm sorry. It is based on a negotiation with the plant in terms of what they expect would be reasonable peaking for them to operate under.

MR. HARVEY: It's based on past experience.

MR. DUNN: Past experience.

MR. HARVEY: They know how much margin they would like -- how much peaking margin they would like to make the core design sufficient and economical.

CHAIRMAN YOUNG: "They" being?

MR. HARVEY: The core design people.

CHAIRMAN YOUNG: Okay.

MR. HARVEY: So they always want more. They always want as much margin as you can give them, and the LOCA analysis establishes a ceiling of how high they can go.

CHAIRMAN YOUNG: Okay.

MS. CURRAN: Okay?

CHAIRMAN YOUNG: Thank you.

MS. CURRAN: If Duke didn't seek to maximize the peak clad temperature, how do you know that you have calculated the maximum peak clad temperature over the entire cycle of -- over the entire MOX cycle, and thus can show compliance with 10 CFR 50.46?

MR. NESBIT: We did our standard practice, in terms of looking at a range in burnups, a range of conditions, and validated that for the peaking limits that we chose to apply, which is basically a total peak of 2.4 with some adjustments for burnup and for elevation, that we were well within the 10 CFR 50.46 acceptance criteria for all of those cases. It's just the standard practice.

And that's another example -- to get to a point that Judge Young brought up earlier. We could theoretically run this calculation to a peak cladding temperature of 2,199 degrees, and say that is our acceptable limit. We don't do that.

We like to keep margin in there for ourselves in case something arises, so that's why the actual limits that we use are reflective of lower peak cladding temperatures than the acceptance criteria in the regulations. And it's just another example of the inherent margin in the whole process.

CHAIRMAN YOUNG: So these are limits you set for yourself.

MR. NESBIT: Yes, they are. We know from experience that with a total peak of about 2.4 that we have adequate operating margin to run a plant efficiently.

MR. HARVEY: Can I add one more point? The MOX lead assemblies -- the total peaking factor of 2.4 is lower than the allowed peaking that we have for the LEU fuel. They allow the LEU fuel to go to a higher peaking than we do for these lead assemblies.

MR. NESBIT: And that's a typical fraction relief as --

MS. CURRAN: How do you know for the limits you have set that you have found the limiting case, if you didn't seek to maximize the peak clad temperature?

CHAIRMAN YOUNG: Maybe what I need to understand is, when you're asking, did you seek to maximize the peak clad temperature, what you mean by that, because it -- I'm not quite fitting that together with the description that the witnesses are giving of these limits that they set for themselves.

MR. NESBIT: We did not seek to maximize the peak cladding temperature. We sought to develop a set of limits that would provide us with adequate operating margin, and with peak cladding temperatures that were well within the regulatory acceptance criteria.

MR. REPKA: Can I ask something that I think might help clarify that? When you set the operating limits, you did seek to calculate the maximum peak clad temperatures for those cases.

MR. NESBIT: Well, yes. For the given combination of power burnup conditions in the plant, it is absolutely a conservative overestimation of peak cladding temperature that would occur if there were a LOCA in the plant at that time.

MR. REPKA: Under those assumptions.

MR. NESBIT: Yes.

MR. HARVEY: I just want to add -- we don't seek necessarily to maximize the peak cladding temperature. We seek to find the limiting case, and that -- whatever -- and that results in a peak cladding temperature. So we try to find, what's the worst case with respect to a LOCA for these MOX lead assemblies?

And we do sensitivities -- it was done by AREVA -- sensitivity calculations to find the limiting case. That limiting case reflects the highest peak clad temperature that we analyzed. So we're not looking, can we make the temperature go up higher? We could. The point is we want to find the worst combination of burnup and power axial elevation that result in the limiting results.

MS. CURRAN: Okay.

JUDGE ELLEMAN: Ms. Curran, I thought I understood your question to be: how do you know you have found that limiting condition? Is that what you were asking?

MS. CURRAN: Yes. Well, I'll turn it over to Dr. Lyman.

DR. LYMAN: Yes. Just in response to what Mr. Dunn said, we were surprised that he said they didn't seek to maximize the peak clad temperature in their sensitivity studies, because we would assume that if he were trying to comply with 50.46, where you have to show that for -- that the peak clad temperature limit of 50.46 is maintained throughout the entire operating cycle for every case that you're going to -- for every case that will occur or could occur for the given plant conditions, that you would have to seek to find the case where the maximum peak cladding temperature occurs, and show that that peak was the limit. And the limiting peak clad temperature is going to be the maximum to show compliance with 50.46.

So we were just surprised by that statement that they weren't seeking to maximize peak clad temperature, and you're seeking clarification of --

CHAIRMAN YOUNG: I'm not sure I --

MR. NESBIT: There's a semantics issue.

CHAIRMAN YOUNG: Hold on. I'm not sure I understand the question. Did you seek to maximize the peak clad temperature in the context of what they're talking -- could you just explain to me?

DR. LYMAN: Well, simply, in their license amendment request they have established a set of plant parameters. They have run large break LOCA cases for a variety of conditions on --

CHAIRMAN YOUNG: We're talking about Table Q14?

DR. LYMAN: -- burnup. Right. And they've chosen a set of cases, calculated the peak cladding temperature in each one of those cases. Our puzzlement comes from the fact that they -- we're not sure if those cases were actually selected with the intent of finding the worst possible and the most limiting case.

MR. NESBIT: They are, and let me explain with an illustration. One of the sensitivity studies that we performed was on the steam generators. The Catawba 2 unit -- we did these large break LOCA analyses to apply to both McGuire Units 1 and 2 and Catawba Units 1 and 2.

We analyzed the different steam generator designs, and we picked the one which gave the worst peak cladding temperature, which was the Catawba 2 design. So that's an example of the way that we applied our sensitivity studies in a conservative manner to ensure that the calculated peak cladding temperature was conservatively high.

CHAIRMAN YOUNG: So these figures in Q14-1 are based --

MR. NESBIT: They all --

CHAIRMAN YOUNG: -- on that --

MR. NESBIT: They all reflect the Catawba Unit 2 plant configuration, because that is the worst case.

MR. HARVEY: We should just add the lead assemblies are going to go in Catawba Unit 1. So these calculations conservatively reflect a steam generator design that is more limiting than the actual plant they're going to go into.

CHAIRMAN YOUNG: Okay. Do you want to add anything else to explain the question in light of what they just said?

DR. LYMAN: No. Our question was simply

in response to Mr. Dunn's statement that they didn't seek to maximize peak clad temperature, and I still haven't heard anything that would contradict the fact that they did not examine every case to ensure that they had -- that they sought to maximize peak clad temperature. That was simply his statement, and we were simply asking what that meant. So we have clarification, and now we're happy to move on.

MR. DUNN: Have we responded to it now? I mean, my -- the example that Steve gave was on steam generators. Within the burnup sensitivity study, the same thing is done. So you looked at five or six times in life for the fuel assembly or the fuel pin, and compare the cladding temperature response that you get at each one, with a, you know, consistent set of other parameters, and you pick the highest -- the burnup that gives you the highest peak cladding temperature.

And then you carry that forward, along with all the other sensitivities. You carry that forward into a determination of what the cladding temperature is as a function of the selected power distribution that the plant could be allowed to operate under.

What I meant by not seeking to maximize

the peak cladding temperature was that beyond that power distribution we don't usually try and always get to 2,200.

MR. NESBIT: Right. We don't take it all the way up to the regulatory limit.

CHAIRMAN YOUNG: Let me ask -- let me see if I can clarify something. Within that power -- those -- the power operating limits, were all of the various factors comparable to the steam generator tube? In other words, were all of those factors a worst-case scenario?

MR. HARVEY: Yes.

MR. DUNN: Yes.

CHAIRMAN YOUNG: So would that be equivalent -- would that to you, Mr. -- Dr. Lyman, be maximizing the peak clad temperature?

DR. LYMAN: Well, if every parameter were chosen for the worst-case scenario, obviously, yes. But that seems not to conform with what Mr. Dunn said, that they didn't seek to maximize it.

MR. NESBIT: Is there a specific parameter that you question that we didn't --

DR. LYMAN: No, we're just struck by that statement.

CHAIRMAN YOUNG: It sounds as though

they're clarifying now and saying that they did use the worst-case scenario for all of the parameters, so that -- would that -- I mean, I don't want to put words in your mouth. But I'm understanding at this point that you're saying, if that's the meaning of the question, you did, in fact, try to seek to maximize peak clad temperatures. Am I understanding that right?

MR. NESBIT: Yes. All we meant by saying we didn't maximize peak cladding temperature is that we didn't run it all the way up to the regulatory limit of 2,200 degrees Fahrenheit.

CHAIRMAN YOUNG: Which would have meant raising the operating --

MR. HARVEY: Exactly.

MR. DUNN: Right.

MR. HARVEY: So within the operating envelope, which is Figure Q14-1, the maximum peak clad temperature that we predict in a curve is 2,019.5. So that is the maximum peak clad temperature for the entire operating envelope.

CHAIRMAN YOUNG: Within the operating limits that you planned to use, and there's nothing that could cause you to go over those.

MR. HARVEY: Efforts were made within that operating envelope to establish the maximum peak -- maximum peak clad temperature.

CHAIRMAN YOUNG: Does that make sense now?

MS. CURRAN: Yes, we're going to move on.

CHAIRMAN YOUNG: Okay. Thanks for clarifying it for me.

MS. CURRAN: I just need one moment before I do to consult with Mr. Repka.

(Pause.)

MS. CURRAN: Mr. Harvey, in paragraph 68 of your rebuttal testimony, I believe you state that the limiting peak clad temperature for MOX fuel LOCA limits calculations is 2,019.5 degrees Fahrenheit, which is below the limiting cases for the co-resident LEU fuel in the Catawba core as analyzed by Westinghouse. Is that correct?

MR. HARVEY: Yes, that is.

MS. CURRAN: What is the significance, in your opinion, of the fact that the limiting peak clad temperature for MOX fuel is below the limiting cases for the co-resident LEU fuel in the Catawba core?

MR. HARVEY: It's just used for reference to indicate that it's not the limiting calculated result. There are different models, and they analyze different fuel types. So a real direct comparison is probably not relevant, but it is an indication of the consequence of the two calculations.

MS. CURRAN: So is it an indication that the PCT for the LEU fuel is limiting of the PCT for the MOX fuel?

MR. HARVEY: As you report in the 50.46, a licensee is required to track the limiting case and report that -- the changes to those results annually or sooner, depending on the magnitude of the change. Accordingly, our limiting calculation is still reflective of the Westinghouse LEU fuel, so that is our analysis or record PCT that we use for 50.46 reporting.

MS. CURRAN: So does that mean you're confident that the MOX fuel is not going to be the limiting case?

MR. HARVEY: A real good calculation would be if you took the same method, take the AREVA method, and you run an LEU calculation at a 2.5 peaking factor, which is what we're allowed to operate -- actually, it's 2.6 in the bottom of the core right now -- you run that at a 2.6 peaking factor, and you run the same calculation with MOX fuel at 2.4 peaking factor, the LEU fuel will always be higher.

MS. CURRAN: What method did Westinghouse

use to calculate the co-resident LEU PCT?

MR. HARVEY: They used the Westinghouse COBRA/TRAC. It's the best estimate LOCA method.

MS. CURRAN: What is it that --

CHAIRMAN YOUNG: Say that again.

MR. HARVEY: The Westinghouse best estimate LOCA methodology is WCOBRA/TRAC. It's a computer code W, Cobra, C-O-B-R-A, slash, T-R-A-C.

MS. CURRAN: Do you know whether fuel relocation effects were considered in the Westinghouse model?

MR. HARVEY: I believe they were.

MS. CURRAN: If relocation of the LEU fuel was, in fact, considered in the Westinghouse calculation, and given that the MOX PCT calculation did not consider relocation, right?

MR. HARVEY: That's correct.

MS. CURRAN: Isn't the comparison you make in paragraph 68 invalid?

MR. HARVEY: I don't understand your question.

MS. CURRAN: Well, isn't it comparing apples to oranges?

MR. NESBIT: There's a whole number of differences in --

MS. CURRAN: I want to hear what Mr. Harvey has to say first.

MR. HARVEY: I'm only comparing two approved methods and showing you the results from those calculations. They are for different fuel types, different peaking levels, so they're not -- they're only to show the two approved models and the limiting calculation.

The reason I brought it up is the limiting PCT -- it determines our analysis of record, and that's the number we need to track for licensing purposes. Under 50.46, we need to track the limiting case. The limiting case remains the Westinghouse calculation. That is the reason I brought that up.

It's not the limiting -- we don't have a limiting case. It's not MOX. It's LEU fuel calculated by Westinghouse.

CHAIRMAN YOUNG: Let me see if I can understand something. You said that the Westinghouse calculations for LEU fuel do take into account fuel relocation?

MR. HARVEY: That's correct.

CHAIRMAN YOUNG: But am I understanding you correctly that the calculations for the MOX fuel did not?

MR. HARVEY: That's also correct.

CHAIRMAN YOUNG: So would the -- if you did apply those, I assume they're getting to what -- how would that affect -- if you used the Westinghouse test or calculation on the MOX fuel, do you know what results you'd get?

MR. HARVEY: The Westinghouse method is a best estimate LOCA method. It is more state of the art, and it has more margin to the 2,200, so it's a more advanced methodology. The MOX method is based on Appendix K, which is a much more conservative analysis.

So it has implied conservatisms in that method, which are one reason why fuel relocation is -- accounting for the fuel relocation is not as important, because the Appendix K methodology that was used for the MOX fuel has a lot of built-in conservatisms.

CHAIRMAN YOUNG: If you had to do the calculation using the same calculation, the Westinghouse COBRA --

MR. HARVEY: COBRA/TRAC.

CHAIRMAN YOUNG: -- is that something that could be done relatively quickly, or --

MR. HARVEY: No, it could not be done

relatively quickly. The best estimate methodology requires you to perform a nominal calculation, and then you have to statistically account for all of the variations and uncertainties. So you do a large number of calculations, and then you do a Monte Carlo simulation to determine the uncertainty on that calculation.

The nominal PCT for the Westinghouse COBRA/TRAC analysis is on the order of 1,500 degrees. The best estimate calculation for an LEU fuel is around 1,500. And when they account for all the uncertainties, it pushes it up closer to 2,200.

MR. NESBIT: Just to add to that, the exercise and evaluation model for LOCA involves a large amount of data, and a lot of the data is proprietary. So we couldn't -- a lot of the data that would be required to analyze the MOX fuel is proprietary to Framatome, and we couldn't turn it over to Westinghouse and have them use that.

I'd also just like to add that there is -- when you start talking about the differences between the two methodologies, the Westinghouse methodology and the Framatome methodology, the fuel relocation is one of the smaller of the differences that you could look at in terms of comparing the two. It's clearly apples and oranges.

CHAIRMAN YOUNG: I was mainly trying to understand it in terms of the questions that she was asking, so -- thank you.

MR. DUNN: Well, if I could add just a little bit to that, too. Although they are very different evaluation models, and they take different approaches, they are still modeling the same set of physics, so to speak, in one case more conservatively than in the other case, or, rather, the conservatisms are brought in in a different way.

CHAIRMAN YOUNG: Right.

MR. DUNN: So one can take a little bit of information from the sensitivity study produced in the LAR that we spent a half hour on a few minutes ago where we showed that for a consistent evaluation model there wasn't a lot of difference in the results of the basic parameters that go into the evaluation between LEU and MOX fuel, where we have a 37-degree difference. I think Mr. Nesbit said 46, but I think it's actually 37 degrees difference between the two, with a somewhat conservative approach to the MOX evaluation that are particularly in the decay heat.

Now, that's one of the things that would be reflected in the best estimate technique is that you would pick up that difference in decay heat. So you could expect some -- a reasonable guess would be this method would produce the same answer for LEU versus MOX fuel.

JUDGE BARATTA: When you -- the Westinghouse method that was employed using Westinghouse COBRA/TRAC, and what you're referring to, is, of course, once you've taken into account the uncertainties, I think you said, to come up with a PCT for the LEU fuel. Do you remember what that value actually was, or is?

MR. DUNN: The value of the PCT for --

JUDGE BARATTA: When you take into account the uncertainties and add them on to get -- you know, get the response or --

MR. DUNN: I think Mr. Harvey would probably be able to address that.

MR. HARVEY: Our limiting analysis of record from the Westinghouse calculation is 2,140, I believe.

CHAIRMAN YOUNG: Excuse me for interrupting. By the way, I'm obviously the lawyer and the -- not the nuclear engineer here, so from time to time if I stop to try to make sure I'm on track, that helps me, so I appreciate your accommodation.

MS. CURRAN: Certainly. It's very important.

(Pause.)

We don't have any more questions for Duke.

CHAIRMAN YOUNG: All right. I guess -- shall we go to Duke and then the Staff, or just, Staff, do you have questions before we go back to Duke?

MR. REPKA: I think we should go to the Staff first, and then come back --

MS. UTTAL: Can we take a short break?

CHAIRMAN YOUNG: Yes, let's take five minutes.

(Whereupon, the proceedings in the foregoing matter went off the record at 9:12 a.m. and went back on the record at 9:25 a.m.)

CHAIRMAN YOUNG: Ready? All right.

Do you want to go first?

JUDGE BARATTA: You mentioned earlier that in the Westinghouse calculation fuel relocation effects on PCT were taken into account. Can you be more specific as to how that was done, or that is done?

MR. HARVEY: I'm not sure if I can.

That's a proprietary model to -- of that fuel vendor, Westinghouse. They do model the phenomena of fuel relocation, and they do take that into account in the uncertainty calculation that determines the maximum PCT.

JUDGE BARATTA: Is it done -- I mean, there's a couple -- when you say "model," is it actually done using a physical model in the code itself, or is it done by using it in the uncertainty analysis? Do you know that?

MR. HARVEY: It's done in the heatup calculation, so it is a physical model. But they treat -- I don't know if I can go any further. They treat that in an uncertainty parameter -- one of the inputs that go into how much relocation occurred.

JUDGE BARATTA: Okay. So, in other words, the amount of relocation that occurs is treated as an uncertainty as opposed to its effect on PCT.

MR. HARVEY: I believe that's correct.

JUDGE BARATTA: Thank you.

CHAIRMAN YOUNG: Judge Ellemen?

JUDGE ELLEMAN: Yes. Several things that relate to the testimony this morning. There was a reference quite recently to the Monte Carlo calculations. And the impression I got from that was that you are using Monte Carlo to establish the spread or the variance in a particular number that has been calculated through another route. Is that how the Monte Carlo is factoring into your calculations?

MR. HARVEY: Are you referring to the Westinghouse calculation now?

JUDGE ELLEMAN: I'm referring to your reference that you made a few minutes ago to the use of Monte Carlo techniques.

MR. HARVEY: That was in reference to the Westinghouse best estimate calculations, which are not the calculations that are used for the MOX fuel. That was in reference to what is the limiting PCT for the co-resident Westinghouse fuel. I stated that was based on a Westinghouse calculation, which is a best estimate plus uncertainty analysis.

JUDGE ELLEMAN: Okay. Where I was heading -- this is my question -- is every calculation you make has a variance associated with it. There's upper and lower limits to it. There's plus and minus something. And do you have any insight into how big those plus and minus numbers are on the calculations you make?

MR. HARVEY: Now, you're talking, again, about the Westinghouse co-resident fuel calculation or the MOX calculation?

JUDGE ELLEMAN: Well, the MOX calculation.

MR. NESBIT: We actually do have insight into the variance, if you will, because, as you're aware, Appendix K was promulgated in 1974. And since that time, there has just been a huge amount of research and analyses done by industry, by the NRC. I've seen one account in 1988 that combined they spent $1.5 billion on it. There was a lot of work that got done that culminated in the development of the best estimate analysis methodologies.

And based on the best estimate analyses that have been performed for the same plants or similar plants, we now have insight into the overall degree of conservatism associated with the Appendix K models such as we applied to the MOX fuel lead assembly calculation.

And as we point out in our testimony, that degree of conservatism is 600 degrees or more, and that's consistent also with the NRC documents that have looked at this, like the compendium of ECCS analyses in 1988.

I don't know -- Mr. Dunn may have something he wants to add to that.

MR. DUNN: I think the only thing that I

would like to add to that is the determination of approximately 600 degrees plus margin between an Appendix K calculation and a mean value for best estimate calculation.

It is, in this case, determined from a -- the comparison of the evaluation model used on the MOX calculations to a Framatome best estimate evaluation model for the same plant. It's not Catawba, but it is the same plant. So it's pretty close to a good -- a good and valid determination of the uncertainty -- cumulative uncertainty.

JUDGE ELLEMAN: Yes. I guess what I was trying to get at is there are -- in the real world, there are perturbations that can occur in any process that slightly alter the results in real life that you get, even though your gross conditions of operation are unchanged and fixed.

And I'm trying to get some idea of how big that variance is, how -- what kind of uncertainty are you potentially looking at? Do you have any insights or comments that you can make on that issue?

MR. NESBIT: I don't know if this is directly related to the issue that you're talking about, but in our testimony we offered a figure that compared the conditions of operations that would correspond to our operating limits at the plant -- you know, the very conservative assumptions we made in calculating the peak cladding temperatures at 2,019, or whatever, and the actual conditions that the plant operates at.

And that's provided in Figure 8 of our testimony on page 36, and that indicates that for that one parameter the actual plant operation is far removed from those conservative limits that we have there.

Beyond that, I think -- again, I'll turn to Mr. Dunn to see if he wants to comment on that. You know, Framatome and -- well, any vendor that does these kind of calculations in the development of their models, etcetera, they perform a series of sensitivity studies to gain insights into the various things that might affect the results of the calculations.

And the evaluation model results that we present here are based on the insights that develop in the development of the evaluation model itself.

MR. DUNN: Another way to come at your question might be to look within the generic applications of our best estimate model. There is something on the order of between 20 and 25 parameters that are varied, each with its own uncertainty distribution within that model. Okay?

Now, we used a technique called non-parametric statistics as opposed to response surface. So what we're doing in that case is we're running cases in which all of the -- those 25 parameters -- and don't quote me on 25. That's what --

JUDGE ELLEMAN: Yes.

MR. DUNN: -- are picked randomly within their distributions, and then combined to produce a case. And then we do a large number of those cases, and we get an estimate of what the -- what an engineer will call a 95 percent confidence number that bounds -- 95 percent of the possibilities is, and that's the level of comparison to the criteria that you -- of 2,200 degrees Fahrenheit.

JUDGE ELLEMAN: Yes, that's --

MR. DUNN: Within that case -- again, it's not McGuire and Catawba that -- the work for McGuire and Catawba is not finished at this point. But on a very similar plant, the mean temperature is about 1,500 degrees Fahrenheit, and the 95/95 -- or the number that we have 95 percent confidence 95 percent of the time will be below runs in the high 1,800s to low 1,900s. Let's say it's 1,900.

JUDGE ELLEMAN: So you're looking at --

MR. DUNN: That says that those variations you might talk about, which aren't -- the peaking factor is varied, the location of the accident within the reactor coolant system is varied, the size of the break is varied, the burnup, if you will, or the exposure of the fuel during the first cycle of operation is varied. Some of the dimensions of the cladding are varied. There's a number of things, most of them system-related, because most of those accidents occur from the system, not the actual fuel.

The fuel is important, of course, because we're analyzing the fuel, but largely fuel is fuel. As long as its powered oxide pellets can change inside cylindrical rods of something like zirconium, you get about the same response, regardless of what you're looking at.

I hope that answers your question.

JUDGE ELLEMAN: Yes, you're certainly getting at what I was asking. And if I understood you correctly, it sounds like if you put in system factors, that a calculation might be something like 1,500 degrees, plus or minus 300 degrees as a 95 percent confidence bound on the integrity of that number. Is that --

MR. DUNN: That's --

JUDGE ELLEMAN: -- essentially what --

MR. DUNN: That's an accurate characterization of what I said, subject to the fact that even in a best estimate model there still remains some bounding baseline approaches that --

JUDGE ELLEMAN: Okay.

MR. NESBIT: And just to clarify one thing, what Mr. Dunn was talking about, again, relates to the best estimate Framatome LOCA methodology, not the deterministic Appendix K methodology that we applied for this particular case.

JUDGE ELLEMAN: Okay.

MR. HARVEY: Can I add one more thing?

JUDGE ELLEMAN: Yes.

MR. HARVEY: When Mr. Dunn was talking about his best estimate calculation, for practical matters, you don't vary all parameters. Some you take at a bounding input to simplify the calculation. So you take -- some of those inputs will be at a bounding condition. So your nominal value -- 1,500 -- includes some parameters that you choose, for practical matters, to be bounding.

JUDGE ELLEMAN: Okay. My second area of questions related to the comments that have been made this morning about conservatisms that have been put in the calculations. On page 24 -- I think it's item 61 of the testimony -- there is a list of five conservatisms.

When you were referring to the conservatisms, is it these five we are talking about? Or are these five simply a small set of a much larger number of conservatisms that are being used?

MR. NESBIT: I think it might have been me that brought it up, so let me just start by saying that what we listed in paragraph 61 are what we considered to be five major conservatisms that are inherent in Appendix K. And I'm going to let Bert, in a second, talk about, you know, the relative magnitude of those.

But I'll also add that a little later in our testimony we also address some conservatisms that are inherent in the way we treated mixed oxide fuel, like assuming the decay heat was the same for MOX fuel even though we knew that it was -- with the MOX fuel.

So there's kind of two different sets of conservatisms we talked about in our testimony. Paragraph 61 addresses Appendix K conservatisms.

Do you want to say anything else about those, Bert?

MR. DUNN: The question I guess was: are

these it, or are there others?

JUDGE ELLEMAN: Yes, right.

MR. DUNN: And there are others.

JUDGE ELLEMAN: And are the others linked in any coherent way that we can look at them, or is it harder to get at them?

MR. DUNN: Well, we have listed them -- the others in a coherent way in our testimony. One of the examples is the initial fuel temperature is a conservatism of, you know, somewhere between 100 to 200 degrees Fahrenheit. And we use -- you know, that's just one of them. It depends upon what base code I am applying to get my initial fuel temperatures from as to how much that margin would be off the mean.

MR. NESBIT: I guess we're not aware of that comprehensive list.

JUDGE ELLEMAN: Yes.

MR. DUNN: I could make one for you.

JUDGE ELLEMAN: Well, I don't know if --

MR. DUNN: It would take a while, but I don't know --

MR. REPKA: I would point out for the record that there -- some of those additional MOX fuel evaluations, specific conservatisms are then addressed in the succeeding paragraph after paragraph 61. So that may get to some of what you're asking for.

JUDGE ELLEMAN: Well, I guess, though, when you are referring to conservatisms you're talking not about the regulatory conservatisms, the ones that are built into your requirements, you're talking about those additional conservatisms that I was asking about and your incorporation of those.

MR. NESBIT: I think we're talking about those.

MR. HARVEY: I think paragraph 61 is -- is pretty much tied to the regulatory --

JUDGE ELLEMAN: Yes. That's correct.

MR. HARVEY: That's Appendix K.

JUDGE ELLEMAN: Yes.

MR. HARVEY: Paragraph 62 gets into some of the MOX-specific conservatisms.

JUDGE ELLEMAN: Additional ones. Okay.

So, really, it's two categories of conservatisms we are alluding to here when we talk about the conservative calculations.

All right. The last question that I wanted to ask, based on your testimony, if -- there have been comments, both in your testimony and you made it today, that a peak clad temperature difference due to MOX of some number -- I think it was six degrees -- is insignificant. And in your testimony, larger numbers are called insignificant.

If one focuses just on MOX, is there some peak clad temperature effect due to MOX where you go from insignificant to significant? And, if so, roughly where is it? Where is the point you start worrying about the effects of MOX?

MR. DUNN: I don't like to have to answer that question relative to MOX in specific, because I don't see much of a difference in MOX performance during a loss of coolant accident versus LEU performance during a loss of coolant accident. And other than that, there are several ways to judge what would be significant and what's insignificant.

One might be to go to the regulations. The regulation is kind of set up 50 degrees Fahrenheit as significant. Most -- but the regulators in this case are trying to be sure they capture everything that is significant.

My personal opinion, based on interactions within the industry, is we need to think in something over 100 degrees before we actually are talking about something that is significant. That within plus or minus 100 degrees it's probably the same answer.

JUDGE ELLEMAN: Okay.

MR. DUNN: And you can get that out of the uncertainty numbers we talked about before as well.

CHAIRMAN YOUNG: I just have question to clarify something for myself. A moment ago you were talking about the best estimate calculations, and you referred to 25, approximately, areas of uncertainty and choosing, I think you said, random values.

And I was thinking back to what you were talking about earlier in the question about maximizing peak clad temperature, and I think you said that in the -- in Table Q14-1, all of the underlying parameters were worst-case scenarios. Is that -- are they the same types of calculations? And normally you did them taking the random values. But when you did these, with regard to MOX use at Duke, that's when you changed from the random values and used the worst case. Am I --

MR. DUNN: I think you're confusing -- Judge Young, I think you're confusing two different basic approaches.

CHAIRMAN YOUNG: That's what I'm trying to get.

MR. DUNN: Okay. When we talked earlier about the Table Q14, we're talking about what's done in what we call a deterministic approach to calculating the LOCA results. The deterministic approach was developed in the 1970s, basically, outlined in the '70s, before best estimate approaches were considered.

CHAIRMAN YOUNG: So there's two different types of --

MR. DUNN: Two entirely different -- in a deterministic approach, you attempt to stack up worst cases, and then do -- do your end results after you stack up a whole bunch of worst-case assumptions.

CHAIRMAN YOUNG: Okay.

MR. DUNN: In the best estimate approach, you mix everything up according to probability distributions for what the individual effect might be, or parameter value might be, and then you do, in our case, on the order of 60 calculations to get a distribution of -- to refine the distribution of peak cladding temperature as a function --

CHAIRMAN YOUNG: Okay.

MR. DUNN: -- of probability.

CHAIRMAN YOUNG: Just for my clarification again, the Q14's deterministic and worst case, where are the calculations? Do we have a list of tables or calculations that were done through best estimates that you did -- through best estimate? Or are you talking generally there?

MR. DUNN: We have not -- yes, we're talking generally. We have not --

CHAIRMAN YOUNG: Okay.

MR. DUNN: -- best estimate LOCA calculation to MOX fuel assembly.

CHAIRMAN YOUNG: Okay. That's -- okay. That's all I wanted to clarify.

Anything else? No?

JUDGE BARATTA: No.

CHAIRMAN YOUNG: Staff, go ahead.

CROSS EXAMINATION

MS. UTTAL: I have one question. Given all of the analyses you've done, and given the core design you've submitted to the Staff, would you expect the peak clad temperature in the MOX LTA to exceed the other fuel in the core? Do you understand the question?

MR. HARVEY: Yes.

MR. NESBIT: Yes. Let me answer it to start, and I'll let my colleagues chime in.

Yes. The MOX fuel would be a lower peak cladding temperature than the other fuel in the core, and the reason for that is twofold. First of all, our analyses have demonstrated that MOX and LEU -- there's just not any significant difference, as we discussed a little bit earlier.

And so the driving thing that's going to drive the peak cladding temperature for these fuel types is power level, first and foremost. And we're operating mixed oxide fuel at a lower power level, a lower peaking factor if you will, which, in fact, we show in our testimony, and we point out that particular figure that's -- no, no, not -- I want to look at the actual peaking figure, which is in -- I think it's towards the end of the testimony.

It's Figure 19 of our testimony, page 65, associated with paragraph 138.

CHAIRMAN YOUNG: Just before you go on, I want to make sure which official exhibit that is.

MR. NESBIT: That's not an exhibit. That's --

MR. REPKA: It's Figure 19.

CHAIRMAN YOUNG: Do you have your individual sheets that they could use? So they could --

MR. REPKA: Yes. And Figure 19 would be the last exhibit. I believe it's Figure --

CHAIRMAN YOUNG: 24?

MR. REPKA: -- 24.

CHAIRMAN YOUNG: It's 24. Thank you.

MR. NESBIT: It's Duke Exhibit 24. Duke Exhibit 24.

MR. REPKA: But I think that's overall Exhibit 50 under the sequential numbering that --

CHAIRMAN YOUNG: No, it's 24.

MR. REPKA: Oh, I'm sorry.

CHAIRMAN YOUNG: Your -- their CVs were 47 through 50.

MR. NESBIT: The point that I was making in responding to the question is that in conformance with our specifications that require that we load the MOX fuel assemblies to non-linear locations, they will be operating at a lower power than the co-resident LEU fuel. And this graph indicates that the LEU -- the chief LEU fuel assembly will be at a significantly higher power throughout.

So with power driving the LOCA response, it would be our expectation that the peak cladding temperature of a LOCA should occur -- the actual peak cladding temperature would be higher in the co-resident fuel.

With that being said, I'm ignoring differences between fuel assembly hydraulics, and I admit that up front. But it is our understanding and experience that these two fuels can be tied fundamentally somewhere, and so that would not be as big an effect as the power itself.

I don't know. Is there anything, Bert, you might want to add to that?

MR. DUNN: The only thing I want to add is I'm not sure your use of the word "yes" was the correct response to her question.

MR. NESBIT: Yes.

MR. DUNN: So listen to the words you said, not necessarily whether it was --

MR. NESBIT: I think the correct response was no, but --

MS. UTTAL: But I was asking you whether you would expect the MOX LTA to exceed the other fuel.

MR. NESBIT: No.

MS. UTTAL: Okay. Thank you.

Thank you.

CHAIRMAN YOUNG: Mr. Repka?

MR. REPKA: Yes. I just have one or two questions.

REDIRECT EXAMINATION

MR. REPKA: There was some discussion earlier in response to some questions by Ms. Curran about the Westinghouse best estimate LOCA evaluation model, and the fact that that best estimate model, which is different from the deterministic Appendix K model used for the MOX fuel assembly, included, among other things, relocation, whereas the Appendix K model does not.

Could you respond to -- and, obviously, this hasn't been done. But in your judgment, if you could do an evaluation model for the MOX fuel assemblies, considering relocation and the other factors that you've identified in your testimony, like the differences in decay heat between MOX and LEU fuel, what would be your expectations regarding whether -- regarding relocation effects on your numbers?

MR. DUNN: I'll answer that relative to our deterministic evaluation model. If we were to add a fuel relocation model into our deterministic evaluation model, we would expect that the peak cladding temperature for the ballooned area, or the ruptured area, which are the same locations on a fuel pin, would increase from the current predictions that we make.

But it would approximate the temperature that we are calculating at the other locations on that fuel pin -- the location that we currently call the

PCT location. We might go into the 10-foot level case when the rupture typically occurs at the location of peak power. That's not always true. You do have to consider the cooling mechanisms as they develop along the pin, but it's going to be very close to the location of peak power.

When rupture occurs, it induces cooling mechanisms appropriate at that location and above that location. So the location of peak cladding temperature for the 10-foot case is generally on the order of nine inches or so below the location of rupture. It could be six inches below the location of rupture.

And those extra cooling mechanisms, in combination with the expanded surface area, in combination with what we would conclude would be reasonable fuel relocation effects, will result in a temperature that's close to, maybe slightly below, could be slightly above, within a reasonable bound of uncertainty well within the conservatisms that we talk about when we include a -- when we do a calculation under Appendix K. And that's our expectations for the result of that type of calculation.

MR. NESBIT: And I'd just like to add to that that if we were to perform a model including fuel relocation effects, whether it be a best estimate or an Appendix K model, or whatever, it would be our expectation that the MOX result would be very similar to the LEU result, for the same reasons that we've just been discussing, that the LOCA behavior is driven by other parameters and it's not really a MOX effect.

MR. REPKA: Let me make sure I understand what Mr. Dunn just said, that the -- you believe that the peak temperature at the ruptured location would still be very close to or below the peak cladding temperature at the non-ruptured location. Is that correct?

MR. DUNN: Yes, that is true.

MR. REPKA: So the non-ruptured location still remains the limiting case?

MR. HARVEY: Can I point out that the FR-2 experiments that we referred to in our direct testimony show the effect that with relocation, the rupture location, the cladding temperature at the rupture location is not the limiting cladding temperature. It shows that the other location on the fuel rod was at its maximum cladding temperature response. So here's an experiment which actually models, which had a relocation phenomenon occurring, and it raised that temperature of that node, but did not exceed the node temperatures in other locations.

MR. REPKA: All right. And you're referring to Figure 11?

MR. NESBIT: Duke Exhibit 16, Figure 11.

MR. HARVEY: That is correct.

MR. REPKA: That's all I have right now.

CHAIRMAN YOUNG: I'd like to just clarify something right here, because I remember when you were talking about that before, and I understand that a major difference - and correct me if I'm wrong - but I understand that a major difference in Dr. Lyman's view and your view is that the peak clad temperature will occur at an area that's not the rupture area. And Table 10, Figure 10, which would be Exhibit 15 shows that fairly clearly. What I'm not totally clear on is in Figure 11, which would be Exhibit 16, it looks like the temperature of the ruptured location is pretty close, and I wanted -- it's Dr. Lyman's suggestion that the temperature would increase by 300 degrees I think is what his testimony is, by virtue of fuel relocation at the rupture location. And it's as close as it appears to be on Exhibit 16, I wasn't all together clear on the significance of -- I think that the total there is slightly over 1,800, but is his theory that it would raise it 300 more degrees?

MR. DUNN: Dr. Lyman's theory would be that the 300 degrees I think would apply to the temperature in Figure 10, and not the temperatures in Figure 11.

MR. NESBIT: The reason -- in Figure 10, you see the dashed line there is the temperature of the ruptured node.

CHAIRMAN YOUNG: Right.

MR. NESBIT: And as you see, after the rupture, there's actually a benefit.

CHAIRMAN YOUNG: It goes down. Right.

MR. NESBIT: And that's because there is no fuel relocation in this particular scenario, so that you can get the benefits, but you don't get --

CHAIRMAN YOUNG: Oh, no relocation.

MR. NESBIT: You don't get any penalties. And so if you were going to put a delta on top of that to account for a fuel relocation effect, it would applied to this curve as Mr. Dunn indicated. I guess I should also point out --

CHAIRMAN YOUNG: Which curve?

MR. DUNN: The dashed curve.

MR. NESBIT: The dashed curve.

CHAIRMAN YOUNG: On 10.

MR. DUNN: On Figure 10.

CHAIRMAN YOUNG: Okay. Exhibit 15. Okay.

MR. NESBIT: But the other thing that I guess we would take issue with is throwing around numbers like 313 degrees --

CHAIRMAN YOUNG: Well, before you get to that, I guess my lack of clarity may be just very simple. The lines on Exhibit 16 are much closer together than on Figure 15. You're saying that the conservatisms would keep them at 1,800, even though the temperature at the rupture location does go up compared to where there's no relocation.

MR. NESBIT: Yes. I'll let Mr. Harvey --

CHAIRMAN YOUNG: Is that --

MR. HARVEY: Yes. What we're saying is with the fuel relocation, the two temperatures, instead of being hundreds of degrees below each other, they're going to be closer together. My reason for that is, in our view, the benefits of the ballooning and rupture approximately equal the negative effects of the fuel relocation, the additional fuel material in the balloon, so it about evens out. Instead of being a big penalty, it's just about a wash.

CHAIRMAN YOUNG: Okay. I think I understand. Thank you.

MR. HARVEY: So, therefore, the calculated number without fuel relocation for the other location; in other words, the solid curve on Figure 11, that number is unchanged and still you're limiting PCT nodes.

CHAIRMAN YOUNG: Right.

MR. HARVEY: You don't have to be concerned about the ruptured node with fuel relocation. That's not limiting. It doesn't affect your peak cladding temperature.

CHAIRMAN YOUNG: Okay. It may get close, but it doesn't go over.

MR. HARVEY: That's right.

CHAIRMAN YOUNG: That's what you're saying. Okay.

MR. DUNN: Just to add one thing, I believe Dr. Lyman's 313 is from a calculated result, not from an experimental result. And we take issue with that calculated result because it included all of the negatives, and none of the benefits.

CHAIRMAN YOUNG: Okay. So this incorporates the benefits which his analysis, you're saying, does not include as a benefit, which you'll explain I'm sure when you --

MR. HARVEY: Yes. This is an experiment, the other one is a code calculation.

CHAIRMAN YOUNG: Okay. You were finished?

MR. REPKA: I'm finished.

CHAIRMAN YOUNG: Ms. Curran, do you have any more questions?

MS. CURRAN: Yes, I do. I think this question is for either Mr. Nesbit or Mr. Dunn. Does Framatome best estimate methodology for LEU fuel LOCA analysis include consideration of fuel relocation?

MR. DUNN: The answer to that is not at this time. The work was submitted in the review process for the best estimate model that supported the concept that calculating the fuel cladding temperature away from the location of rupture was sufficient and somewhat conservative. The model at this time also does not include the calculation of swelling and rupture because work was also submitted on that topic which demonstrated that the location of rupture, the extra cooling that occurs at that location makes it a non-limiting location.

CHAIRMAN YOUNG: A non-what?

MR. DUNN: A less severe --

CHAIRMAN YOUNG: I didn't hear you.

MR. DUNN: I'm sorry.

CHAIRMAN YOUNG: I just didn't hear the word.

MR. DUNN: A non-limiting location.

CHAIRMAN YOUNG: Limiting. Thank you.

MS. CURRAN: So relocation is an actual phenomenon that occurs - right - with LEU fuel.

MR. DUNN: I think I would have to respond to say that relocation has been shown sufficiently to state that we have to consider it in our deliberations as to how to assemble an evaluation model. In other words, it has to be provided for in some sense.

MS. CURRAN: And it appears that Westinghouse chose to model it, while Framatome chose to basically analyze it away. Is that right?

MR. DUNN: Well, Framatome chose to demonstrate that the ruptured location was not a limiting location, and then did not choose to complicate its evaluation model with the model we're discussing, the relocation model.

MR. NESBIT: I'd just like to add that again, the analyses that we're talking about in this application are the deterministic Appendix K analyses, not the best estimate analyses of Framatome, or the best estimate analyses of Westinghouse. And to our knowledge, nobody that has an Appendix K model includes fuel relocation effects.

MS. CURRAN: But it is true, isn't it,

that Appendix K does not technically apply to Plutonium fuel. Is that right?

MR. NESBIT: What's the basis for you saying that?

MS. CURRAN: Because of what the regulations say. Haven't you applied for an exemption?

MR. NESBIT: Actually, the exemptions we applied for in that area were to make the regulations apply for fuel containing Plutonium, instead of low enriched Uranium.

MS. CURRAN: That's right, because there's no comparable regulation to Appendix K for Plutonium fuel. Isn't that right?

MR. NESBIT: I'm not sure what your point is.

MS. CURRAN: Well, what's the answer to the question?

MR. NESBIT: I don't know the answer to your question, because it's sort of a regulatory interpretation question. It's our view that the applications we've made and the associated exempt requests do make it appropriate to apply the 50.46 criteria and Appendix K to our analyses. And I think the NRC has agreed with that in their safety evaluation. I don't think that's the subject of this contention even.

MS. CURRAN: If you look in your exemption application, you considered the appropriateness of the Appendix K criteria for this particular type of fuel that you're proposing to use. That's right, isn't it?

MR. NESBIT: I think it's fair to say that we considered Appendix K to be just as appropriate for MOX fuel as it is for LEU fuel.

MS. CURRAN: And it's also potentially appropriate to consider other factors that are unique in MOX fuel that could be added to the Appendix K analysis. Is that correct?

MR. NESBIT: There's been no demonstration of any other factor that needs to be added apart from those which we considered and adjusted for in our application and in our analysis.

MR. DUNN: Well, I'd like to add, I think you are correct if there were such a thing, because as we've gone in here, we sought to demonstrate that those items don't exist, so there isn't any need to advance any of the criteria of Appendix K. Now an example would be the Baker-Just correlation, if somehow you demonstrated that it was not conservative for a given type of fuel, then you might want to adjust Appendix K's requirement to do that. In this case, it remains conservative by about 50 percent.

MR. NESBIT: But I think it's also important to point out that Appendix K doesn't say thou shalt not account for fuel relocation. It's silent on the issue.

MS. CURRAN: Do you think we could just take a 10 minute break while we consider whether we have a few more questions?

CHAIRMAN YOUNG: Okay.

MS. CURRAN: Thank you.

(Whereupon, the proceedings in the above-entitled matter went off the record at 10:06:22 a.m. and went back on the record at 10:19:25 a.m.)

MS. CURRAN: All right. I believe that a little earlier I heard one of you say in general that the impact of fuel relocation on the clad temperature at the rupture location would be close to the original PCT under relocation conditions within a reasonable bound of uncertainty. Is that right?

MR. DUNN: Yes, ma'am.

MS. CURRAN: Okay. Do you have any experimental basis for this conclusion?

MR. DUNN: One experimental basis is within our testimony.

MS. CURRAN: And what is that?

MR. DUNN: That's the test performed in FR-2 reactor. It's Duke Exhibit 15 and 16.

MS. CURRAN: Do you have any experimental MOX data?

MR. DUNN: We have data that's for MOX that supports the general applicability of doing MOX pellet tests on MOX fuel; in other words, the basic phenomena that are taking place to go forward and lead to fuel relocation have been examined between -- to some extent between MOX and LEU fuel. And those portions behave similarly. Do we have specific fuel relocation studies on MOX fuel which would simulate a LOCA? I do not believe that those types of experiments have been done.

MS. CURRAN: Okay. Thank you.

DR. LYMAN: Sorry. In the interest of time, the second question is just that we'd like you to confirm that the 600 degree Fahrenheit margin that you testified was present in the AREVA best estimate LOCA analysis relative to Appendix K analysis did not consider fuel relocation effects.

MR. DUNN: Probably. The Appendix K model that we applied on one side of that difference did not include fuel relocation effects. By the time we dropped down to the temperatures for the best estimate calculation that we're getting the mean from, we're below the temperatures at which you would expect swelling and rupture to occur, so you wouldn't have the conditions in that calculation necessary for fuel relocation. It would be down to 1,400, 1,500 degrees.

MS. CURRAN: That's all the questions that we have.

MR. REPKA: I have nothing.

CHAIRMAN YOUNG: Thank you.

JUDGE BARATTA: All right. Let me start with respect to a couple of clarifications of what I've heard this morning, and I'd like to go into some clarifications with respect to your pre-filed testimony, if I could. To the extent that the work that you mentioned with respect to the Westinghouse COBRA/TRAC, I gathered that the resulting filling ratio that's used as an input parameter as opposed to a calculated value, and I think if I recall, and I may not be recalling this correctly from your testimony, that for LEU fuel values are typically on the order of say .5 or thereabouts for that filling fraction. Do you know off-hand if the value that was used in the Westinghouse COBRA/TRAC was of that nature or considerably higher? And I phrase it that way because it might be proprietary. That's why I -- if you can give me a specific value, fine. But if you can't --

MR. HARVEY: I don't know if I can give you the value.

JUDGE BARATTA: Okay. Can you say whether it's on the order of --

MR. HARVEY: It is on that order.

JUDGE BARATTA: Up to .5 as opposed to --

MR. HARVEY: In that vicinity, yes.

JUDGE BARATTA: Okay. All right. That was the first question. The second question concerns the relative difference that Mr. Dunn cited with respect to an Appendix K calculation versus a mean value for a best estimate calculation, the information you were -- or based on your experience with the Framatome codes. And I just was curious as to whether or not that's typical; in other words, if you were to go and look at somebody else's best estimate code and compare it to Appendix K, would you see comparable - I don't know whether it's 550 degrees or 600 degrees, that's comparable to me.

MR. DUNN: I think we have the information from Mr. Harvey relative to the results of the Westinghouse best estimate calculation of a mean, and if you look at Appendix K-type calculations, they're all -- they all result in temperature, peak cladding temperatures in the 2000 to 2,100 range typically. So, Bob, you have 1,500, I believe.

MR. HARVEY: Fifteen hundred and change is the best estimate PCT for the Westinghouse analysis for Catawba, McGuire-Catawba. And again, that is also considering the worst steam generator design, as well. It's another analysis which takes the worst plant features from all four units and confines them, so that has an inherent conservatism in that, as well. So if you do the numbers, it comes out to five or six hundred --

JUDGE BARATTA: Okay. So that's correct that that would be the least of those two typical --

MR. NESBIT: Well, let me add to that, that if you look in NUREG 1230, is a compendium of ECCS Research for realistic LOCA analyses, there's other analyses in here and the same type of margins are referred to in that document, as well.

JUDGE BARATTA: Okay. Before I go on to their testimony, are there any points that either of you want to clarify on those two points that I just raised, or should I go on to --

CHAIRMAN YOUNG: Go ahead.

MS. CURRAN: I would like to have a

follow-up question on that one, on that point.

CHAIRMAN YOUNG: If it would be easier, why don't you go ahead and then we'll keep it on the same.

MS. CURRAN: I think, Mr. Harvey, you said that the best estimate by Westinghouse is 1,500 degrees. Is that --

MR. HARVEY: That is the nominal -- that's a best estimate number. There's two numbers you get from a best estimate, and that's you get the 50 percentile value, which is the nominal expected result, and then you get the 95-95, the uncertainty adjusted value, which is much higher.

MS. CURRAN: And that's the number in Table K-6 of the LAR, which is Exhibit 1, 20.56.

MR. HARVEY: Yes, that's the number.

MS. CURRAN: Okay. That was it.

JUDGE BARATTA: All right. Going to your testimony and looking at the pre-filed testimony, as well as your rebuttal testimony, and Mr. Dunn in his paragraph 48, there's a statement - I believe this is on page 20, although I'm not sure. I'm looking at an electronic copy so the pagination may be slightly different, but it's paragraph 48 there.

You say that M-5 is also being used in

foreign nuclear power reactors including MOX fuel applications. Could you be more specific as to where?

MR. DUNN: The primary -- excuse me, Judge Baratta, did you say -- I didn't read that. Did you say it's being used in foreign reactors with MOX fuel?

JUDGE BARATTA: It says, "M-5 is also being used in foreign nuclear power reactors, including MOX fuel applications."

MR. DUNN: The MOX fuel applications are primarily in Germany.

JUDGE BARATTA: Okay. So it's the German reactors.

MR. DUNN: Yes.

MR. NESBIT: There's four German reactors currently using M-5 clad MOX fuel.

JUDGE BARATTA: Okay.

MR. NESBIT: I believe we may have addressed that in our rebuttal testimony.

JUDGE ELLEMAN: It shows up later.

JUDGE BARATTA: Oh, does it? Okay.

JUDGE ELLEMAN: That question related to one of my questions. Have any of these irradiation accumulated high burn-ups to this point on the M-5 clad --

MR. NESBIT: I don't know the answer to

that question.

MR. DUNN: We have accumulated high burn-ups on M-5 cladding. Whether or not those high burn-ups were with MOX fuel, I can't answer you.

JUDGE ELLEMAN: No, I -- well, it wouldn't have to be with -- okay.

MR. DUNN: We have burn-ups towards 70 gigawatts days per metric ton on M-5.

JUDGE ELLEMAN: On M-5.

MR. NESBIT: I believe M-5 clad MOX fuel has been in German reactors for several years, but I don't have any specific information on the burn-up.

JUDGE ELLEMAN: But it would presumably be in the range of 30 to 40 gigawatt days per metric ton, somewhere around there.

MR. NESBIT: Yes.

JUDGE ELLEMAN: Okay.

JUDGE BARATTA: Okay. Any further points on that, or can I go --

CHAIRMAN YOUNG: Go ahead.

JUDGE BARATTA: All right. In paragraph 54, I may be misreading what you're saying here, but it seems as though you imply that Appendix K requires the evaluation model unirradiated cladding properties, and maximizes the --

MR. DUNN: I don't think Appendix K requires you to use unirradiated cladding properties. Appendix K says that you should not under-estimate the incidence or degree of clad swelling rupture.

JUDGE BARATTA: Okay.

MR. DUNN: The vendors use unirradiated properties because that does that.

JUDGE BARATTA: Okay. That's the point I wanted to get a clarification on. Okay. In paragraph 58, there's mention of a 40 degree difference between MOX fuel and LEU fuel comparison cases. And I admit based on what we've heard this morning that that 40 degree difference is less than the 50 or 100 degree difference that, depending upon who you are, you would conclude is significant. I was just curious though, is there any specific factor or factors that you could attribute the difference to?

MR. DUNN: I don't believe that we're going to be able to pinpoint the specific reason for that 40 degrees. The fuel temperature comparison is extremely close. There's no real difference in this particular case for the two in the fuel temperatures. The mathematical modeling has a little sensitivity to points for an advancement in the bottom of the core, and some small differences between the fuel pellets -

I'm sorry - between the fuel pin performance alter the sensitivity of those filling as quench front movement actually, Judge Baratta. And we've only modeled it with a certain amount of accuracy, with a certain amount of disparatization, and I think we changed the pattern of that a little bit, and that's what resulted in the 40 degree change. I don't really think it's a real change. And if we took a mean of our prediction; a running average through the temperature prediction, which might be a better thing to do than actually use the specific peaks, I think we would show very little difference between the two calculations.

JUDGE BARATTA: You mentioned the grid that was used to predict quench front propagation. Is that correct?

MR. DUNN: That's correct.

JUDGE BARATTA: Okay. And by that, you're talking about the numerical grid that was used in the calculation. Is that correct?

MR. DUNN: Yes, I am.

MR. NESBIT: Judge Baratta, just let me add that there's actually a comparison of the two peak cladding temperature predictions in our Exhibit 1, with the LAR analysis. It's on page 3-55, Figure 3-10. And I think that comparison structures and it bears out what Mr. Dunn just mentioned, that fundamentally calculations, Appendix K calculations are not smooth, straight lines, but that if you look at these two, they are vertical overlays.

JUDGE BARATTA: You're referring to Figure 3-8 and 3-9 it appears.

MR. NESBIT: I think I'm referring to Figure 3-10. Yes, I am referring to Figure 3-10 on page 3-55.

JUDGE BARATTA: All right.

MR. NESBIT: That's the MOX LEU case.

MR. DUNN: In my response, I mentioned the fuel temperatures are the same. What I meant was the initial fuel temperature distribution was essentially the same between the two.

JUDGE BARATTA: That's the fuel pellets.

MR. DUNN: Fuel pellets.

JUDGE BARATTA: And that was both. Okay. So that's within the fuel pellets. That's the regular distribution primarily.

MR. DUNN: Yes.

JUDGE BARATTA: All right. In paragraph 61 which we discussed somewhat earlier, you list the major conservatisms, including regulation within the approved Appendix K methodology. In glancing through the Appendix K, I thought there's also mention of the -- assuming there's no heat transfer until you actually get rebut as opposed to rewaxing of the surface. Is that --

MR. DUNN: That is correct.

JUDGE BARATTA: Okay. And was that also -- you didn't list that in your list of items.

MR. DUNN: We did not list that, and that's probably something that could have been listed there. There is a portion even in our real transfer where you go to low heat transfer for a period of time towards the end of blow-down, and the beginning of refill. But it's certainly not as extended as in a deterministic calculation.

JUDGE BARATTA: Okay. And then the next paragraph you make mention of the best estimate calculation on three new Westinghouse designed plants. Was that also done with the Westinghouse COBRA/TRAC --

MR. DUNN: The best estimate model we're quoting here is the Framatome best estimate model.

JUDGE BARATTA: Okay. I'm sorry. Thank you. All right. In looking at 63 here, it says -- paragraph 63, "Because the PCT is strongly insulated -- estimated to be a conservatism of up to 75 degrees Fahrenheit on PCT." How did you estimate that 75 degrees?

MR. DUNN: I believe I did just a 3 percent power balance against the assumed vapor temperature and kept the heat transfer process, I was assuming the same as it got there.

JUDGE BARATTA: Okay. So you assumed the heat transfer regime was the same, and therefore, the correlations and such were the same.

MR. DUNN: Yes.

JUDGE BARATTA: So the only change was the heat input.

MR. DUNN: Change the heat source and just looked at the Delta T.

JUDGE BARATTA: Okay. All right. You make -- there's a rather broad statement that appears in paragraph 66 under "Differences in LEU and MOX fuel behavior."

MR. DUNN: Judge Baratta, I would like to say that I said up to 75.

JUDGE BARATTA: Okay.

MR. DUNN: So that would be the upper end, I think. You know, I didn't mean that 75 --

JUDGE BARATTA: Yes. I apologize if I misquoted you there. I was trying to get the basis for how you came up with this range rather, as opposed to the specific number. In paragraph 66 there's a rather broad statement, which says "This has determined that each behavior, these are inapplicable to the dry-base LOCA calculation or the LOCA evaluation. Clearly insignificant in terms of impact on the LOCA calculations, and/or specifically addressed later in the testimony."

Some of the items when you get into their -- there's kind of a general promise that's not applicable, did you, in fact, go through each of these items and evaluate them in detail, and it just isn't in your testimony. IS there a summary there? I wasn't quite sure about the methodology that was used. I was hoping to get clarification on that.

MR. NESBIT: The testimony is a summary of the evaluation that we performed on the list of potentially phenomena that may essentially affect the difference between MOX and LEU. We didn't run any LOCA calculation, sensitivity studies or anything on these. In most cases, it was readily apparent, the effect or actually the lack of effect based on work that we had already done.

JUDGE BARATTA: So in part, it was based on a review of the literature, engineering judgment, and such. Is that fair to say?

MR. NESBIT: It was a mix. And a good example item is the very first item about rod centerline temperature. We had already calculated that and shown that in our LAR to show that there really wasn't a big difference between the fuel temperatures of MOX and LEU. And we just presented again the information that we had already presented to demonstrate that fact.

JUDGE BARATTA: Okay. In paragraph 73 there's mention, and I've also heard it earlier, that the MOX fuel lead assembly core design would ensure that the MOX assemblies are not the peak power assemblies, that is the highest heat generation rate, and at four during the depletion. I gather -- is it correct then to say that that's going to be invoked, required by the core -- help me with your terminology. I would call it the tech specs, but you put it in the core limit report? Okay. So that's where that will specify thou shalt not put them in there.

MR. NESBIT: That's correct.

JUDGE BARATTA: And I assume that's where all the restrictions on where those can go will appear. Is that correct?

MR. HARVEY: Yes.

JUDGE BARATTA: One of the ones that I

think also appeared earlier is that the MOX assemblies are not going to be in rodded locations. Is that also --

MR. NESBIT: Yes, we plan to load the MOX assemblies in unrodded locations.

JUDGE BARATTA: Okay. And that's also going to be in the core operating limit --

MR. NESBIT: That's actually in our final fuel cycle design process. It's specified out as one of the requirements in there.

JUDGE BARATTA: Okay. Going to paragraph 84, it says that -- this is a discussion of the M-5 cladding. It says, "The test shows that the 2,200 degrees F PCT 17 percent local oxidation limit specified as acceptance criteria apply to M-5 cladding, as well as to the Zercoloid-4." Do you have a reference for that, where those results --

MR. NESBIT: Go to 3 for the impact topical report at BAW 10227.

JUDGE BARATTA: Okay. That was one that was provided, I believe, earlier in the proceedings. Is that the same report?

MR. NESBIT: It was noted by us, Judge, as potentially relevant information. I honestly can't remember if we provided it, or if we just provided the reference to the ADAMS number.

MR. REPKA: It is referenced again in paragraph 29 on page 11, if you want the full reference.

JUDGE BARATTA: Okay. I just wanted to clarify where that came from. Going now to 98, it says in paragraph 98, and it's discussing again this issue of fuel pellet relocation, "An assumption of very high packing factor or filling ratio within the balloon sections required have developed any concern about the amount of heat generation following relocation." Can you provide a range of values for the packing fraction? In other words, is it closer to the nominal values, or not nominal values but a value of .5, or is it more like .7 or .8?

MR. DUNN: I believe somewhere in our testimony we indicated that packing factors up to about 65 percent might be reasonable. That comes from the FR-2. Do you remember where we --

MR. NESBIT: Yes, that's right.

MR. HARVEY: It's on page 58, paragraph 120.

JUDGE BARATTA: Okay. And those are ones that are typical, but that's -- I mean, that you could get. What I'm asking here is could you be -- it says an assumption of a very high packing factor or filling ratio. What's very high?

MR. NESBIT: Well, let me just try to address that by stating that we haven't performed a specific fuel relocation analysis to say this filling ratio is okay and this one isn't okay. But what we do know is that the numbers that are being cited by the Intervenor in this case, the 313 degrees, are based on a filling ratio of 70 percent, for example. Obviously, we don't know all the details of the analysis that IRSN did, and IRSN is not here for us to ask them about it, but that's an example of using a very high filling ratio and coming up with a number that when we -- even though we don't agree that that is an appropriate calculation of the Delta PCT, if you will, on the ruptured node, we think it's a very conservative representation. We still can take that and add it to our ruptured node cladding temperature, and still show that we're within the 2,200 degree limit.

JUDGE BARATTA: All right. Looking at, and I've got to figure out which exhibit number this would, it's Exhibit 19, which is Figure 14 in your testimony. In the paragraph 111, it says that, "Typical rupture temperatures", I believe this was also stated earlier in this morning's testimony or question, I should say on cross examination; that typically rupture temperatures for the Catawba plant lie between 1,500 and 1,600 degrees placing the rupture temperature during the transition of the cladding from the Alpha phase to the Beta phase. Approximately where on that figure would that lie? I mean, I realize that some of that data is proprietary so you can't be extremely specific, but --

MR. DUNN: I would like on the bottom of the trough, if you will, between the two peaks. There's approximately at the mid-fraction of the phase transition. In other words, at that point the material is approximately 50 percent Alpha, and 50 percent Beta. The typical rupture locations occur just a little bit beyond that point, the point at the bottom, and backwards maybe of about halfway up the strain curve.

JUDGE BARATTA: Okay.

MR. DUNN: And I'm being a little approximate here.

JUDGE BARATTA: Yes, I understand. That's sufficient. I wanted to make sure that it was consistent with the physical -- physically reasonable, so that's the minimum strain there that you could --

and temperatures, things like that. That would have been the spot, but since it wasn't labeled on there, it wasn't clear if that was the spot.

Okay. I think that -- well, do you want to go ahead and -- since I've monopolized things at this point. Would you --

JUDGE ELLEMAN: I have just several questions that I would characterize as clarification questions. In Figure 17, which is on page 57, and on Figure 16 on page 56, you have micrographs for radiated LEU fuel and MOX fuel. Because of the low thermal conductivity of Uranium Oxide, I would expect any fuel pellet that is irradiated as thermal neutron fluxes comparable to a power reactor would show centerline melting of the pellet. There would be transfers, cracks radiating from the center. There would be center void as a result of recrystallizing the material, but none of t his shows in the radiographs. What am I missing on these?

MR. NESBIT: We don't have centerline fuel, no.

JUDGE ELLEMAN: You don't get centerline fuel?

MR. NESBIT: No. We've included our core design in our analysis.

MR. HARVEY: Not only during normal operation, also during any populated transient, we're not allowed to have fuel centerline.

JUDGE ELLEMAN: You get no recrystalization crystal growth in the center of the fuel at normal operating conditions?

DR. McCOY: I can imagine some crystal growth at operation temperatures.

JUDGE ELLEMAN: Yes, but it isn't showing in these radiographs, so it must not have occurred here. But these are radiographs of pellets radiated at reactor --

DR. McCOY: They're optical photographs, not radiographs.

JUDGE ELLEMAN: Right. Okay. Cross-sections of the --

MR. NESBIT: But they are operating reactor fuel.

JUDGE ELLEMAN: Okay. All right. On page 61, item 130, I had trouble with the sentence near the bottom that says, "Higher power at end of life is adverse for fuel relocation." I couldn't relate the second part of that sentence to the first part of it. And maybe it would help if you simply went through why is higher power at end of life adverse to fuel relocation?

MR. NESBIT: It's right here. The concern over fuel relocation arises out of the possibility that you'll accumulate fragments of fuel larger than the original pellet itself in one location, one spot in the cladding where it's ballooned out. And because you have more fuel there, you have more of a heat source, more power, so the point that has been made is that if you're operating at a higher power, then these fragments that come down will have higher --

JUDGE BARATTA: Yes.

MR. NESBIT: Because if they operate at a higher power, they generate more fission products.

JUDGE BARATTA: But that's not just end of life the power, that's steady state operating power too.

MR. NESBIT: That is correct. And in fact, in retrospect, the end of life illusion came from one of the IRSN presentations that was one of the bases of the BREDL contentions in the first place, where they highlighted that as an end of life effect. But I would agree with you. I would agree with you that fuel relocation does not just occur at end of life, and high powers don't just occur at end of life, so it would be an issue at any point in time.

JUDGE ELLEMAN: Okay. All right. On page 9, item 25, there's "Significantly, there has never been a MOX fuel failure attributable to fuel pellet material." That wording suggests there have been failures. And what are we talking about here, pinholes in the fuel clad, fracturing of the clad?

MR. NESBIT: Yes. Actually, the MOX fuel performance history is summarized in one of the topical reports that we referenced in our license application, which is the MOX fuel design report, which coincidentally just got its safety evaluation from the NRC earlier this month. The kind of fuel failures that had been observed in the MOX fuel that's operating in Europe had been the same as the kind of fuel failures that we observed typically, fretting, debris, rubbing, things like that. To my knowledge, nothing - and this is also consistent with the literature - there's been nothing about one of these failures that's been attributed to any pellet-type of an effect. But it's the same garden variety fuel failures that we've occasionally experienced.

JUDGE ELLEMAN: And are there enough data to imply that the frequency of occurrence is essentially like the LEU fuel?

MR. NESBIT: It's based on a database of

hundreds of thousands of rods. I think it's up around 600,000 rods. It's a pretty substantial database.

JUDGE ELLEMAN: Okay. Let's see. That may be all I have. Yes.

CHAIRMAN YOUNG: I'd like to clarify some things too, sort of in different areas mainly, but one thing that you were talking a moment ago about the centerline. And in Dr. Lyman's pre-filed direct testimony, question 16 on page 10, he talks about increased centerline temperature and stored energy; therefore, reducing the margin of a design basis LOCA limit. Could you speak to that in the context of your earlier comments?

MR. DUNN: Could you give us the reference page again?

CHAIRMAN YOUNG: Page 10, question 16, the second paragraph at the end.

MR. NESBIT: I believe we addressed that in our rebuttal testimony.

CHAIRMAN YOUNG: You may have.

MR. NESBIT: Yes, let's just go back to our direct testimony, because I know that's where we went with that. It is Figures 4 and 5 in our direct testimony, and that's Exhibits 9 and 10.

CHAIRMAN YOUNG: What pages are they on in your testimony?

MR. NESBIT: That's on pages 28 and 29.

CHAIRMAN YOUNG: Thank you.

MR. NESBIT: And the point that's made there is that you hear a lot made about the difference between MOX and LEU thermal conductivity, but the reality is that there's really not that big a difference, as Figure 5 shows. That's a plot. It's almost an overlay. The MOX is slightly lower in terms of thermal conductivity than LEU, both get no burn-up, and at 40 gigawatt per day ton burn-up which were the two points they were evaluated at. And the result of that is that when you look at an actual profile of the temperature through the fuel pellet in Figure 4 and zero on the X-axis there is the center of the fuel pellet, and it goes out in the first dashed line there as the edge of the fuel pellet. And as you can see, the temperatures are virtually the same, so it's really not a significant effect.

DR. McCOY: In Exhibit 10, I'd point out that we're supposed to be comparing not the upper pair of curves and the lower pair of curves, but the two curves within one pair.

CHAIRMAN YOUNG: Right. I understand. Now the main area that I wanted to get some clarification on - in your direct testimony at paragraph 164 and following -- well, you actually start talking about it a little bit earlier. And your point is, as I understand it, that if you think you're saying that if you did all the testing that Dr. Lyman is suggesting you do, it would be difficult to ever get to a lead test assembly stage, and that the lead test assembly stage, if you will, provides some of the testing within - and this is what I understand you to be saying - within enough margin of safety that it's appropriate to go forward with the lead test assembly.

MR. NESBIT: I would agree with everything you said, and I would also add that in this particular --

CHAIRMAN YOUNG: Well, I just want to make sure I'm accurately reflecting --

MR. NESBIT: There is another key thing that I was trying to communicate with the chicken versus egg analysis, and that is to say that the tests that are prepared by Dr. Lyman require that you have prototypical irradiated fuel. Well, in order to get prototypical irradiated fuel, you have to put it in a reactor.

CHAIRMAN YOUNG: And I know there's some testimony somewhere about waiting to get the --

there's approval then sought for the needs of some of the irradiated rods from Europe.

MR. NESBIT: Well, Dr. Lyman has suggested that as a possible way to get around this issue of while we need prototypical irradiated fuel rods, so we can use some irradiated fuel rods from Europe because they have MOX fuel over there, and we can use that. But in the same breath, he points out that fuel from Europeans is reactor grade mixed oxide fuel, and not weapons grade mixed oxide fuel as we propose to use here in the United States, and that those issues have to be addressed, as well. So leaving aside any discussion of the practicalities of the testing, the need for the testing, whether you can actually get your hands on this fuel because it's not our's, it's their's. I do think that the fundamental proposal that he's making ignores the fact that at some point you have to take the step of irradiating fuel in a prototypical environment, and this is the right time to do it for this program.

CHAIRMAN YOUNG: Starting with this concept, I noticed a place in paragraph 164 of your testimony where you talked about the PHEBUS test, P-H-E-B-U-S test that had been proposed, and you talk about the time and preparation, and so forth. And that certainly needs, making some comparisons between your testimony and looking at what Dr. Lyman said about those tests, and he makes reference in question 8 of his direct testimony to the greater local linear heat generation rate of the MOX fuel requires a great coolant flow around the ballooned area to ensure core coolability. And I was looking at some of the slides from Exhibit D, and I want to do the same thing when we get to Dr. Lyman, but I wanted to, while you're here, get some of your thoughts on some of these things so I can have a clear understanding.

Maybe somewhat repetitive if you've already testified to that, you can point me where - my understanding of your testimony was that around the ballooned area, I thought you had said that there would be actually more turbulence so that it would add to greater cooling. And in looking at the slides, I noticed some diagrams of fuel assemblies, which I didn't know if these were just - let's see, in Exhibit 28, page 29 - that's BREDL exhibit. Exhibit 28 is trial Exhibit D, which is also official Exhibit 28.

MR. HARVEY: Is that page 29 on my presentation?

CHAIRMAN YOUNG: Yes. It's near the end of it. The page numbers are down on the bottom right.

I wanted to make sure I had an understanding of what the fuel assemblies look like compared to this, because again just as a lay person, I'm looking at this and there don't appear to be any spaces between the tubes. And I wanted to get your point of view on this whole coolant flow around ballooned area, because you seem to be saying two different things, and I wanted to make sure we were on the same -- using the same model or the correct model.

MR. NESBIT: We're saying something different from what they're saying.

CHAIRMAN YOUNG: Right.

MR. NESBIT: Okay. We're internally consistent. Okay. That's what I thought. I'll probably let Mr. Dunn address this. I would just point out that the figures from the BREDL exhibit are what I would call a cartoon, you know, it's not data. It may be based on -- I don't know what it's based on. And once again, we don't have the IRSN people here to testify to what this is all about, but it is our position that --

CHAIRMAN YOUNG: I got the impression that your testimony was assuming enough space so that if there was ballooning, there would still be space around it, and the turbulence would be caused by the narrowing. And that the increased flow would add to the cooling is what I understood you to be saying.

MR. NESBIT: That's all correct. What we're saying is that the - and I'll let Bert address this in a little more detail - but fundamentally, we have looked at the potential impact on the peak cladding temperature calculation, and we've shown that the narrowing that will occur is not enough to adversely impact the cooling of the cladding such that we would exceed the limits in the short term. And we've also looked at the long term, and we address that in our rebuttal testimony. I notice they corrected their testimony so they took the long-term component out of there, but clearly, there is no issue associated with long-term cooling.

Bert or Bob, you guys want to chime in on the issue of the blocking of the flow channel?

CHAIRMAN YOUNG: Let me add another factor to add to your -- is there anything -- well, have you addressed the issue of the extent of ballooning that Dr. Lyman has raised, such that you still think that there would be that space there? And was that based on modeling or data?

MR. NESBIT: Yes, we modeled the extent of the reduction in the flow area of the channel, and it was 51 or 52 percent, or something like that, which is well within the amount that's needed for adequate cooling.

CHAIRMAN YOUNG: Do you know, in fact, whether the fuel assemblies that IRSN slides are based on are different than your planned fuel assemblies or not?

MR. DUNN: I don't know that at this time. I'd have to go back and - if the test report is here - review it to give a definitive explanation.

CHAIRMAN YOUNG: Okay.

MR. DUNN: They're probably, but these are not going to be M-5s, for example. These will be 4, so there's going to be some differences in the fuel assembly, and some differences in how they're packed in there.

CHAIRMAN YOUNG: Okay. Well, I was trying to get an understanding of the difference, the stark difference between you on that particular point.

MR. DUNN: I'd like to add a little bit on the question on the blockage and the lack of flow area, et cetera, in this particular diagram that IRSN has presented. The demo experimental evidence, and I'm integrating PHEBUS test and some other tests, those are just different test theories, is that blockage -- I'm sorry, the occurrence of swelling and rupture in fuel pins within a fuel bundle induces added cooling mechanisms. Turbulence is one effect that we have there. We also have droplet mechanical interaction with the blockage, which changes the liquid that's flowing within the steam. You have steam that's containing liquid droplets in it, rising up rather violently in the core during reflux. Those droplets are here, structures that are in there way, grids they will always hit. I think the mechanical straps that hold the fuel assemblies together, and rupture they will run into, as well. When they do that, those droplets shatter and become smaller. Smaller droplets have a higher surface area per pound and they vaporize more quickly, so they cool the steam that's providing most of the coolant for the cladding.

In tests, we see that blockages up to about 40 or 45 percent contribute a little bit to cooling. Then we start to see a fairly substantial increase in cooling. They told me I have to get this on the verbal record, so I'm sorry I'm using my hands there, but we start to see a substantial increase in cooling. And then as you get up to higher blockages, in fact, the restriction of the flow starts to become important again, and at somewhere around 90 percent we actually see about the same type of cooling that we see in an unruptured or unswelled condition.

CHAIRMAN YOUNG: Does it ever go higher at any point?

MR. DUNN: Yes, in-between maybe 35 to 90 percent blockage.

MR. NESBIT: Let's clarify what you mean by higher?

CHAIRMAN YOUNG: Yes, that was not well put. Does it ever reach a level that the cooling is less effective?

MR. DUNN: Well, we don't think it's really possible to get much more than 90 percent blockage, in fairly postulated conditions, but if one were to block at 100 percent over an extended axial length, yes, I think it would be worse.

CHAIRMAN YOUNG: Let's see.

MR. NESBIT: I did get back and confirm the number from our paragraph 56 in the direct testimony. Our model, again, was a conservative model ballooning that we would expect to over-estimate the amount of ballooning and flow blockage. For the case that resulted in the peak cladding temperature, we had a 52 percent blockage of the flow channel.

CHAIRMAN YOUNG: Okay. Let's see. This

is related to the coolable core geometry issue. Correct or not correct? You're nodding.

MR. NESBIT: I'm sorry, yes. We -- probably that's more of a question that you should put to BREDL, but our understanding is that -- well, there's two effects there. There's the effect during the actual LOCA transient that we're calculating temperatures and properly accounting for those effects. And then there's the issue of the long term, but I think we shared in our rebuttal testimony there really isn't an issue there with respect to long-term cooling.

CHAIRMAN YOUNG: The sentence that you used about the PHEBUS testing in paragraph 164, starting with the sentence, "PHEBUS tests are complex", going to the end of that paragraph, I guess from time to time in your testimony you make statements that are different from your statements about particular conservatisms and particular benefits that might happen with MOX fuel. And you talk about, for example, I think one place you say that they don't -- no LOCAs that are significant at a large break, and they're very unlikely. And then here you talk about the PHEBUS testing, complex and requiring preparation, and the time it would take, and the resolution of the chicken versus egg paradox.

I want to make sure that -- I want to get some sense of where these types of statements, and this one in particular, fit with regard to adding together all the factors that would mitigate, that would suggest that there are not the problems with MOX fuel of the sort that Dr. Lyman suggests. Are these type of statements - is this a necessary ingredient to your overall conclusion that you don't think it's necessary to go further with any tests, or they're of a different nature than the ones about conservatisms, and benefits and so forth.

MR. NESBIT: These statements have nothing to do with the need or the lack of need for the added -- well, let me rephrase it another way. These statements have nothing to do with the adequacy of the analyses that we have done, submitted, and had reviewed and approved by the NRC that justify going forward with the lead assembly program, nothing whatsoever.

I would add that if we had identified the need to do some additional testing prior to putting the lead assemblies in there, we would do that testing first, irrespective of the fact that it might take time and money to do it, because from our perspective, there is no higher priority than nuclear safety. And we're the ones that are there on the front line there. They're our plants.

With that being said, the purpose for adding this into the testimony at all was simply to point out that the proposal that BREDL was advancing, which might on the surface look attractive or practical possibly, really isn't a practical alternative. It's certainly not necessary as the rest of our testimony demonstrates, but it's also not just a simple matter of let's do a few tests before we load some lead test assemblies. While we're stopping the program to wait for this, surplus Plutonium is not being disposed of, and it will take a long time.

CHAIRMAN YOUNG: Just one last question, and your statement leads me to it, so I want to make sure I understand as much as I can about your testimony at this point. The contention about consequences in releases I think was withdrawn, but implicitly I think you both sort of get into that on some level. And I can't find the place where Dr. Lyman made a statement about the consequences of failure of the lead test program, the significance of that.

I just wanted to understand, if I could, from you what your -- to the extent that you address it, what your implicit understanding is should something unforeseen happen with the lead test assemblies, and what -- do you understand what I'm trying to understand?

MR. NESBIT: Yes. In fact, I found the exact place that you're talking about. I believe it is the Rebuttal 7 on page 4 of BREDL's rebuttal, where they make a statement that "BREDL also notes that a loss of coolant action at Catawba when MOX LPAs are present in the core would likely put an end to the MOX program in the United States."

CHAIRMAN YOUNG: Which page are you on again?

MR. NESBIT: I'm on page 4 of the BREDL rebuttal.

CHAIRMAN YOUNG: Okay. I was looking at the wrong thing. Okay.

MR. NESBIT: And they conclude by saying, "Thus, it would prudent to ensure that the likelihood of such an occurrence is low." Well, we were somewhat perplexed by the statement. From our perspective, whether or not you perform the kind of testing that BREDL has proposed, will have nothing whatsoever to do with the likelihood of whether there's a LOCA at

Catawba or not. There's no connection whatsoever. The tests, if they were performed, would simply provide information about some of the thermal hydraulic and mechanical phenomena that might occur if such a LOCA were to occur, but it doesn't have anything to do with the likelihood of the event.

CHAIRMAN YOUNG: Well, if the tests were to show, for example, that there were more significant, that the heat increase would be greater, and you did have a LOCA and the peak clad temperature went above what you predicted it would, I'm trying to understand the whole picture, where --

MR. NESBIT: That's really quite a hypothetical. I mean, there's a lot -- if I can take you along there. We do the test. We find out that contrary to every reasonable engineering expectation there is some significant MOX fuel relocation effect that we haven't accounted for, we have a LOCA at Catawba, and despite the fact that all the best estimate evidence indicates that even if you do, you're never going to get a -- you won't probably reach the temperatures where you have fuel relocation in the first place.

CHAIRMAN YOUNG: Here's what I'm trying to do. When you were talking about using all worst case scenarios before, I'm adding - since this has been raised, I'm trying to get some clarification, understanding if you added that worst case scenario, is there an understanding of what would happen. You say it would not -- well, I'm just trying to get a better understanding of that.

MR. NESBIT: Yes. To state it succinctly, there's no connection with the likelihood of the occurrence. And if all these things stack together and we actually did have a LOCA at Catawba with the MOX fuel lead assemblies and the core, I think that probably the MOX program would be the least of our worries at that point in time. That would be a significant event for our plant that we would be dealing with, and not -- the consequences of that would be significant for us from an economic perspective.

MR. DUNN: At the risk of just going to numbers, if you had that type of an event it would be one of the other 193 fuel assemblies, well several of the other 193 fuel assemblies that would lead to the adverse consequence.

MR. NESBIT: That's correct.

MR. DUNN: Because a LOCA will not just happen in a given fuel assembly.

MR. NESBIT: As we talked about before, we would certainly expect that the higher peak cladding temperatures, more oxidation would occur in one of the other fuels.

MR. REPKA: Judge Young, if I may, just a little point of confusion in the questioning. I do not read Dr. Lyman's testimony in any way as going to the consequences of a LOCA in terms of public health consequences, which was the issue in Contention 2. I read it as a statement about the consequences for the MOX fuel program.

CHAIRMAN YOUNG: And it may be. I guess I just -- there seemed to be sort of implicit and underlying in a lot of this well, what would happen? And I just wanted to get a better understanding of what were some of the assumptions.

MR. REPKA: In fact, I think it begs the question in terms of what would happen in terms of an accident scenario for the beyond design basis accident which this panel obviously is not testifying to. Or two, what would happen in terms of the Catawba reactor and the use of MOX fuel in the future, which I think is a different policy question.

MR. NESBIT: And I will add that our reactor is designed to accommodate that kind of an accident without adverse public health and safety consequences. There would be significant economic consequences to our company, but again, there's no connection between the testing they propose and the probability of having that kind of an accident.

CHAIRMAN YOUNG: Well, when we get to Dr. Lyman, he can say what he meant. But while you were here, I just wanted to get your take on that, as well. Thank you. Any further --

MS. CURRAN: I have two follow-up questions.

CHAIRMAN YOUNG: Go ahead.

MS. CURRAN: I'd like to go to Item 6 which was just above paragraph 84 of your direct testimony. I believe you said that this issue was addressed in the M-5 topical report. Is that correct?

MR. DUNN: Item?

MS. CURRAN: Item 6, loss of ductility as measured by ring compression tests as a function of clad activation and surface conditions for all burn-ups.

MR. DUNN: Your question is for the criteria as measured to M-5 were addressed in the M-5 topical report. And the criteria were determined to be just -- it wasn't done specifically at that time to do ring compression tests.

MR. NESBIT: Is there a specific question about that.

MS. CURRAN: Yes. So, in fact, Item 6 was not addressed in M-5 topical report.

MR. NESBIT: Well, loss of ductility was.

MR. DUNN: The loss of ductility was addressed in M-5 topical report. The specific means of ring compression tests, that has been done since then.

MS. CURRAN: But let me just clarify that the M-5 topical report did not evaluate loss of ductility a measured by ring compression tests. Is that right?

MR. DUNN: That would be correct.

MS. CURRAN: Okay. One more question. Going to paragraph 154 of your direct testimony --

MR. DUNN: Paragraph 154?

MS. CURRAN: Yes. And this was you, Mr. Dunn. I'm just going to read what you said here. "Adding LAR and predicting for the Catawba MOX fuel results gives an estimated PCT of .70 degrees Fahrenheit and a local oxidation of 10 percent." Can you verify that if you add the change in peak clad temperature to the ruptured node that it will also satisfy the maximum clad oxidation limit?

MR. DUNN: I believe in constructing this answer, I added the differential oxidation predicted by IRSN to our base oxidation.

MR. NESBIT: Are you speaking of the local limit or --

MR. DUNN: No, that means you'd be comparing 10 percent to 17 percent, and it would match.

DR. LYMAN: No. The question isn't adding the differential peak clad oxidation from the IRSN calculation, which is not appropriate to add directly. If you used the peak clad temperature that you calculate, and if your basis for estimating the peak clad oxidation, you your loss of content and your need to limit on peak clad --

MR. DUNN: I'm sorry, Dr. Lyman. I don't think I understood what you just asked.

DR. LYMAN: If you took as you said the pessimistic IRSN increase in peak clad temperature of 313 degrees Fahrenheit, you add it to the first rupture peak clad temperature of 1750 degrees Fahrenheit, which would actually give a 2100, around 2070 degrees Fahrenheit, if on that basis you calculated using your code, the peak clad oxidation at the first rupture location - what would the increase in local oxidation be? In other words, if you didn't use the IRSN change in peak clad oxidation, but you based your calculation on an increase in peak clad temperature.

MR. DUNN: I'm going to say I think it would be about that 10 or 11 percent that -- or 9 percent or something that's in there. I think the IRSN is using the Baker-Just correlation in doing this. That's the same correlation I would use. The temperature adjustment range I believe is reasonably close. There could be a little variation in the particular temperatures creating 1 or 2 percent changes in the oxide thickness, but I'll answer if I were to recalculate this with my code, it'll be around 10 percent, perhaps a little lower. The IRSN oxidation predictions do seem high to me. They seem to be too high.

MR. NESBIT: Well, they start out at 100 microns.

MS. CURRAN: That's all the questions we have.

CHAIRMAN YOUNG: Mr. Repka.

MR. REPKA: I have one very quick clarifying question I believe for Mr. Nesbit. In response to a question that Judge Elleman asked, you were discussing the history, the performance history of MOX fuel rods in Europe, and you were talking about the variety of fuel failures that had been experienced, and what was their nature. Judge Elleman asked you whether there was any indication from the database as to the frequency of those failures and I believe you answered the question something along the lines of it's a very large database, and you gave some numbers. But I don't know that you ever answered directly his question regarding the frequency.

MR. NESBIT: My recollection, and I don't have the source document in front of me, is that we're talking about failures on the order of one in 50,000 or one in 100,000 rods, which is commensurate with what you would expect.

MR. REPKA: Thank you. That's all I have.

MS. UTTAL: Staff has nothing.

CHAIRMAN YOUNG: Thank you. Anything further from either of you? Would it be an appropriate time to take an early lunch break, or would you like to get started with Dr. Lyman?

MS. CURRAN: I would like to request that we take the lunch break now.

MR. REPKA: I have no objection.

CHAIRMAN YOUNG: Okay. Then be back at 12:30.

(Whereupon, the proceedings in the above-entitled matter went off the record at 11:31 a.m. and went back on the record at 12:41 p.m.)

CHAIRMAN YOUNG: Let's go on the record.

WHEREUPON,

DR. EDWIN LYMAN

was recalled for examination by Counsel for Licensee, having been previously duly sworn, was reexamined and retestified as follow:

CHAIRMAN YOUNG: Did you have any preliminary questions before we go to cross examination?

MS. CURRAN: No, I'll save my questions until afterwards.

CHAIRMAN YOUNG: All right. Then let's start with you, Mr. Repka.

MR. REPKA: Okay. Thank you.

CROSS EXAMINATION

BY MR. REPKA:

Q Dr. Lyman, have you ever developed an Appendix K LOCA evaluation model?

A No.

Q Have you ever worked with a LOCA model computer code?

A No.

Q Have you personally performed any LOCA evaluations?

A Could you be more specific?

Q Have you personally been involved in work on a LOCA design-basis accident analysis to meet Appendix K requirements?

A In other words, have I actually used a computer code to conduct an Appendix K analysis?

Q Yes.

A No.

Q Have you ever modeled fuel pellet behavior?

A In what sense?

Q Performed calculations or developed models related to the performance of fuel pellets?

A I performed calculations. I haven't used computer codes to simulate fuel pellet performance or evolution.

Q Have you conducted any experiments on fuel pellets or fuel rods?

A No.

Q Do you have any degrees in materials science or materials engineering?

A No.

Q Is it fair to say that your testimony in this case is based upon your reviews of the literature related to LOCA analysis?

A Yes, I would say that's fair.

Q You've obviously reviewed the Duke Energy testimony in the case and there are some discussions in that testimony related to the LEU evaluation model that was adapted for the MOX fuel LOCA analysis. Am I correct that you've reviewed that testimony?

A Yes.

Q And in your testimony, you have not challenged any aspect of that evaluation model other than the lack of a specific model related to fuel relocation. Is that correct?

A Yes, the focus of our Contention 1 does center on the absence of fuel relocation effects in Duke's calculations.

CHAIRMAN YOUNG: Can I ask you to speak up just a little bit?

THE WITNESS: I'm sorry.

BY MR. REPKA:

Q Duke has stated in its testimony that certain differences between MOX fuel and LEU fuel related to neutron power characteristics were modeled using LEU characteristics which are conservative for MOX. You don't dispute that, do you?

A No, I don't.

Q And similarly, Duke has stated that it's used LEU decay heat characteristics and that this is conservative for MOX fuel in a LOCA analysis. You don't dispute that, do you?

A Yes -- No, I don't dispute that.

Q Duke has estimated that decay heat conservatisms alone can be estimated at up to 75 degree Fahrenheit. Is that correct?

A That's the number contained in Duke's testimony.

Q And you haven't offered any challenge to that.

A No.

Q Your focus again is entirely upon the non-conservatism related to fuel relocation and I believe you've offered a number of 313 degrees Fahrenheit. Is that correct?

A The number 313 degrees Fahrenheit was the nominal figure that was extracted from the IRSN study which is attached to our testimony.

Q That would be your Exhibit E or Exhibit 29.

A That's correct.

Q Okay. And that number was based upon an IRSN calculation.

A That's correct.

Q That was not based upon any specific IRSN experiment.

A That was drawn from an IRSN study which was based on simulations.

Q It's a calculation.

A Yes.

Q Do you know whether that calculation included the effects of cooling benefits related to clad rupture and fuel relocation?

A Well, as I was pointed out on Duke's rebuttal testimony, there's a transcript of the dialogue following the presentation of that paper where IRSN said that those effects were not considered.

CHAIRMAN YOUNG: I'm sorry. I missed that last part. IRSN said what?

THE WITNESS: I'm sorry. Those cooling effects were not considered in that calculation.

CHAIRMAN YOUNG: Were not?

THE WITNESS: Were not considered.

CHAIRMAN YOUNG: Were not considered.

BY MR. REPKA:

Q By that, you mean the enhanced cooling effects related to rupture and relocation?

A That's correct. I think Duke's testimony has that page as one of their attachments.

Q And you don't dispute that those enhanced cooling effects exist, do you?

A I don't dispute that they exist, however, it's not clear that the balance between the mitigation associated with those cooling effects and any reduced cooling effects due to flow blockage have been thoroughly accounted for.

Q Right. And you've made no attempt to do that balance, that assessment. Correct?

A That's correct.

Q Now that IRSN calculation related to LEU fuel. Is that correct?

A That's correct.

Q So this fuel relocation issue is an LEU fuel issue, isn't it?

A Well, the fuel relocation issue is an issue associated with fuels that experience moderately high burn-ups. It's been observed in LEU fuel because LEU fuel is the only fuel where such tests have been performed and would be likely to observe it. It is also likely to be an effect for MOX fuel.

Q So you haven't attempted to quantify the difference between the effect for LEU fuel and MOX fuel, have you?

A I haven't quantified the effect, but I've pointed out some qualitative features that could have an impact on that difference.

Q Okay. Let me come back to that. But you've answered my prior question and you said that it's an LEU -- I asked whether it's an LEU issue and you said it's an issue for higher burn-ups, correct, fuels with higher burn-ups?

A Yes. Well, it's been observed, as I pointed out in my testimony, in LEU fuels with rod average burn-ups of about 48,000 MWd/ton.

Q Do you have a copy of your testimony in front of you, your July 1st testimony?

A Yes.

Q Could you turn your attention to page five, the answer 11?

A Yes.

Q In that answer, I believe, you refer there to the fact that -- and I'll just read it. "Fuel relocation phenomenon has been observed in LEU fuel for rod burn-ups exceeding around 48 GWd/metric ton."

A Yes.

Q And you cite the Grandjean/Hach-Wunderle study from 2001 which is, I believe, Exhibit 29. Is that correct?

A Yes.

Q Do you have a copy of that exhibit in front of you?

A Sorry. Give me one second.

MS. CURRAN: Exhibit E.

BY MR. REPKA:

Q Well, actually. I'm sorry. Let me correct that. Let me go to your Exhibit G which is Exhibit 31 overall.

A Yes.

CHAIRMAN YOUNG: It will the one that I marked as 31.

BY MR. REPKA:

Q Exhibit G is --

A I think I have it.

Q -- the proceedings of the Nuclear Safety Research Conference.

A Yes.

Q And that involves the paper by Mailliat and Schwartz.

A Mailliat and Schwartz, yes.

Q And that's an IPSN study. Correct? Presentation?

A Yes.

Q Could I refer you to page 432 of that paper?

A Yes.

Q There's a discussion of fuel relocation on that page and if you go down to the bottom of that discussion which appears just above "An Increased Cladding Deformation," that subheading. There's a sentence that reads -- Actually, could you just read that sentence for me?

A I'm sorry. Could you direct me to it?

Q It begins, "Finally, fuel relocation process..."

A "Finally, fuel relocation process is not specific of high burn-up fuel. It was also observed for fuel rod, fuel rod having a burn-up as low as 48 MWd/ton."

Q So it's 48 MWd/ton whereas your testimony says "49 GWd/ton." Can you explain the difference between those two numbers?

A Yes, that's a mistake in my testimony.

Q Okay, but the significance of 48 MWd/ton is what? Isn't it true that that's a fuel with relatively little burn-up?

A That's correct.

Q That's practically fresh fuel.

A Yes, but without knowing the circumstances surrounding the radiation preparation, the microstructure of that fuel, I wouldn't be able to comment on the explanation for why that was seen.

Q But the net effect of that is that relocation is an issue that applies to LEU fuel, is it not?

A In general, yes. I don't know. That particular result may be an outlier again. I can't comment without knowing more about the circumstances of how that test was conducted.

Q But I certainly can conclude that that data does not support a theory that the relocation issue is anyway unique to high burn-up fuels or MOX fuels. Correct?

A Not unique, but I think the point to the extent that -- Well, let's put it this way. The fuel cracking that occurs during normal irradiation, that initial cracking occurs relatively soon after the rod reaches full power. That will cause some cracking and therefore if there was a LOCA type event leading to relocation, you do have some particle formation.

The hypothesis that we are pursuing is that in high burn-up fuel that the appearance of the rim generates a different type of fuel microstructure that could contribute an additional source of pellet and fragment formation for the relocation phenomena. So it's qualitatively and possibly quantitatively different for high burn-ups than it would be at low burn-up. So I'd say that the fuel relocation effect we're concerned about primarily is not what's described in that test.

Q Well, your testimony on page 5(a)(11) says that the IPSN and IRSN supports the idea that relocation is something that's associated with the development of the high burn-up rim region. And if in fact, this data shows that relocation occurs at much, much lower burn-ups, how can you equate that with the development of a high burn-up rim region?

A Well, what I said is that the character of the relocation is different. The low burn-up would be relatively large fragments. The development of fragments at higher burn-ups could generate smaller fragments. In fact, it's the fine fragments that are of the greatest concern because they can lead to higher filling ratios. So I would maintain that those are actually different qualitatively and possibly quantitatively different regimes. For considering fuel relocation, I couldn't conclude without further data how those two regimes would compare with respect to their impact on fuel relocation.

Q But do you have any data or report that supports the idea that connects the high burn-up to the development of the rim region to the fuel fragmentation?

A Yes. I would say that there's considerable data indicating that the high burn-up structure that occurs in the rim is associated with the generation of fine fragments during the rod pellet heat-up that would occur during LOCA.

Q But not the reference that you submitted with your own testimony.

A Well, I would --

Q As you just said, it relates to low burn-up.

A That particular reference, yes, but I wouldn't say that. I would say, "In the references that I've included, there is certainly an indication that there's concern about the impact of relocation of high burn-ups effects in relation to relocation." In fact, the IRSN presentation which was given to the staff which is our Exhibit D, clearly, if you look at the title --

CHAIRMAN YOUNG: That's 28.

MS. CURRAN: Exhibit D is 28. Yes.

THE WITNESS: For instance, page 22 of Exhibit D, if you look at the third checked item that says, "Filling.." These are pending issues associated with fuel relocation. The third item is "Filling ratio of clad balloon at high burn-up with fragmentation of U-02 rim or MOX agglomerates."

BY MR. REPKA:

Q Question mark.

A It's question mark. These are pending issues. These are the types of uncertainties which we've pointed out to support our call for additional LOCA testing on irradiated MOX fuel.

Q You don't have any specific data, do you though, that supports this idea of the fragmentation related to the rim region? You have a question on one slide from IRSN.

A Well, since this point was challenged and I was aware that there was considerable literature about this phenomenon, I brought in another document which I'd like to read an excerpt from that's the CABRI test data. There has been accident testing with regard to reactivity insertion accidents on both high burn-up LEU and high burn-up MOX fuel. And it's through that test program that there's been more insight about the impact of the rim region both in high burn-up LEU fuel and the MOX agglomerate high burn-up structure and its impact on the fuel fragmentation. Now the caveat, the CABRI tests, of course, is reactivity insertion accidents which are associated with much more rapid rising in temperature than loss of coolant accident, but the information that's come out from those tests about high burn-up fuel structure, I think, is relevant to our understanding of what occurs on the LOCA.

CHAIRMAN YOUNG: Where were you reading? Were you reading from a certain part of this?

THE WITNESS: No, I haven't started reading it.

CHAIRMAN YOUNG: Okay.

MS. CURRAN: Could we identify the document and mark it as an exhibit for identification purposes at this point?

CHAIRMAN YOUNG: You could give it your number and then once we make a ruling on its admission then we'll give it an official number.

MS. CURRAN: Okay. Did you read the title yet, Dr. Lyman?

THE WITNESS: No, the title of this document is "Synthesis of CABRI-RIA Tests Interpretation" and the primary author is J. Papin, P-A-P-I-N.

CHAIRMAN YOUNG: That's sufficient for identification I think.

MS. CURRAN: I'm not sure what number to give it or where we left off. I guess it was the --

CHAIRMAN YOUNG: When we get to it, it will be 51 assuming it's introduced. Is there any objection to this?

MR. REPKA: No.

CHAIRMAN YOUNG: Then consider it admitted and we'll mark it as 51.

MS. UTTAL: Your Honor, I don't know where it came from or whether it was provided in discovery. Was it provided in discovery? How did we get this?

MS. CURRAN: Dr. Lyman, how did you find this document?

THE WITNESS: Oh, I found this on the Internet in response to Duke's rebuttal and also the staff's rebuttal in which they asserted a lack of any experimental information that would tie fragmentation to the high burn-up rim structure and I believe that to contribute to the record here that it was necessary to document that. This is information that the staff and Framatome certainly are well aware of. So if they weren't willing to offer it, we will.

CHAIRMAN YOUNG: Were you objecting?

MS. UTTAL: Well, Your Honor, if they just got it today, that's something else. But if they had it yesterday, then I don't know why they waited this long to give it to us.

CHAIRMAN YOUNG: When did you find it?

THE WITNESS: It was probably the day before yesterday. I don't recall exactly.

MS. UTTAL: I'm not objecting, Your Honor.

CHAIRMAN YOUNG: Okay. Then it will be entered as Exhibit 51.

(The document referred to was marked for identification as Exhibit No. 51, and received in evidence.)

THE WITNESS: But the relevance of this document is I like its description of the rim structure and this is section 3.3. Unfortunately, the document doesn't have page numbers in my version, but this actually page --

CHAIRMAN YOUNG: The back of the third page.

THE WITNESS: Yes. I would just like to read this into the record because I think it's a good description. It's entitled "Fuel and Microstructure Evolution and Fission Gas Behavior." The second sentence there starts with "Furthermore, there is a strong increase of the fraction of the fission gases stored at grain boundaries mainly due to the formation of the typical high burn-up structure, HBS, in the rim zone and in the MOX agglomerates. This leads to a high gas content in over-pressurized micrometer intergranular pores (for UO_2 fuel at 60 to 65 GWd/ton. Poor gas content in the rim zone amounts to eight to ten percent of the pellet total gas retention) which may contribute to transient fuel swelling and gas release under fast transients.

In most of the CABRI tests (and also in the NSRR tests) fuel fragmentation with grain boundary separation and structure changes are evidenced in the external zones of UO_2 fuel or in the matrix of MOX fuel as shown in figure 5. This effect is understood as a result of the high over-pressure that is developed in the small intergranular bubbles under fast heating rates and which induces high stress fields between the grains.

Depending on gas pressure and fuel

constraint, grain boundary cracking and grain boundary separation may occur and lead to fission gases availability (intergranular bubbles and large pores) for internal solid fuel pressurization and swelling." Then it goes on to describe issues that are more specific to the reactivity insertion accidents.

I raise this because it's a good description of the rim structure which this paper equates the rim structure and high burn-up LEU fuel to the high burn-up structure and the MOX agglomerates. It provides a mechanism for fragmentation of fine particles in the event of rapid temperature increases.

Now again, I'm not saying that's the type of fragmentation that we see in the CABRI tests. I'd like to point out that in a CABRI test on a high burn-up MOX rod that there was a failure and there was significant fuel dispersal into the coolant of fine particles.

I'm not saying that is necessarily an indication of what might happen in loss of coolant accident, but this does demonstrate that the high burn-up rim region in LEU fuel and the high burn-up structure in the plutonium agglomerates in MOX fuel does undergo a micro-cracking as a result of the fission gas accumulation and in the event of a rapid gas expansion can lead to fragmentation. So that's the connection that I attribute the IRSN statement with the question mark in our Exhibit 4 to. It's the French understanding of this phenomenon.

BY MR. REPKA:

Q And to make sure I have this correct, you agree that this was a reactivity insertion test, was it not?

A I do.

Q And that's different from a LOCA test.

A That is different. I said that.

Q And it's different because it involves increasing the power which increases the heat which increases the thermal stresses, does it not?

A That's correct, but this is also a description of the rim structure and presents a mechanism for fragmentation. I don't know at the lower limit on the rate of temperature increase is below which you would not experience this fragmentation. That's something which I think needs to be studied. However, we do have the staff -- Actually, we have another document if you'd like to see some indication of the impact of fission gas expansion during loss of coolant accident on cracking. That would be the first Yan, Burtseva, Billone paper.

CHAIRMAN YOUNG: While we're on this, could you just help me out here? Is there a reference to specific temperatures here?

THE WITNESS: In the CABRI document?

CHAIRMAN YOUNG: Yes.

THE WITNESS: Well, I'm sure there is. Certainly in other documentation of these types of tests, that can be found. In a reactivity insertion accident, the temperature ramp, you actually can see that on this. It's Figure 8.

Well, no. That's actually the sodium temperature. I don't know if it's in this document, but certainly other documentation would clarify what the particular insult to the fuel is during a reactivity insertion. This first one.

As another illustration of the potential impact of this microstructure on fragmentation in a loss of coolant accident, in other words, a temperature ramp is more indicative of what we're talking about here, I would point to the results of experiments at Argonne National Laboratory which were documented in a paper provided to us by the staff in discovery.

This paper, some photographs of which were included in one of the staff exhibits, but not the particular photograph I'm interested in. It's entitled "LOCA Results for Advanced Alloy in High Burn-up Zircaloy Cladding." The authors are Yan, Burtseva and Billone and it dates from 2003, I believe, although the copy given to us from the staff is not dated.

CHAIRMAN YOUNG: Any objection to this document?

MS. UTTAL: No.

MR. REPKA: No.

CHAIRMAN YOUNG: Let's just go ahead and enter it as 52.

(The document referred to was marked for identification as Exhibit No. 52, and received in evidence.)

THE WITNESS: I'd like to bring your attention to Figure 4. There's a caption to Figure 4 and there's also some description on the opposing page. Again there are no numbers. I'd like to read -- Well, first of all, if you look at Figure 4, you see there's an image of the Limerick boiling water reactor fuel that was tested under LOCA conditions at Argonne before and after a test, test number ICL No. 2. These are actually different segments which makes things a little confusing, but the point that's trying to be made here is that there's a region which is possibly a region of enhanced fission gas storage that leads to circumferential tearing during the LOCA.

I'll just read the paragraph beginning "Figure 4:." This is opposite to Figure 4. It is at the top of the page. It says, "Figure 4 shows the fuel structure approximately 130 millimeters below the burst center (45 millimeters above the bottom end cap) as compared to the fuel structure of the as-received Limerick fuel. The structures are similar except that the post-LOCA fuel shows a ring of circumferential tearing around mid-radius. This tearing may have occurred as the cladding tried to move a small distance less than 0.1 millimeters away from the fuel and/or because of the effects of fission product gases on the fuel (see dark ring near mid radius for the pre-LOCA fuel in Figure 4a)."

"Figure 5 shows a high magnification of the small area of the mid-radius of the Limerick pre-LOCA fuel with a high concentration of fission gas within the grains as well as some fission gas bubbles on grain boundaries. Figure 5b shows an enlargement of the region of post-LOCA fuel with the circumferential tearing."

Now they are offering two different explanations for the circumferential tearing. One is the effect of fission product gases on the fuel. So I'm just offering this as a potential indication that during under LOCA conditions, a region with high concentration of fission gas within the grains can lead to actual fuel tearing during the LOCA. Whether that leads to significant fragmentation is not 100 percent clear, although you can see from Figure 6 of this document the extent of fragmentation that did occur during that test including the generation of considerable numbers of small particles.

Those are the two pieces of information that I was able to obtain rapidly to support the connection between a high burn-up rim region and the potential for some fuel damage, tearing or cracking due to the expansion of the gas in the high burn-up region under LOCA conditions. I don't think this data constitutes sufficient body of data to reach a definitive conclusion. Again, this is one of the reasons why we believe more testing on MOX fuels is necessary.

CHAIRMAN YOUNG: Could I just clarify one thing? In Figure 4 and Figure 6, the white area between the outer line and the cross section of the rod or pellet, is that space or is that --

THE WITNESS: No, I believe --

CHAIRMAN YOUNG: I'm trying to figure out where the cladding fits in. Is that a part of that or not?

THE WITNESS: No, I don't think that's -- Well, I would assume that's the cladding, but I'm not positive without -- These are areas that are away from the burst region so the burst cladding wouldn't be apparent. But I believe that is the cladding, yes.

CHAIRMAN YOUNG: I just want to make sure I knew what I was looking at here. Thanks.

BY MR. REPKA:

Q Are you done?

A Yes.

Q I want to go back to the CABRI test which I think you've already said was a reactivity insertion test which involves an increase in power within the rod. In contrast, wouldn't you agree that a LOCA test would have actually involved a decrease in power in the short term?

A A decrease in power, but as we know, an increase in temperature. So what is a controlling parameter, I don't know.

Q Let me ask the question. Wouldn't it

involve a decrease in temperature of the pellet from the beginning of the LOCA?

A Initially, but later on, there's an increase as we know.

Q In fact, that's modeled in Figure 1 of your testimony.

A Right.

Q You can see in the short term there's a decrease in pellet temperature which is very different from a reactivity insertion test, is it not?

A No, I agree, but we're looking at the entire course of the event. So again, there's scarce data here. The few data that I was able to find about the potential impact on the high burn-up fuel during LOCA results looks like there is some tearing that's due to expansion. I would welcome further interpretation of those results, but these are fairly recent results and they're suggestive.

Q But can you equate the two tests, a LOCA test and a reactivity insertion test, in terms of, I believe you termed it, the insult to the fuel pellet? Do you think that they are equivalent?

A No, they are not equivalent. They have different characteristics, but they don't have to be equivalent. I don't think --

Q But the factors are substantially different, aren't they?

A Yes, they're different. I'm not attributing the consequences, and I think I've said it several times now, of the outcome of the CABRI tests. I'm not saying that the same outcome that you would have during a LOCA test. I'm simply providing some substantiation of the connection between microstructural changes in the fuel at high burn-up due to the accumulation of fission gases and that do provide the potential for grain boundary expansion under certain stressful conditions. Whether the LOCA challenges the pellet in a way that will cause that substantial microcracking, again I said I don't think it's clear, but the results from Argonne has suggested.

Q Tell me if you know. What is the time span in a reactivity insertion test, the overall, beginning to end?

A Before -- I mean it's on the order of a few seconds.

Q A few seconds. If I said it was 0.3 seconds, would you be surprised?

A No, I think that's still in the order of a few seconds.

Q And compare that in a LOCA test, what's the overall timeframe of the LOCA test?

A LOCA test is on the order of several hundred.

Q Thanks. Turning back to some of the other calculations -- Well, actually before I do that, let me just ask you. You've obviously reviewed the testimony again of Duke and must be familiar with the testimony of Dr. McCoy related to the rim region and the materials involved. Correct?

A Yes.

Q Based upon what you're saying today, do you disagree with his conclusion that the rim region is tougher, I believe, are the words he used?

A No, I don't disagree with the conclusion that it's tougher, but that isn't the only relevant parameter. I don't think he addressed the -- It's not just the change in the mechanical properties of the rim region that's relevant, but it's also this driving force which as we've seen can be supplied by the expansion of the trapped fission gas bubbles. So the fact that mechanically, it's a tougher material does not necessarily mean that it will not experience microcracking under certain conditions. I think the CABRI test, that's demonstrated that.

Q It has demonstrated that or it has not demonstrated that?

A It has demonstrated that since you do have at high burn-up the dispersion of fine fuel particles that didn't occur at lower burn-ups.

Q Under that conditions of that test.

A Right.

Q In your opinion.

A Yes.

Q Let me return to some of the other exhibits you've included with your testimony. Exhibit 29 was the March 2001 paper. That's the IRSN calculation. I think we already talked about that. That calculates at least the potential for a 313 degree F delta PCT related to fuel relocation. Correct?

A Yes.

Q And that began with a calculation, not a test.

A A calculation, yes.

Q Now did that paper include any delta PCT related to MOX fuel?

A No.

Q You've also referenced Exhibit 27 which was an NRC memorandum from Ashok Thadani.

A Yes.
Q Do you recall that exhibit?
A That's research information letter 202.
Q Now that paper talks about some potential impacts on PCT related to fuel relocation for LEU fuel. Correct?
A Yes.
Q Does it include any estimate with respect to delta PCT for MOX fuel?
A No, it doesn't.
Q You've included Exhibit 30 which is a May 2004 presentation from Argonne. Do you remember that exhibit?
A Yes.
Q And that included some calculations from IRSN related to potential impacts on PCT due to fuel relocation. Correct?
A Yes.
Q Do you remember what the delta PCTs they were attributing to for LEU fuel were?
A Yes, they gave a figure of 100 to 150 degrees Celsius. I wasn't present at that presentation so I don't know. It wasn't clear to me if that was a result of new analysis or simply a restatement of older findings.

Q Right, but you read the paper.
A I saw the overheads. That's all I got.
Q Are you familiar with that that paper did make an estimate of a delta PCT related to MOX fuel as well?
A Yes.
Q And what was that?
A I think that showed as much as an additional 10 degrees Celsius.
Q Which would be about 18 degrees F?
A Yes.
Q And do you consider that to be a significant change in a LOCA analysis?
A I do, but that -- I don't know what the basis for their calculation is. As it was pointed out this morning, that difference is attributed to the higher initial energy which I think we all agree is not a factor for the particular positions of the MOX LTAs that Duke is proposing.
The concern that we have is the potential for a more severe MOX effect due to the greater generation of fine fragments and higher filling ratios. I don't know if that consideration is reflected in this calculation or not.
Q But you do agree that the calculation that

was provided was only 18 degrees F, number one. Correct?

A Yes.

Q And you do agree, I just heard you say, that that was attributed to the higher initial stored energy which does not apply in the case of the Catawba Leap assemblies. Correct?

A That's right.

Q Duke provided in Exhibit No. 3 which is an EDF presentation from Aix-en-Provence in 2001.

A Yes.

Q Have you reviewed that presentation?

A Yes.

Q And I believe that there's a delta PCT calculated in that paper as well related to fuel relocation. Are you familiar with that?

A Yes, I don't remember the number, but it's smaller than IRSN's.

Q Right, it's smaller. It was something on the order of 54 degrees F.

A Okay.

Q You don't disagree.

A I don't disagree that's what that paper said.

Q Right, but that's for LEU fuel. Correct?

A I'm sorry.

Q That's for LEU fuel.

A Yes.

Q And did it present anything related to MOX fuel?

A No. Not to my recollection, but I'd like to have it before me. I actually don't have Duke's exhibits. Thank you.

Q Okay. I want to turn your attention to Duke's Exhibits 10 and 11.

A Wait. I'm sorry. We weren't done with that one.

Q Yeah, we are actually.

A No, I wasn't done. I'd like to look at it before I conclude my statement. Thank you. Yes, so the EDF, you asked me if it involved MOX and I don't see anything about MOX here. I would like to point out --

Q I said LEU fuel.

A Okay.

Q And there was a calculation, correct, not a test?

A Yes. And I would like to point out that IRSN was asked about the difference between the EDF results and their results and this is actually at the end of Duke's Exhibit 4, second page from the end where IRSN said that --

CHAIRMAN YOUNG: It says "Summary" on it, the page we're looking at.

THE WITNESS: No, unfortunately the page numbers are -- It's the second to last page of Duke's Exhibit 4. It says, "Discussion" at the top.

CHAIRMAN YOUNG: The second to the last page of Exhibit 4 that I have says, "High Burn-up Fuel LOCA Calculations to Evaluate the Possible..." That's the second to the last one. It says "Summary."

THE WITNESS: Do you see those -- I'm sorry. I don't know what the problem is. Duke's Exhibit 4 is a paper and a set of overhead slides and after the last overhead slide, there's something that says, "Discussion." It says on the upper right-hand, "NEA/CSNI/ARC."

CHAIRMAN YOUNG: Do you have that on yours?

THE WITNESS: I'm sorry. You can't find it.

CHAIRMAN YOUNG: It's not on mine.

JUDGE BARATTA: Evidently, it's not on the printed copies. It is on the electronic copy which is what I'm looking at.

MS. CURRAN: He's looking at a printed copy, the one that we got from Duke. Do you have that same thing?

BY MR. REPKA:

Q It's right after the last presentation slide. It's the first page you're looking at.

A Yes.

Q And it says, "Discussion" at the top.

A Yes.

Q And you're looking at the answer by C. Grandjean.

A And G. Hach.

Q It says, "Added after."

A Yes.

Q Okay.

A Right. There is some discussion there about why those two calculations came to different conclusions.

Q Wait. Which goes to now to their different assumptions.

A Right. And IRSN or Mr. Hach thought there was too much -- for temperature variation in the EDF model so there's some dispute there about the assumptions. I just want to point that out that there was some discussion on the differences.

Q And one of those is that IRSN assumed no gap resistance. Correct?

A I'd say I can't come to that conclusion from what's written here.

Q Okay, but you're not familiar with the assumptions that IRSN did make.

A I find that the way that statement is phased too vague to be able to conclude what those differences are.

Q You find the statement in the exhibit too vague to ascertain what the differences are between the assumptions of EDF and IRSN. Is that what you just said?

A Well, with respect to whether there was a gap resistance or not.

Q Right. But how much do you personally know about the assumptions that IRSN did make?

A Oh, just what was presented in their paper and what I can remember.

Q Okay. Let's look at Duke's Figures 10 and 11 which you've referenced in your rebuttal testimony. That's Exhibits 15 and 16. Do you have those?

A Yes.

CHAIRMAN YOUNG: It's the same as their Figures 10 and 11.

MS. CURRAN: It's in the text.

THE WITNESS: Yes, but I -- The copy of the bound testimony I have has no Figures 10 or 11 in it.

CHAIRMAN YOUNG: Pages 46 and 47.

THE WITNESS: Yes, it's blank.

MS. CURRAN: Oh, it's blank.

THE WITNESS: Okay. Sorry.

BY MR. REPKA:

Q Okay? Now you've discussed these figures in your rebuttal testimony in paragraph 3. Correct?

A Yes.

Q And we had some discussion of this this morning with the Duke witnesses that you were here present for. Correct?

A Yes.

Q It appears to us that what you have done is -- Let me back up. These are actual tests, not calculations. Do you agree?

A Yes.

Q And Figure 10 is a no relocation case. Correct?

A Yes, that's what it says.

Q Do you know anything about the test conditions for this test?

A No, only what was handed out by the staff yesterday late as an exhibit which I hadn't had time to assess.

Q So when you wrote your testimony on July 8 you didn't know what the test conditions were for the test that's illustrated in Figure 10.

A Well, all I know is what was presented in Duke's testimony.

Q Right. And Figure 11, this is a separate test. Correct? This is the test where relocation was involved, was observed. Do you agree with that?

A Yes.

Q And do you know anything about the test conditions for this test?

A Only what was said in the testimony and what was mentioned this morning.

Q Right. Do you know what the initial power was applied in these cases?

A I don't recall the exact numbers, but the second, it was said this morning was higher than the first.

Q Right. And if I told you that they were in different conditions such as different initial power, would you agree that you cannot directly extract and compare data from one test to the other?

A Yes.

Q So when you in your rebuttal paragraph 3 where you've taken you said about 300 degree higher, that appears to be based on a comparison of the data from Figures 10 to 11. Correct?

A That would be if you directly compare them, but since that number was a reasonable comparison to the IRSN calculation I figured it was.

Q Right. So now you'd substitute numbers now for the IRSN calculation instead of the test.

A Not substitute, but I thought it was a reasonable inference. I'm happy to retract the direct comparison that I made there. I think though that the most important point of those two figures is that it does demonstrate that in the high exposure relocation case we have a much smaller difference between the peak temperature of the rupture location, the peak temperature of the nonruptured nonballoon location and that does contradict statements that Duke had made earlier that seem to suggest that the ruptured location is always considerably less limiting than the nonruptured location.

Q Well, with respect to Figure 10, would you agree with me that what this does show in the no relocation case at least is that the cooling effect associated with the swelling and the rupture as evidenced by the dotted line curve of the temperatures at the ruptured ballooned location?

A Yes, that's a reasonable explanation.

Q And Figure 11 is showing you through this dotted line the same cooling effect simply at a slightly different level.

A It looks like it's substantially different.

Q But in both cases, the peak clad temperature for the LOCA analysis purposes, the PCT, is not at the ruptured ballooned location. Correct?

A That's true, but if you look at Figure 11, the difference appears just visually to be on the order of 15, maybe 20, degrees which according to Mr. Dunn's opinion this morning that's not a significant difference.

Q Right, but still bounded by the peak clad temperature at the nonruptured node.

A It appears to be.

Q Now another thing you've done in your rebuttal testimony 3 is you've added a 20 degree factor without a lot of explanation, but you just said here, "The PCT in a rod or relocation occurs, appears to be about 20 degrees up greater than the maximum temperature at the rupture location." I assume you're extracting that from Figure 11 by the difference between the dotted line and the solid line.

A Right.

Q Now do you know that's always the case?

A No, of course not. As a matter of fact, it's not always the case that the ruptured node is non-limiting. As a matter of fact, there are cases when the ruptured node is limiting. It's case dependent. If you want evidence to that effect, I can introduce it.

Q So how can you justify adding 20 degrees on a difference between two different locations and simply adding that to the temperature at the ruptured node?

A That was just a representative, back-of-the-envelope attempt to use the information that we had.

Q Would you agree though that those two curves are completely independent of each other physically?

A No, I wouldn't agree that they are completely independent of each other.

Q So you believe that there's a physical mechanism by which the ruptured node is impacting the temperature of a nonruptured node.

A I would say to the extent that the fuel rod is an integral -- I would say I would not conclude that there's absolutely no. To say they are completely independent would mean that there is absolutely no physical effect that propagates the space of less than a foot between these two regions and I'd say that's probably not the case. I would not say they are completely independent. I'll stop there.

Q Okay. Now you've added all these numbers and you understand, I think, the difference now between the 1841 and the 1750 here. Is that correct?

A I've heard an explanation on that. I understand the difference.

Q Okay, but you've taken the higher, the 1841, and you've added all these numbers and you've come to the back-of-the-envelope of 2174 degrees F.

A For the peak clad temperature, yes.

Q For the peak temperature at the ruptured node plus the 20 degrees to give you some estimate of a real PCT for LOCA purposes.

A It's just representative. Again, I don't believe these back-of-the-envelope calculations are obviously not precise. It's just an attempt to provide a reasonable assessment of these effects.

Q And then the number you come to, 2174, is still below the acceptance limit of 2200, isn't it?

A That's correct.

Q So what are we to make of that? Are we to conclude that's too close to the 2200 to allow licensing?

A No, I think the point is just to demonstrate that relocation has a significant impact and the uncertainty associated with ignoring relocation could be significant in terms of Duke's LOCA analysis. After all, the difference between 2200 and 2174 which is 26 degrees again by Mr. Dunn's standard is insignificant.

CHAIRMAN YOUNG: Could I just clarify something that occurs to me that would be helpful to know right now? Are there any figures on peak clad temperature in nonruptured rods?

MR. REPKA: Well, both figures -- Would you mind, Mr. Harvey, answering that question?

CHAIRMAN YOUNG: If there is no problem, it would just help me to understand this issue of the effect on the temperature of the rupture.

MR. HARVEY: The ruptured temperature that occurs in the LOCA is on the order of 1500 or 1600 degrees. So to not have fuel rod rupture, our temperatures would have be calculated below 1500 or 1600. So those cases are really unimportant because they are so low in temperature.

Generally, all the cases that we analyze in the large break LOCA event are going to lead to fuel rod rupture. If you don't have fuel rod rupture, that means your temperatures are below 1500 or 1600 degrees Fahrenheit.

CHAIRMAN YOUNG: The reason I ask that question was because Mr. Repka was asking Dr. Lyman about the effect on the nonruptured location. So it occurred to me that what we don't have is the temperature curve of a nonruptured rod that has no rupture on it. Maybe it just --

(Discussion off record.)

MR. REPKA: Yeah, I think the answer is anything that's at a temperature that high that would be important for a PCT calculation, the assumption is that they're going to be ruptured because you're at the temperatures of which that's an issue. For nonruptured rods, it would have to be down like 1300 degrees or 1400, 1500, less than 1500.

CHAIRMAN YOUNG: So your question was limited to the effect of relocation on peak clad temperature.

MR. REPKA: Right.

CHAIRMAN YOUNG: Okay.

MR. REPKA: Relocation can have an effect at the ruptured node and my question was limited to whether it could have an effect at the nonruptured node or you would be measuring temperature or analyzing calculated temperature at different points. Is the nonruptured location impacted by the ruptured location was the thrust of my question.

CHAIRMAN YOUNG: Right, and the reason I was asking it, and maybe I'm off track here but just to clarify, was the curve of the nonballooned location up to 150 some seconds is pretty much similar in the two diagrams. So I was wondering whether if you had a nonruptured tube, it would have the same peaks that sort of go along the same curve as the temperature changes on the ruptured part of the tube. Do you see what I'm saying? Does it make any sense?

MR. REPKA: You would want to be showing the exact same test conditions as Figure 11 and having a curve at a nonruptured location for a fuel rod in which there was no rupture and to see how that compares to the solid line curve on the Figure 11. That's what you're asking.

CHAIRMAN YOUNG: Right, that's what I was wondering.

MR. REPKA: There is no curve in the evidence that shows that.

CHAIRMAN YOUNG: Okay.

MR. REPKA: And the reason is because if it gets that hot, it will rupture. But the short answer is there is no such drawing.

CHAIRMAN YOUNG: The reason I was asking that, and maybe this is just to clarify it for me, but from these, I can't tell whether the rupture itself has some effect on the peak clad temperature or whether it's going up anyway and the ruptured area just sort of goes up with it. Does that make sense?

MR. REPKA: Yeah, what you're trying to tell is whether it clearly has an effect at the rupture location. The question is does that rupture have an effect on the nonruptured location and is that acting independently of the rupture. That is, is the PCT which at the nonruptured location independent?

CHAIRMAN YOUNG: Right.

MR. REPKA: And the testimony of our witness is that, yes, they are independent, but I would rather they answer that than --

CHAIRMAN YOUNG: Okay, well, I just wanted to clarify.

MS. CURRAN: And I think Dr. Lyman also answered the same question.

THE WITNESS: Yeah, I mean I think that depends, if there's relocation, on the height of the column of rubble and depends on the length of the balloon as well. So I certainly couldn't conclude without seeing any analysis that those two are independent.

MR. REPKA: Mr. Dunn and Mr. Harvey are going to confer. But maybe if you would give them a little leave, they could answer that question.

(Discussion off record.)

MR. REPKA: You may want to turn to Figure 1 and Mr. Dunn would like to address it.

CHAIRMAN YOUNG: Exhibit 6.

MR. DUNN: On Figure 1, we have the results of the calculation for the LOCA limits case. In this case, it's the one we've been talking about before that had a peak clad temperature of about 2020 degrees and a ruptured node temperature of 1750.

The solid line is the peak cladding temperature line. That's the one that's in the middle. The upper line is the fuel temperature. This solid line is at a location on the fuel pan that is just below the rupture location. The rupture here actually occurs above the location of peak cladding temperature.

It's because cooling is full but it's also because of the nature of the power shape. When you force power up as high as approximately 10 feet in the core, the power tends to drop off very quickly after that so that the regions below the location of peak power are relatively hotter or more powerful than the regions above it. So we get a relatively higher power and the cooling effects induced by the rupture are not applied below the rupture location.

CHAIRMAN YOUNG: And so if there were no rupture, you're saying -- Well, you may not be saying this. If there were no rupture, the temperature would still follow the same general curve as the peak clad temperature on ruptured locations. Is that what you're saying?

MR. DUNN: I would agree with that, ma'am? I'm sorry, Judge.

CHAIRMAN YOUNG: That's okay.

MR. DUNN: Judge Young, I'm sorry.

CHAIRMAN YOUNG: That's okay.

MR. DUNN: The one I can think of easiest is if we had a pin that looked very much like these pins but didn't have any internal pressure in it. So that the stress wouldn't develop to rupture the fix. I think its transient would be very much, not identical because there are small little things, like what we predict here. (Witness indicating.)

CHAIRMAN YOUNG: Okay. Thanks. I appreciate that clarification because when you're talking about a fix, I couldn't help but wonder. Thank you very much. Thank you.

MR. REPKA: Okay.

CHAIRMAN YOUNG: And, Dr. Lyman, feel free to add anything. I just was trying to understand in my own mind what was going on.

THE WITNESS: No, I think I already added. I guess I would just like to emphasize that although Duke is trying to represent as if the peak temperature at the burst location is never limiting, that's not the case. There are cases where the burst rupture location is limiting and that depends on the temperature of the transient.

I might as well offer this email which we got as a result of Duke discovery which makes that clear in the case of relocation. This is an email that we received from Duke. I'm not sure how you would like me to identify it for the record.

CHAIRMAN YOUNG: Just say who it's from

and the date.

THE WITNESS: Yes, this is an email that was sent from someone named Mitchell E. Nissley of Westinghouse to, I believe, it was the PIRT panel on high burn-up fuel and it was cc'ed to the NRC staff, some of whom are present here. This email was discussing relocation effects.

On the second page of this handout, the second to last paragraph, I'd just like to read this. "The first part of this makes clear that Westinghouse has included fuel relocation on local power generation, fuel thermal conductivity and gap conductance in its best estimate plus uncertainties. Large break LOCA model approved by the U.S. NRC. When the analysis is done statistically such that all parameters are not at their worst value simultaneously as done on a deterministic calculation, the burst node is typically nonlimiting except in very high temperature transients." So this shows that there are calculations in which it's obviously not that common that the burst node is limiting, but it does happen.

CHAIRMAN YOUNG: If the high temperature transients, do you know what level of temperature you're talking about here?

THE WITNESS: No, I'm afraid all I have is this email and, of course, the Westinghouse model as we've heard is priority. So I don't have any information about that.

CHAIRMAN YOUNG: I mean just generally what would be considered.

THE WITNESS: Well, I assume this is in the context of a design basis LOCA that's by definition restricted to less than 2200 degrees Fahrenheit. So I would assume that meant those LOCAs that take you into a regime, I would guess, above around 2000 or 1900 to 2000 degrees Fahrenheit.

CHAIRMAN YOUNG: Thank you. Was there any objection to this document?

MS. UTTAL: No.

CHAIRMAN YOUNG: Let's go ahead and mark it and enter it as 53. Is that right?

(The document referred to was marked for identification as Exhibit No. 53, and received in evidence.)

JUDGE BARATTA: While we're on this exhibit, could I ask a question or two for clarification because there was some discussion that appears in here that I don't know whether you have any amplifying remarks on? But it says on the second page of the email, "The above supports the conclusion that medium is an appropriate ranking" - this referring to the PIRT that was done, I guess, for the NRSA - "for high burn-up UO_2 fuel. A higher ranking for MOX may be appropriate." Do you have any understanding as to why that statement was made?

THE WITNESS: Yes. The PIRT Panel, there was some opinion that that higher ranking for MOX might be appropriate because I believe of the potential for greater generation of fine fragments and I think I even referred to that in my testimony.

JUDGE BARATTA: Do you know if that was for weapons grade or reactor grade MOX?

THE WITNESS: I think that was either. I don't think that was --

JUDGE BARATTA: So it was generic.

THE WITNESS: Yes. So I believe that is another reflection of the opinion that there may be the potential for greater generation of fine fragments in MOX fuel and that could amply fuel relocation effects.

JUDGE BARATTA: Thank you.

CROSS EXAMINATION (Con't)

BY MR. REPKA:

Q Just one question about this document,

Exhibit 53, he read a sentence from here that "the burst node is typically nonlimiting except in very high temperature transients." Do you recall that?

A Yes.

Q Now what that says is in these high temperature transients, whatever they are, but we don't know what they are, the burst node may be the limiting temperature. Correct? It may be the peak clad temperature in that case.

A That's the way I read it, yes.

Q Right. But does anything in here say that the relocation effect at the burst node is impacting the clad temperature at a nonrupture location?

A No, but that's not relevant if the burst node is the limiting -- That the peak clad temperature would be at the burst node in that case.

Q And with respect to the reference over on page one of this exhibit that the author of the email states, "I think the ranking of medium is appropriate for high burn-up UO_2 fuel," etc. etc.

A Yes.

Q Would you agree that there was a difference of opinion amongst the PIRT members as to the relative ranking of the issue?

A Yes, and I think we pointed that out that there was disagreement. But I think that only underscores the fact that there's disagreement among experts in the absence of experimental data that would help to resolve some of these issues.

Q Right, but it's just to raise a point that you haven't already raised in your earlier testimony that there was this disagreement.

A I'm sorry. Could you repeat that?

Q This doesn't raise an issue you haven't already raised before.

A No, I cite this email because of that indication that shows that the burst node can be nonlimiting under certain circumstances.

Q Way back before we went off --

A Limiting. I'm sorry.

CHAIRMAN YOUNG: Can be?

THE WITNESS: Can be limiting under certain circumstances.

BY MR. REPKA:

Q Before we went off on this last digression, we were talking about the calculation where you added these various figures and came up with the 2174 degree F. Do you remember that conversation?

A Yes.

Q And I think I asked you what are we to make of it all and you said that well it shows you that relocation has a significant impact. I think that was what you said. Correct?

A Yes.

Q When you said it has a significant impact, do you mean to say that it may have a significant impact or do you mean to say it does have a significant impact?

A Well, I think from all the information I've seen, from the IRSN calculations, they strongly suggest that if relocation occurs it will have a significant impact on the parameters that are necessary or parameters that must be determined for compliance with 50.46. Obviously, there are many uncertainties associated with that.

I think we've discussed some of them, but I think in general my opinion is that relocation is a significant effect. And, in particular, the difference between MOX and uranium is of particular concern in the context of this proceeding in that if relocation is ignored in a calculation of an evaluation of the performance of the MOX LTA as under designed basis LOCA conditions, that it may be nonconservative to a significant degree.

Q And in your calculation, your back-of-the-envelope calculation you called it, you did use the 300 degree number that roughly equated to the 313 calculation of IRSN. Correct?

A Yes.

Q And I think you stated earlier already that you either didn't know or now understand that the IRSN calculation did not include any of the beneficial cooling effects of relocation.

A The IRSN calculations were -- There is obviously certain methodology. My initial approach from reading that document was to conclude that the clad temperatures that they presented in that document were in fact the peak clad temperatures that should be compared with the compliance limit. In fact, they do so in that document.

So the clear implication is that they believed the clad temperatures they calculated in that document were relevant with regards to peak clad temperature. I agree that without considering these cooling effects you are not getting a complete picture, but I would also acknowledge that some of the flow blockage issues that were raised this morning need also to be considered in an integral approach.

But even putting that aside, even if we take the effect of the cooling that Duke has put forward on the burst node in their calculation where the burst node is not the limiting one and an impact, a several hundred degree Fahrenheit temperature increase is going to be highly significant not only with respect to PCT compliance, but also with respect to peak clad oxidation compliance.

Q And in any calculation for PCT compliance, you've not included any consideration for the decay heat conservatism in the Duke Mox calculation.

A That's correct. And I 100 percent agree with Duke that in adapting Appendix K appropriately to MOX fuel not only relocation effects should be considered, but also the decay heat. However, we heard according to the testimony taking into account the decay heat would be an up-to-75-degree-Fahrenheit effect which could be dwarfed by the most severe relocation effects. But I do agree that they should both be considered together.

Q And then you referenced some of the blockage issues. You've raised that in your testimony and you've included the diagram from IRSN that we discussed this morning. Do you dispute Mr. Dunn's testimony that they've done calculations to show that blockage up to 90 percent will have no significant impact?

A Well, I would just point out our Exhibit L.

MS. CURRAN: It is Exhibit 36.

THE WITNESS: Sorry. Yes, this is 36. This was the very recent presentation by IRSN at the meeting in Argonne two months ago where IRSN continues to express concerns about the understanding of core coolability if relocation occurs. And I would just like to direct attention to page 23 which I cite in my testimony where IRSN continues to regard the coolability of blocked regions as a pending question in the event of fuel relocation.

They highlight that IRSN believes that impact of fuel relocation, fuel rod balloons, has a fully questionable effect on the coolability of the blocked region. So I would interpret IRSN's concern as a concern that the analyses that were conducted on acceptable levels of blockage that did not take fuel relocation into account are questionable.

Q And do you know whether IRSN had looked at the Duke MOX calculations?

A No, I have no knowledge of that.

Q Do you know whether they are familiar with the Framatome assessments of cooling issues?

A I assume they are, but I have no knowledge of that.

Q But you don't know that. Then have you talked to Mr. Dunn about the Framatome cooling analyses?

A No, only what I've heard in this hearing.

Q So you don't have any basis other than IRSN's statement to say that there's a question and to say that that question hasn't been answered.

A That's correct.

CHAIRMAN YOUNG: Do you -- If I could just. I asked this morning whether the assembly configuration that IRSN was referring was similar to the Catawba to the lead test assembly configuration. Do you know?

THE WITNESS: Well, if you look at the references where the drawing, photograph, actually came from, it came from Japanese studies.

CHAIRMAN YOUNG: Which photograph?

THE WITNESS: The one that you had pointed to which was in our Exhibit D. I'm sorry. I don't have the numbers.

MS. CURRAN: Oh, I do. Exhibit D is Exhibit 28.

CHAIRMAN YOUNG: I have it here.

THE WITNESS: Exhibit D, you pointed to

one of the pictures in there

CHAIRMAN YOUNG: Cross sections.

MS. CURRAN: Page 29.

CHAIRMAN YOUNG: Oh, no. The assembly.

THE WITNESS: Right. That is what you're referring to. Is that correct?

CHAIRMAN YOUNG: I was just wondering if they had just claimed whether there's any variation in the distance between rods in different assemblies. I know there was some discussion about assembly.

THE WITNESS: Well, in general, pressurized water reactor and fuel assemblies there are slight differences in the structure, in the pitch, and the distance between fuel rods, fuel cladding, thickness, but small differences generally. And the basic PWR array in a fuel assembly, I believe, is pretty standard.

CHAIRMAN YOUNG: So you think that the discussion about blockage would apply to the same type of assembly.

THE WITNESS: Yes, in general. I would also like to point out in that context in our Exhibit 36 which we're just talking about, page 20, that IRSN discusses the balance between the reduction in flow in blocked regions which would cause a reduction the heat removal and it considers the balance between that and the effects that Mr. Dunn was talking about this morning including the droplet coolant from the shattering of droplets. So IRSN is fully cognizant of these different effects. I think one of the issues that they want to resolve is how that balance is maintained in the effect of fuel relocation.

CHAIRMAN YOUNG: Thank you.

MR. REPKA: I don't have anything further for Dr. Lyman right now. I would say that I want to at least consider the possibility later of asking to bring back our witness to talk about some of the CABRI tests and the other paper that BREDL has just given us for the first time here today.

CHAIRMAN YOUNG: Yes. I think we were going -- Last time, we went back to Duke. Do you want to go back to Ms. Curran or do you want to go to the staff next?

MS. UTTAL: Let me see if we have any questions.

CHAIRMAN YOUNG: Okay.

MS. UTTAL: We have no questions.

CHAIRMAN YOUNG: Do you have any more questions?

MS. CURRAN: Yeah, I will have some

questions and actually it might take a little while. So I wondered if Dr. Lyman needed a short break.

THE WITNESS: I'm okay.

MS. CURRAN: You're okay.

CHAIRMAN YOUNG: Let's take five minutes. Off the record.

(Whereupon, the foregoing matter went off the record at 2:06 p.m. and went back on the record at 2:26 p.m.)

CHAIRMAN YOUNG: Back on the record. Ms. Curran go ahead.

MS. CURRAN: Okay.

BY MS. CURRAN:

Q Dr. Lyman, what training and/or experience do you have that gives you expertise to testify in this proceeding regarding Intention 1?

A Well, I've been studying various issues associated with the safety and security of mixed oxide fuel for more than a decade. I was a co-author of one of the first papers to explore the possibility of using MOX fuel to dispose of separated plutonium. I've, since then, conducted my own analysis of various safety issues associated with the US Program to use mixed oxide fuel including an analysis of the consequences of severe accidents. I've attended numerous meetings over the last 12 years regarding safety issues associated with mixed oxide fuel.

I've attended many NRC discussions of these matters and I've presented papers at conferences.

Q And does your educational background give you the skills that you need to do this work?

A I've a PhD in physics which fosters the spirit of inquiry and gives one the technical skills to, I believe, understand on a technical level a number of issues associated with nuclear fuel, nuclear power plants, and MOX fuel.

Q Okay. Duke has testified that even of the pessimistic IRS and predictions are simply added to the current Cotaba (phonetic) MOX fuel LOCA evaluations, the results remain well below the acceptance criteria of 10 CFR 50.46. In your opinion, is it scientifically valid to add the IRS and predictions for the increase in peak clad oxidation in the Cotaba MOX fuel calculations for peak clad oxidation in order to determine whether the 10 CFR 50.46 acceptance criteria for peak clad oxidation are met?

A No, I don't believe it's valid to simply add the increase in peak clad oxidation that was calculated by IRSN for a particular scenario to Duke's own calculation for a clad oxidation conclude that that's the maximum peak clad oxidation effect associated with relocation. The proper way to do that calculation would be to actually use the -- Duke's own time, temperature curves, modify those for relocation effects and then estimate the total clad oxidation associated with their LOCA transient. It's not a linear process that you can simply add the results of the clad oxidation -- transient clad oxidation from the IRSN predictions and simply add them to Duke's predictions, so I don't believe that's an appropriate way to bound the impact of relocation on the peak clad oxidation of the MOX fuel lead test assemblies.

Q Dr. Lyman, you have stated that there's an unacceptable level of uncertainty with respect to the behavior of MOX fuel under LOCA conditions; is that correct?

A Yes, that is what I believe.

Q Do you seek a perfect level of certainty?

A No, I don't seek a perfect level of certainty. I seek reasonable assurance that the MOX fuel lead test assemblies will under design basis loss of pull and accident conditions comply with 10 CFR 50.46 criteria. To obtain that reasonable assurance, a certain amount of experimental data is required. The issue associated with MOX fuel is that there is considerably less operating experience with MOX fuel and considerably less testing data associated with MOX fuel under extreme conditions compared to the more than 50 years worth of data accumulated for light water reactor uranium oxide fuels including numerous test series of uranium oxide fuels under loss of coal and accident conditions. The data base for MOX fuel simply does not compare to that of uranium fuel and in that context, I believe that the MOX fuel data base is too sparse at this time to support the application for MOX lead test assemblies.

Q Duke says that there is substantial conservatism in its Appendix K design basis LOCA calculations which add up to approximately 600 degrees Fahrenheit PCT margin relative to best estimate calculations. Is this 600 degree Fahrenheit margin relevant to this proceeding?

A No, that margin is not relevant to this proceeding. Duke had the option to do a best estimate calculation of the design basis LOCA analysis for the MOX fuel lead test assemblies and they did not do so. They chose to use the Appendix K deterministic model. So while the best estimate results may be interesting they simply have no bearing in this proceeding. All that matters in this proceeding is whether the application of Appendix K models to the MOX fuel assembly design basis LOCA analysis provides adequate assurance that those lead test assemblies will comply with the regulatory criteria.

Q In his rebuttal testimony Mr. Harvey said that the NRC does not -- the NRC staff does not endorse the higher PCT values calculated by Grandjean at all. Do you agree?

A I don't agree with that statement. In the exhibits that we presented, including the Exhibit B in particular which is Number --

Q 26. Exhibit B you're saying?

A Yes, Exhibit B and Exhibit -- and Exhibit C, I read those documents to indicate that there is considerable concern within the international community in general and within NRC in particular, on recent information about the potential impact of relocation on design basis LOCA.

CHAIRMAN YOUNG: Which official exhibit numbers were those?

MS. CURRAN: Oh, Exhibit B is Number 26. Exhibit C is 27.

CHAIRMAN YOUNG: Thank you. Go ahead.

THE WITNESS: In particular, I would -- right in Attachment 5, which is part of Exhibit C, what's labeled Attachment 5, page 2, the NRC states that, "It is likely that new test data will be needed to fully resolve the issue, that is the issue of the impact of fuel relocation on LOCA". I would say that that's pretty strong language indicating the need for new test data to fully resolve this issue. As far as the magnitude of the uncertainty associated with fuel relocation, I'd point you to Table 1 of Attachment 5, which is on page 4, where this may not be an endorsement of the IRSN results, if you look at the fifth item from the bottom on Table 1, it shows the fuel relocation value or estimated Delta PCT of plus 313 degrees Fahrenheit, which was based on the IRSN calculations and I think its presence in this table indicates that the NRC staff had some confidence in the order of magnitude of that calculation.

BY MS. CURRAN:

Q In his rebuttal testimony, Mr. Harvey states that the current Appendix K approach relies on the known conservatisms to more than offset any of the potential non-conservatisms. Do you think this statement is applicable to the use of Appendix K analysis with respect to MOX fuel?

A No, I don't think that that statement is applicable. Appendix K explicitly references uranium oxide pellets and an application of Appendix K models to MOX fuel. I believe that there are effects which could have the significant impact on a MOX fuel design basis LOCA analysis that are not included or modeled in Appendix K. And my use Appendix K models with MOX fuel, that fully accounting for effects that could have a significant impact on the MOX fuel analysis.

Substantial non-conservatism is being introduced in the Appendix K model that upsets the balance of conservative versus non-conservative assumptions involved in the Appendix K applications to uranium fuel. These assumptions -- these effects not modeled in Appendix K, as we know are fuel relocation primarily and in the other direction, is the MOX decay heat which, as Duke has testified, is likely to lower the PCT by something on the order of or up to 75 degrees Fahrenheit. Both those effects are relevant in the application of Appendix K to MOX fuel but as I pointed out the potential positive impact of fuel relocation on Delta PCT is considerably greater than Duke's estimate for the maximum negative impact on Delta PCT of the -- using the MOX decay heat.

Q In paragraph 16 of his rebuttal testimony, Mr. Nesbitt states that the difference between the result of an Appendix K analysis for LEU fuel and one for MOX fuel if relocation is considered in both cases, would only be a matter of a relatively small degree. Do you agree?

A No, I don't agree with that statement. First of all, relatively small is a subjective term. So it's hard to pin it down. But I believe that from all the information I've seen that we've presented at this hearing that the potential impact could be large if it is verified that the amount of fine particles in the packing fraction or filling ratio associated with MOX fuel relocation is significantly greater than for LEU fuel at the same burn-up.

If that is the case, there could be up to a several hundred degree difference in the impact on PCT associated with relocation. However, I do not believe there is sufficient experimental evidence to quantify the difference between MOX and LEU with regard to impact on fuel relocation and that is why we believe that fuel relocation studies involving MOX fuel under LOCA conditions are called for.

Q In paragraph 20 of his rebuttal testimony, Mr. Dunn says that the increase in LOCA linear heat generation rate within the ballooned area will not lead to significant increases in PCT in that area. Do you agree with this conclusion?

A No, I don't agree with that conclusion. Again, given the information that we've presented at this hearing the potential impact on PCT of considering fuel relocation is significant. Certainly increases on the order of 100 degrees Fahrenheit, which Mr. Dunn described today as what he -- or what the industry considers in the general realm of significance, certainly that type of increase could easily be the result of fuel relocation and again, referring to the IRSN impacts could be considerably higher than that. So I think -- I don't think there's any question in my mind that the impact of fuel relocation could be highly significant on the ability of MOX fuel assemblies to comply with the 50.46 criteria.

Q In paragraph 21 of his rebuttal testimony, Mr. Nesbitt states that you have ignored the clear evidence that the Cotaba MOX fuel lead assemblies would be better than their co-resident LEU fuel assemblies in the aspect of the peak linear heat generation rate. Do you agree?

A I don't agree. We acknowledge that for the loading of four MOX lead test assemblies and the

Contaba Core, they will be loaded in non-limiting positions where the power will be somewhat lower and that this will counteract the tendency of MOX fuel to have a higher linear heat generation rate and higher central line fuel temperature for the same position in the core.

However, that effect could well be significantly lower than the impact considering relocation in the event that MOX fuel relocation turns out to be significantly greater than LEU fuel relocation because of the generation of fine fragments in the MOX fuel case.

Q Do you think that Duke will be able to avoid exceeding the 10 CFR 50.46 PCT limits by adjusting plant operating parameters such as power peaking?

A I don't believe that Duke will be able to accommodate the potential -- the potential peak cladding temperature for MOX fuel lead test assemblies unless it has an accurate assessment of the impact of fuel relocation on those fuel assemblies. Only with that kind of information in hand, could you determine whether there is a possibility that the MOX fuel assemblies are likely to exceed 50.46 criteria and them make adjustments accordingly in the operating limits, in the loading pattern of the MOX assemblies.

So without better information, it's going to be difficult for Duke to provide high assurance that the modifications they've made will insure that the MOX lead test assemblies comply with 50.46 criteria.

Q Duke says it disagrees with your theory that MOX fuel pellets may experience greater fragmentation than LEU fuel pellets. Is this a theory?

A The concept that MOX fuel may create greater fragmentation and greater generation of fine fragments than LEU fuel is not a theory. It's been --

CHAIRMAN YOUNG: You said than LEU fuel?

THE WITNESS: Than, greater, greater generation of fine fragments in MOX fuel compared to LEU fuel is not a theory. There is experimental evidence to support that and we have provided that in the exhibits that we distributed earlier. I don't have the numbers.

BY MS. CURRAN:

Q Would this be the CABRI paper?

A In the CABRI paper and the Argonne paper. In addition, the NRC staff in their testimony did discuss or admit that there would be or that there was a possibility that there could be greater generation of fine fragments in MOX fuel because of the high burn-up structure within plutonium conglomerates. So I believe there's substantial basis for thus concern.

Unfortunately, there isn't enough experimental evidence to conclusively verify it and to make a quantitative estimate of the potential impact of MOX fuel properties on relocation.

Q In paragraph 24 of his rebuttal testimony, Dr. McCoy asserts that Duke's position that no differentiation in pellet cracking and fragmentation should be made between MOX and LEU fuel is supported by the micro-graphs of irradiated fuel that Duke provided in its testimony. Do you agree?

A I don't agree that the micro-graphs provided by Duke in its testimony demonstrate that there's no concern associated with the greater generation of fine fragments in MOX fuel than in LEU fuel. As I discussed earlier, the cracking pattern developed during normal irradiation is not the only mechanism by which fine fragments could be generated. The other mechanism is the -- as I described before, the high burn-up structure within the rim layer and high burn-up uranium fuel or within the plutonium conglomerates and MOX fuel, in which case the generation of fission gases and their movement to grain boundaries provides a mechanism by which if those gasses expand during a temperature increase they cause grain separation and fragmentation and again, I would refer to the CABRI paper and the Argonne paper for some demonstration of that aspect.

Q In paragraph 34 of his rebuttal testimony, Dr. McCoy asserts that a small difference in thermal conductivity between MOX and LEU fuel translates into a small difference in thermal gradients or thermal stresses during glow-down and therefore, the effect of these stresses on pellet fragmentation will similar to that in LEU fuel. Do you agree?

MR. REPKA: Can I ask a question, a point of procedure here? Are we in rebuttal now or are we in redirect? Where are we because this is certainly the --

MS. CURRAN: I'm doing surrebuttal which I understood you to have done with your --

MR. REPKA: I understand.

THE WITNESS: The difference -- the impact of the differences in thermal conductivity between MOX and uranium fuel and their impact on fragmentation behavior during a loss of con-accident, have not been evaluated by Duke and it's not apparent to me that the difference in thermal conductivity could not have an impact on the generation of greater thermal stresses and more fragmentation in MOX fuel than uranium fuel. I believe that's another effect that needs to be further modeled and further examined experimentally.

BY MS. CURRAN:

Q In paragraph 35 of their rebuttal testimony, Dr. McCoy and Mr. Dunn quote the testimony of the -- of NRC staff witness, Dr. Ralph Meyer, with the proposition that it appears that small particles or fines are blown out of the burst opening when the rod depressurizes. They say that according to Dr. Meyer, there would be few or no small particles the ballooned region and it is these small particles that have been postulated to make a difference between the mass of fuel and a balloon in MOX fuel and LEU fuel. Do you agree that there is a reasonable basis for the conclusions that these witnesses share?

A No, I don't believe that the information provided by Dr. Meyer indicates that there's no basis for concern and with regard to the generation of fine fragments and their impact on relocation in the LOCA.

He offers a photograph, a single photograph where all one can see are large fragments through a hole in the burst opening. But I'd like to refer you to the exhibit that we offered, the LOCA results for advanced alloy and high burn-up zircoloid (phonetic) cladding.

Q That's Exhibit 52?

A Exhibit 52, in particular the discussion on page one, two, three, four -- on page five. The heading is LOCA In-cell Integral Test Results ICO Number Two Test Sample. The photograph that Dr. Meyer referred to was from what's called ICL Number 1 at Argonne. That actually was not an integral LOCA test but was a high -- was a ramp to burst test in argonne gas, so it was not a complete integral LOCA test. But that aside, ICL Number 2 was an integral LOCA test and if you read the first paragraph the fourth sentence, it says, "During the post-test handling of this fuel, some fuel particles less than .3 millimeters in size, fell out through the burst opening".

Now I read that -- I would interpret that statement to mean that during the post-test handling fuel particles less than .3 millimeters in size fell out through the burst opening, therefore they must have still been within the fuel during the test. So clearly not every small fuel particle and these are again the size of particles that we're concerned about, less than one millimeter compared to the large chunks, the fragments characteristic of the normal cracking during irradiation in the fuel which are several millimeters or on the order of a millimeter greater in size, these are small fragments, less than .3 millimeters, yet they still remained in the fuel and fell out when the fuel was handled post-test.

So again, I would interpret that to mean that there are some small fragments that do remain within the fuel during the test and so, therefore, the observation that Dr. Meyer made is not that compelling to me. I also made the observation in my rebuttal testimony that in the case of MOX fuel where you would have a more or less homogenous distribution of material through the entire pellet, because it occurs in the plutonium conglomerates that are distributed through the whole pellet, it's hard to see why there would be selective leaking of all the small particles through that bed of fragments in the case of MOX fuel, whereas in the case of uranium fuel where the rim is located near the cladding, you might have a greater loss of small particles when the burst occurs because of the location of the finer particles near the burst opening. So for those two reasons, I'm not convinced that there is not a significant quantity of small fines left in the fuel as a result of relocation during the -- after the burst and when the temperature continues to rise until it reaches the peak clad temperature.

Q In paragraph 36 of his rebuttal testimony, Mr. Harvey describes the conclusions of a PIRT panel from 2001 regarding the importance of fuel relocation as a safety issue. He says that none of the PIRT panel members rated fuel relocation as important. Two rated it as low medium and our rated it as of low importance. Do you agree with his description?

A I believe the characterization of the PIRT panel was inaccurate. The panel consisted of, to my understanding --

CHAIRMAN YOUNG: Which exhibit?

THE WITNESS: I'm going to be discussing Exhibit -- the e-mail.

MS. CURRAN: Oh, yes, Exhibit 53.

THE WITNESS: Exhibit 53, but before I get to that, the PIRT panel consisted of several different groups of experts rating the importance of various phenomena with regard to loss of coolant accidents in this e-mail which is Exhibit 53. This is actually the second e-mail which starts on page 2. There's a discussion of the difference in opinion between two of the groups on the importance of fuel relocation in doing integral LOCA tests.

BY MS. CURRAN:

Q Dr. Lyman, just back up for a minute and identify that e-mail.

A I'm sorry. Yes, this e-mail is from Ralph Meyer to a group of people -- to the PIRT participants.

Q And what's the date on it?

A And the date is October 13th, 2000.

Q Okay.

A So it's clear that these are an experimental group ranked fuel relocation as significant but an analytical group ranked it well, as medium, I guess of moderate significance and said it had a modest effect on LOCA linear heat rate. And it was the opinion of Dr. Meyer that the analytical group ranked this too low and so this appears to me to be perhaps a difference of opinion between a different set of experts with different perceptions about the importance of experimental data and the relevance and importance of experimental data.

So I'd just like to point out that this difference has been noted and it was really different groups of experts but this only strengthens my argument that there is uncertainty concerning the importance of fuel relocation effects and that only underscores the fact that there's a need for more data to resolve that uncertainty. When a group of the intelligent people, experts disagree, it means that there's still a lot of uncertainty in the data that allows that disagreement to flourish.

Q In paragraph 51 of his rebuttal testimony, Mr. Harvey says that the differences between the irradiated and unirradiated test results at power burst facility or PBF have been explained. What is Duke's explanation and is there a different staff explanation?

A Yes, in the rebuttals of Duke and the staff, they seem to give different explanations for the observation of the power burst facility test that erradiated cladding experienced greater balloon size than unirradiated cladding. Duke cites an explanation having to do with the lack of -- or the lack of temperature variation in the azimuthal (phonetic) direction as the contributing factor to this difference between erradiated and unerradiated cladding. However, the staff offers a different explanation stating that that explanation is probably wrong and offering a different one having to do with the temperature at the time of the rupture of the balloon.

CHAIRMAN YOUNG: Where -- can you give us a reference?

THE WITNESS: Yeah, I'm sorry. In the rebuttal testimony of Duke, this would be paragraph 49. In the rebuttal testimony of the staff, -- I'm sorry, could I have a copy of the rebuttal testimony of the staff, please?

MS. CURRAN: You don't have it?

THE WITNESS: No. Sorry about this. That would be Answer 8 on page 5. So the fact that there still seems to be some disagreement about the interpretation of these tests which were done more than 20 years ago, indicates to me that the phenomenon is not completely -- there's not a complete consensus on the origin's phenomenon and so I wouldn't conclude that it's fully understood the difference between unerradiated and erradiated clad and their swelling properties is not fully understood at this point so IRSN's concern regarding the impact of these -- the PBF tests on an understanding of blockage phenomena during design basis LOCAs I think is -- it's warranted that further experimental studies be pursued to isolate the particular phenomena responsible for this effect.

BY MS. CURRAN:

Q In paragraph 60 of his rebuttal testimony, Mr. Dunn asserts that your comments about EPRI's and AVREVA's lack of cooperation with NRC regarding the provision of M5 clad LEU testing data is largely immaterial to the technical issues raised in contention 1. Do you agree?

A I don't agree that it's largely immaterial simply because the MOX fuel LTAs will have M5 cladding, so any data with regard to M5 cladding will have some relevance to their performance in loss coolant accidents.

Q In paragraph 61, Mr. Dunn states that he is mystified by your assertion concerning a lack of cooperation by AVERVA because the text of the letter that you provide as an exhibit eludes to no such cooperation. On what did you base your conclusion?

A I believe there's no such lack of cooperation is that.

Q I'm sorry, on such lack of cooperation.

A Yes.

Q Yeah.

A Well, I base my conclusion there's a lack of cooperation in the fact that the NRC staff asked AVERVA for these samples of erradiated M5 clad fuel for testing at Argonne last September and as of the date of Duke's testimony they had not even concluded a memorandum of understanding that governs the supply of that fuel, so I would interpret this at least 10-month gap as not -- as an absence of I'd say active cooperation.

Also I'd like to offer another letter as evidence just to buttress this point, there are two letters actually just to supplement the record as long as it was brought up by Mr. Dunn. The first is a letter dated May 5th, 2003 from James F. Mallay, the Director of Regulatory Affairs at Framatone to Ralph Meyer. In this letter, Mr. Mallay expresses some concern that tests that were done on unerradiated M5 cladding at Argonne were done without the consent of Framatone. They were concerned because these tests apparently showed there was some undesirable behavior of M5 cladding under certain circumstances during high temperature oxidation. That's a letter of May 5th.

The letter of December 15th, 2003, again, from James Mallay of Framatone to Ralph Meyer, states that -- this is the second paragraph, "You should appreciate Framatone's reluctance to proceed with serious discussions, however, about providing erradiated fuel. As you know, we've always been willing to cooperate on this program, but we expect the spirit of cooperation to be extended in both directions. We want you to understand that we have several reservations about continuing this cooperation to include erradiated fuel".

And the first reason was, "We were dismayed when it was discovered that our M5 cladding was altered or etched to perform some comparative tests with another type of cladding". Putting aside Framatone's concern, I just believe that this paper trail documents a lack of active cooperation on the part of Framatone with getting a complete set of data on the performance of M5, of erradiated M5 or on high burn-up fuel with M5 cladding. I would think that Framatone would welcome, you know, generation of additional experimental data to clear up some of these issues and the fact that this fuel has not been provided yet, I'd say documents a lack of cooperation. I would like to see these tests proceed because I believe they have a bearing not only on this proceeding but also on the use of M5 cladding in other situations. So that's the basis for my belief that cooperation is not going as smoothly as it could be on supplying this fuel to Argonne.

CHAIRMAN YOUNG: Any objections to these

two proposed exhibits?

MR. REPKA: No.

MS. CURRAN: So A would be marked as --

CHAIRMAN YOUNG: 54, is 54 the next one? Okay, so the May 5th one will be 54, admitted and the December 15, 2003 will be received as Exhibit 55.

(The documents referred to were marked for identification as Exhibit Numbers 54 and 55 and were received in evidence.)

BY MS. CURRAN:

Q In paragraph 73 of his rebuttal testimony, Mr. Nesbit accuses you of simplistically superimposing a PCT increase of 313 degrees Fahrenheit on a reported MOX fuel PCT of 2,018 degrees Farhenheit which he says is incorrect. Is he right?

A As I said before, that was following the approach of the IRSN paper which did not distinguish between the temperature at that burst rupture location and a peak clad temperature at an unruptured location and the reason for that is it didn't take in to account the cooling effects at the burst as was discussed. So I did follow that approach initially. However, I do accept the criticism and I acknowledge that that back of the envelope calculation could and should be improved.

CHAIRMAN YOUNG: Do you have an idea to what extent or what difference the cooling effects would make?

THE WITNESS: Well, just taking Framatone's own, you know, estimates into account, I've been basing my revised calculation on the value of 1841 degrees Fahrenheit which appeared in Table 35 of the LAR. This was, after all, the calculation that was referenced in the staff safety evaluation and also referred to in their testimony as establishing the LOCA limits for the MOX fuel assemblies. We've heard this morning that Duke does not believe that calculation does establish the LOCA limits that the rupture temperature at the -- or the peak temperature at the rupture location that their maximum would only be 1750 degrees Fahrenheit.

However if for the sake of argument we use 1841 degrees Farhenheit, if you consider an impact of 313 degrees Farhenheit on that first rupture temperature, you are -- you may still be in compliance with the peak cladding temperature limit but you are going to seriously challenge the ability of the fuel to satisfy the maximum clad oxidation limit because the rate of oxidation is so much greater at that higher temperature and so that my current area of concern is the ability MOX fuel -- of the LTAs to comply with the peak clad oxidation limit in 50.46 at the rupture location if relocation is taken into account.

BY MS. CURRAN:

Q Along that line, do you -- have you identified further studies that support your concern?

A Well, Argonne National Laboratory has done recent studies of oxidation of M5 cladding at high temperature, at 1,000 degrees Celsius, 1100 degrees Celcius and 1200 degrees Celcius and I guess I'd like to introduce those papers into evidence.

MR. REPKA: You know, this is now the third or fourth paper that BREDL is introducing into evidence in this proceeding that were not identified in the original testimony, were not identified in the rebuttal testimony, were not identified in any response to discovery and while we're certainly willing to look at it and haven't objected to the others, we do have a concern with the timing of how this is being presented.

CHAIRMAN YOUNG: And I guess I would say there was a responsibility to supplement discovery, so I think the better practice would have been to provide these as soon as you had them. With that said, and I recognize your concern and I agree it's better not to -- it's like a judge I used to clerk for used to say, we stopped doing trial by ambush a long time ago. With that said, are you going to object?

MR. REPKA: In this case, I will object. I think that we have to draw the line somewhere and again, as with the other reports I certainly would like the opportunity to come back later today and to the extent we can digest these reports, respond to them to some degree.

MS. UTTAL: Your Honor, I have an objection also. It's a little different. These two reports have to do with embrittlement. I don't believe embrittlement was raised as an issue in the contentions that form the scope of this proceeding and I would object to the content of these articles being beyond the scope of this proceeding.

CHAIRMAN YOUNG: Let's deal with the two objections separately, but as I understand it, they're being offered because they apparently contain something on maximum clad oxidation, some limit, something relevant to that. Did I understand correctly?

THE WITNESS: Yes, we're not proposing any of the data referring to embrittlement studies on these. They're not what we're putting forward. It's just the calculation of clad oxidation of the unerradiated cladding.

CHAIRMAN YOUNG: When did you find these?

MS. CURRAN: We'd have to ask Dr. Lyman.

THE WITNESS: Within the last two days.

MS. CURRAN: And I just would like to add that we have -- we provided Duke and the staff with everything that we had that we planned to use when we had it. We didn't hold anything back to ambush them. Dr. Lyman --

CHAIRMAN YOUNG: Well, wouldn't it have been better to give it to them this morning or yesterday?

MS. CURRAN: That I concede.

MS. UTTAL: Your Honor, I also --

CHAIRMAN YOUNG: Especially since we had time yesterday.

MS. UTTAL: I also object on the same grounds that Mr. Repka objected on, the fact that they are being given to us, again, at this late date.

CHAIRMAN YOUNG: We'll be right back.

(Off the record.)

CHAIRMAN YOUNG: Right before we went back on the record the last time, I was just asking how many more exhibits there were. And you indicated you had several. And you indicated you had one.

MR. REPKA: It was a potential.

CHAIRMAN YOUNG: Okay. And I was just in the process of saying let's just exchange them with each other at that point. And then everyone came in. And so we went ahead and went on the record without doing that. How many more do you have Ms. Curran?

MS. CURRAN: I have one more.

CHAIRMAN YOUNG: One more? As I said before, we don't look kindly on not supplementing the record, on not providing everything you have as soon as you have it so that we can proceed more efficiently.

And I've taken this position before. On the other hand, we are reticent to completely ignore evidence that's been put before us, given the possibility that it may be something relevant that we should consider as a part of the complete record on the issues before us.

Certainly the embriddlement issues are not before us. The only issue that this would be relevant for that we would give it any weight, would be as it relates to the maximum clad oxidation limit issue.

We, at this point, will take under advisement your objections to these exhibits. We'll go ahead and take the testimony. We will consider your objections to be all the testimony related to this.

And we'll allow both the staff in Duke to respond to it in whatever manner is most appropriate in your estimation after you've had a chance to look at it and consider it.

And then we will consider whatever arguments you may wish to provide on how relevant it is in terms of how much weight we should give that in our ultimate decision, and on how much weight we should give the evidence period, should we ultimately admit it.

And I think we would also like to go ahead and get out on the table any further exhibits at this point so that there won't be any more surprises.

MS. CURRAN: I only have one.

MS. UTTAL: Judge, I have one more thing. If the Board determines to accept these two exhibits, I would ask that they be redacted and only admitted and redacted that so that the only information in these exhibits is the information relating to the oxidation issue.

CHAIRMAN YOUNG: Do you have any objection to that Ms. Curran?

MS. CURRAN: No. But it seems to me that the Board can read the relevant portions of the exhibit. I don't know why it needs to be redacted. We're not relying on the irrelevant portion.

CHAIRMAN YOUNG: Well, we can say we won't consider them. Mr. Repka, since I asked Ms. Curran and Dr. Lemon when did you obtain this document?

DR. LYMAN: I got it the day before I left, which was --

MR. REPKA: Yes, just this week. And that's a document that his staff, I think, introduced an excerpt of it. And this is the rest of the document. And we have not, even at this point, decided whether we are going to introduce it.

CHAIRMAN YOUNG: Okay.

MR. REPKA: It's one of the Staff's rebuttal exhibits. I believe it was C. So whatever number that became was an excerpt from that particular report.

CHAIRMAN YOUNG: Okay. With regard to all these documents, I mean, obviously there is some leeway on rebuttal documents. On the other hand, if you have something you know you may well use, under the duty to supplement it, it's always better to share those, especially if you want to avoid this type of objection.

Given the significance of the issues before us and our desire not to ignore any issues that may prove to be significant, we're not going to sustain the objection.

We'll take it under advisement. And we'll allow further substantive response and arguments on this in your proposed findings of fact and conclusions of law. Those are two separate things.

MR. REPKA: Okay. And I need to correct myself. This document apparently not a piece of what the Staff had introduced.

CHAIRMAN YOUNG: Okay.

MR. REPKA: So, I'm passing it at now, because I think it's something we may consider in connection with addressing these other documents. But it's not something at this particular moment we're offering.

CHAIRMAN YOUNG: Okay. Let's see, we've got numbers for -- let's mark these for identification as Exhibits A, B, C, and D at this point with the March 23rd, 2004 -- well, let's start with the January -- January 31st, 2004 article Post-Quench Ductility

Results for -- and I won't read all of it -- for identification as Exhibit A.

(Whereupon, the above referenced to document was marked as Exhibit A for identification.)

CHAIRMAN YOUNG: The March 23rd, 2004 Post-Quench Ductility results paper for identification as Exhibit B.

(Whereupon, the above referenced to document was marked as Exhibit B for identification.)

CHAIRMAN YOUNG: The NEANSC.2003 12 for identification as Exhibit C.

(Whereupon, the above referenced to document was marked as Exhibit C for identification.)

CHAIRMAN YOUNG: The Duke's Exhibit results of the FR-2 test on LWR fuel rod behavior for identification as Exhibit D.

(Whereupon, the above referenced to document was marked as Exhibit D for identification.)

CHAIRMAN YOUNG: And mark it in the taken under advisement file. So, let's go ahead and all the testimony relating to these will be in the same category.

MS. CURRAN: Okay. Dr. Lyman, would you just please explain the relevance of these two documents, the two YAN documents?

DR. LYMAN: Yes. We only offered these because they provide concise tables, show the measured oxidation of M5 cladding at 1,100 and 1,200 degrees Celsius.

In the document which is A, it's the oxidized at 1,100. Is that A?

MS. CURRAN: Yes.

DR. LYMAN: Okay. The relevant pages are simply three and four. And, just to demonstrate the much faster, the much greater rate of oxidation that 1,200 degrees Celsius in M5 cladding than at 1,000, it turns out this unexpected.

In fact, the experiments just confirmed the correlations which are already in common use. So, these documents aren't actually -- all they do is provide the latest confirmation of those correlations.

But they do show, if you compare table 2 on page four, document, A -- which is the oxidation of M5 and 1,000 degrees Celsius -- with table four on page 10 of Exhibit B, we find that the amount of time it takes to reach 13 percent equivalent clad reacted under the CATHARE hovel correlation, which corresponds to 17 percent under the Baker-Just Correlation.

At 1,000 degrees Celsius it takes M5 almost something like 3,000 seconds to reach an equivalent clad reactive of 13. And this is just a rough interpellation.

On the other hand, that you can see from Exhibit A. From Exhibit B, again, table four on page ten, the top line, you see it takes only 166 seconds for M5 to reach 13 percent equivalent clad reactive.

So, therefore the rate of oxidation is much more rapid, and illustrates why a difference of 1,841 degrees Fahrenheit for the temp of the burst rupture location and 1,841 plus 313, which is 2,154 degrees Fahrenheit, which is nearly 1,200 degrees Celsius makes a big difference.

And so that's why the impact of relocation oxidation -- and if evaluated correctly -- could provide a significant challenge to the peak clad oxidation of the MOX-LTAs.

So that's why that temperature variation, even if it's only the non-limited PCT location, the challenge the P clad oxidation limit or the burst limit.

And that's why I'm concerned about it.

MS. CURRAN: Okay. I think we heard this morning that fuel relocation was considered by Westinghouse in its analysis of the limiting PCT case for co-resident LEU fuel.

Knowing that, do you believe the comparison of PCTs from MOX fuel and LEU fuel that are found in table 3-6 of the LAR, which is Duke's Exhibit 1 is valid?

DR. LYMAN: No. I do not believe that the comparisons in table 3-6 are valid, because we are comparing a result of a deterministic calculation of P clad temperature and maximum oxidation for the MOX-LTA using a model that does not take fuel relocation into account with the 95th percentile P clad temperature for the co-resident LEU fuel using a Westinghouse best estimate calculation that does take fuel relocation into account.

Since fuel relocation could well increase P clad temperature in P clad oxidation, if it were taken into account in the MOX deterministic calculation, that would clearly affect the comparison with the resident LEU fuel, which already takes relocation into account.

I'd further like to add that the fact that the LOCA analysis of record for the Catawba core, which is the Westinghouse best estimate model, the fact that that actually indeed takes fuel relocation affects into account was very surprising to us when I discovered that yesterday morning.

And both Duke and the Staff had been asserting throughout this entire proceeding that fuel relocation does not import an effect for either LEU or for MOX fuel, and would not be accounted for.

The fact that they actually do account for it in their LEU fuel calculation but not in their MOX fuel calculation only increases our concern that there is a bias and a non-conservatism associated with the MOX fuel evaluation in comparison to the LEU fuel evaluation.

MS. CURRAN: Thank you. In paragraph 42 of their rebuttal testimony, Dr. McCoy and Dunn say that the connection you make between PCMI performance and pellet clad bonding is speculation because the document that you site, Exhibit J, -- it's also Exhibit 34 -- does not refer to pellet clad bonding as a possible explanation for the greater resistance of

MOX fuel to clad failures due to PCMI, do you agree?

DR. LYMAN: No, I don't agree. I offered the information about the experiments. We observed the tendency of MOX fuel to be more resistant to clad failure as a result of PCMI simply as a suggestion that this may be related to fuel clad bonding issues that we did raise earlier in this proceeding.

The exhibit that we presented, that Ms. Curran just read from, indicates that even in the most recent discussion of this issues there is some uncertainty as to the origin or some uncertainty as to why MOX fuel is better with respect to or more resistant to clad failure as a result of pellet clad mechanical interaction.

It seems that there's not a technical consensus that the phenomena is completely understood. So I simply offered the fact that this observation may be related to fuel clad bonding in MOX pellets as simply that, an observation.

The exhibit that we've just distributed, which I guess is Exhibit C --

MS. CURRAN: Yes.

DR. LYMAN: -- is a document which offers a little more clarification of the length between pellet clad interaction and pellet clad mechanical interaction. That would be found on page 17 of Exhibit C.

MS. CURRAN: Could you just identify that document for us?

DR. LYMAN: Yes. This document is entitled Status of NSC activities in the field of fuel behavior, dated May 2003. On page 17 of that document you will find the discussion of pellet clad chemical interaction, which is a chemical bond forming between the internally oxidized cladding and the fuel.

I read this discussion to mean that there is a belief that the pellet clad chemical interaction may have an impact on pellet clad mechanical interaction, especially from a sentence in the paragraph entitled pellet cladding chemical interaction that says, "thus a full understanding" -- well, I'll start from here.

"For weak bonding and soft mechanical properties, the bonding layer can act as a lubricant, thus minimizing stress and strain concentrations in the cladding."

"Thus a full understanding of this bonding layer is necessary to predict the likely outcome of fuel swelling and pellet expansion in transients." So I interpret this to mean that a full understanding of why MOX fuel is more resistant to pellet clad mechanical failure during transient could have some relation to the pellet clad chemical interaction.

And, therefore, this difference, if it is attributed to pellet clad chemical interaction, could have an impact on the difference between MOX and LEU pellet clad interaction and the behavior in LOCAs, which is another type of accident, I understand.

But, it's simply that connection that MOX fuel and LEU fuel may have a pellet clad bond of a different nature. And that is exhibited in MOX fuel's better resistance to pellet clad mechanical interactions.

This is just a hypothesis. But that's the point I'd like to make. And I think this document strengthens the argument that there may be an impact of the chemical bond on mechanical interaction.

CHAIRMAN YOUNG: Can I ask you to back up for just one minute? What portion of Exhibit 1 was it that you were referring to in your earlier discussion about LEU versus MOX and your discussion right before that?

I think you were saying that Duke took into account -- was it fuel relocation? I was not listening as well as I should have because I was looking for the Exhibit.

DR. LYMAN: I'm sorry.

MS. CURRAN: It was the table, table 3-6, I believe. That was what I asked Dr. Lyman about.

DR. LYMAN: I'm sorry. Right, of the license amendment request, which is Exhibit 1. I'm sorry. Shall I go over that exhibit?

CHAIRMAN YOUNG: If you don't mind. I'm sorry. I was trying to find the Exhibit.

DR. LYMAN: Right. Table 3-6 compares the result of the deterministic or one of the deterministic appendix K calculations that Duke conducted for the MOX fuel lead assembly.

And it compared that to the 95^{th} percentile results for the resident low in rich uranium assembly, and showed that there was a difference that the MOX fuel P clad temperature is actually bounded by the resident fuel temperature.

CHAIRMAN YOUNG: Oh, so the difference was the different methods of analysis?

DR. LYMAN: Right. And the fact that the Staff even, in its safety evaluation, used that table implicitly in a discussion of why MOX fuel was bounded by LEU fuel.

If your read the Staff's safety evaluation in the section on loss of coolant accidents, I believe it's implicitly assumed that that table has some bearing on the notion that MOX fuel PCT and the clad oxidation will be bounded by the resident fuel calculation.

But, what we learned this morning in Duke's cross examination and what's also clear from the email which we offered as Exhibit --

MS. CURRAN: Exhibit 53.

DR. LYMAN: -- Exhibit 53, is that the Westinghouse large break LOCA best estimate model considers fuel relocation effects. So that table actually compares a MOX calculation, but did not consider fuel relocation with an LEU calculation that did.

Therefore, that underestimates the potential PCT and clad oxidation of the values of the MOX relative to the LEU column.

JUDGE BARATTA: I guess I'm a little confused by your argument there. I was going to save this for later. But I think I'd like to interject it now because, in comparing the two analyses in that table, one is basically appendix K analysis, see if there's a best estimate in accordance with the new appendix K.

And we heard earlier, I believe, that the approach taken in the old appendix K, which you called deterministic, -- excuse me, which has been referred to by others as well, as deterministic -- was viewed as conservative and did not account for a number of factors, including fuel relocation.

But, that was taken care of because of many of the conservative approximations, which are not included in the best estimate calculations, on being the one that I pointed out that Duke at omitted from the list of conservatisms in the calculation, which is the so-called heat transfer lock out feature that's in appendix K.

Now, looking at that table, and using this rule of thumb that a 50 degree difference probably isn't significant, I might -- based on that -- conclude that really, they are about the same.

In other words, what was said earlier that appendix K accounts for fuel relocation because of the fact that it's got a lot of conservatives in it -- not because it accounts for it specifically -- and the fact that the resident analysis accounts for fuel relocation explicitly, and yet the results come out about the same.

It says that the assumption that it

doesn't have to be counter for an appendix K is reasonable provided -- and this is the point that -- this is really my question for you -- provided the effect and MOX is comparable to that LEU.

And you've alluded to the fact that it might not be. And I want to really spend a little bit of time, if I could, on that specific issue. In other words, the contention as admitted really deals with the question that there's some sort of fundamental differences between the two that leads one to improperly or -- maybe not improperly, but leads one -- the failure to account for these differences, one of which is this filling fraction that occurs when you have this a fracture that occurs during a normal operation and then, of course, when you have a LOCA on the upper bursting that fills in that region.

Could I ask you to explain, based upon what we've seen, and the evidence you've presented in your testimony, what is it that causes that? You've looked at some photo marked graphs of the fuel.

And you've seen some tearing and things like that, etcetera. Could you expand on that?

DR. LYMAN: And, to take you on what would be the physical mechanism that might account for greater generation of --

JUDGE BARATTA: Right. Not only that, but what you think, based upon the RSN data and other data as to what is not only the difference in the physical mechanism, but the differences in the -- I guess ultimately it's the filling fracture.

DR. LYMAN: Right.

JUDGE BARATTA: In other words, is it closer to .9 or .7 or something like that?

DR. LYMAN: Right. Well, you know, I'm curious about what that would be as well. I just don't think we know. I mean, there is test data on relocation that's provided the little bit of information that we have.

The FR-2 test for example, gave some indication of filling fraction for uranium fuel. So we have some limited amount of LEU data. We have some suggestion that MOX fuel, because of the presence of plutonium conglomerates, the greater volume of fuel, may experience these high burn up changes that could generate fine particles in a LOCA.

We have that as a suggestion. But, as far as a quantitative estimate, it's just not something I think that can be done analytically. So, all we have is a strong evidence that the relocation affects the filling fraction could be higher for MOX fuel if there's a greater generation of fragments, and those fragments are the medium distribution and the smaller, leading to a greater filling fraction.

And that, presumably, because of the extreme sensitivity of the PCT and clad oxidation on filling fraction was demonstrated by RSN. It's pretty strongly dependent on that value.

So, putting those together, there's a reasonable -- I think it's reasonable to believe that the effect could be stronger. But I find it frustrating if there's an absence of experimental data that could help to resolve that fact.

JUDGE BARATTA: The next question I have is that we've heard that -- well, we haven't been given the specific value because it apparently is thought to be -- we've heard that the filling fractions used in the Westinghouse -- calculation is an input value and that it's something on the order of .5.

And the test data that has been -- the two figures that were admitted as exhibits -- the one figure, I guess figure 11 -- I forget what the exhibit designation is of that -- that it's like point something, .65 or .61 on that order.

Do you consider that a significant

difference based on your knowledge so far?

DR. LYMAN: Between 50 percent and 61.5 percent?

JUDGE BARATTA: Right.

DR. LYMAN: Well, the only calculations have before me are the ISRM ones. And they do look at three ranges from 40 degrees -- 40, 61.5, and 70. And there is between 40 and 70 quite a large span in the impact on P cladding temperature, probably a couple hundred degrees Fahrenheit.

JUDGE BARATTA: And that's largely associated with the resulting -- the larger amount of heat that's being generated as opposed to any other.

DR. LYMAN: Right. I mean, what happens in the relocation is you have a balloon which can be significantly greater than the original pellet diameter.

Then you have rubble which fills that balloon. The density of the rubble is less than the original pellet density. But the total cross sectional area is greater, maybe four times greater.

So, clearly the greater packing density or filling fraction of the rubble, the more decay you are going to have in that section. And so, that's the reason for the dependence on the filling fraction.

JUDGE BARATTA: Okay. And you're not concerned about the ballooning being greater in MOX versus LEU because I believe we heard that -- in -- testimony -- that Duke used a factor of .9, which appears to be well beyond any --

DR. LYMAN: Well, I think you are actually putting two things. The impact on ballooning on fuel relocation would be that the larger the balloon obviously the more space there is that fuel to fall.

JUDGE BARATTA: Right.

DR. LYMAN: The .9 figure that Duke was using had to do with their evaluation of what's the maximum blockage that would still leave the --

JUDGE BARATTA: The present --

DR. LYMAN: The coolant, right.

JUDGE BARATTA: The flow area.

DR. LYMAN: I think, in this context, what's relevant is Duke's claim that it uses the properties of eradiated and five in estimating the balloon size, and that that's bounding because it's most ductile when it's eradiated.

But, the fact is that, because they haven't considered relocation in their MOX deterministic model, -- well, let me put it another way. We'd like to see a deterministic analysis done with an uncertainty estimate based on the parameters that Duke uses based on its default parameters, including the M5 balloon size that they calculate. We'd like to see that calculation done.

CHAIRMAN YOUNG: Say that over again.

JUDGE BARATTA: Yes, could you say that over again? I didn't quite catch it.

DR. LYMAN: I'm sorry. We think that Duke needs to bound the uncertainty associated with fuel relocation in the MOX calculation. And to do that it should use all the relevant parameters, including the M5 balloon size, the maximum balloon size that's credible.

It should look at a range of filling fractions and estimate at least the uncertainty on the 50-46 parameters associated with considering relocation.

The fact that we don't know how much finer the MOX fuel fragments would be, how much more fine material there would be, what the filling ration difference would be for MOX fuel and LEU fuel.

That would have to constitute an uncertainty in the calculation. And that uncertainty would be clearly greater than 50 degrees Fahrenheit since -- at least from the IRSN calculation -- a variation from a filling fraction of 40 percent to 70 percent is much greater than 50 degrees Fahrenheit.

So we believe that is a significant uncertainty that should be evaluated.

CHAIRMAN YOUNG: Just let me interject. When you say you want them to do that, is that -- to what degree would that satisfy your concerns if they did that?

DR. LYMAN: Well, at least that would put a bound on the uncertainty. But I suspect that that would be an unacceptable level of uncertainty that can only be narrowed to an acceptable level through the acquisition of more experimental data.

Since the analysis hasn't been done, I don't know if that's the case. But I think there's strong reason to believe that that uncertainty would be unacceptable.

JUDGE BARATTA: Let's explore this a little further. How would you propose -- for example, I guess getting down to the nitty gritty, there are basically three codes that are talked to.

One is the Framatome, the best estimate methodology, which currently is not -- the fuel relocation model is not recognized by the NRC, but is in the process of being, I guess, reviewed.

It's in some capacity we've heard. You have the best estimate open methodology that Westinghouse developed, the so-called COBRA/TRAC, which does have an accepted fuel relocation model in it.

Although, the amount of relocation is determined by the analyst and not by the code, at least based on what we've heard. And then you have the Framatome ANP old appendix K methodology, which is what was used for the LEU test assembly.

So, how would you propose one would do your sensitivity analysis?

DR. LYMAN: Well, given that the Westinghouse code does have the fuel relocation model in it, it would seem to be a logical choice. But that's not the only difference between them and deterministic model, as we've discussed.

And using that may raise more questions than it answers. I mean, we believe that appendix K is basically suitable for use for MOX fuel assemblies if you identify those features that are not modeled in appendix K that could have a large impact on the MOX fuel analysis.

One is relocation. The other is much smaller, but not a negligible degree. It's the difference in decay heat. Modification -- and, gain, since these are proprietary codes, and I don't have any facility with how they're used or in understanding their basis, it would be hard for me to judge how easy it would be to modify the deterministic code to add a fuel relocation model.

Although, looking at what IRSN has done, their process is much more similar to that. I think they use a deterministic code. And they put in a fuel relocation model with a variable filling fraction, other parameters.

That would really be a question, I think, for Duke. If they were to do that, what would be the most efficient way.

JUDGE BARATTA: Admittedly, since the Westinghouse COBRA/TRAC models are different than the appendix K Framatome ANP code, would there be any benefit from -- I mean, what you're asking for is to get a feel for, from what I understand you're saying, a feel for the effect of filling fraction and balloon size.

And I'm not referring to the flow block, more to the heat transfers within the fuel region itself, due to different filling fractions. Would it be a benefit to do that type of calculation recognizing that there are other substandard differences between the Framatome code and the Westinghouse code, at least in order to get this feel for the effect?

DR. LYMAN: I guess, because these codes are proprietary, and I really don't -- you know, I'm just not privvied to -- I just have no ability to judge how easily they could be modified.

I really feel like I'm not in a position to judge what the best way would be.

JUDGE BARATTA: Okay.

DR. LYMAN: It seems like it would be more straightforward to modify the deterministic code if you only want to look at the effect of fuel relocation on . that calculation, that might be the most straightforward way.

But, it's really a question for those entities that actually run those codes. In other words, Framatome or Westinghouse.

JUDGE BARATTA: Now, refresh my memory if you could. The ISRN or IPSN calculations were done with which code?

DR. LYMAN: It's, I guess, a CATHARE2. The specifications are described to some extent in our exhibit.

JUDGE BARATTA: And their calculations account --

CHAIRMAN YOUNG: Wait, which exhibit?

JUDGE BARATTA: Our Exhibit E, which is --

MS. CURRAN: Exhibit E.

CHAIRMAN YOUNG: Exhibit E or B did you say?

JUDGE BARATTA: E.

CHAIRMAN YOUNG: E?

MS. CURRAN: And 29.

JUDGE BARATTA: I could just read from that, the introduction to that exhibit. "In a preliminary step, in view of obtaining some insight into the fuel rod performance following fuel relocation and the PWR high burn up UO-2 rod under LOCA, calculations were being performed using the French CATHARE2 code with specific modifications in the fuel routine so as to describe the fuel accumulation after burst in the ruptured mesh of the broad cladding."

So it sounds like they took a code which did not have relocation and modified to account for it.

JUDGE BARATTA: Is there any more description of it?

DR. LYMAN: CATHARE? I believe so.

JUDGE BARATTA: Well, I was talking about their modification to be specific.

DR. LYMAN: Oh. You know, there's some detail. You could sit down and do it from scratch based on the description here. You know, they describe they methodology to some extent.

JUDGE BARATTA: Let me ask some additional questions -- ask later for clarification. But it's related somewhat to this, not pertaining to the sensitivity side, but taking a slightly different approach.

Again, this goes back to those two figures, 10 and 11, that are in there. And it does bear on what was done with the CATHARE code. Because I believe the CATHARE code -- if I understood correctly what was said earlier -- only took into account the detrimental aspects of fuel relocation in that a higher heat generation rate, but did not take into account the model they put in there, did not take into account the beneficial effects that some people have alluded to, which are the cooling due to the turbulence of the burst region and such. Is that correct?

DR. LYMAN: That is correct.

JUDGE BARATTA: All right. Now, going back to those figures, I thought I heard this morning some indication that in the experiment that was described in figure ten, which is, I believe, Exhibit 15, that that did not have fuel relocation in it.

So, it should, in principle, have the beneficial effects of the increased turbulence, which tends to enhance heat transfer we heard.

DR. LYMAN: Right.

JUDGE BARATTA: Whereas, Exhibit 16, which is figure 11, that had the bolt effects in there, including the fuel relocation. And I think we also heard that the approximate difference of the heat was on the order of about 300 degrees between those two.

DR. LYMAN: Well, actually, since those two were -- as Duke pointed out today -- those two graphs actually came from different runs. And there's a different initial rod power. So they can't be directly compared that way.

JUDGE BARATTA: Okay. Well, I guess I wonder, the magnitudes, wouldn't they be determined by the rod power, but the differences would be determined more by the relative local powers?

DR. LYMAN: The difference between figures 10 and 11?

JUDGE BARATTA: Now, within a figure.

DR. LYMAN: Yes. Between the burst location and the P clad temperature. Yes. I mean, that -- in the case with no relocation, the benefit of cooling was apparent from the fact that the rupture location was considerably lower temperature than the P clad temperature.

In the case where there was relocation, those two curves did not diverge nearly as much. And I don't know what the explanation for that is. But that's an observation.

JUDGE BARATTA: I'm trying to determine is it possible, I mean, it could just be coincidence, that on the IRSN/IPSN calculations you have this 300 degrees.

DR. LYMAN: Right.

JUDGE BARATTA: And there's also about a 300 degree difference in --

DR. LYMAN: Well, it turns out to be -- it is coincidental in the extent because they had different initial conditions they can't be directly compared.

But I think the order of magnitude is -- that does give a sense of the order of magnitude associated with relocation. There may be some, even though they can't be directly compared.

That does give an idea of -- then again, it's hard to say without.

JUDGE BARATTA: Okay, let me -- I understood. We're talking order of magnitude numbers as opposed to a delta TCP associated with relocations.

DR. LYMAN: I mean, let's look at it this way. In figure eleven the P clad temperature at the unruptured note is about 100 degrees Fahrenheit greater than it is.

In figure 11 it is about 100 degrees greater than it is in figure ten. So, to the extent that reflects the difference in initial conditions and rod power, there is a great difference between the temperature at the ruptured location from the no-relocation case and the one where there is relocation.

And that's a much greater difference than the difference in P clad temperature between the two curves. So you might say that -- and, again, without actually going through trying to validate those data or understand them, you know, in connection with a simulation, it's hard to say.

I think it might say that the greater impact on the rupture location clad temperature may be related to the fuel relocation and not just the greater power of that experiment.

JUDGE BARATTA: Okay. Now, that suggests that the impact of the fuel relocation is on the order of a couple of hundred -- and I say, it could be anywhere from 200 to 400, as apposed to literally 200.

And you had said earlier that the 75 degrees associated with using the LEU decay heat curve with appropriate multiplier, as opposed to the MOX decay heat curve, was worth about 75 degrees.

DR. LYMAN: Up to 75.

JUDGE BARATTA: Up to 74, okay. So, obviously, it doesn't compensate based upon the data that would appear to be one could draw from that at as well as the IPSN.

On the other hand, I believe we heard earlier today that the Duke had determined that it was appropriate to use more limiting factors for the MOX fuel locations and their core design than appeared in the original analysis that was done and put into the LAR in that table 3-5, which is what we talked about a few minutes. I think that's correct.

DR. LYMAN: That's what Duke did say. But we have some question about --

JUDGE BARATTA: I know you question they didn't maximize it to see what it would take to get to the point of --

DR. LYMAN: Right, because they didn't seek out to look for the most limiting burst rupture temperature. Then they may not have found the case when it occurred.

JUDGE BARATTA: Okay. But, in the cases they did find, it would appear on the surface, anyway, that if you added three or 400 degrees to those, you would still be below the 2,200. Is that --

DR. LYMAN: Yes. It would be below the 2,200. But the impact on the oxidation rate would be substantial on that. That would take more work, I think. To assess its affect in relation to the limit on P clad oxidation.

JUDGE BARATTA: Yes. Because, looking at appendix in the regulations, there's more than just simply evaluating it from the outside. You have to do it from the inside and the outside. Is that not the case?

DR. LYMAN: It's not in the regulation. But it's in the regulatory guidance that you have to consider double sided oxidation as well as the initial, not only the transient oxidation, but the initial oxidation.

JUDGE BARATTA: Okay. Thank you. I

apologize for taking so much time.

DR. LYMAN: No, please.

CHAIRMAN YOUNG: Actually, I was just about to ask a question to clarify earlier. And let me do that while we're still on here. Actually two areas, pardon me.

First, on this table 3-6, the different types of analysis are used. And Duke testified that the conservatisms in the deterministic part K method compensate for not including the fuel relocation calculations in that.

In looking at all this, I guess one of the things I'm sort of thinking, and I want you to tell me, one, if the way I'm thinking you think it's on the wrong track, and two, I just wanted to let you know what kinds of information that would be helpful to me.

If you had a chart, if you had all the conservatisms identified, and maybe give ranges for how much they would affect the ultimate calculations, and then you looked at all the various benefits and did the same kind of ranges to the extent possible, and then put on the other side -- what I understand at this point, your concern.

Did I understand you correctly to concede that the P clad temperature is not a concern at this point? It's mainly the maximum clad oxidation?

DR. LYMAN: No, I didn't concede that. I mean, in the kinds of back of the envelope calculations that I've been doing as part of this proceeding, I certainly wouldn't concede that there aren't' circumstances where fuel relocation may challenge the P clad temperature.

We don't know what the maximum impact of fuel relocation for the MOX LTAs would be, because we don't know what the maximum filling fractions are. We don't know if -- primarily that's our main concern.

So, I certainly wouldn't concede that. These back of the envelope calculations are suggestions, but they are not substitutes for running the codes that are actually should be used for establishing compliance or non-compliance.

CHAIRMAN YOUNG: Okay. So, in my mind at this point I'm thinking on the one had of sort of somehow pulling together all the evidence, the testimony, and the exhibits that indicate the conservatism -- that describe conservatisms in how much they may be responsible for benefits and any other factors that would be favorable to Duke's point of view.

And, on the other side, I'm looking at any information that has been suggested about how fuel relocation might go against Duke's argument that there's no significant affect on P clad temperature or, I gather, the maximum clad oxidation limit, which are the two main things that we're looking at in this case about the affects of fuel behavior.

And let me just finish up. I want to ask you how that fits and how you understand and how I'm understanding you on these issues. In another part of my mind, based on what you've said in response to the -- you would like for Duke to do an uncertainty analysis taking into account the M5 cladding, the filling fractions, arrange those, the uncertainties of particular section 50.46 parameters, and come up with figures that would relate both to P clad temperature and to max clad oxidation limit, and see what results that produces.

There, that's sort of what I'm thinking at this point, having heard the evidence to this point. Does that make sense? And, am I understanding you correctly in what you want?

DR. LYMAN: Yes, for the most part. We did say that, you know, we'd like to get a better understanding of the potential impact of relocation on MOX fuel.

However, at this point, given the lack of experimental data on MOX fuel and the relocation conditions, we think that result would ultimately be somewhat unsatisfying.

In other words, there would be an error bar which would be fairly large. And, as we've maintained throughout this entire proceeding, we believe that there's, because of the lack of relevant MOX data compared to the data accumulated for uranium, that it's reasonable to accumulate more data on MOX fuel behavior before deciding whether or not the LTA's will be safe for operations.

So, simply doing the analysis without having the experimental data to limit those uncertainties, I think, may not get us too much further, except to appreciate the potential impact of relocation on the MOX LTA calculation.

CHAIRMAN YOUNG: Is there -- I'm sorry, go ahead and finish.

DR. LYMAN: You know, the other thing I wanted to point is that there is a -- you know, you spoke about having a table of the conservatisms and non-conservatisms in appendix K.

CHAIRMAN YOUNG: Actually, that was going to be my next question. Assuming you could come up with ranges for the various conservatisms and benefits --

DR. LYMAN: Well, we have on actually.

CHAIRMAN YOUNG: Oh.

DR. LYMAN: It's not mine, but it's the Staff's. And this is in our Exhibit C.

MS. CURRAN: Exhibit C is Exhibit 27.

CHAIRMAN YOUNG: Twenty-seven? Hold on a minute.

MS. UTTAL: Judge, can I interrupt for one second? I'm getting quite nervous that we're never going to get to my panel. And I was wondering whether -- we've been talking to Dr. Lyman for just about four hours now -- whether it's possible to put a time limit on it and take a short dinner break.

And then we move on to the Staff's witnesses.

CHAIRMAN YOUNG: After I finish my question I think we can talk about a time.

JUDGE BARATTA: I've got one.

CHAIRMAN YOUNG: But I would like to sort of stay on my train of thought for a minute and --

MS. UTTAL: Thank you.

CHAIRMAN YOUNG: -- find Exhibit 27 first. The bench is not that big, and now we have less room.

Okay, Exhibit 27.

DR. LYMAN: This is the very second to last page, which is a table one, attachment five.

CHAIRMAN YOUNG: The second to last page?

DR. LYMAN: It's page four of attachment five.

CHAIRMAN YOUNG: Okay. So this does represent all the various --

DR. LYMAN: Well, the Staff's assessment. This is a Staff document. It shows the variety of, you know, conservatisms and non-conservatisms in appendix K, and their impact on P clad temperature.

I'm not endorsing this. But, I would say that our approach to the application of appendix and the MOX LTAs would be to identify those effects that may have a significant -- may differentiate significantly between MOX and uranium and augment their appendix K calculation with just those effects, because those are the important ones to acknowledge in a MOX fuel calculation.

CHAIRMAN YOUNG: You're saying add in those of which there is some information at the present time? Whatever information there is at the present time, did I understand you correctly?

DR. LYMAN: No, those effects that may be important in distinguishing between MOX and LEU with respect to look. In other words, you go down this list, some effects deal more with the global thermal hydraulic behavior of the reactor.

And I think would not be significantly impacted by the difference between MOX or LEU. So I don't think those need to be remedied in adapting appendix K for the MOX case.

The one that is important is fuel relocation. That's the most important. And that, as we've hopefully successfully argued, there is a potential that there will be a different MOX and LEU response it would be significant.

So that's on effect from this list that I would say should be considered in adapting appendix K for the MOX LTA case.

CHAIRMAN YOUNG: Well, let me just see if I can get back to the way I'm thinking about this and how you think it may be reasonable and not reasonable. If there is a way to assess ranges for the various uncertainties as well as benefits that Duke and the staff have talked about, would it be possible to compare all that with say a worst case scenario of the possible effects of -- just too sort of take your point of view on it.

Take the worst case scenario with regard to relocation and the filling fractions and the M5 cladding, and all the various things that play into that, and come up with any kind of reasonable assessment of Duke's argument that the conservatisms more than what I think they say is the most pessimistic view of the effects of fuel relocation.

And part of the reason I'm telling you this is because at this point, anyway, unless corrected, this is how I'm starting to think about it. And so, I will be looking through evidence to try to see how reasonable is Duke's argument, and how much have you presented to show what the worst case scenario could be with the understanding that you're wanting more analysis based on actual data.

And then I'll just cut to the chase in terms of what I'm looking at. I asked Duke earlier about your statement in your rebuttal testimony that it would be the end of -- let's see, I've got it.

It would be the end of -- it would likely put an end to the MOX program in the United States. Duke is arguing that this chicken and egg argument, that the lead test assembly program is designed to test a lot of the things that you're wanting them to test in advance of doing the lead test assembly program.

And so, even though consequences as in original contention to are not at issue now, there's this implicit underlying -- especially in your comment -- underlying sort of assumption that we're talking somehow about more than -- maybe I'm wrong.

And anyone can correct me if I'm wrong -- but that we're somehow talking about more than whether there's a chance of reaching the P clad temperature and maximum clad oxidation limits in 50.46, but something more than that somehow as to why going forward with the lead test assemblies, and see what information and enlightenment they provide, is not enough.

Now, I've given you a lot of stuff here partially to take into account the Staff's concern about time, and partially to let you respond to sort of my thinking at this point and how I'm looking at analyzing this subject.

DR. LYMAN: Okay. So let me --

CHAIRMAN YOUNG: By a lot of people, actually.

DR. LYMAN: Let me try to be brief. On your first point, I would restate our view that the Staff -- appendix K has a lot of conservatisms built into it. But the Staff chose -- I mean, Duke chose to use an appendix K analysis and not their best estimate analysis for the MOX LTA LOCA.

So, for better or worse, that is the analysis that has been put forward by DUKE, the appendix K. When we say that fuel relocation, which is not modeled in appendix K, should be for applying appendix K to MOX, we are not saying that every conservatism and non-conservatism is appendix K is open to evaluation.

That's not what we're saying. We are only saying that those aspects which could make appendix K non conservative if applied to MOX should be evaluated.

Because, appendix K as it stands, when it applies to uranium oxide fuel as state din the regulations, there's some history to it. There's some implicit balance between the conservatism and non-conservatism that the Staff has spoken of.

That balance could be affected by transferring that to MOX fuel if there are affects that are unique or more severe to MOX fuel. And so that's why I would say that I agree with your approach.

Except I don't think all the conservatisms in appendix K are open to consideration now unless Duke decides to withdraw the analysis it has and do a best estimate plus estimation of all the uncertainties, in which case fuel relocation, I think, would have to be part of that best estimate as it is in the Westinghouse best estimate model because the impact is clearly greater than 50 degrees Fahrenheit.

So, the uncertainty would have to be including according to the regulation.

CHAIRMAN YOUNG: Let me see if I misunderstood something. You said some of the conservatisms are not open for -- I can't remember the word you used, question or analysis --

DR. LYMAN: Reevaluation.

CHAIRMAN YOUNG: -- reevaluation. But, as I understand Duke's position, it's that even if they are not related in some way to fuel relocation, if they provide more of a margin or cushion, then they would still compensate for any.

DR. LYMAN: But you see, we believe that isn't relevant. Duke did an appendix K calculation. They have to show compliance with 50.46. Any conservatisms they haven't taken credit for in appendix K are simply not relevant in that context.

The only way they could address that if

they chose, would be to do a best estimate analysis and to discard the appendix K analysis they have done. It's simply not open.

Those are not relevant to this proceeding. The conservatisms in appendix K are not relevant in your consideration. What's relevant in your consideration is whether the MOX LTAs -- whether you can have reasonable assurance that the MOX LTAs will not exceed the 50.46 criteria. That is your only consideration.

CHAIRMAN YOUNG: But, aren't they just one part of it, though? Even if you did them, wouldn't that just be one part of the appendix K? And wouldn't that necessarily be considered along with any other parts?

DR. LYMAN: Yes. You see, I don't believe that's true. I believe that the application of appendix K, which is now established for uranium oxide fuel, if that is applied to MOX fuel, that will change the -- could change the balance of conservatisms and non-conservatisms in appendix K, mainly, by not considering fuel relocation effects.

So, again, we're not arguing that appendix K is appropriate or necessary to open up all the conservatisms in appendix K for reconsideration in applying appendix K to MOX.

We're just saying look at those aspects which could have a significant impact on the MOX analysis. And that would be fuel relocation and decay heat.

CHAIRMAN YOUNG: But, anything that Duke has said about the benefits of MOX -- you would --

DR. LYMAN: Okay. That's a different question.

CHAIRMAN YOUNG: That you would consider --

DR. LYMAN: No, that's a different question. That's certainly reasonable. If Duke wanted to take credit for the conservatisms in their appendix K analysis having to do with MOX, I think that's all well and good. And that should be part of it, absolutely.

CHAIRMAN YOUNG: And then on the analysis the way I just summarized the analysis that you wanted done, did I leave anything out of that?

DR. LYMAN: Well, I wouldn't say we were looking at worst case. I don't think that's the best characterization of what we're saying. We want a reasonable assessment of relocation based on the best available experimental data.

Now, quickly in response to the chicken and egg argument --

CHAIRMAN YOUNG: Right, that was --

DR. LYMAN: -- the point I was trying to make here, is I don't think there's a chicken and egg paradox in what we're saying. We believe that, the MOX fuel LTAs, it's not just a minor variant of the population of fuel assemblies that are now approved for use in U.S. reactors.

It's a substantially different fuel with a different microstructure, a different physical material, and for that reason it can't be regarded in the same way as an LTA approval, which is the change of mid-span mix and group position or something like that.

This is a substantial change to the nature of the fuel. And, for that reason, there are test facilities, both in the United States, and abroad where experimental fuels are not only radiated, the are also subject to severe accident and loss of coolant accident, and other accident testing.

CHAIRMAN YOUNG: Prior to lead test assembly?

DR. LYMAN: Prior to lead test assembly, simply because there are questions about the ability of those LTAs to comply with safety criteria with regulatory criteria to present accidents.

And we believe MOX fuel falls into that category. And the fact that there is relatively little on MOX fuel under accident conditions relative to the long experience with uranium fuel that's been acquired over the history of commercial nuclear power in this country, that that leads to an effective discrepancy in the confidence we can have that we know enough about the LTAs to put them in a U.S. reactor.

And the point I tried to make about the loss of coolant accident is simply we don't want -- the goal is to make sure that U.S. commercial reactors are not test reactors.

There are different safety criteria for test reactors. We have test reactors in Idaho in the middle of the desert with a very low populations that are around them.

And that's the advanced test reactor. That's where testing of experimental fuels should be performed in a commercial reactor in a densely populated area, that's one of the fastest growing metropolitan areas in the country.

That's not an appropriate place for a test. So my point was only that, if indeed that is a test, and the test fails, and that there's a loss of coolant accident, and we find out in the course of that test t the MOX fuel assemblies do not conform to the regulatory criteria that they embrittle, that they shatter, that they cause coolant flow blockage, and an unacceptable outcome, that not only would that be an unacceptable outcome from the point of view of safety, but it would be the end to the MOX disposition program that Duke thinks so highly of.

CHAIRMAN YOUNG: I'm sorry, if what occurred?

DR. LYMAN: If there was a loss of coolant accident and we found out that the four MOX LTAs actually, because of relocation effects, exceeded the P clad oxidation limit and embrittled or went during re -- so you end up with shattered fuel assemblies, fuel coolant interactions, and those undesirable consequences of an unmitigated LOCA.

CHAIRMAN YOUNG: I have one more question. And then Judge Baratta has a couple questions. Then we're going to try to break around five. He's my last question.

The last thing you said about the test reactors reminded me of some of Duke's testimony about the early use of MOX fuel in the '60's and their discussion about the different isotopic nature of reactor grade versus weapons grade plutonium.

And I believe that they suggested that there are some benefits because of that different concentration of -- I can't remember which, 240 was it? Anyway, can you respond to that?

DR. LYMAN: So, this question is --

CHAIRMAN YOUNG: Does that make and difference? In other words, you're talking about test reactors. And, as I understand it, there have been some tests of reactor grade MOX fuel an test reactors and to --

DR. LYMAN: Well, but they --

CHAIRMAN YOUNG: -- what extent would that --

DR. LYMAN: If they're what you're of, you mean the MOX fuel loading and I believe in the Saxton, no in Ginna, for example. Is that what you're talking about?

CHAIRMAN YOUNG: Well, my recollection of the testimony is that not that specific. I guess the reason I was asking you that was you're talking about test reactors.

And Duke, early on in through testimony, did mention that there had been previous use of MOX fuel. And then later, in another place, I believe, in their testimony, they did compare reactor grade and weapons grade, I believe, in making an argument that weapons grade had some benefits in terms of the isotopic ratios and the effect that that had on the --

DR. LYMAN: But I feel like they are two different -- So what you're saying is the fact that there was some reactor grade MOX loaded in U.S. reactors in the 60's?

CHAIRMAN YOUNG: What I'm asking is, given that you were saying that this needs to be done in test reactors, and I'm presuming that there was some experience with MOX fuel in test reactors back in the 60's and 70's -- whenever -- and --

DR. LYMAN: Oh, I see.

CHAIRMAN YOUNG: -- given that even though they are not the same, weapons grade and reactor grade --

DR. LYMAN: Right.

CHAIRMAN YOUNG: That there's some comparisons that can be made, isn't some of that information useful?

DR. LYMAN: Oh, I see. Well, there haven't been the kinds of tests where we're calling or we think need to be done, in other words, integral

LOCA tests on high burn up MOX fuel.

Now, the IRSN has proposed a test series that would involve those. There's the Halden reactor in Norway which does conduct some limited testing, looking at things fuel relocation effects.

These facilities are available for exploring these types of effects. I'm not aware of -- there were, you know, tests of radiations of MOX fuel, certainly not only in the 60's, but we've been eradiating MOX fuel in the advanced test reactor weapons grade MOX fuel for several years now.

That fuel, it turns out, that program started based on the belief that Los Alamos National Laboratory would be fabricating lead test assemblies for the MOX program.

It turns out they weren't able to fabricate them. That's why we have to export the fabrication of these assemblies to France. But, our own reactor could have provided an opportunity for producing high burn up fuel.

Then we could test, for example, in Oregon, in our own integral LOCA testing program. That was a missed opportunity because the fuel that's been eradiated in that reactor several years is not prototypic of a fuel we're going to be using in a reactor, not mostly because of bad planning and management by DOE.

So, the fact is that it's not a better planned program would have been able -- would be further along in resolving some of these experimental issues for the context of the MOX disposition process in the United States.

And it's not -- let's say that the experimental data is limited now in resolving those issues could lead to a delay in the loading of lead test assemblies, etcetera, is not necessarily the fault of those who are calling for better information on this program.

CHAIRMAN YOUNG: Well, I wasn't really getting into delay. I was just more getting into the things that are appropriate for test reactors according to what you're saying and the degree to which.

As I understand you, you're saying that the kind of information that you're calling for is the type of information that should be done in the test reactors.

And that's your response to the chicken versus egg argument that Duke made.

DR. LYMAN: Well, and response is that you don't need to use the LTAs. I mean, there's no plan actually to use -- to take the MOX LTAs and use them for LOCA test.

We'd like that. That would be too late for this application. But it could provide valuable information to the next phase of this program. But there's no plan to do that.

However, there is MOX eradiation in Europe of similar but not identical fuel but with similar microstructure, and test facilities that are capable of doing these types of experiments.

So, there's no chicken and egg paradox in that there is fuel available that could provide information for these tests if the will were there.

CHAIRMAN YOUNG: Thank you.

JUDGE BARATTA: Okay. Let me -- hypothetically now, let's assume for the moment that CATHARE2 code is modeled from hydraulic heat transfer models or sufficiently similar to what's used in most of the other computer codes for analysis.

IPSN and ISRN have already done, for lack of a better term, sensitivity analysis of at least the detrimental effect of the fuel relocation as a function of the filling fraction.

That's I think what I heard you say, is

that a correct --

DR. LYMAN: Yes, for a certain scenario.

JUDGE BARATTA: Right. Now, what you haven't -- what hasn't been done is that hasn't been translated into oxidation?

DR. LYMAN: No. Actually ISRN did look at the impact on clad oxidation for that particular scenario. But if you look at -- my argument is that this is something that should be reactor specific.

And, if you look at the actual LOCA scenario that was run by IRSN --

CHAIRMAN YOUNG: Where is that?

DR. LYMAN: That would be in our Exhibit E.

MS. CURRAN: Which is Exhibit 29.

DR. LYMAN: If you look at the graphs --

CHAIRMAN YOUNG: Okay, 29.

DR. LYMAN: The graphs at the end, you see anyway, this is a different LOCA model than Framatome's.

JUDGE BARATTA: The graphs at the --

DR. LYMAN: For instance, look at -- you see the figures at the end?

JUDGE BARATTA: Yes.

DR. LYMAN: Of figure one, for example.

So that shows -- that looks like a considerably different time temperature curve for the clad temperature than for instance, Duke's figure one.

JUDGE BARATTA: Okay. Well, of course, the CATHARE2 code is not clear whether that's in appendix K, the old appendix K or --

DR. LYMAN: Right, exactly.

JUDGE BARATTA: So that alone could account for it.

DR. LYMAN: You see the clad temperature drops pretty fast after about 140 seconds where Duke's doesn't. The slope is much shallower in figure one. So, for that reason alone, the calculation of clad oxidation from ISRN can't be translated directly into Duke's -- imported directly into Duke's analysis.

JUDGE BARATTA: Okay. That would be a fair observation. Now it lead me to go to -- continue to if you mentioned something about the uncertainty. And it seems as though the uncertainty is only associated with the filling fraction, right?

DR. LYMAN: In the ISRN calculation, yes.

JUDGE BARATTA: Right. And if -- let's carry this hypothetical situation a little further. Let's say that if someone were to do a calculation in which that was treated as the only uncertainty, and then they calculated in a manner consistent with what either Duke or Framatone has done, or Westinghouse has done for this core, for the design basis LOCA analysis for Catawba and got a rang of values of delta PCT associated as a function of towing fraction, is that the type of sensitivity study that you have in mind?

DR. LYMAN: Yes. I'd like to see a similar analysis done for the MOX LTAs in this case. The problem is that without -- I think I said this before -- without having better data on the role of impact of MOX characteristics on relocation, it's not going to advance our understanding of whether there is, in fact, a significant difference.

Or that isn't necessarily relevant in the -- well, no, you need to know. You need to have a good idea of what the MOX filling ration -- a credible MOX filling ration is to be able to calculate the impact of relocation on the MOX PCT and clad oxidation.

And that's something no one knows. There's suggestive evidence that it could be greater than the ones that have been encountered for LEU. So, without having more information about that, you more or less replicate IRSM's calculation.

But, it wouldn't necessarily give you

insight into the MOX aspect.

JUDGE BARATTA: If, on the other hand, it turned out that those values were not a linear function of towing fraction, in fact actually somehow saturated at some point, and that that dealt the PCT when -- of two conservative calculation for the PCT, which is still below the 2,200 degrees, then it would at least say, okay, things are okay.

True, we don't understand what the throwing fraction is, it probably might be worthwhile to do that. Is that --

DR. LYMAN: Well, right. If it's saturated at a high value and you had reason to believe the uranium filling fraction were near the upper end, you know, where it starts to saturate.

Then, if MOX had a greater filling ratio, it wouldn't have much of an impact. I agree. But we don't know that.

JUDGE BARATTA: But you agree that a calculation could tell you that as opposed to having to have calculation plus experiment.

DR. LYMAN: Yes.

JUDGE BARATTA: In other words, the factors that go into doing that calculation are sufficiently well known with the exception of that filling fraction. Is that correct?

DR. LYMAN: I mean, you know, that could help to value the uncertainties. I mean, there's value in doing analysis to some extent. And that's a good example.

JUDGE BARATTA: Thank you.

CHAIRMAN YOUNG: Do you want to -- we sort of interrupted you some time ago.

MS. CURRAN: Well, Dr. Lyman has actually answered the rest of the questions that I had.

CHAIRMAN YOUNG: Okay. We could take a break now or we could continue. I don't know how many --

MR. REPKA: I think the Staff has not yet had a chance to examine Dr. Lyman. I don't know if they want to.

JUDGE ELLEMAN: I'd like to defer my questions until after the Staff speaks. Why don't we take a break.

CHAIRMAN YOUNG: Take a break? If there's nobody else who just has quick questions, and the Staff intends to take some time cross examining, then I think it would make sense to take a break now. Yes, Ms. Uttal?

MS. UTTAL: One minute. The Staff doesn't have any questions.

MR. REPKA: I'll ask just a couple quick questions. Then we'll ask. Dr. Lyman, there's some discussion of Exhibit 27, which is the so-called to Thadani Memo in attachment five.

Is there, on that table of estimates of potential delta PCTs, any estimate related to MOX fuel?

DR. LYMAN: No, there isn't.

MR. REPKA: In the course of your discussion earlier, you said that certainly all the conservatisms or non-conservatisms of appendix K aren't in issue in this proceeding.

I happen to agree with you on that. But, you agreed that any conservatism related to decay heat deriving from using LEU figures as opposed MOX figures would be a fair conservatism to take credit for.

DR. LYMAN: Yes, I did.

MR. REPKA: And, would you also agree that any conservatism derived from using the power related to the co-resident LEU fuel relative to the MOX fuel would be a conservatism that could be credited?

DR. LYMAN: Yes. You meant the fact that the MOX fuel being non-limiting?

MR. REPKA: It's non-limiting locations and it's operated at lower power than --

DR. LYMAN: Yes, for this core.

MR. REPKA: Right. Yet it's calculated, for the appendix K, it's assumed to be at the LEU power levels? That's a yes?

DR. LYMAN: Yes.

MR. REPKA: Okay. And, as far as the documents you've put in, and we discussed earlier, I think that the only delta PCT that has been mentioned related to MOX fuel is an 18 degree F number from Oregon, is that correct?

DR. LYMAN: From IRSN.

MR. REPKA: Yes.

DR. LYMAN: Right.

MR. REPKA: At the Oregon Conference.

DR. LYMAN: At the Oregon, yes.

MR. REPKA: Right. And that's the one that didn't consider the power levels, correct?

DR. LYMAN: I think, from what's written on that view graph, it seems to attribute it to greater power level for MOX.

CHAIRMAN YOUNG: Which exhibit are you talking about now?

MR. REPKA: This was BREDL F, which is

essentially Duke 5 or exhibit 30. It was the ISRN calculation presented at Oregon in May, 2004.

CHAIRMAN YOUNG: Thank you, that's all. I just wanted it on the record.

MR. REPKA: That' really all the questions I have right now.

CHAIRMAN YOUNG: All right. It sounds like there's a consensus here for a break at this point. And we'll come back and start with the Staff, Mr. Fernandez?

MR. FERANDEZ: I thought Judge Elleman had questions to ask of Dr. Lyman?

CHAIRMAN YOUNG: Did you?

JUDGE ELLEMAN: I do, but I'd rather give the Staff the opportunity to ask their questions and get any other questions out of the way and then let me him answer my questions.

MS. UTTAL: We don't have any questions Judge Elleman.

JUDGE ELLEMAN: Okay.

CHAIRMAN YOUNG: So, do you want to go ahead with your first?

JUDGE ELLEMAN: Well, I can. Do you want to break now first or do you --

MS. UTTAL: Can we finish with Dr. Lyman?

CHAIRMAN YOUNG: Okay.

MS. UTTAL: And hopefully in just a short while. Because it's going to really be a short time.

JUDGE ELLEMAN: Okay. Dr. Lyman, you've given us a lot of information, both in your exhibits and in the material that was passed out today that relates to relocation of fuel.

But, what is the lesson? What is the message you want us to get out of all of that material?

DR. LYMAN: The fundamental is that Duke used a deterministic appendix K model for calculating the 50.46 acceptance criteria that did not consider fuel relocation in it.

The information we provided supports the potential that MOX fuel may experience more severe fuel relocation effects than uranium fuel. And therefore, by ignoring fuel relocation in that deterministic model, it may become non-conservative when applied to MOX fuel.

So, therefore, the message we'd like to get across is that, in the evaluation of the design basis logo for the MOX LTAs, fuel relocation effects should be considered.

There should be an acknowledgement of the uncertainty associated with the impact of MOX fuel behavior on fuel relocation and on its impact, and on its impact on the 50.46 acceptance criteria, and that there should be a requirement to address some of the experimental uncertainties and try to limit the uncertainties in the analysis, which could be quite considerable.

Fuel relocation is a potentially large effect for MOX fuel. And therefore, it should be accounted for in this analysis to the extent possible where uncertainties exist because of gaps in the experimental database, there needs to be an acknowledgement of that and an attempt to address it.

JUDGE ELLEMAN: I haven't had a chance to look closely at either your exhibits or what you passed out. But, in a cursory examination, it looks to me like most of the data presented is for LEU fuel and that references to MOX fuel are predictions or -- maybe speculations isn't the right word -- but, extensions of the experimental database, which is an LEU database. Is that fair?

DR. LYMAN: Yes, that's fair. And that reflects the inequity in the availability of experimental data from the two types of fuel. Or in a quality.

I'd also like to point out that, because Duke actually does take fuel relocation into account in its LEU analyses, we pointed out, as Duke disclosed earlier.

Therefore, Duke is accounting for fuel relocation as far as the resident fuel. So that does leave the question of what would be the impact on MOX fuel.

Right now the LTA/LAR gives the impression that the resident LEU fuel is bounding. But that, at least partially, may be due to the fact that MOX relocation effects are not accounted for.

And, if they were, it's possible that the MOX LTAs would become bounding with regard to the ECCS criteria.

JUDGE ELLEMAN: If I were to propose that the primary purpose of our panel and this hearing is to examine differences between LEU and MOX fuel, and to focus on safety implications of any differences that appear, would you feel that's a fair statement of our mission, or is that an insufficient statement?

DR. LYMAN: Well, I'd say that wasn't insufficient. I'd say that's incomplete. I mean, I think what's before you is a license amendment request and a LOCA analysis for the MOX LTAs that both Duke and the Staff claim provide adequate or reasonable assurance that the MOX LTAs will comply with NRC's regulatory criteria.

I think your charge is really to examine whether that reasonable assurance standard is met for the MOX LTA LOCA analysis, given the fact that it does not consider relocation effects, that they could be severe for MOX fuel, and that there's inadequate experimental data to support a full understanding of how bad those effects are.

JUDGE ELLEMAN: Let me propose a hypothetical point of new and then follow that with a question and get your impression on the question. You've generated a lot information related to relocation effects and what the possible impact of that might be.

Nothing today forces a reexamination of LEU fuel. The criteria, the requirements, they're out there. And people are generally pleased with them. And so the data are a building, as far as LEU fuel is concerned.

Something does force a consideration of MOX fuel safety. And that's our hearing. And you're saying that, as a part of our hearing, we should take these LEU data and apply them as best they can be extended to MOX and to make calculations or requirements for the MOX fuel that would evaluate the effect of things like fuel displacement or relocation or whatever.

Now, if that were done, aren't we, in effect, holding the MOX hearing hostage? Aren't we using MOX as a surrogate for upgrading the requirements for LEU fuel, but doing it off the record in some way so that it bypasses the regulatory process?

DR. LYMAN: No. I don't agree. That's certainly not what we're proposing. First of all, we know that the LOCA analysis of record for the resident LEU fuel assemblies, the Westinghouse robust fuel assemblies and the Catawba core, that considers fuel relocation effects.

So, what we are calling for is that a consistent treatment be carried out for the MOX LTAs, because relocation is a significant phenomenon. And it's simply not appropriate that the resident LEU fuel assembly analysis considers relocation and the MOX analysis doesn't, considering that there's evidence that MOX relocation could be more severe.

So, you know, we're certainly saying open up appendix K to relocation. That's not what we're saying. We're simply saying that appendix K was a based in the regulations.

It explicitly refers to Uranium oxide pellets. Duke want to take appendix K and use it wholesale to evaluate mixed oxide fuel pellets. In what ways is that application potentially non-conservative?

We've identified one aspect where it could be the case, the lack of consideration in relocation for MOX. We're just talking about the application of appendix K to MOX fuel.

We're not talking about opening appendix K, changing the criteria, mandating relocation. That's not what we're talking about. We're talking about simply applying appendix K to MOX, looking at relevant phenomena that are not modeled in appendix K, but may be important for consideration in the evaluation of the MOX calculation.

And, the fact that Duke itself considers fuel relocation effects for LEU should only strengthen our argument that fuel relocation should be also considered in the MOX LTA.

Otherwise, we don't have a sense of -- we don't have a uniform evaluation. Of course the evaluation isn't uniform because one is a best estimate and the other is deterministic.

But, the important effect that we've identified in relocation, at least as a starting point, should be addressed to improve the consistency of their analysis.

JUDGE ELLEMAN: When I think back to the pre-hearing conference, two issues jump into mind. One was the data you presented from IRSN, which appeared to show slumping of MOX fuel at lower temperatures than occurs with LEU fuels.

Since that time, have you uncovered any experimental evidence or any new insights on those results that you can specifically direct us to in the exhibits and in the papers.

DR. LYMAN: Well, I'm sorry. I'd like to offer a correction. At the pre-hearing conference that observation actually applied to relocation in a different context.

And it was unfortunate that IRSN used the same word for two completely different phenomena.

JUDGE ELLEMAN: Okay.

DR. LYMAN: In that case that was in a severe accident. And the relocation they were referring to was the relocation of the melt to the lower vessel head.

JUDGE ELLEMAN: Okay, so that one did not appear --

DR. LYMAN: Right.

JUDGE ELLEMAN: -- in contention one?

DR. LYMAN: That's right. But, with regard to relocation of the fuel pellets fragments during a LOCA, which is the subject to today's hearing, the information we've offered is what we have found, opinions, all the scientific support we could find for the notion that there may be more severe relocation effects for MOX because of the potential for greater generation of fine grain material, because of the higher local bur ups within the plutonium conglomerates in MOX fuel.

JUDGE ELLEMAN: And that elucidation is contained in your exhibits and the papers passed out that we have today.

DR. LYMAN: Yes.

JUDGE ELLEMAN: Okay. The other issue I remember -- and, again, correct me if I'm not in the right particular contention -- was a concern over the sue of M5 cladding, and that its behavior was going to be different than Zircaloy claddings and others of which we were more familiar.

In your testimony, I hadn't seen anything that related to M5 cladding. And I hadn't seen anything anywhere until these two papers came around just recently.

Has that issue dropped off your radar scale as an issue of concern?

DR. LYMAN: Well, the issue was raised again from the ISRN presentation that indicated that there was some reason to believe M5 cladding would form larger balloons than Zircaloy four, because of its greater retained utility as a function of burn up.

We don't think that comparison is really that relevant to the issue at hand here. I mean, it's interesting in the comparison of how M5 and Zircaloy clad fuel may behave in a LOCA.

But, I think the crux of the issue here is the appropriate evaluation of fuel relocation effects for the MOX LTAs using the appropriate data for M5 cladding and what is known about MOX fuel behavior.

So the comparison is not really the relevant issue. It's simply the behavior of M5 cladding. Duke has said that in the evaluation of M5 cladding they use it in its most ductile or uneradiated state.

We'd just like to see that, the data derived from those estimates used in the relocation model. The other aspect is the oxidation rate. The reason why we handed out these papers today is because they are just M5 specific oxidation rates which would be appropriate to any calculation of the clad oxidation associated with MOX fuel LTAs and LOCA if there is relocation.

JUDGE ELLEMAN: Just one more question. And this is really just clarification question. In your testimony, in representing the effect of the swelling of a fuel element, you used an aerial representation an R squared scaling to evaluate the impact.

And it wasn't clear to me why you'd use area rather than volume for some other scale.

DR. LYMAN: I believe that's because we have a length of fuel. And so it's really the heat generation rate per unit length that we do the relevant parameter.

So, what you're looking at is the scaling with respect to that we change in the linear heat generation, the local heat generation.

JUDGE ELLEMAN: That's it.

CHAIRMAN YOUNG: Okay. Before we break, I just want to say a couple things. We don't want -- I'm sure I can speak for all of us -- we don't want to cut the staff short.

And, after a little longer time for Duke's panel. And also Duke has indicated the desire to recall its panel. And we also, of course, want to have as complete a record as possible on the issues that are before us.

So, I'd like to ask everyone on the break to look at your calendars. Because we have discussed the possibility if we do nee do come back, of combining that with our August 10^{th} close session, either doing it for a part of that day, or for the following day, August 11^{th}, since at least all the parties are going to be here. And that's after July 27^{th}.

MS. UTTAL: I can tell you right now that the date is not going to be good for one of my witnesses.

CHAIRMAN YOUNG: Either one? August 10^{th} or 11^{th}?

MR. REPKA: Let me make another suggestion. Obviously we can go tonight with the Staff's panel. And, yes, Duke would like to recall its panel, as I said before, as the party with the burden of persuasion, we think we are entitle to go last.

CHAIRMAN YOUNG: Right. And I think you are right.

MR. REPKA: I throw out another idea. And I don't think that will take that long if we have a chance this evening to do it. I won't put a minute's estimate on it.

But I don't think it will take too terribly long. But another alternative is we could perhaps submit something by way of further written testimony to respond to the further issues raised in the rebuttal and the new documents and do that in lieu of another hearing date.

But that's a possibility I just throw out for suggestion.

JUDGE BARATTA: Would you consider that to be acceptable in light of the fact that you do have the right to go last?

MR. REPKA: Yes. I believe it would be. I think that it would be preferable if we could do it tonight and answer any Board questions. But I'm willing to let everybody discuss.

And I'll certainly discuss it with my witnesses over the break as well. And maybe we can revisit that. But I think at least potentially that could be acceptable.

CHAIRMAN YOUNG: There's also the issue of the time and method for responding to exhibits A through D, the various ones of your parts. And we are, I need to say, limited.

Tonight we have to end by nine o'clock, because that's what we have told security. And I don't think we can go any further. And I also don't want to deprive you of attentive ears and minds.

MS. CURRAN: I just wanted to make a suggestion that we break now, come back and do as much as we possibly can and that we reserve -- if we find we can't finish tonight, we are all meeting again tomorrow, aren't we?

CHAIRMAN YOUNG: Well, the Staff's witness is not going to be here tomorrow. That's the problem with tomorrow.

MS. CURRAN: But, in terms of scheduling logistics of how we go on --

MS. UTTAL: We can do the schedule.

MS. CURRAN: -- you can do that tomorrow.

CHAIRMAN YOUNG: So I do have everyone's schedules?

MS. UTTAL: I will make sure.

MS. CURRAN: I just want to suggest spending our precious time tonight doing that.

CHAIRMAN YOUNG: Right. Good idea. Before we part, I want to throw one question out there. I don't want an answer. I just everyone to think about it. In clarifying with Dr. Lyman, some of his earlier testimony and also with regard to Duke's chicken and egg issue, the issue of test reactors came up.

And it just occurred to me to wonder whether there are any standards or criteria for what test reactors do and what lead test assembly program functions are, and what levels of uncertainties are involved.

I don't know whether there are or not. But it's come out. So that struck me as a good thing to clarify. If anyone has any knowledge on it --

MR. REPKA: Can I make an initial response to that? Because we discussed that when I came up. I think you asked the question and there was a characterization of the Duke testimony.

And it's generally in the neighborhood of paragraph 132. And there was a reference that reactor grade MOX fuel had been used in test reactors. But, in fact, that testimony was that it has been used in commercial reactors.

So I think there was a little bit of a

mischaracterization of what the testimony says.

CHAIRMAN YOUNG: I wasn't trying to characterize the testimony. I was just trying to clarify after hearing from Dr. Lyman whether there's anything out there, any sort of general understanding or standards or whatever there might be on that.

MR. REPKA: Okay.

CHAIRMAN YOUNG: All right. Then let's be back at -- it's quarter after five now. Can we be back at six maybe? Okay.

(Whereupon, at 5:15 p.m. the above-entitled matter was concluded.)

CERTIFICATE

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

Name of Proceeding: Duke Energy Corporation

Docket Number: 50-412/414-OLA;

ASLBP No. 03-815-03-OLA

Location: Rockville, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Rebecca Silberman
Official Reporter
Neal R. Gross & Co., Inc.