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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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OPEN SESSION

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THURSDAY,

NOVEMBER 14, 2002

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. Graham Wallis, Chairman, presiding.

COMMITTEE MEMBERS:

GRAHAM B. WALLIS, Chairman

SANJOY BANERJEE, Consultant

THOMAS S. KRESS, Member

FREDERICK MOODY, Consultant

VICTOR H. RANSOM, Member

VIRGIL E. SCHROCK, Consultant

ACRS STAFF PRESENT:

PAUL BOEHNERT, Staff Engineer

ALSO PRESENT:

KEN CARLSON, Framatome ANP

RALPH CARUSO, NRC

HUEIMING CHOW, Framatome ANP

SARAH E. COLPO, NRC

JERRY HOLM, Framatome ANP

RALPH R. LANDRY, NRC

JAMES F. MALLAY, Framatome ANP

ROBERT MARTIN, Framatome

BILL NUTT, Framatome ANP

LARRY O'DELL, Framatome ANP

YURI ORECHWA, NRC

C-O-N-T-E-N-T-S

P-R-O-C-E-E-D-I-N-G-S

(8:31 a.m.)

CHAIRMAN WALLIS: This is a continuation of the meeting of the Thermal Hydraulics Subcommittee of the ACRS. And we're going to continue our investigation of the Framatome S-RELAP5 Realistic LB LOCA Code.

I have a request from Jim Mallay to start us off this morning.

MR. MALLAY: Thank you, Mr. Chairman.

I'm Jim Mallay. I'm Director of Regulatory Affairs for Framatome. And I just wanted to say a few words about yesterday's discussion.

Specifically, a number of you had mentioned that -- and I guess I'd say insisted on the fact that our documentation be presented a little more clearly. During that discussion, I think you provided a different perspective on how our documents are read. Specifically, we need to better communicate to knowledgeable third parties about how we actually apply our equations.

In some respects, the discussion yesterday was a little frustrating for us from Framatome for two reasons. First, we expended a great deal of effort in preparing excellent documentation. In fact, the NRC staff complimented us on several occasions about the clarity and completeness of the documents we submitted.

Second, those of us who are here attending this meeting have a reasonably thorough understanding of the implementation of the methodology. Therefore, I guess I'd have to admit that we had a bit of a blind spot concerning your comments about not adequately communicating what we have done in the model.

As the discussion proceeded, we realized you were exactly correct however. We assumed too much on the part of the reader. Therefore, Framatome will correct this situation. Because of the work involved, obviously, to change this extensive documentation and, of course, our ongoing obligation to fulfill many of our contracts, the revision process cannot be accomplished in the near term.

Just so you understand a little bit about our overall strategy, we plan to expand the use of S-RELAP5 to all of our thermal hydraulic safety analyses. Assuming acceptance of this realistic LOCA model, our next step is to apply the S-RELAP model to BWR non-LOCA analysis. Subsequently, we will then plan to apply this model to LOCA analyses for BWRs, and eventually to a realistic LOCA application. We therefore envision a series of future submittals based on this same basic platform.

Getting back to our commitment, however, we plan to revise the theory manual, which is EMF-2100, which presents the equations and how they're applied. This will be done prior to the next formal submittal of the S-RELAP code. Specifically, we will provide a revised report to the NRC at a time that is sufficiently prior to our next formal submittal of S-RELAP so that final clarifications can be incorporated at that time.

Our goal is to present the equations actually used, including the loss factors, which you will see later on are so very important to the success of the model, and how two-phase flows are handled. We will explain more clearly the conversion of the complex geometries that we talked about yesterday to the one-dimensional straight-line approach used in S-RELAP5. Other similar changes will be made to help the reader fully understand the implementation of the model.

So I guess in conclusion, I appreciate your pointing out some of the shortcomings in how we've explained how the model is actually put together. So, we will fix that.

CHAIRMAN WALLIS: All right. Thank you very much. So we will see this documentation again?

MR. MALLAY: Yes.

CHAIRMAN WALLIS: I can't resist reminding you that we had this conversation the last time or the time before. There were some promises to improve documentation when we first saw it, and that was I think a year or two ago.

MR. MALLAY: That is true. We've had this discussion on at least two previous occasions. I think the context, or at least from my point of view, the context of the conversation was a little different. It was more toward the theoretical basis of the equations, which of course we went over in some detail yesterday.

I think the perspective we got yesterday was how do you really use these equations in the model? And I think that's the first time I really got that message. So, that's what we'll do.

CHAIRMAN WALLIS: Well, certainly I think also there should be more attention to -- what approximations are being made? And there appears sometimes to be a claim that some equation is just truly basic and general when it is not. It's limited. Maybe it's appropriate, but it's not the basic general equation.

MR. MALLAY: That's certainly true.

CHAIRMAN WALLIS: Okay.

MR. SCHROCK: So, Graham, I'd just like to say to Jim that I found the preparation for answering questions that I posed in writing was woefully lacking. And the person who made the presentation was not familiar with the issues involved. The nature of the response was a series of rather vague view graphs, which didn't even put them in the context of the questions that had been posed.

So, I mention that because that's what I'm going to say in my report. I don't think there was an adequate response to questions, which in fact are serious questions.

MR. MALLAY: Okay. We understand what you're saying. There may have been some lack of appreciation about what the questions were in themselves. But, we understand.

MR. SCHROCK: Well, the way to resolve that is to ask for clarification if the questions are unclear.

MR. MALLAY: We understand.

CHAIRMAN WALLIS: So are we ready now?

DR. MOODY: On the upside, I want to

appreciate the answers that were given to my couple of questions on the early blowdown. I never was quite sure what had been done in RELAP to fix that problem. One of my questions did not apply on part forces, but at least I felt like that was well ordered. And I felt much better after the explanation.

CHAIRMAN WALLIS: So are we ready to proceed with the original plan?

I understand we're going to get an overview of the code, and why it's good, and why it works, and how it's been assessed.

MR. HOLM: Graham, this is Jerry Holm. We were also asked a question about the use of the Forslund-Rohsenow equation.

CHAIRMAN WALLIS: Oh, yes.

MR. HOLM: It's not on the agenda, but we thought we'd --

CHAIRMAN WALLIS: Want to do that first?

MR. HOLM: -- do that first.

CHAIRMAN WALLIS: Yes, please do that.

MR. HOLM: Okay, so Ken Carlson will do that.

CHAIRMAN WALLIS: The next time I see Warren Rohsenow, I've got to ask him what he thinks of this equation.

MR. CARLSON: Basically, the -- well, I have it written down that the question was --

COURT REPORTER: Excuse me. Are both switches on?

MR. CARLSON: Oh. Sorry.

We're going to discuss the applicability of the Forslund-Rohsenow of dispersed flow film boiling. I believe the question was: Why is Forslund-Rohsenow a dry-wall contact model? So, I'll briefly go through the purpose of the Forslund's experiment.

Observations by the experimentalists briefly touch on the experimental procedures, and in the end show a plot of Forslund's data compared to T_{min}.

COURT REPORTER: If you lean towards this one, it would be much better.

MR. CARLSON: Oh, okay. I'm sorry. I'm not qualified to work this. It's pretty obvious on that.

And these are just statements that we're taking from the introduction to one of Forslund's papers. Forslund wrote a report that was basically a precursor to the one that was published in the ASME journal.

They were interested in looking at previous experimentalists, was looking at film boiling with nitrogen. And so, there was a regime that was of interest because there was a dip in the wall temperature. When they noticed it was around 10 to 20 percent quality, they -- you see a break-up of the liquid core into droplets and filaments. And Forslund also observed that the droplets were prevented from touching the surface by what he termed as Leidenfrost effect.

He also -- just more verbiage on the terminology. He felt like film boiling is also applied to this high quality region, since it's assumed that a vapor film covers the heating surface.

In his last statement, he says it is this high quality dispersed from this region that is the subject of this current investigation. And I put this last statement in because there seems to be some -- or at least maybe an unclear conclusion when he talks about a low quality region that he's applying this heat transfer coefficient to.

One of the ways to ensure that he was going to get a high quality of data in the film boiling region, he would measure the minimum heat flux that would support film boiling. And he was going to run at two different mass fluxes: 70,000 pounds per hour foot squared and 190,000 pounds --

CHAIRMAN WALLIS: One of the conditions of the experiment, isn't this more fluid and more pressure?

MR. CARLSON: Excuse me. It's nitrogen. He's running at approximately 25 psi.

CHAIRMAN WALLIS: And Hynek also used liquid nitrogen?

MR. CARLSON: He used liquid nitrogen. He was running at, I think in his report he said -

CHAIRMAN WALLIS: Thirty psi.

MR. CARLSON: Fifteen psig, which would be about 30, 29 to 30 psia.

MR. SCHROCK: Do you have any idea what density ratio that would correspond to? What's the equivalent for water pressure?

MR. CARLSON: Well, you know --

MR. SCHROCK: You're going to get at that.

MR. CARLSON: -- I actually did that slide. It seemed like it was, it was around, saturation around 250, wasn't it? Something like that. I don't really remember. I'd have to look at the presentation I did before.

MR. SCHROCK: Thank you.

MR. CARLSON: But I think it's around 250 psi or saturation equivalent to that.

So, Hynek followed up on some of Forslund's work, made some observations, and he did calculate a T_{min}. He did use a different mass flux, so there will be some variation between the T_{min} that Hyneck reported verses a T_{min} that you would back out of this. But I don't expect it to be significant.

MR. SCHROCK: Was Hynek also nitrogen?

MR. CARLSON: Yes. There were three experimentalists. Lavarty was the first, who did film boiling experiments, then Forslund, and then Hynek came in afterwards and summarized some of Lavarty's and Forslund's work. And also extended -- well, he applied Forslund's correlation to water and another fluid. I'm not -- I'd have to look at his paper to report that. He came up with different multipliers, coefficients on the correlation to look at, to make it fit the data for water.

Forslund wanted to make sure that he always had a stable film boiling, so they would start off the experiment by first turning on the power to the test section, and run it up to approximately room temperature since that was way above the Leidenfrost temperature, then instigate the nitrogen flow.

And it would basically go through CHF in the apparatus, or in the valve mechanism before heat would get into the test section. So, they had stable film boiling throughout the experimental test section.

Now, I have -- there's an error on the slide. It should be T_{min}. And it's approximately 220. T_{sat} was about 150 ranking, 150, 160. And all of his data even at the low flow rates are way above it. The T_{min} that he measured --

DR. BANERJEE: What are the units of temperature?

MR. CARLSON: Rankines.

CHAIRMAN WALLIS: It just seemed to me that T_{min} wasn't a magic constant, but it should depend upon the velocity and various other things.

MR. CARLSON: Right. The correlations I've seen for T_{min} have been cast in terms of latent heat of vaporization and heat capacity, surface tension.

CHAIRMAN WALLIS: They have gravity in them. Some of them have gravity in there, which would seem inappropriate in force convection.

MR. CARLSON: Pardon me?

CHAIRMAN WALLIS: Some of them have gravity in the T_{min} as if it were sitting on a flat surface.

MR. CARLSON: That's right.

CHAIRMAN WALLIS: But this is a force convection experiment, which seems the mechanism is completely different. It would be the same in outer space as it would be on earth.

So, I will never believe a T_{min} that has a "g" in it for a force convection experiment, although quite often it does.

MR. CARLSON: Quite often it does. Well, quite often film boiling correlations have "g" in it as well.

CHAIRMAN WALLIS: I know. Wrongly, they use it wrongly.

MR. CARLSON: We're applying it to a vertical --

CHAIRMAN WALLIS: It seems nautically inappropriate. Do the experiment in space you get the same answer.

MR. CARLSON: Yes. Let's assume -- I have assumed anyway that the part of the coefficient in front of film boiling style coefficients is to account for gravity and really shouldn't be there.

CHAIRMAN WALLIS: So all these temperatures are way above T_{min} in these tests, right?

MR. CARLSON: Yes. The two test series he ran at two different heat fluxes -- or four different heat fluxes: 20,000 BTUs per hour foot squared, 15, 10, and 5. He measured -- well, actually he measured, under his flow rate conditions and under his test conditions, a T_{min}. The T_{min} would come in at -- was it what, 3200?

CHAIRMAN WALLIS: Those asymptotes are for vapor alone I take it?

MR. CARLSON: I believe so, yes.

CHAIRMAN WALLIS: Then he correlated his data in some dimensionless form that was mechanistic. Then the real question is: How do you take this and apply it to water?

MR. CARLSON: Hynek, I believe just fit the data to water using various data sets available at the time. I think Bennett was one of them. And as far as I know, he just looked at what a multiplier was. Rohsenow described a multiplier of K1 times K2, which was basically an effective compensation for a particular fluid that we were looking at.

CHAIRMAN WALLIS: So Forslund had a dimensionless mechanistic correlation, and then someone else checked it and it also worked for water?

MR. CARLSON: Changing the coefficients,

yes.

CHAIRMAN WALLIS: So there is work with water, which corroborates this?

MR. CARLSON: Well, he didn't run --

CHAIRMAN WALLIS: No, Forslund didn't use water. Someone else did.

MR. CARLSON: I think Hynek had looked at other data sets, but I don't believe he generated new data sets.

CHAIRMAN WALLIS: So your bottom line is that the wall was not wet, is that it?

MR. CARLSON: Yes.

CHAIRMAN WALLIS: This is important in the precursory cooling and rewet, is that what it is? And the droplets that spit up in front of the quench front?

MR. CARLSON: Yes.

CHAIRMAN WALLIS: Okay. Any questions? Can we move on?

DR. RANSOM: Did you ask this question?

CHAIRMAN WALLIS: No.

DR. RANSON: How did this question come up?

CHAIRMAN WALLIS: What's the origin of the question?

MR. CARLSON: What's the origin of the question, Jerry?

MR. HOLM: This is Jerry Holm from Framatome.

This was a question asked of us by the NRC staff. And at this point, we still have not reached agreement with them on this point. That's why it was forwarded to us by --

DR. RANSOM: What? On the applicability of this correlation for use in the film boiling review?

MR. HOLM: Right. We are still disagreeing that it's a dry-wall contact verses a wet-wall contact.

CHAIRMAN WALLIS: But it still gives you the heat transfer coefficient whatever it is. Isn't that --

MR. HOLM: We would take a bottom line of "A" -- you know, we used it in our assessments so it's validated that way. And then staff asked us to go off and do a sensitivity study. It turns out that it is actually not very important. If you set the coefficient to zero, it only affects the PCTs by a few degrees.

So, I think at this point we're agreeing to disagree on because it's low impact.

CHAIRMAN WALLIS: So you're saying if you get steam cooling instead of precursory cooling by film boiling it doesn't make any difference?

MR. CARLSON: Yes, not much difference. In the low-flow cases, there is a very small change in PCTs, less than three degrees.

In the high-flow cases, there was a bit more. Forslund-Rohsenow is more important for, once you turn it over to the PCT, it acts as the precursor for quenching. So without Forslund- Rohsenow, you change it to either never quench in the upper regions of the experiment or quench at such a late time.

MR. LANDRY: Dr. Wallis?

CHAIRMAN WALLIS: Yes.

MR. LANDRY: Ralph Landry from the staff.

The reason we disagree with the correlation is not concerning PCT and the actual quench. The point at which we disagree with use of the correlation is when you're at a wall temperature above T_{min}.

Reading Forslund and Rohsenow's paper, it very clearly states that the concern here is with dispersed flow film boiling region where heat is transferred from the wall to a possibly super heated vapor, and from this vapor to liquid droplets. Superimposed on this two-step process is an additional amount of heat that transferred from the wall -- to the wall, directly to the liquid droplets.

And the fact that Forslund-Rohsenow experiments were run at extremely high mass fluxes compared to the mass flux that will occur in slow reflood process, the mass fluxes are in order of 10 to 100 times the mass flux one would see in the low reflood rate calculation.

We have looked at a number of papers. We provided to Framatome a list of 35 papers, and I have 18 of them with me right now, which all disagree with use of this correlation that, temperatures above T_{min}. We simply don't agree with them that it is valid when the wall temperature is above T_{min}.

We have discussed this matter with Professor Griffith, who is cited in the paper as one of the reviewers. We talked with Pete last week and Pete very strongly disagrees with use of this correlation in rod bundles at these high mass fluxes, and stated that this correlation is a method of desuperheating vapor that should not ever be used in contact with a wall.

When we asked Framatome to do the

calculations, we specified that the calculations, which Ken has alluded to, were to set a multiplier on Forslund-Rohsenow to zero when T_{wall} was greater than T_{min}. We are not disputing the correlation when T_{wall} is between T_{min} and the quench. It is when the T_{wall} is above T_{min} that we have the disagreement with use of this correlation.

When that is done, it affects the -- and I was going to talk about this this afternoon too. The effect is to raise the temperature on the order of 5 to 18 degrees Fahrenheit over the temperature that occurs if you allow Forslund-Rohsenow to be included in the heat transfer model. It extends the quench time, but it has no effect whatsoever on PCT.

So on that basis, the staff's position is we do not agree with Framatome on the use of Froslund-Rohsenow above T_{min}. However, the effect is so small that we have agreed to disagree.

CHAIRMAN WALLIS: Well, I'm glad you're doing such a thorough job of review. I'm a little puzzled about your statement of T_{min}, that you don't use it above T_{min} because the figure we just saw showed all the data points way above T_{min}. And, I thought the whole idea of the correlation was to provide a correlation when you were above T_{min} rather than below it.

MR. LANDRY: Our reading of a number of the other papers dealing with dispersed flow film boiling indicates that the Forslund-Rohsenow correlation should not be used above T_{min} and you should rely on other heat transfer mechanisms.

CHAIRMAN WALLIS: But you saw the figure though just now, and all the data are way above T_{min}. So, I'm puzzled. But I haven't seen all these papers.

MR. LANDRY: But those figures were taken at very low temperatures. This was done with liquid nitrogen in a small tube. And it is now being applied to water at very high temperature in a bundle.

We do not feel that this can be directly taken from the experimental conditions to the conditions that occur --

CHAIRMAN WALLIS: Well, you can work it out with Framatome. We're not being asked to give an opinion on this particular issue.

MR. LANDRY: That's why the staff's view is that we have simply agreed to disagree that this does not affect PCT. It only affects the time to quench and has a minimal effect on the temperature beyond --

CHAIRMAN WALLIS: It doesn't affect it for this particular application. You might for other applications have to examine it more carefully.

MR. LANDRY: That's right. And that's why we have identified in the SER our disagreement over this correlation.

CHAIRMAN WALLIS: Okay.

DR. BANERJEE: Ralph, there was an extensive review of this by Yadigaroglu and Andreani.

MR. LANDRY: That's one of the papers I have right here.

DR. BANERJEE: What was their view of it?

MR. LANDRY: They did not --

DR. BANERJEE: Did they come up with any sort of suggestion?

MR. LANDRY: I'd have to go back and read the exact statement, but they did not agree with use of this correlation about T_{min}.

DR. RANSOM: On this figure, the dashed curves are never explained, are they?

MR. CARLSON: Oh, the dashed curves.

CHAIRMAN WALLIS: Maybe that's a prediction of some sort?

MR. CARLSON: I think that's the prediction, but I would have to look at the paper to--

DR. RANSOM: Prediction by Forslund?

MR. CARLSON: By his computations, yes.

DR. RANSOM: For the nitrogen case or for water?

MR. CARLSON: The nitrogen case.

CHAIRMAN WALLIS: This is nitrogen. Well, maybe anyone who is interested can get these papers from Ralph and look at them.

I think we have to move on with this particular part of the meeting. We'll finish this part and move on to the main schedule.

Can we go back to the main plan?

MR. BOEHNERT: Okay. Now, I understand we go into closed session, is that correct? So, anyone who doesn't have an agreement with Framatome to be here should leave.

And transcriber, we'll go into closed session.

(Whereupon, at 8:59 a.m., proceedings went into Closed Session.)

CHAIRMAN WALLIS: Okay. Let's go back into open session. We're now going to hear from the staff. Ralph Landry of NRR will start off.

MR. BOEHNERT: Oh, by the way. Just for everybody's information, we are in open session now.

CHAIRMAN WALLIS: Are we going to be closed any --

MR. LANDRY: Thank you, Dr. Wallis. I am Ralph Landry, from NRR.

THE REPORTER: Excuse me. Do you have your mike on?

MR. LANDRY: Yes.

THE REPORTER: There's two switches on there.

MR. BOEHNERT: There's two switches on there, Ralph. Make sure both are in the on position. Try it now.

MR. LANDRY: Is that better?

DR. BANERJEE: Move the thing a little bit to the right or middle.

MR. BOEHNERT: Oh, wait a minute. Mine are straight. You don't have it lined up straight. There we go.

MR. LANDRY: This okay?

MR. BOEHNERT: That's good.

MR. LANDRY: You're sure, Paul?

MR. BOEHNERT: I'm sure. That's really good.

CHAIRMAN WALLIS: But you passed the first two tests. Now, we'll get on with the serious part.

MR. LANDRY: Well, that's definitely -- that was sure the whole content of what I was going to do. Now, the next speaker will be --

(Laughter)

MR. LANDRY: I'm Ralph Landry. I'm from NRR and today I'm going to be presenting a summary of the staff's Safety Evaluation Report on the Framatome ANP S-RELAP5 Realistic Large Break LOCA Methodology.

Today, I want to go through just a brief review of some of the milestones we reviewed. There are some members here and consultants who were not involved in the early stages. So I'd like to just highlight some of the milestones, not spend a lot of time on that.

I'm going to talk a little bit about the SER structure in particular, then give an overview of some of the thermal-hydraulic review. We'll have a review of the uncertainty analysis and a discussion of some of the staff parametric studies that were performed and our conclusions to date.

The staff that have been involved in this review include myself, Sarah Colpo, who has done a great part in looking at parametric studies and looking at some of the internal coding, and reviewed some of the material internal to the code.

Tony Attard assisted with review of much of the transfer modeling that's in the code. We had Yuri Orechwa reviewing the uncertainty analysis and statistical approach, and Len Ward, from ISL, Incorporated, assisted us with general overview of thermal-hydraulics in the code.

A brief overview of some of the milestones. We received the documentation and the code in August of 2001. Just over a year ago we began this review. We've provided acceptance letter on the code to Framatome in October of 2001.

The acceptance letter is merely a statement that, yes, there is sufficient material here to permit us to perform a review. It is not acceptance of the code or acceptance that anything there is correct.

It's simply a statement that there is sufficient material to proceed with the review. Framatome made presentations to the staff in October and to the ACRS Thermal-Hydraulic Subcommittee in

January of 2002.

We issued the full set of RAIs to Framatome in July of 2002. Framatome prepared their responses in August. We were meeting on the draft SER yesterday and today with the subcommittee, and our intention was to go to the full committee in December of 2002, and to issue the final SER in December.

Now, this is assuming that we resolve some of the issues we talked about this morning. The structure of this Safety Evaluation Report, in performing a review of a code of this nature you have to keep in mind that the review we do is not of every single detail in the code, and every single detail in methodology.

We simply do not have the staff, the time, the capability to perform a review of that nature. What we do is perform a review of select portions of more of snapshot views of parts of the documentation, parts of the code, parts of the modeling, parts of the uncertainty analysis, assessment and so on.

We are not in the position to review every single detail. If we were doing that, that would be performing the quality assurance function, which the applicant must perform on their own. So we have to keep in mind that when we perform this review and what we report in the SER is a snapshot of parts of the code and parts of the documentation and the methodology that's followed.

The SER follows the format that is described by CSAU. This morning Larry O'Dell from Framatome went through step by step the CSAU process. This is the material contained in NEWREG-5249. It defines a 14-step process by which a methodology is presented and determined to satisfy the requirements of 50.46, and determine what the uncertainty is in that methodology.

The SER provides an overview of the PIRT structure. We give an overview of the thermal-hydraulic phenomena modeling that we've reviewed. Again, this does not cover everything we review. This is only giving an overview of select parts of our review.

If we provided detail of everything we reviewed our SER would be several hundred pages long. So we're trying to be reasonable. And we give an overview of selected assessments. We give an overview of some of the coding examination which was performed and some of the parametric studies which we perform, and we give an overview of the uncertainty determination methodology and the conclusions by the staff.

You've heard a great deal today already about the PIRT. So I don't want to go through too much detail on what the PIRT contains. A Phenomena Identification and Ranking Table was developed and included in the methodology report.

The omissions of NEWREG-CR-5249 have been included in the PIRT. Those things that were omitted in the standard PIRT developed for the new reg have been fulfilled and included in the PIRT developed and supplied by Framatome.

Specifically, the PIRT does address the hot bundle containing the hot rod, as we discussed this morning. The plant calculations are done at a realistic peak linear heat generation rate.

The standard PIRT was done at a linear heat generations rate down at around five kilowatts per foot, five to seven to nine, somewhere in that range, and we expect plants to be more in the range of the teens, 12, 14, 15 kilowatts per foot.

Calculations have been performed at the realistic and at low containment back pressures. This is an issue which was discussed somewhat this morning and which we do discuss in the SER, and that is the downcomer boiling question that can occur, especially at low containment back pressures.

The PIRT that is presented represents phenomena according to the transient phases, blowdown phase, refill, reflood, post-CHFE transfer phase, reflood heat transfer and rewet. As you also heard this morning, a frozen code version has been provided and has been specified.

This was a concern that was raised a few years ago in our code review that we were performing when we discovered that a code that we were reviewing was not frozen.

In fact, the code was undergoing major revisions, a major revision in very fundamental aspects, which made it very difficult because we realized at that point that we were reviewing a moving target, and it's very hard to review a moving target.

So we've been very, very adamant with some of the vendors that has come in since that point that we will not even begin a review until they assure us that the code we are reviewing is a frozen code version. And Framatome identified and indicated this morning the version of the code which has been supplied for this review.

Our SER very specifically states that we have reviewed the S-RELAP5 MOD2 and then identifies the version of the code. That is to insure that when our SER is picked up and applied, people understand that our review approval is for this specific version of the code and no other.

Framatome ANP has provided documentation on the frozen code version, such that evaluation of the code's applicability to the postulated large break LOCA transient scenario could be performed.

I will have some more comments on the documentation. I know comments were made this morning regarding documentation. Comments were made based on presentations yesterday, and there's some dissatisfaction.

We have pointed out also that there are areas where the documentation needs to be repaired, and indeed, Framatome has committed to make changes in documentation based on some of the things that we discovered.

I'd like to turn to some of the thermal-hydraulic models that we've looked at. The heat transferring modeling was evaluated by requesting that Framatome identify the heat transfer correlation used from transient initiation to quench at the hotspot.

Specifically, what we are interested in: those of us that have been involved in code work for a number of years have looked at some of the code results and code modeling techniques and realized that one thing that we don't recall ever seeing was a code modeler take a transient, whatever transient it might be, and follow it from the beginning of the transient to the end of the transient, the heat transfer correlations that are being invoked throughout the transient, what correlations are being brought in and are those correlations being used within their range of validity.

And to do that we said, identify to us time-wise throughout the transient what correlations you're using, what are the sources of the data and the range of validity of the correlations and what are the parameters that exist when you're invoking those correlations throughout the transient so that we can see that the correlations are being used properly with correlations that are being used within an accepted range of validity.

In doing this, Framatome, as one of the thoughts, provided this diagram which shows for the hotspot the mesh point temperature versus time, and this is looking at a void fraction range over the time.

It's not looking at specific void fraction at a specific time, but what is the average void fraction over a time interval. So we can see that throughout the transient, small time steps were the -- where we see major changes in void fracture, what are the void fractures that are occurring at the hotspot.

Now, correlated against this in the documentation is a table listing time step, time block, void fraction, heat transfer correlation, the data range of validity for that correlation and the data parameters, the phenomenal parameters that exist in -- during those time blocks for the entire transit.

We were able to go through this and then look at the material and say, gee, there are a couple of these correlations that are outside -- or we think are outside the range of validity.

We began a series of discussions with Framatome and they were able to come back and show us that through further assessments that they had extended the range of validity of some of the correlations through assessment cases that were run.

So we said, okay, those correlations, even though they might appear to be outside their initial range of validity, are within a range of validity because they've been assessed against other data.

DR. KRESS: Now, is this for a given break size?

MR. LANDRY: This is -- yes. This was -- I didn't write down which break size this was. This was the break that resulted in the peak cladding temperature. This is that --

DR. KRESS: That was the one that ended up with the heat point.

MR. LANDRY: Correct.

DR. KRESS: So as you move across in time -- oh. As you move across in time you're looking at different locations in the core? Those are not all one location?

MR. LANDRY: No. These are at that one mesh point. This is at the --

CHAIRMAN WALLIS: It's all at the peak clad temperature --

MR. LANDRY: This is at the mesh point at which the peak clad temperature occurs.

DR. KRESS: It finally occurred.

MR. LANDRY: So what you're looking at is the temperature trace --

DR. KRESS: Of that particular node.

MR. LANDRY: -- at that point through time.

DR. KRESS: Okay.

MR. LANDRY: We tried to figure out what is the valid way to determine what correlations are being used, and felt that if you take the point at which peak clad temperature or the node at which peak clad temperature occurs from the start to the end of the transient, what correlations are coming in, but then we added onto this.

There are other plots and so this is only one. They then showed us plots for that rod, the hot rod, up and down the rod what are the correlations that are occurring at the time of peak cladding temperature, so that you can see -- this is the PCT time.

This is the -- this gives us the void fraction. We can go back and check the void fraction and see what correlations are being used there. But we can also look up and down the rod because you know that there is quenching occurring at some point in the rod at that particular time, and what correlations are being used up and down the rod, also, so that you have correlations versus time, or there's a hotspot and correlation versus distance up and down the rod at the time of peak, also.

We were trying to get a handle on, in a almost global sense, what is going on in the code at the highest temperature mesh point. Is what is going on reasonable? Are the correlations that are being used, being used correctly?

DR. BANERJEE: Are those different hatchings, just different phases or what is the --

MR. LANDRY: The different hatchings are indicated over here in the legend.

DR. BANERJEE: Right.

MR. LANDRY: They indicate the different void fraction ranges.

CHAIRMAN WALLIS: Okay.

DR. BANERJEE: The first one goes from zero to one, right? Or does it?

MR. LANDRY: That's just in this very narrow time.

DR. BANERJEE: Right.

MR. LANDRY: In this very narrow time --

DR. BANERJEE: They've what?

MR. LANDRY: This is the blowdown period.

DR. BANERJEE: Right.

MR. LANDRY: You're blowing -- you're decompressing the system so you're going from water solid to total steam. After that point, though, the ranges on the void fraction become very narrow.

DR. BANERJEE: But what is distinguishing each hatched area?

CHAIRMAN WALLIS: They're going up or down.

DR. BANERJEE: They overlap.

CHAIRMAN WALLIS: The trend is up or down, it seems to me; are they climbing the mountain or going down the mountain. They're on the top.

DR. BANERJEE: I see.

MR. LANDRY: But really, it's showing you the way the void fraction is going up and down at the hotspot throughout the transient.

CHAIRMAN WALLIS: But it's hardly varied at all. It's between .98 and 1 or something, most all the time.

MR. LANDRY: Which is a --

CHAIRMAN WALLIS: Very high -- very high void fraction.

MR. LANDRY: Which is a very good conclusion that you can see that you have at the hotspot an almost totally voided system for the entire period of the transient until you quench the rod. At this point the void fraction starts dropping very fast, because you're quenching.

Quench front is approaching. Once you hit quench you drop very rapidly.

CHAIRMAN WALLIS: Now, is this all the same equation that describes this heat transfer?

MR. LANDRY: No. These are -- each of these -- there's a different correlation in each of these --

CHAIRMAN WALLIS: In each of these regions a different correlation? But the void fraction's much the same in most of the regions.

MR. LANDRY: The void fractions vary a little bit and different correlations are being brought in. We raised a number of questions on the correlations, and as we got into the discussion this morning, discussion of Forslund-Rohsenow, because there are different heat transfer modes occurring in each one of these void sections.

MR. CARUSO: This is Ralph Caruso. I'm just going to help Ralph Landry out a little bit. He's got void fraction plotted up there, but there's a lot of other things that are going on. Flow rates, mass flow rates up through the channels are also changing quite a bit, and these also affect the heat transfer readings and the correlations that are used.

So although he's just got void fraction here plotted, realize there's a lot of other stuff that's changing at the same time.

DR. BANERJEE: So is there a typical correlation which is being exercised in each of these regions, or is it all Forslund-Rohsenow, all this?

MR. LANDRY: No. There are --

DR. BANERJEE: So it's different --

MR. LANDRY: -- there are a fair number of them.

MR. CARUSO: If you look -- let's see.

MR. LANDRY: I did not put a listing of all of the correlations up here because that material's proprietary. We wanted to keep the discussion here open.

DR. BANERJEE: Okay.

MR. CARUSO: I'm looking at one of the RAIs and I've got one, two, three, four, five -- I think about five different correlations coming in and out.

DR. BANERJEE: Okay. We can find out details later.

MR. LANDRY: You can look in the RAI answers.

DR. BANERJEE: Right.

MR. LANDRY: This is from RAI No. 2. If you read the response to RAI No. 2 and then Action Item 1 or Action Item 2, you get even more detail of what is occurring.

CHAIRMAN WALLIS: So your documentation has spelling errors in it.

MR. LANDRY: Oh, okay. I switched over and instead of using Word Perfect to prepare these I was using one of Bill Gates' products.

(Laughter)

MR. LANDRY: Which does not do spell-checking. PowerPoint does --

DR. BANERJEE: Oh, PowerPoint doesn't.

MR. LANDRY: PowerPoint does not do spell-checking for you as you move along. So I'll say a comment that was similar to one said this morning by the applicant when they were asked about a bunch of dark lines in a figure.

I think if you look at the mis-spelled words throughout the document, it spells out, "We love Bill Gates." The dominant phase in large break LOCA is reflood, and in particular disperse flow film boiling heat transfer.

And we're going to talk more about the reflood in a little bit when Sarah Colpo comes up in some of the studies that she has done. The applicant switched, as you heard this morning, from using the more common Dittus-Boelter correlation to the

Sleicher-Rouse correlation.

We spent a time looking at that correlation. We asked for a copy of the paper and we had questions on the uncertainty analysis for that correlation, because everybody knows Dittus-Boelter. It's been around for years.

The dispersed flow regime uses Bromley and Forslund-Rohsenow, but interpolates between the two over a particular range.

CHAIRMAN WALLIS: Bromley is one of those anomalous correlations that has gravity in it, although this is forced convection?

MR. LANDRY: Yes.

MR. SCHROCK: Bromley was really analysis. It wasn't correlation, but it was for a different problem.

MR. LANDRY: Yes.

CHAIRMAN WALLIS: Yes.

MR. SCHROCK: Film boiling on a horizontal cylinder.

MR. LANDRY: I think Professor Schrock is trying to get me on my soapbox right now.

MR. BOEHNERT: But you're not taking the bait, right?

(Laughter)

MR. LANDRY: Well, the dispute -- the discussion we had this morning on an application of Forslund-Rohsenow brings up a concern that the staff has, and that's a concern with using the right correlation at the right time and for the right reasons.

We went through a long discussion on this, this morning. One of the problems that we see with Forslund-Rohsenow, one, it's a correlation model that was developed for liquid nitrogen in a tube at a very high mass flux and a low void fraction.

You see a correlation that is now being applied for water in a channel between rods at low mass flux at very high surface temperatures. The difficulty I have is you're taking a correlation developed for one fluid and applying it to another at a significantly different surface tension, significantly different viscosity, significantly different latent heat vaporization and you're saying that these bubbles -- or excuse me -- these droplets that may be a different size are able to penetrate a thermal boundary layer at a much lower velocity and much less turbulence.

This just doesn't make sense. One of the difficulties that we see in the heat transfer models is a lot -- and everybody uses these. It's not unique to Framatome. We're seeing heat transfer models that are used, that are developed for boiling in a radiator of an automobile.

We see models using correlations that are developed for Freon, liquid nitrogen, inside various sized tubes and even capillary tubes, all of these things being applied to flow in a rod bundle.

One of the important programs, at least in my view, is to look at the work that you heard about Tuesday afternoon that Dr. Hochreiter is doing at Penn State. He is doing work on reflood heat transfer in a more or less prototypical rod bundle configuration using water at typical flow rates and typical wall super heats.

So that information is going to be much more prototypical of the kind of phenomena you would see occurring in a rod bundle under reflood conditions.

DR. BANERJEE: Why wasn't FLECHT sufficient? I mean, they have a lot of data and stuff.

MR. LANDRY: They have a lot of data, but it wasn't really a heat transfer problem. There are some other problems with FLECHT. There was a tremendous leakage. When we were doing the review of AP600 we raised a number of questions about use of FLECHT and FLECHT SEASET for levels -- two-phase level swell, because it was so hard to characterize leakage.

And the same with the G-2 test and some of the other tests. You can look at these tests and get some data, but are they really fundamental heat transfer research data? We spent a great deal of time and a great deal of effort studying ECC performance.

But we're still using a lot of heat transfer correlations that go way, way back and were not developed for this particular problem.

MR. SCHROCK: Well, there are some other correlations in the literature for rod bundles, but nobody seems to want to use them in codes.

MR. LANDRY: I think there is a certain inertia, industrial inertia that these correlations -- everybody's using these.

MR. SCHROCK: Very large inertia.

MR. LANDRY: People are satisfied with them because we're getting globally reasonable results. We're able to predict a lot of the tests and a lot of the separate effects, integral system tests, et cetera, that we use to validate the codes. So why change?

MR. SCHROCK: These things preceded the codes in many instances. ASME has had a series of monograms, heat transferring rod bundles. I edited the first one of those in 1969. So none of these codes existed in 1969, for example.

MR. LANDRY: Well, none of these codes existed when Bromley's work was done, either.

MR. SCHROCK: Well, people don't look at what's in the literature enough, I think is the problem.

MR. LANDRY: This morning --

MR. SCHROCK: If the literature wasn't NRC-generated, it doesn't get the same level of attention.

DR. BANERJEE: But Forslund and Rohsenow was not NRC-generated, though.

MR. SCHROCK: Well, I'm not making any universal comparisons.

(Laughter)

MR. LANDRY: This morning, one of the questions that was raised concerned material that had been presented by Joe Kelly a few years ago regarding the Lahey correction. Steve Bajorek of research has talked with Joe Kelly about that.

Steve, can you enlighten us a little bit?

DR. BANERJEE: These were the experiments where the interfacial area was measured.

MR. BAJOREK: Well, I'll try to, Ralph. I mean, I only saw your slide this morning for the first time. But I think what you were alluding to was the problem with the Lahey bubble-pumping model and the sub-cooled boiling correlations.

The problem that's associated with that is that when you try to apply that at relatively low pressures, 40, 50 psi or lower, it cannot really split the heat flux between the sensible heating of the fluid and the latent heat very well.

The term that's in question is like a rho-L, a liquid density times an enthalpy difference, a delta enthalpy over a -- on top of a rho-G H-sub-FG. At high pressures it seems to do a reasonable job and do a -- and split the heat flux between heating of the liquid and vapor generation relatively well.

However, when you get down at low pressures the rho-L over rho-G dominates and until you get to a -- almost a saturation, all of your energy is going into heating up the liquid.

And all of a sudden, what your code does is switch when you get a low pressures to nearly saturation, to taking all of the energy, putting it into the liquid phase to all into vapor, and your code acts in a very oscillatory fashion, all the heat going to the liquid and then suddenly all the vapor generation, you get very large voids in your calculation, and that instability is what Joe is referring to.

MR. LANDRY: Does that answer your question, Sanjoy?

DR. BANERJEE: Yes, it's exactly in line with what I --

MR. LANDRY: Okay.

DR. BANERJEE: -- my understanding is, that it doesn't give you the right split.

MR. LANDRY: This morning you heard from Framatome about the decay heat model that they're using. They're using ANS 5.1 1979 model and they're using it in a conservative fashion. We looked at the counter-current flow limit model that is used in the code and felt that the CCFL model was being used fine in the core, but there's no CCFL model in the downcomer.

We had a number of questions and spent quite a bit of time speaking with Framatome about the lack of a CCFL model in the downcomer. Our concern was that even though the calculations which they showed us, showed that they did not have CCFL model -- CCFL violation very often in the downcomer, there were a couple of instances in one large plant calculation where they did have CCFL violation, but these were just three short violations of a CCFL.

So we felt that it was important that the analyst be alerted and Framatome has agreed to put in the code a flag, so that if CCFL is violated in the downcomer, the analyst will be alerted so that the analyst can determine, is this CCFL violation of such a magnitude that it's going to affect my result, or is it just an instantaneous, very brief violation that's not going to have an affect on the result and it can be ignored.

We felt that that is sufficient to simply alert the analyst through the violation of CCFL so that if it is a problem, something can be done. If it's not a problem, it can move along. We did a great deal of looking at boiling in the downcomer, as you heard some talk about this morning.

And we've talked about the nodalization in the downcomer. We requested that Framatome go back and renodalize their downcomer from the three node model which they had initially presented, to a six-note and to a nine-node model.

In those studies in varying the containment back pressure at the same time, we noticed that there is a less than a 100-degree Fahrenheit change in PCT when you vary the downcomer nodalization back pressure, and the form loss coefficient.

So we felt that since the most conservative calculation that they had was the three-node model, that that was acceptable to us. They go to the six-node or nine nodes they go -- they get a lower PCT. So our conclusion was the three-node model which they were using was conservative.

Framatome did confirm that they do not include a direct, negative bias and uncertainty methods simulating ECCS bypass, so that they are conservative. This was a concern we had during the review.

MR. BOEHNERT: Ralph, what did you mean, they're using ANS 1979 in a conservative manner?

MR. LANDRY: They way that the -- the way they've included the actinides, decay heat generation, they've included Plutonium-239, U-238. All the components that they put in are giving a conservative prediction of decay heat. They're not using they're statistical decay heat model.

MR. SCHROCK: I understood they make the

assumption that it's all U-235 and infinite irradiation. Is that not true?

MR. LANDRY: Yes.

MR. SCHROCK: So that's the conservatism. That gives you a higher value than if you have Plutonium contributing.

MR. BOEHNERT: Now, if they came in and said they wanted to use ANS 94, would you find that all right?

MR. LANDRY: We'd have to re-review it.

MR. BOEHNERT: But there's nothing says they can't.

MR. LANDRY: If they came in and made the argument, we would review it. I can't say without looking at it.

MR. BOEHNERT: No. No. I'm just saying -- yes. I understand.

MR. LANDRY: We would review what they've presented.

MR. BOEHNERT: Okay.

MR. LANDRY: Okay. The uncertainty analysis, I'm going to ask Yuri Orechwa to present to you. After Yuri's presentation we're going to talk about the assessment matrix, and in particular, what we want to talk about is the assessment which we performed in-house.

We looked at the assessment that was provided by the applicant. As I said earlier, because we have to focus on particular parts of the presentation to us, we focus most heavily on our review of the assessment cases, on those that are the latest tests that were run, the SETF, CCTF and UPTF, the NRC sponsored 2D-3D Program.

We thought that these were the best data and these are the closest to full scale. So while we looked at the whole assessment that was done, we focused most heavily in our assessment review on the 2D-3D assessment cases.

We did include spot-checking of the coding and comparison of that spot-check with the documentation, and Sarah will have some words on that later. We found some inconsistencies between -- excuse me -- what was coded and what was documented and Framatome has agreed to go back and fix the documentation, because there was documentation errors. We ran --

CHAIRMAN WALLIS: Can I go back to that? You mean that the code, what was actually encoded, was not correct?

MR. LANDRY: What was encoded was correct.

CHAIRMAN WALLIS: It was the documentation that was wrong?

MR. LANDRY: What was written in the documentation was wrong. We included in our review running numerous parametric studies using the S-RELAP5 code. Sarah's going to go through those.

As was discussed this morning with some of the assessment discussion, Sarah looked at three particular parameters, three sub-routines, which were medium to low priority and one that was a very high priority, according to the PIRT, and found results that are consistent with what we would expect from a high priority phenomenon.

CHAIRMAN WALLIS: So you were able to get their code and input text and everything and run it?

MR. LANDRY: That's right.

CHAIRMAN WALLIS: You had the right platform to run it on?

MR. LANDRY: That's right. We were running it on an HP. So I was able to go into the source code, put multipliers in the source code, then recompile the code -- it was in the same compiler -- and rerun cases. And Sarah's going to present some of those discussions.

This morning there was a lot of time spent talking about assessment, what is adequate assessment. Unfortunately, what is not done in this country when an assessment is performed is to go to some of the international information that's available.

People in this country tend to use certain tests that everybody uses to assess a code. This is particularly troubling because it may be out of a parental view of the assessment.

But years ago in Paris while I was working for the Nuclear Energy Agency, Klaus Wolfert and I started a program to determine at that time what was called, how good is good enough, attempted to define what is the proper assessment to perform on a computer code.

That work, after I left, was continued and completed under Ralph Caruso while he was in Paris. That work developed massive tables of phenomena that could occur, not only in LOCA but in a number of different transients for PWRs and BWRs, phases that would occur during transience and LOCAs, the phenomena that would occur, the data from all the international projects that could be found, how good are those data, which data directly indicate the phenomena that are being studied and what is the quality of the data.

This is a massive effort that is available to code modelers. And I don't know of any that are using it, at least in the United States. It could easily help out in the discussions like we had this morning of how do you know you've assessed the code enough.

How do you know you've assessed properly? By looking at that information that's available and saying, gee, maybe this test that I'm using is not the best test; there is a test in country XYZ that might be better.

Now, of course, the difficulty is when you're dealing in the international community, getting the data. The data are not always easily available. One of the complaints -- and you heard part of the complaint this morning -- one of the complaints that has been voiced by the code modelers throughout the world has been the quality of data that are now available.

Last May in France when the best estimate code modelers met to discuss the state of best estimate code assessment, virtually every country complained about the same thing. We have all these identified tests and data, but the data are becoming very degraded and very poor.

Accessibility of the good quality --

qualified data to use for assessment is going downhill fast. Any effort, as Framatome has talked about, of getting data, putting the data on CDs, getting various sets of data from various sources, so that if we have a data set for LOFT test L22, part of it is corrupted, well, if we can get a data tape from somebody else of the same test, maybe that data set is corrupted somewhere else and we can extract the good data from all these different tests and put together a good data set for as many tests as we can before the tapes are all lost. So anyway, that's my soapbox.

Next, though, I'd like to turn to a discussion of the uncertainty analysis and turn the floor over to Yuri Orechwa.

CHAIRMAN WALLIS: Thank you very much. You'll be back with your conclusions at the end.

MR. LANDRY: Of course.

(Pause)

MR. ORECHWA: Is that going to work?

MR. LANDRY: Sure.

MR. ORECHWA: Okay. Can you hear me, lady? Okay. What I'm going to discuss is the construction of S-RELAP5, realistic large break LOCA, best estimate analysis methodology. In the words of Bette Davis, fasten your seatbelts, we're in for a bumpy, bumpy ride.

All right. To start, let me remind you what we were supposed to review. In the words of Framatome, the basis of the analysis is the entire methodology, not just the code. I think for the last day or so you've been beating to death the code.

Let's talk a little bit more about the methodology. Framatome says the methodology is statistics-based. Okay. Given they're statistics-based, they are going to use a non-parametric statistical approach.

I want to touch on all these three points. The framework for this discussion is the following. We can draw the following picture so you get a little bit more of an understanding how this hangs together. The methodology contains the code and data.

How are you going to use the code and data? You can use it in two ways. You can go the deterministic way and use Appendix K type analysis. You can go and use best estimate, do a statistical approach with regard to the -- with respect to the data.

Having chosen statistical, you have two choices of how to do your statistics, non-parametric and parametric. Within that, you still have two choices. You can take classical and Bayesian. If you want to bring in all your engineering judgment, you have the Bayesian option.

If you take your classical option, shut up about engineering judgment. It's as simple as that. Okay.

DR. RANSOM: Aren't you talking about different codes, though?

MR. ORECHWA: No.

DR. RANSOM: When you talk about Appendix K versus best estimate.

MR. ORECHWA: What I'm talking about, I don't care if it's RELAP5 track or anything. Forget the code. Code is going to be a tool. I want to talk in a generic way. The code is going to produce numbers. We're going to evaluate those numbers with respect to data.

And I will go through that a little bit later. I hope to make it a bit more inter-ocular. All right. So here is where Framatome is going to be and they will take the classical approach, because the other hasn't been really developed yet.

Okay. The next view graph is for you, Graham, so listen up. This was prepared for you.

CHAIRMAN WALLIS: Am I allowed to ask

questions, then?

MR. ORECHWA: Yes.

DR. BANERJEE: Only if they're intelligent.

CHAIRMAN WALLIS: Ah, the rules of man.

(Laughter)

MR. ORECHWA: Okay. Let's talk for a minute -- let's start at the end and just talk about the two difference between the parametric and non-parametric approach. What we're talking about is a tolerance limit.

Tolerance limit is a number, 5, .7, whatever. It has three parameters, beta, the fraction of the population of interest, or you can interpret it as a probability, gamma, the confidence level that you have in that probability or fraction of the population, and n, the number of observations in the sample; those three things.

What do you do in a non-parametric approach? You start with an assumption. Non-parametric approach and everything starts with an assumption. You're going to hear this over and over again.

It says the population is continuous, redistributed, nothing else. It's a continuous function. No --

CHAIRMAN WALLIS: Does it have to be?

MR. ORECHWA: Yes, it has to. Because you're going to use order statistics, you cannot order two values which have the same value. So no throwing dice. This is an important assumption. If it isn't true, you can't do this.

CHAIRMAN WALLIS: You mean, you can't do it.

MR. ORECHWA: No one can. You don't -- you can't define an order statistic.

CHAIRMAN WALLIS: Well, it seemed to me that if you're asking for a --

MR. ORECHWA: Don't seem. You can't define an order statistic.

CHAIRMAN WALLIS: -- you're asking for a 95^{th} percentile.

MR. ORECHWA: Wait for the percent.

CHAIRMAN WALLIS: Then all you need is the 95^{th} percentile exists, and it doesn't mean to say that the rest of the distribution has to be continual.

MR. ORECHWA: You start -- you have to go through the proof. You start with the assumption of a continuous function, okay?

CHAIRMAN WALLIS: Okay. Well, let us

agree to start with an assumption.

MR. ORECHWA: Statistical theory then tells you that there is a functional relationship between these three numbers. So you have not taken a sample yet. They don't know what the data is, nothing, but given the fraction of the population and the confidence level, I can compute the end as to how many samples I should take.

So I haven't done anything yet. I'm still sitting at home. I haven't gone anywhere. Okay. Once I have n, then I go take my sample. I order my values and I get my winner. So you're starting with a choice of what your beta and gamma, what your probability and what your confidence is.

In the parametric method, what do you do? The assumption in the parametric method is that the population distribution is known. I put quotes on it because we never know the distribution. We have -- we know something roughly.

To know the distribution, statistical theory says you go out and you get some data. How many data points do I take? That I choose, a priori. It's a hypothesis. So say I need five or 50. I don't derive that.

I go out and get data. Based on that

data, I estimate a distribution, say normal, a vector of means and variance, co-variance matrixes. There are no co-variances here. I don't know what the distribution is.

They're only here and they came from the data. So in the parametric method you're starting with the data.

DR. BANERJEE: So you can derive from that the distribution if you know --

MR. ORECHWA: You derive the distribution. You take the sample. You derive the parameters of the distribution. That's why it's parametric. Once you have the parameters, based on this distribution you say, for a 95 confidence what is going to be beta. You compute. Given that, you say, I want this confidence level.

CHAIRMAN WALLIS: So in the parametric method you need more information because you have to estimate --

MR. ORECHWA: Up front.

CHAIRMAN WALLIS: -- you have to estimate the distribution.

MR. ORECHWA: Yes.

CHAIRMAN WALLIS: So if you did both of them with the same problem you'd expect your answers to be compatible and reasonably descriptive --

MR. ORECHWA: No.

CHAIRMAN WALLIS: -- of the same problem.

MR. ORECHWA: No. You have -- you're starting with --

CHAIRMAN WALLIS: I don't expect to get a different answer.

MR. ORECHWA: -- far less information. You were starting with nothing.

CHAIRMAN WALLIS: Yes. But if you want -- yes. But then you look at data afterwards. You can always look at data when, you know, you have the data afterwards.

MR. ORECHWA: Oh, I see what you mean. The data should come close, yes.

CHAIRMAN WALLIS: What does it tell you about, and it should be consistent.

MR. ORECHWA: The thing will be consistent.

CHAIRMAN WALLIS: Right.

MR. ORECHWA: But you have far less information when in a non-parametric method. See, the trick is this -- it's in the end. Here you are, it's predetermined what end you're going to choose for this. Here, you need to choose it and then go out and compute.

DR. BANERJEE: Usually, you have to have enough data to get the higher order moments to get the distribution like --

MR. ORECHWA: For this.

DR. BANERJEE: Usually, the skewness and the peakedness is needed, as well, to get the proper distribution for the parametric approach. So you need quite a bit of data.

MR. ORECHWA: Yes. Once you have your data you can do whatever you want.

DR. BANERJEE: Yes. But you need a lot of data to get a good estimate.

MR. ORECHWA: Well, I don't know. That depends how good your data is. The point is, it's not the quality of the data; it's you have to go get data first. And you have to decide how much data with almost no information except maybe some thought in a dream or something.

Here, you choose what you want and it tells you how many data you need. Okay. So that's the story. All right. Thank you very much. All right. Let's change --

CHAIRMAN WALLIS: Which of those ways is the straight and narrow and which is the primrose path of dalliance?

(Laughter)

CHAIRMAN WALLIS: Either way is acceptable, right?

MR. ORECHWA: Of course.

CHAIRMAN WALLIS: Okay. Good. There's no judgment.

MR. ORECHWA: No, there is no judgment in this.

CHAIRMAN WALLIS: Okay. Good.

MR. ORECHWA: But it's just, you have to realize what information is being carried through and how you're arriving at it. Okay. And in different cases it might be -- you know -- in some cases you may not be able to even do one of the non-conservative -- it's just that type of thing.

But there is no panacea in either of them. That's the issue here. Okay. Let me now try something out on you guys. All right. We're going to attack the methodology itself. Again, I'm not going to solve any codes.

The code is basically not the issue here. What I want to do is give a formal solution to the problem. What is the problem? The objective is to estimate the performance figure of merit, peak cladding temperature, oxidation, whatever, at some thermal-hydraulic conditions.

I mean, that's what RELAP does. That's what basically it solves. The tool is RELAP or some other codes. What I want to focus on is what are the ingredients in the methodology. We have measured results of a test.

We have the computed results of the test. We have measured results of a LOCA. We don't have this. If we had this, we wouldn't be here. This is what we want. But we have computed the results, and we could compute anything.

So you can just go out and compute. How do we get this? Let me just -- the notation I'm going to use. On this side we have whether the parameter is measured or calculated. These are the thermal-hydraulic conditions.

Are they tests or are they LOCA? By LOCA, I mean we have a manifold which is all LOCA and in between there are test specs spattered around. Okay. Now, in order to solve this, I'm going to solve it formally, like mathematicians do formal, you know.

My advisor used to call it Polish mathematics because at that time in transfer theory there were two Polish guys. They worked in bannock space and the answer was always the resolvent on the source equals the resolvent operator, which we all know is lambda I minus inverse. And that was the answer.

But I'm going to do Polish mathematics for that reason. Let me bring the sum total of my education -- I need to bring the sum total of my education to this -- bear on this problem.

CHAIRMAN WALLIS: That's a big package, then.

MR. ORECHWA: You will find out what it is. My high school teacher, my algebra high school teacher told me, it's all a matter of expressing what you don't know in terms of what you do know. That's principle one.

Then I went on to university with this principle and I was not a very serious student, but I had the good fortune to go to a university where mathematics was taken very serious, and teaching was taken very serious.

And in my -- I think it was second year algebra class, something to do with Jordan canonical forms or whatever. I don't remember. Teacher proved the theorem, goes through the theorem and then we're discussing kind of the results of it and implications, and the student asks the question, and the question was basically conjecture.

Well, professor says, okay, let's just see if we can prove this. So he writes down, if blah, blah, blah, then such and such and such. And then he starts proving, proving, proving, and the thing is just not going anywhere.

It just isn't happening. So then he turns around and says, now you shall mathematics in action. He goes up, changes a word in the if statement, goes back to the proof, QED falls right out. So the key is, you got to get the right assumptions up there.

CHAIRMAN WALLIS: So he worked back from the answer.

MR. ORECHWA: That's right. So what do we need here? What did I take away from that? See, the teaching was so good you could pick things up by osmosis, even for -- what I'm going to assume is that in our manifold of LOCA conditions that the test data is dense, okay, in the mathematical sense that it's dense.

So whenever you're at some place, some LOCA place, you're close to a test. It's like if you -- it's like a cherry pie, okay. The tests are the cherries spread out. Then -- okay. Then I went to graduate school.

So we still can't connect this. Now, we have a -- we know what we're supposed to do. We have an assumption. How do we --

CHAIRMAN WALLIS: I'm still waiting to know what insight you got when you came to the NRC after all this schooling.

(Laughter)

MR. ORECHWA: I went to graduate school and what a rude awakening in graduate school. These guys really expected you to do something. It's not just messing around like that. And you're up against the wall with this analytic expression and you learn very quickly, well, you expand in Taylor series. Okay. And then finally --

CHAIRMAN WALLIS: Well, you waited to graduate school before you heard about Taylor?

MR. ORECHWA: Of course. I had to do something. So and then Feinman (phonetic) says you should never consider anything beyond first order and you always listened to him, of course. Only losers go and work in higher orders.

CHAIRMAN WALLIS: So you've at last discovered the differential calculus, huh?

MR. ORECHWA: Right. So --

(Laughter)

CHAIRMAN WALLIS: Well, we've got to get to the point of this, Yuri.

MR. ORECHWA: So the point of this is the following. Now, what tools do we have? We have that the tests are dense in LOCA space and we have a Taylor series, first order Taylor extension. We can expand this, the LOCAs about the tests. Okay.

And we get an expression. If we take the ratio of that expression we get our -- and throw out all higher terms and all that, we can get the following relationship. All right. You can do that for homework.

Now, you may laugh, Graham, but I'd like to know what is in the solution algorithm of RELAP that goes beyond the assumption of density and Taylor approximation, or can be formulated from that. You're integrating in time.

You're going from one thermal-hydraulic condition. You want to know what it is from T to T plus delta-T. How do you get -- you have to solve it at those other thermal-hydraulic conditions. All you do generally is do a thermal -- Taylor expansion.

So I don't think it's that far, making those assumptions from what you do fundamentally at the most basic level.

MR. LANDRY: Except when you change flow regimes.

MR. ORECHWA: Yes, flow regimes. But given a flow regime, I mean, when you're solving the equation at a node for one point, they're doing nothing else. So we're talking about the basic characteristics of the whole thing.

Now, okay. So here we have what we wanted, okay, and we have three terms. And I'd like to interpret these terms. And here's where you're going to learn that I did learn something when I came to NRC.

Okay. This is the calculation of the parameter calculated of LOCA. This is what RELAP calculates for one shot. Okay. I'm going to assume that this has been beaten to death. All the models are good and all the whatever it is.

Everything is fine. It comes up with an answer close to it. Let me look at --

DR. BANERJEE: Dr. Orechwa, are you going to take it away?

MR. ORECHWA: No. I'm going to come back to it. I want to first discuss this, okay.

DR. BANERJEE: Say there's a vector

anywhere else.

MR. ORECHWA: Yes. It's a big, big vector. It has lots of these. It's thermal-hydraulic conditions, velocities, densities, voids, et cetera, et cetera, whatever defines your thermal-hydraulic condition.

DR. BANERJEE: Right.

MR. ORECHWA: Whatever you need in order to compute the cladding temperature.

CHAIRMAN WALLIS: So your first slide -- your last slide was talking about the sensitivity of P measured LOCA to changes in theta?

MR. ORECHWA: Yes. Right. Let me -- now, if we look at what is the difference between scaling and validation. If we are at fixed thermal-hydraulic conditions and we take measurements at those conditions and we do a calculation, we're doing validation. Okay.

If we are looking at measurements, at test conditions and the LOCA conditions, we're going from one thermal-hydraulic condition to the next, okay. That's scaling. All right. At least that's what I call it. If -- so we're here, P, at theta.

Here we are P at theta plus delta theta. Okay. That is, we need to get from this thermal-hydraulic condition to this, we need to know the derivative. That is the quantity that's necessary, to go from point A to point B.

Here we are at the same theta. You don't need anything. So if you look back on the previous slide -- if I can get it up there -- okay, this term is just a ratio at the same thermal-hydraulic conditions, okay, and these are ratios.

So we need the -- I mean, not ratios. These are derivatives. We need to know at the places where we have data we need to know the derivative of the quantity. This is -- it's the same place, the same thermal-hydraulic conditions, but how do they change in the measurements; how does it change.

CHAIRMAN WALLIS: Now, theta is an n-dimensional variable.

MR. ORECHWA: Yes. It's n-dimensional. You're right.

DR. BANERJEE: So is that a summation, like --

MR. ORECHWA: I don't want to go there, okay. Let's just stick to heuristic.

DR. BANERJEE: Okay.

MR. ORECHWA: I'm trying to show what form it was in.

CHAIRMAN WALLIS: I'm trying to figure out what your formula is. I'm having trouble with the formula.

DR. BANERJEE: But don't go away from that.

CHAIRMAN WALLIS: We need to understand what you're doing there.

MR. ORECHWA: What I'm saying is, let's define this, what you do -- what I'm doing is the next slide, what I learned at NRC.

CHAIRMAN WALLIS: All right.

DR. BANERJEE: But don't go away.

CHAIRMAN WALLIS: Don't go away from that.

MR. ORECHWA: Well, I'll bring it back, but can I bring this up?

CHAIRMAN WALLIS: You seem to be claiming that you --

MR. BOEHNERT: Yes, go ahead.

CHAIRMAN WALLIS: -- can do something about predicting the LOCA just from DP/Dtheta --

MR. ORECHWA: No. Wait. Wait. Wait. Wait. Wait. Wait.

CHAIRMAN WALLIS: -- during --

MR. ORECHWA: Wait. Wait. Wait. Wait. Wait. Wait. Don't get carried away. Don't get carried away.

CHAIRMAN WALLIS: Well, you -- no, you have to answer it. You keep flashing up your key place and then take it away. You can't play that game.

MR. ORECHWA: All right. I was going --

DR. BANERJEE: Keep it on the other one.

MR. ORECHWA: Yes, that's okay. I'll please him; just leave it.

CHAIRMAN WALLIS: You're claiming that what you know about --

MR. ORECHWA: What I'm claiming --

CHAIRMAN WALLIS: -- what you know about the --

MR. ORECHWA: -- formally, where's the -- if -- in order to know what we want to know at LOCA conditions --

CHAIRMAN WALLIS: Plus what happens in a real LOCA.

MR. ORECHWA: A real LOCA. We calculate what happens.

CHAIRMAN WALLIS: Right.

MR. ORECHWA: At the real LOCA. We correct this information by looking at the ratio at this ratio.

CHAIRMAN WALLIS: That's an assumption.

MR. ORECHWA: No.

CHAIRMAN WALLIS: Yes, it is.

MR. ORECHWA: This is all -- what did these guys show for the last few days.

CHAIRMAN WALLIS: It's assumption and similarities.

MR. ORECHWA: This is --

CHAIRMAN WALLIS: Assumption of scalability, then.

MR. ORECHWA: No. This is the uncertainty. This is past versus -- past calculated versus measured at a test.

CHAIRMAN WALLIS: How does the test --

MR. ORECHWA: Scaling, I said, is you go from -- to get further out to the next, to the next thermal-hydraulic condition.

CHAIRMAN WALLIS: Yes, but you --

MR. ORECHWA: You start at a test.

CHAIRMAN WALLIS: You first assume scalability in your first four factors.

MR. ORECHWA: No. No.

CHAIRMAN WALLIS: Yes, because you're relating P measured LOCA. You're saying the correction factor for P measured LOCA to P calc is the same as for P measured test for P calc, plus some sensitivity to delta theta.

MR. ORECHWA: Nonsense.

CHAIRMAN WALLIS: Well, you're trying to explain it so we understand it doesn't do any good if we don't understand it.

MR. ORECHWA: Well, I'm trying to explain it. You have to be receptive --

CHAIRMAN WALLIS: Yes. So you have to be patient.

MR. ORECHWA: -- to my explanation. Okay.

CHAIRMAN WALLIS: No, that's not the way education works. We have to understand it.

MR. ORECHWA: But that's a calculus -- not a correlation --

CHAIRMAN WALLIS: Well, if we understood -- don't understand it, we can't do anything with it at all. Anyway, I understand this figure. That doesn't say anything. Let's go to the -- does the equation say anything. That's what I'm trying to find out.

MR. ORECHWA: Okay. Here's the equation in pieces, okay?

CHAIRMAN WALLIS: Okay.

MR. ORECHWA: This -- the calculation of the LOCA with the applicability, the question is, can you do that, do you -- is my code good enough to get close to the answer. We know it is slightly off; let's say slightly off.

Formally, you would correct that by comparing a test, the measurements of the test to a calculation of the test. Formally, you would do that. What would you compare in order to show scalability? Like I said, scalability, you're going from some thermal-hydraulic conditions to another.

To get from one to the other you need to know the derivative from -- at where you're starting. That's this picture that you don't like.

CHAIRMAN WALLIS: Well, scaling to me means going from one size, like a test, to another size.

MR. ORECHWA: Right.

CHAIRMAN WALLIS: And I don't understand how -- DP/Dtheta in a test or DP/Dtheta coded for a test tells you anything about the real LOCA because it's at a different scale. It doesn't say anything about --

MR. ORECHWA: The real LOCA is the point -- look, can't you understand, there is a manifold. There's a manifold with a bunch of --

CHAIRMAN WALLIS: Well, you don't seem to understand my question.

MR. ORECHWA: Yes, I do, but --

CHAIRMAN WALLIS: Well, then you listen to the question.

MR. ORECHWA: -- and I'm telling you --

CHAIRMAN WALLIS: You listen to the question, please, and listen to the question. The third bullet you have DP/Dtheta measured test, which is a function of the test, right? You have DP/Dtheta calculated test, which is a function of the test.

MR. ORECHWA: Right.

CHAIRMAN WALLIS: But you don't understand -- I don't understand how something measured at a scale or calculated at a scale, low scale, can't tell you directly information about what happens without scaling.

MR. ORECHWA: At a different --

DR. KRESS: You have an assumption that all these data points bunch around the real answer.

MR. ORECHWA: That's right. It's dense.

DR. KRESS: And they sort of --

MR. ORECHWA: That's the whole assumption of density.

DR. KRESS: That's your dense assumption in there and you just have to look at it as a bunch --

MR. ORECHWA: It's a formal argument and the point is that in scaling you're going to need a little bit more than just a ratio. You need to know how you -- because you're going to a different place than where the tests are.

CHAIRMAN WALLIS: Sensitivity to theta.

MR. ORECHWA: Right.

CHAIRMAN WALLIS: Sensitivity to changes in theta.

MR. ORECHWA: And this is exactly. These two terms, if you ratio them, it's like elasticity in economics.

CHAIRMAN WALLIS: Well, again, you see, my problem is that this equation here is only a function of the lower scale. It only measures things at the lower scale. DP/Dtheta at the lower scale, whether it's tests or measurement, it doesn't tell me what DP/Dtheta is at the high scale.

MR. ORECHWA: Look, the point is, what type of information do you need in scaling? In scaling you need derivative information. For uncertainty, you just need the ratio to compute the bias and the distribution of the bias in order to do the correction.

CHAIRMAN WALLIS: I'm not sure if it's

derivative.

MR. ORECHWA: In this case --

CHAIRMAN WALLIS: It could be that at the higher scale some other phenomenon happens.

MR. ORECHWA: No. Forget --

DR. BANERJEE: No. I think your assumption is there's a one to one mapping from the --

CHAIRMAN WALLIS: We've already assumed that it's good scaling.

DR. BANERJEE: -- test into the LOCA scaling.

CHAIRMAN WALLIS: We've already assumed it's good scaling.

DR. BANERJEE: I don't think that bias ratio will hold. You can divide P measured by P calc, I know.

MR. ORECHWA: This is one component, what I'm saying.

DR. BANERJEE: No, that's okay, but the left-hand side, if you go to the previous equation, the right-hand equation -- if you wrote -- if you divided the left-hand side by P calc LOCA, all it means is that the distortion is the bias. If you measured --

MR. ORECHWA: That's right.

DR. BANERJEE: Is a distortion of the bias, but it assumes there is a mapping from these to that. And what Graham is saying, suppose there's a nonlinearity here.

MR. ORECHWA: The mapping is taking care of this.

DR. BANERJEE: Yes, right.

CHAIRMAN WALLIS: Right.

MR. ORECHWA: But there is a --

DR. BANERJEE: Where there is a mapping, but that assumes there is a mapping.

MR. ORECHWA: There is a mapping, exactly.

DR. BANERJEE: Yes.

MR. ORECHWA: That's why you're dense and the mapping is the Taylor expression.

DR. BANERJEE: The question he's asking is that it can be phenomenon which is not there.

MR. ORECHWA: Yes, that's right. And then you --

DR. BANERJEE: In which case, you cannot map.

MR. ORECHWA: Right. Just listen. The issue is uncertainty. The question is, where are the uncertainties coming from.

CHAIRMAN WALLIS: But it's not a question of scaling.

MR. ORECHWA: You mean, scaling is not something that is an uncertainty in all this business?

CHAIRMAN WALLIS: No. I say that the question, scaling question is whether or not your phenomena and the test are the same, and equate -- the same equation as on the first test.

MR. ORECHWA: Are you going to accept this? Given my formalism, what Framatome is doing, in my view, they're -- this is their big RELAP, S-RELAP calculation. They go through a bunch of uncertainty analysis with separate effects tests.

The discussion of scaling is about five pages and it says there is none. This is one.

CHAIRMAN WALLIS: Okay. Does your equation give any insight into whether there is or is not scaling?

MR. ORECHWA: My equation says that you have -- I'm not saying what there is. I'm telling you what to look at. I'm saying you got to look at the derivative of the parameter of interest in relation to the thermal-hydraulic parameters, in principle.

CHAIRMAN WALLIS: Which is -- that's a sensitivity --

MR. ORECHWA: How you do that is a

different question.

CHAIRMAN WALLIS: It's a sensitivity study.

MR. ORECHWA: I'm giving a formal argument and I gave a formal relationship where all these three things come in, your base calculation, your uncertainty analysis and the uncertainty associated with the scaling. Okay.

These should be addressed if you're going to talk about uncertainty with regard to a code, in my view. Okay. Please. I won't go into scaling. I don't want to go down that road.

CHAIRMAN WALLIS: So you're saying that Framatome should --

MR. ORECHWA: Graham, you won't follow me.

CHAIRMAN WALLIS: -- you're going to want to require that Framatome evaluate these DP/Dthetas in some way?

MR. ORECHWA: No.

CHAIRMAN WALLIS: No?

MR. ORECHWA: I want them to evaluate scaling a little bit more than in five pages, given all the work that's done.

CHAIRMAN WALLIS: How do you want them to evaluate scaling?

MR. ORECHWA: That's not my problem. I'm only a reviewer.

CHAIRMAN WALLIS: Why are you telling me all this stuff about DP/Dtheta if it isn't relevant?

MR. ORECHWA: I'm saying that if you -- the ratio of two -- just one point. Okay.

CHAIRMAN WALLIS: We're going to move onto the next slide.

MR. ORECHWA: The derivative, you have to have more information about the test than just the data. That's the whole thing, and you just want to throw scaling out. All right. Any --

CHAIRMAN WALLIS: I think scaling is an important question and it should be evaluated in a rigorous way.

MR. ORECHWA: Anyway, anything else, Graham?

CHAIRMAN WALLIS: Well, I'm still eager to learn, but I'm not sure --

MR. ORECHWA: That I'm the proper teacher?

CHAIRMAN WALLIS: -- what I'm learning.

MR. ORECHWA: Or the proper --

CHAIRMAN WALLIS: So maybe we should go on to your next slide.

MR. ORECHWA: Yes, all right. Let me just get organized here for a second. Okay. So where are we? All right. Let's go now to the answers, all right?

DR. BANERJEE: So let's assume that the expression you wrote was correct.

MR. ORECHWA: Formally correct.

CHAIRMAN WALLIS: Formally correct.

MR. ORECHWA: I emphasize the --

DR. BANERJEE: So what is the consequences of that?

MR. ORECHWA: The consequences are if you're looking at -- if you are checking data, okay, that if you're uncertainty, the first uncertainty with regard to bias is just a ratio of the values at the thermal-hydraulic conditions, if you are trying to correct for scaling, involved in that expression are derivatives.

These always contain a lot more uncertainty, the over-analyzed data. And so by just saying that they don't matter to me is implausible.

DR. BANERJEE: So what you're saying is that the bias is amplified in some way by --

MR. ORECHWA: By scaling.

DR. BANERJEE: -- by these other derivatives there.

MR. ORECHWA: In principle it is. This is usually -- it's much more difficult to you, deal with derivatives of the data than with the data itself.

DR. BANERJEE: So the sensitivity of the calculations and the sensitivity of the tests, of the experiments --

MR. ORECHWA: Yes.

DR. BANERJEE: -- at the test scale have to be added in some way to increase the bias.

MR. ORECHWA: That's right, how do the tests connect. Remember, the assumption is that the tests are dense in the manifold of all the parameters over which we consider LOCAs may have. All right. So then in that context, with that assumption that it's dense, we can do certain things.

Whether you have in reality that kind of data and whether you can make those statements, that's a completely different issue.

DR. BANERJEE: So all you've done is a Taylor series expansion about --

MR. ORECHWA: About the test.

DR. BANERJEE: -- the measurement.

MR. ORECHWA: Right.

DR. BANERJEE: Test and calculation at test.

MR. ORECHWA: Right, because we don't have the -- we are interested not at the test, but the --

DR. BANERJEE: Then why didn't you also do a Taylor series expansion of the calculations at LOCA, then?

MR. ORECHWA: Because you -- what is your reference point? You say I know the tests. You're interested in what you calculate. So you know, when you expand, what are you going to expand about? You expand about what you know.

DR. BANERJEE: Right. You know the test, but you also have the calculations at test conditions. You have measurement at test conditions, calculations at test conditions, and both of these you have done tests --

MR. ORECHWA: You also expand the test -- you expand the terms about the test condition. So you take the derivative at the test conditions.

DR. BANERJEE: Yes.

MR. ORECHWA: See, so you need more information at the test --

DR. BANERJEE: I guess if you wrote that expression as the quotient on the left-hand side it would make more sense, because then you are looking at the distortion of the bias.

MR. ORECHWA: Well, fine. I look at it differently.

DR. BANERJEE: Yes.

MR. ORECHWA: Okay. I'm looking at it, a correction to --

DR. BANERJEE: Well, I can see how you come to that expression.

MR. ORECHWA: Okay.

DR. BANERJEE: Yes.

MR. ORECHWA: The point is to look at what the information content is of that expression, and it basically follows what is it, the same terms that we use in CS whatever the methodology.

MR. BOEHNERT: CSAU.

MR. ORECHWA: CSAU methodology.

DR. KRESS: Part of the trouble is your delta theta may be very large, and Taylor --

MR. ORECHWA: Well, that's a computation, yes.

DR. KRESS: -- series breaks that -- yes.

MR. ORECHWA: You're not going to compute anything like that.

DR. KRESS: No. No.

MR. ORECHWA: That's not the point.

DR. KRESS: But in principle this would be a way to look at it.

MR. ORECHWA: But this is a way of looking what type information do you want and what does it mean, the type of information. Okay.

CHAIRMAN WALLIS: Well, I'm very -- I would think you would want to express delta P measured LOCA as a function of delta theta.

MR. ORECHWA: All right. If you want to, Graham, you can --

CHAIRMAN WALLIS: I don't see any delta on the P measured LOCAs.

MR. ORECHWA: -- you can express it any way you want.

CHAIRMAN WALLIS: Okay.

DR. BANERJEE: Anyway, what he is saying there is the change -- there's an increase in the bias that --

CHAIRMAN WALLIS: Well, he must have a delta P measured LOCA. I mean, I think what he's saying is that you need to look at the variations in these DP/Dtheta in order to tell how sensitive your P measured LOCA is to your delta theta.

MR. ORECHWA: The LOCA --

CHAIRMAN WALLIS: I don't see any delta P measured LOCA here. So I'm not quite sure what I'm seeing.

DR. BANERJEE: Because he doesn't have a delta P.

DR. KRESS: He doesn't have that. He can't --

DR. BANERJEE: He is not -- he is expanding about the test scaling.

MR. ORECHWA: LOCA is anything outside the test in the manifold of the thermal-hydraulic parameter.

DR. BANERJEE: Which is why I said you should express the left-hand side of the --

CHAIRMAN WALLIS: Well, I guess if we're not going to use it we should move away from this equation.

MR. ORECHWA: But I -- you know -- it's a cautionary tale.

CHAIRMAN WALLIS: Is he going to use it?

DR. KRESS: I don't know.

CHAIRMAN WALLIS: I don't think so.

MR. ORECHWA: All right. Let's get to the answers.

CHAIRMAN WALLIS: Yes -- we'll think about it.

MR. ORECHWA: All right. Food for

thought, Graham.

CHAIRMAN WALLIS: All right.

DR. KRESS: We'll think about it.

MR. ORECHWA: Good.

DR. BANERJEE: He's used the binomial expansion, as well, just to the first.

DR. KRESS: He just took the first term, then.

CHAIRMAN WALLIS: We'll see what we can do with it.

DR. BANERJEE: I think we can do that.

MR. ORECHWA: It's a homework problem. Let's go.

CHAIRMAN WALLIS: It's a homework problem, yes. Okay. So now, we're getting back to Framatome.

MR. ORECHWA: Framatome, and what Framatome said. Okay. Initially, in their initial submission they gave, this was the bottom line. The methodology, which uses S-RELAP5 data, it uses a statistical approach and that statistical approach is non-parametric.

And they came up with this and I already about a year ago discussed this with you. We went over it, that the results based on 59 cases is okay if you are only considering one variable, PCT. If you want to make a probabilistic statement about the output variables, 59 is enough -- not enough.

It's just basically, you need more information, okay? And remember that you're starting, this is a derived quantity. The F that I showed the relation between the beta, gamma and n is different slightly for a different number of outputs.

Okay. So this finally after many, many months of back and forth and et cetera, et cetera, Framatome kind of backed off and they appealed to Regulatory Guide 1.157 in the following statement, which they always write.

What I bolded here is the words I want to emphasize what this thing is about, no matter what it says. It talks about probability and it talks about criteria over and over. Probability and criteria. If you now look at the currently -- where is it -- current submission of Framatome they want us to accept, at least that's what the last information that I got, is the following, that there are still three criteria.

There are still 59 samples. Fifty-nine samples, 95/95 PCT is fine. This they say, given these 59 samples, where we make this statement, it happens that the result for the -- what is it --

maximum nodal oxidation is this, and it compares very favorably with the limit.

Same thing here, that this is a result and this is favorable. Let me -- there is no -- where is the word "probability" here? This is an example of 59. This is not statistics. Somebody ran 59 cases and got a result.

If they run 59 cases again they're going to get a different result. This is not probability.

DR. KRESS: But each of those 59 cases represent to some extent the full distribution.

MR. ORECHWA: Then okay. Let's do it. Let me show you what the answer -- we'll look in the back of the book.

DR. KRESS: Okay. You got to look for the answer.

MR. ORECHWA: If we look in back of the book we get this: "Number of runs 59, number of criteria, .95/.95, 95/95. In order -- remember how non-parametric statistics goes. It starts over here. For using the relationship for this we need 124 runs.

Given that Framatome doesn't want to get off of 59, if I choose my confidence level at 95, my -- this is the probability. If I choose my probability, this is the confidence.

CHAIRMAN WALLIS: That is true, apart from the fact that they submitted these graphs and the statistical distributions showing how far away you were from the 17 percent with the total observation that PCT appeared to be a far more stringent criterion, based on all these runs, than these other criteria.

Therefore, there was a very high probability that if you met the PCT criterion, you're going to meet the other ones because in order to get to 17 percent oxidation you'd have to be way off scale in terms of the results. So that was additional information that they submitted.

DR. KRESS: Yes. That's what I was saying, that they --

MR. ORECHWA: But you can't use that information if you're going to do non-parametric statistics.

CHAIRMAN WALLIS: But you've got to use new information if it's relevant.

MR. ORECHWA: If it's relevant? How do you get the probability out of that information?

CHAIRMAN WALLIS: Well, I think --

DR. KRESS: The curve you get for --

MR. ORECHWA: No. No. Wait --

DR. KRESS: It's an approximation of the distribution, to some extent.

MR. ORECHWA: Fine. What I'm telling you is the methodology on page 1, what they said, what it -- what they're claiming, we will use a methodology which is primary over the -- even over the code.

CHAIRMAN WALLIS: Well, let's go to --

MR. ORECHWA: We will use statistics, we will use non-parametric statistics. We will arrive at an answer using those.

CHAIRMAN WALLIS: Well, you're --

DR. KRESS: We agree with that -- we agree with you that that's not -- in principle that's wrong.

MR. ORECHWA: Okay. That's all.

DR. KRESS: Yes, we'll agree with that. But we also agree that the new information can be used to justify the 59 runs is sufficient for all three.

CHAIRMAN WALLIS: For instance, let's look at this table --

MR. ORECHWA: Why not --

CHAIRMAN WALLIS: -- your number 124 assumes that these phenomena are independent.

MR. ORECHWA: Doesn't assume anything.

CHAIRMAN WALLIS: Oh, yes, it does.

MR. ORECHWA: It does not.

CHAIRMAN WALLIS: If they are tightly correlated --

MR. ORECHWA: It assume --

CHAIRMAN WALLIS: -- as you are -- if peak clad temperature and oxidation are exactly dependent, one on the other, if you're in the 95^{th} percent --

MR. ORECHWA: Graham, get that out of your head.

CHAIRMAN WALLIS: Listen to me. Listen to me. Well, I'm going to put it on the record and you're going to be quiet.

MR. ORECHWA: Fine. Put it on the record.

CHAIRMAN WALLIS: Right. That if peak clad temperature and oxidation are exactly a function of each other, you can draw straight -- you can plot on a graph one against the other and you get one straight line, then if your results are in the 95^{th} percentile at peak clad temperature they would also be 95^{th} percentile of oxidation level.

Then in that case you only need 59 runs and you succeed with both of them. If they're independent you need more runs; depends upon how they're related to each other.

MR. ORECHWA: Let me go on the record. You didn't learn your lesson on square one. You --

CHAIRMAN WALLIS: But that's so general it doesn't tell me anything at all.

MR. ORECHWA: It doesn't -- it tells you exactly. You get your 50 n runs based on knowing nothing about the distribution, nothing except that it's continuous. You're bringing in information after the fact, after you've chosen n.

DR. KRESS: That's right.

MR. ORECHWA: That's right, but there is a method of doing that. You can't go on and bring in information and change this thing.

CHAIRMAN WALLIS: The same -- this information which is brought after doing the runs.

MR. ORECHWA: This --

CHAIRMAN WALLIS: The correlations.

MR. ORECHWA: -- this is what you're starting with, nothing. That's the whole thing about non-parametric statistics. Why is it non-parametrics? No parameters. What is correlation? It is a parameter in the distribution that you don't know when you're starting out. Get that through your head. (Polish phrase.)

CHAIRMAN WALLIS: But after you have done the runs you learn something. You happen to learn --

MR. ORECHWA: Yes. Then you use phase-in

methods. You update.

CHAIRMAN WALLIS: Well, if you --

MR. ORECHWA: But what are you updating then?

CHAIRMAN WALLIS: That's what we're doing.

MR. ORECHWA: Then you got to do the statistics properly. They said they're going to do statistics. There are methods.

CHAIRMAN WALLIS: Well, I think -- I agree with you. I think it would be very good if instead of this kind of qualitative argument, we could have a more rigorous statistic argument. But I think you'll find that when you do that, that the number of runs is decreased --

MR. ORECHWA: Well --

CHAIRMAN WALLIS: -- from your value.

MR. ORECHWA: What?

CHAIRMAN WALLIS: That I think it would be useful if Framatome --

MR. ORECHWA: But you got to do it.

CHAIRMAN WALLIS: -- instead of representing these qualitative arguments based on some curves, could actually put some numbers in a statistical way on -- to buttress their conclusions. I think that would be very helpful.

MR. ORECHWA: Look at --

CHAIRMAN WALLIS: That's a useful argument.

MR. ORECHWA: I agree that the data that has been presented is probably okay, but they are not -- they're presenting a sort of a good feel type. They're not presenting a probability.

CHAIRMAN WALLIS: That's right. That's why I agree. I agree.

MR. ORECHWA: Which is what Reg Guide calls.

CHAIRMAN WALLIS: It would be very useful if they would do that, but they will not -- if they use that information they will not conclude that they need 124 runs.

MR. ORECHWA: They're going to have to -- that's right, because what they're going to have to do is parametric statistics and they're going to choose n before that.

CHAIRMAN WALLIS: That's a useful idea.

MR. ORECHWA: This is a whole point, that these two things are like night and day. It's like choosing, what is it, forward and backward differencing. You're doing the same thing, but you can end up in very different territories, analogous.

But the regulation or whatever it is, it says probability of the criteria. It doesn't say, feel good because I got a number that is small.

CHAIRMAN WALLIS: It just says --

MR. ORECHWA: We're going to go to another reactor and what are you going to get then?

DR. BANERJEE: There's a point you made, though, which is sort of quite interesting, which is that they have assumed implicitly, I think, a one to one scaling, the slide you showed there.

MR. ORECHWA: Yes, but that's outside of this, my argument with Graham, though.

DR. BANERJEE: That's irrelevant, whether it is outside, but you showed the slide.

MR. ORECHWA: Okay.

DR. RANSOM: That's the question I had. What is going to happen to this other influence coefficient type of thing? Why did you present that? Do you have some conclusion based on your slide five?

DR. BANERJEE: Yes.

MR. ORECHWA: My conclusion is --

DR. RANSOM: In which you presented the --

MR. ORECHWA: -- that in my view that there are certain ways -- that certain things have not been looked at that would contribute to uncertainty.

DR. RANSOM: Are you proposing a way to evaluate these derivatives?

MR. ORECHWA: Absolutely not.

DR. RANSOM: No.

MR. ORECHWA: I'm just saying that there is an area that -- you know -- I don't have the solution.

DR. BANERJEE: What you showed was a slide that -- you showed two slides. One is a slide which said that implicit in the arguments of Framatome are scaling is one, and they've based it on some full-scale tests.

MR. ORECHWA: Right. Right.

DR. BANERJEE: And there are other aspects which are not, and but it may not be that all aspects are full-scale. And then you proposed a sort of a formal relationship for bias which at least allowed you to get a better idea about the scaling. Now, you don't want to stand behind that equation you showed?

MR. ORECHWA: What I tried to show formally, what type of information is important if you're going to consider scaling.

DR. BANERJEE: Right. And that --

MR. ORECHWA: And that now -- and I stand by that that type of information is important. Now, how you do it --

DR. BANERJEE: That's besides the point, yes. But what you showed was I think that just because you have a certain bias based on your test experiments, some of the test experiments would be full-scale, doesn't mean that the LOCA bias measurements to calculations will be the same.

That's basically what you showed, but that you have to look at the sensitivities of both your test scale measurements and your test scale calculations.

MR. ORECHWA: Right.

DR. BANERJEE: Is that correct?

MR. ORECHWA: Right. Right.

DR. BANERJEE: Or right, I mean?

MR. ORECHWA: That's right. That's right. Let me suggest something. I'll stick my neck out on this. I haven't thought it completely through. So Graham, don't jump on my ass right now. I think that the part of scaling, if you look at in response -- I'll set this down -- is analogous a little bit to R squared in regression, I think. It's at least analogous, not one form.

DR. BANERJEE: There may only be four or five values of theta that actually affect --

MR. ORECHWA: Right. Right. No, this could be -- maybe none of them do. Maybe it is one. I don't know. Maybe it's fine.

DR. BANERJEE: But whether this applies to Framatome --

MR. ORECHWA: But that's not --

DR. BANERJEE: -- the problem is not the issue here.

MR. ORECHWA: My presentation, other than the result, okay, of 59 cases and PCT and like that, that's Framatome. The rest is a generic -- are genetic issues, how to deal with uncertainty. But when you say you're going to take a statistical approach, you make certain decisions.

When you come to parametric, non-parametric, it's a crossroads. One you go down one you got to follow it. You can't mix the two. If you want to bring in information, you go and do the Bayesian.

That's a completely different story again. If you are going to follow statistics, so --

CHAIRMAN WALLIS: So I think it --

MR. ORECHWA: I'm telling you the way it is. You want to apply -- you want to whittle it down. That's -- you know -- you're -- but I go on the record to tell you what the story is.

CHAIRMAN WALLIS: Yes. But I think we're saying that the statistical probabilities that you estimate are not independent of what you learn about how these three outputs are related to each other.

MR. ORECHWA: That's right. But then you have to do the analysis.

CHAIRMAN WALLIS: That's right. That's what -- I agree.

MR. ORECHWA: Accordingly to come up with probability.

CHAIRMAN WALLIS: I think we're agreeing. It would be very useful if instead of just saying, look, it's .8 percent compared with 17 percent, the affect could have been put on the basis of some probability.

MR. ORECHWA: I mean, that's what the Reg Guide asks for.

CHAIRMAN WALLIS: Right.

MR. ORECHWA: Okay.

CHAIRMAN WALLIS: So I think we --

MR. ORECHWA: But I mean, my conclusion --

CHAIRMAN WALLIS: -- I think we've appreciated that from your presentation.

MR. ORECHWA: My conclusion is, let's look at the statistics that they are presenting and what does it result in. It does not result in -- you know -- the key thing is, 95 insures that greater probability that the other criteria will not, if. The thing is, it's if then.

CHAIRMAN WALLIS: But if it were that they could look at the actual -- infer some probability distribution for nodal oxidation, from the 59 points that they do have, and if they can then use a statistical argument which has numbers on it, then you might be satisfied, right?

MR. ORECHWA: Well, 59 -- take 59 cases and do it the classical, statistical, parametric way. You should --

CHAIRMAN WALLIS: Right. Okay. So I think it's about time to take a break now?

MR. ORECHWA: Yes, please. I've got to --

CHAIRMAN WALLIS: Listen to anyone else? Do you have any other question, Vic, maybe? Sanjoy? So we could take a break until five past 3:00.

Thank you very much, Yuri.

(Whereupon, the foregoing meeting went off the record at 2:52 p.m. and went back on the record at 3:07 p.m.)

CHAIRMAN WALLIS: Let's come back into

session to hear from Sarah Colpo.

MS. COLPO: Is this one working?

CHAIRMAN WALLIS: I don't know. It's best if you use the mike which you carry around. Do you have a place you can put it?

MS. COLPO: No, that's the thing.

DR. KRESS: Then you have to do this.

CHAIRMAN WALLIS: Then you have --

DR. KRESS: Doesn't it hang around your neck? No.

CHAIRMAN WALLIS: It doesn't hang around your neck?

DR. BANERJEE: Might strangle you.

MS. COLPO: Yes, that's what I'm worried about.

CHAIRMAN WALLIS: Okay. Well, speak into the other one. If you have to stand up maybe you can just grab it and walk around with it.

MS. COLPO: Okay. All right. My name is Sarah Colpo. I'm a reactor engineer in the Reactor Systems Branch of NRR. And my role for this effort was to review the 2D/3-D assessment and also to do a code documentation comparison.

And I did some parametric studies where it was my job to investigate the importance of some of the parameters in the code and report what I found to other members of the team. I want to be clear here that it was not within the scope of my review to make decisions about what to do with this information.

DR. BANERJEE: Do we have a handout?

DR. KRESS: Yes. You didn't get one? Looks like this. Did you get a handout?

DR. BANERJEE: No. Sarah, will you be using this form?

MS. COLPO: I will. Only a couple times, but I will be using it.

(Pause)

MS. COLPO: Okay. In my review of the writeup of the 2D-3D assessment, I compared the writeup to the plots that Framatome ANP provided to see if what they said made any sense.

When it didn't make any sense at all for me I spoke with senior engineers until I understood what was going on, and then went from there. The bottom line for me from that review was that the codes were mostly conservative.

The results were mostly conservative, but I didn't -- I guess being new to this game I have a hard time seeing them as realistic, because I guess I have different expectations since I'm new, I guess.

CHAIRMAN WALLIS: Your expectations are for a closer comparison with data to be realistic, or?

MS. COLPO: Yes, that's what I was thinking.

CHAIRMAN WALLIS: You were surprised by the degree of scatter or?

MS. COLPO: Yes. You know, and like I said, I'm new. So you know, this may be leaps and bounds better than what was around before, but just, I had maybe different expectations. There was one case where the code was not conservative when Framatome ran a UPTF test.

It ended up that there were large oscillations in the pressure and in the lower plenum level in mass. So Framatome suggested that the large oscillations were due to the level tracking model, which is in the bottom node of the lower plenum model.

They thought they'd go ahead and turn that off and when they did the oscillations dampened, but the mass and level in the core were still much lower than the data. So to investigate that, they implemented a 2D lower plenum model.

The results improved. However, in that case the levels in mass in the core was nonconservative. So even though the -- looking at the way that they've chosen to model it, it was mostly conservative, there were occasions where it wasn't.

I have to say that Framatome and the RAI response said that they don't have the intention of modeling the lower plenum as a 2D part of their methodology.

CHAIRMAN WALLIS: I'm wondering what we should conclude from this.

MS. COLPO: Well, that their 1D is good enough.

CHAIRMAN WALLIS: It's not good enough or is good enough? They think it's good enough?

MS. COLPO: They think it's good enough.

DR. RANSOM: 1D where?

DR. BANERJEE: Lower plenum?

DR. RANSOM: Lower plenum.

DR. BANERJEE: Were you -- did you -- when you say conservative, you meant that the predictions in the core were lower in level or something than the experiments?

MS. COLPO: Were --

DR. BANERJEE: Is that what you meant by conservative?

MS. COLPO: Right, that they weren't making assumptions that were the wrong directions.

CHAIRMAN WALLIS: No. The results are conservative, presumably.

MS. COLPO: Pardon me?

CHAIRMAN WALLIS: Not the assumptions. It's the results; you found at the core that the level was predicted to be higher than measured or something? Is that what you mean by conservative?

MS. COLPO: Or --

CHAIRMAN WALLIS: The other way around?

MS. COLPO: -- or the other way around, yes.

CHAIRMAN WALLIS: The other way around is conservative?

MS. COLPO: Right.

DR. RANSOM: There was too much entrainment of water being carried out of the vessel or?

MS. COLPO: Right.

CHAIRMAN WALLIS: So then you passed on your observations to the more experienced members of this team and --

MS. COLPO: The senior engineer.

CHAIRMAN WALLIS: -- they had to decide whether or not to reach some conclusion or how to reach conclusions?

MS. COLPO: Right. I didn't have that difficult task. That was beyond my scope of responsibility.

CHAIRMAN WALLIS: It's a little difficult for this group to reach conclusions, so -- because we haven't seen this in the degree of detail that you have. So is this written up somewhere?

MS. COLPO: It's in the -- I can't remember which documentation chunk it's in, but --

CHAIRMAN WALLIS: I don't know if the individual staff reviews ever get through a documentation that's accessible to ACRS. Maybe we will ask. We will ask Mr. Landry what he concluded from what you told him.

May we ask you now, Ralph?

MR. LANDRY: I think let's go through and hear the whole presentation first.

CHAIRMAN WALLIS: She's going to do the whole thing first. Okay. Okay. We'll come back to you.

MR. LANDRY: Because you have to look at the entire package of the RAIs and the responses to the RAIs to see where we ultimately concluded that --

CHAIRMAN WALLIS: Okay.

MR. LANDRY: -- the realistic large break

LOCA methodology was conservative overall in its predicted capability.

MS. COLPO: All right. So the next thing I worked on was a spot-check of the code and the documentation for consistency. I looked at things, I mean, from just as basic as typos up to, you know, were the units correct. Were the equations matching with what was in the documentation?

And what I found was that there were occasions where the documentation didn't match the code, and that's not to say that the code was wrong, but the documentation was wrong. And Framatome --

CHAIRMAN WALLIS: It seems to me very strange that the code is always right and it's always the documentation that's wrong.

MS. COLPO: Well, I picked the wrong choice of --

CHAIRMAN WALLIS: I would think you'd write the equation first and then put it in the code.

MS. COLPO: Well.

CHAIRMAN WALLIS: Seems to be the other way around. You write the code and then you figure out what the equation must have been.

DR. RANSOM: Well, I wonder if it's possible that different people did the writeup from the development side.

CHAIRMAN WALLIS: That's probably it. It's almost certain.

MR. LANDRY: If I may. Ralph Landry again. The code is written from calculation notebooks and developmental materials. Afterwards, the documentation is prepared for the code. The documentation is not prepared and then the code taken from the documentation.

This has been an ongoing problem that we've had in the past with the National Laboratories back in the early days of the code, that the code should be written. And it was very difficult to get documentation prepared on what was in the code.

And somewhere errors get introduced into the documentation because they're working from hand notes, hand calculations, calculation notebooks and the code, to then write and prepare the documentation of what is in the code.

So this is not a surprise that there are errors in documentation, but not in the code. But if we look at the code then we discover the documentation doesn't match exactly.

MR. SCHROCK: What is the reference for what is correct? Or how do you know when you look at the code whether it's right or wrong?

MS. COLPO: Well, what I did was ask questions. If there was something that appeared in the equations that didn't appear in the documentation I asked the general question, explain this parameter, and then it can go either way.

Well, is it wrong in the code or is it wrong just in the documentation? Did that answer your question?

MR. SCHROCK: I'm not quite sure.

MS. COLPO: Okay.

MR. LANDRY: Virgil, what we end up doing with the RAIs is give the applicant the opportunity to explain to us which is correct, rather than the staff go out and determine which is the correct.

The onus is on the applicant to explain which is correct, and we can then look at the response and look at literature and say, are they describing the correct correlation that we are familiar with from the literature. And can we then conclude that, yes, they're right, the documentation is wrong.

MR. SCHROCK: How do you guard against the possibility that they agree with one another, but are in fact wrong?

MR. LANDRY: That the code and

documentation are the same?

MR. SCHROCK: Are in agreement, but --

MR. LANDRY: But they're both incorrect?

MR. SCHROCK: -- but are incorrect. Yes.

MR. LANDRY: That was one of the reasons we started to do this spot-checking. The committee has requested the staff numerous times to look at individual lines in the coding and make sure things were coded right.

So we started down this path and found in some of the subroutines that there were lines of coding which did not agree with the documentation. We did not go back and start checking the individual lines then against literature when both were in agreement to see that, yes, this was coded right.

As I said earlier, we have to do a snapshot review. We have to pick out particular items to look at and determine, are they correct or not.

MR. SCHROCK: Okay. So the bottom line is, you don't claim it's exhaustive. It's --

MR. LANDRY: No. No.

MR. SCHROCK: -- we've got some measure of errors that are discovered.

MR. LANDRY: This was to give us a snapshot view of, can we spot-check and see something was done correctly. And in our spot-checking, which Sarah did, Sarah found a number of instances where she said, why does this -- what's in the code show something different than this in the documentation, and Framatome would come back then and say, because such and such.

DR. KRESS: The other problem would probably get uncovered by your comparisons with tests, for examples, and by your cross-checking with another code to see if you get the same kind of results, if you had both wrong, document and the code; if something's wrong in there, is in the wrong correlation, for example.

MR. LANDRY: Right.

DR. KRESS: Or wrong form on it, then it would show up in some of your other tests, probably.

MR. LANDRY: Yes. And well, that can give you a gross error.

DR. KRESS: Yes.

MR. LANDRY: Some of the really find errors that may not show, but that is another way and that's another reason why we do confirmatory calculations, and another reason why we did some of the stuff that Sarah's going to talk to, if she can get to it, looking at some of the parameters in parametric studies that she did with the code.

CHAIRMAN WALLIS: But presumably, sometimes you do look at the original document. You have all these papers and you're interested in Forslund-Rohsenow particularly --

MR. LANDRY: Right.

CHAIRMAN WALLIS: -- I would imagine you can't help looking at the equation that's in the published paper, and probably noticing when the documentation is not the same.

MR. ATTARD: Yes. Dr. Wallis, my understanding -- Tony Attard from Reactor Systems. I did that exact thing when Sarah first brought a couple of these questions. I went back to the sources and various textbooks actually, and what happened quite a number of times is that the expression in -- or the equation in the submittals was written slightly different than what it was in the textbook, okay.

And that was enough to kind of just throw things off a bit. But in reality there was just a parameter change from one to the other. So we did check that at the equation level.

MS. COLPO: All right. The next thing I looked at in what I'm spending probably the rest of my time here talking about is the parametric studies.

What they did was focus their review on the most significant parameters.

I varied FWDRAG, VISCOL and post-DNB subroutines. FWDRAG calculate to the wall drag terms. VISCOL calculates the water liquid density or viscosity, I mean. And post --

CHAIRMAN WALLIS: Excuse me. These wall drag terms are what they've been calling the loss coefficients? Or are they something else?

MS. COLPO: I'm not sure.

CHAIRMAN WALLIS: Well, they are a generalized friction factor loss or the places where they don't have loss coefficients? Or what are the FWDRAG? They're wall friction, but most of your -- many of your components have a K loss factor rather than a wall friction or both of them or --

A PARTICIPANT: They got both.

CHAIRMAN WALLIS: They got both. Okay. So you're not varying the loss coefficients. You're varying the friction drag.

DR. BANERJEE: You mean, the total frictional drag and the total losses are about the same in magnitude going around the circuit or what? No -- yes. You said they were very similar.

DR. MARTIN: In the --

MR. BOEHNERT: You need to get the mike, I'm sorry, to get you on the record.

DR. MARTIN: In the momentum equation --

DR. BANERJEE: Right.

DR. MARTIN: -- the formulation, there's -- the order of magnitude obviously is different.

DR. BANERJEE: What is the relative order of magnitude?

DR. MARTIN: Well, I guess it depends on what you're looking at. If you're in a straight pipe but no formula, it's at -- well, at zero.

DR. BANERJEE: Right. But in the typical circuit.

DR. MARTIN: Typical circuit.

DR. BANERJEE: Right. Pick one that you did a calculation for, one of your cases, anyone.

DR. MARTIN: I would say they're on the -- if you're going through a component like a bend it can be on the same order of magnitude. They're not -- you know -- they're not talking about ten to the six type things, and you're probably only talking about ten to the three. It's probably less than that, ten to two.

CHAIRMAN WALLIS: What do you mean by ten to the three?

DR. MARTIN: They can vary kind of in that

-- I mean --

CHAIRMAN WALLIS: Oh, by a factor of 1,000?

DR. MARTIN: At the most maybe 100, if you have -- as long as you have something. And I'm just -- and I'm gauging that based on looking at output, where they -- you know -- we'll have --

DR. BANERJEE: So let's say that --

DR. MARTIN: -- a list of what the F-wall F and form F, we have those outputs and I'm just going on experience there and looking at the output and seeing numbers that are kind of in the ballpark, but sometimes they may be off by 100.

DR. BANERJEE: Well, you want to compare K with 2FL divided by D, right, their equivalent?

DR. MARTIN: Yes. FL over D and --

DR. BANERJEE: Yes, 2FL by the end of the -- or 4 by D if you wish -- against K. And let's say, take a couple of typical cases, we come in from the code leg, go down the down-comer into the lower plenum, what is the relative magnitude of those two in typical terms, K versus 2FL by D?

DR. MARTIN: Well, you'll have a friction loss along the walls everywhere.

DR. BANERJEE: Right.

DR. MARTIN: Just what we've talked about. We'll apply the form loss, or basically our guideline is we will go to IDLECHECK or Crane or something to determine the form losses. So certainly, when the -- at the cold leg to the down-comer there is a calculated form loss there that's put in there.

Then you won't have anything unless there's a geometry change, you know, up the area of the down-comer will vary again at the appropriate junction. There'll be something there.

DR. BANERJEE: No. I'm just trying to get a feel for it.

DR. MARTIN: And I'm saying it's --

DR. BANERJEE: What's the relative magnitude of these?

DR. MARTIN: -- it's going to be very close, but you know, depending what you have, it may be -- you know -- up to 100 DIP off.

DR. BANERJEE: Let me make a statement and see if it's correct, then. Except in the core, form losses dominate frictional losses. Is this correct or not?

DR. MARTIN: Yes, when you have them. I mean, in straight pipes you're not going to have them, right. I mean, they're --

DR. BANERJEE: Yes. But let's say for a circuit, leaving aside the core, if I make the statement, form losses will dominate over frictional losses, is that correct for the whole circuit or not?

DR. MARTIN: I'll agree with you.

DR. BANERJEE: Okay. In the core it's the other way around. Okay.

MS. COLPO: Okay. In these parametric studies the FWDRAG subroutine had the most significant affect on the peak cladding temperature, and given that that was the case, the FWDRAG was the subroutine that I chose to focus my parametric studies on.

DR. KRESS: Now, FWDRAG is a subroutine?

MS. COLPO: It's a subroutine that calculates the --

DR. KRESS: How do you parametrizie the subroutine with --

MS. COLPO: Well, what I did was, the -- I went into the code and at the very bottom where it computes the wall drag term. I introduced a multiplier.

DR. KRESS: Oh, I see. Okay.

MS. COLPO: Of -- well, depending on which case we're talking about, of two or ten or .1.

DR. KRESS: You went to the bottom line

result.

MS. COLPO: Right. Right.

DR. RANSOM: Normally, the code calculates one for the liquid and one for the vapor.

MS. COLPO: I did them both.

DR. RANSOM: You did them both. I had one more question: why you selected these particular parameters?

MS. COLPO: Well, I -- to tell you the truth, I chose VISCOL because I've always -- just my own personal choice that I've always thought liquid viscosity seemed to be a pretty important parameter. And so that was my own curiosity.

I chose the wall drag because I just think it would be important, and I chose post-DNB because I just thought it seemed like it would be an important one; nothing more than that.

MR. SCHROCK: So VISCOL essentially --

DR. RANSOM: So of you chose interface drag you probably found a really big affect.

MS. COLPO: Actually, I believe somebody has already looked at interface drag, interfacial drag. Is that correct, Ralph?

MR. LANDRY: It's been looked at but we haven't looked at it with this code.

DR. RANSOM: I was just curious why, you know, you chose the parameters you did and I guess just to see what the sensitivities were?

MS. COLPO: Right.

CHAIRMAN WALLIS: The interface drag makes quite a difference in the things like pool swell and, you know, entrainment and carryover.

MS. COLPO: Well, unfortunately, I didn't choose that one. I could certainly do that.

DR. BANERJEE: This is just a frictional drag. So you -- because you were interested in the core, primarily, I take it?

MS. COLPO: Yes.

DR. BANERJEE: Okay. So you didn't change any of the loss factors.

MS. COLPO: No.

MR. LANDRY: Keep in mind that we were trying to keep this fairly easy to understand. This was -- for this type of review this was a first shot at doing something of this nature. So we were trying to keep it at a range where we understood what was going on and where we thought we could see an affect. We wanted to see what would happen with the code.

DR. RANSOM: Well, the other one would be, were the ranges that you chose consistent with the non-parametric studies that they made using the statistical approach?

MR. LANDRY: No. We chose ranges to try to make an affect.

DR. RANSOM: I mean, were they bigger or smaller or -- is that correct?

MS. COLPO: I didn't even look at the statistical --

R. RANSOM: What Framatome did?

MS. COLPO: Yes.

DR. RANSOM: Or what they used for their multipliers or range?

MS. COLPO: I just, like Ralph said, I just picked ones to see where I would get an affect, or if there would be an affect at all.

MR. LANDRY: Every time we do this we get a little smarter. So we're --

DR. RANSOM: No, I'm not objecting to doing it, but I'm wondering, what do you make of it.

MR. LANDRY: Yes. We wanted to see an affect, and from this we have some ideas. And next time we review a code we have further ideas where to go.

CHAIRMAN WALLIS: Yes. See, you're -- this is a good step and you're learning as you go along.

MR. CARUSO: There is a strong element of staff development associated with this.

CHAIRMAN WALLIS: We're interested to have you describe the picture that's up on the screen.

MS. COLPO: Well, I'll be happy to tell you all about that. What this plot shows, and going back to the statement that I chose different subroutines to look at, and found that FWDRAG has the most significant affects.

As you can see, the peak clad temperature was higher for FWDRAG. It occurred more than 100 seconds later than the --

CHAIRMAN WALLIS: But that was with FWDRAG, what, ten times as much or something?

MS. COLPO: Right.

CHAIRMAN WALLIS: Ten times as much?

MS. COLPO: Right.

CHAIRMAN WALLIS: Okay.

MS. COLPO: And it just looks like a different transient. It doesn't quench at the same time the other ones do. So I thought FWDRAG's the one to look at.

DR. RANSOM: Well, did you multiply FWDRAG just in the core or through everywhere, everywhere it's used?

MS. COLPO: Wherever it's used, that's where it would come up, because in that bottom line where it has the final -- this is what the wall drag is, I just put a multiplier in, two, ten, .1 and so on.

CHAIRMAN WALLIS: But then the other ones that those black, red and blue, those are for VISCOL changes or something?

MS. COLPO: VISCOL and post-DNB.

CHAIRMAN WALLIS: Oh, those are a factor of ten on both of those things?

MS. COLPO: Actually, there is a factor of two or three on those. Now, the reason that I put up the ten was that I also did the same two or three on the FWDRAG and it had a significant affect, too. So I wanted to emphasize on this slide that it had the most significant affect.

And I guess if I had put the same multipliers in you would have still seen the same idea. It's just accentuated here a bit more. Anymore questions on that?

MR. SCHROCK: Is viscol simply the liquid viscosity? Did I understand that --

MS. COLPO: The water liquid viscosity,

correct.

MR. SCHROCK: And you put a multiplying factor of three on that?

MS. COLPO: Two and three.

MR. SCHROCK: Two and three.

MS. COLPO: Yes. Actually, I did more than that. I started out with .5, you know, to see. It wasn't a whole lot of difference.

MR. SCHROCK: It's not --

MS. COLPO: I just kept playing with it until I saw something happen.

CHAIRMAN WALLIS: Well, the title says, "PCT independent of location."

MS. COLPO: Right.

CHAIRMAN WALLIS: Does that mean that you fixed the place where you raise the temperature or?

MS. COLPO: What that means is that this was run with the Westinghouse three-loop model that Framatome ANP gave us.

CHAIRMAN WALLIS: Right.

MS. COLPO: And there was a script that could go through and look anywhere in the core and pick the highest peak cladding temperature.

CHAIRMAN WALLIS: Ah, that's what it means, is that this -- it searches for the peak clad temperature, independent of where it may be?

MS. COLPO: Yes.

CHAIRMAN WALLIS: That's what the typo means? Okay.

MS. COLPO: Right.

DR. KRESS: Are you --

CHAIRMAN WALLIS: So you're --

DR. KRESS: I'm sorry.

CHAIRMAN WALLIS: -- so you're finding the real peak clad temperature.

MS. COLPO: Right.

CHAIRMAN WALLIS: Yes.

DR. KRESS: But when you vary viscosity aren't you just going for a wild ride?

CHAIRMAN WALLIS: Maybe it's also an interface drag.

DR. BANERJEE: You vary the Reynolds number.

CHAIRMAN WALLIS: And all kinds of things.

DR. BANERJEE: You vary the Reynolds number, too.

CHAIRMAN WALLIS: Maybe it's also an interface drag.

DR. BANERJEE: Maybe. I don't know. I'd have to look at the formulation.

MS. COLPO: Framatome, like I said, provided the staff with the Westinghouse three-loop large break LOCA model, and I ran a base case with this model and then inserted the multipliers of two and ten in the FWDRAG subroutine of the code and recompiled and reran the plant deck.

Those results were interesting and prompted some further investigations, and I'll show them on the next slide. But just to say right now that for further investigation Framatome also provided the input deck for the FLECHT SEASET test 31504.

I ran a base case with that model and inserted multipliers of .1, two and ten into the FWDRAG subroutine of the code, recompiled and reran the FLECHT SEASET model, and this study will be the focus of the rest of my presentation.

Of course, one of the differences between the two cases is the PWR case is a model of the whole primary coolant system. Whereas, the FLECHT SEASET test is basically just a lower plenum and a core and an upper plenum and that's it.

So it really focuses the investigation on what happens when you increase or decrease the wall drag in the core.

DR. RANSOM: Do you know if in the FLECHT

SEASET case they had boundary conditions of just pressure? Or did they have a velocity boundary condition?

MS. COLPO: They had pressure, temperature and velocity, and I'll get to that. I have a nodalization diagram that shows that. These are the results from running the PWR cases. The heavy black line is the base case with no multiplier or just one as the multiplier on FWDRAG.

The blue line is the case modified to calculate ten times the wall drag. As you can see, the peak clad temperature is increased with increasing wall drag. This is explained by saying that the wall drag retarded reflood by slowing down the progress of the clinch front.

Also, the ten times wall drag case looks like a totally different transient and it resulted in over 100 degree higher peak clad temperature occurring later, and once again, doesn't quench before the end of the calculation.

So because of these results I was interested in running some cases where reflood would be the focus, and that's what I was just talking about in running the FLECHT SEASET model runs.

This slide shows the FLECHT SEASET cases that I ran with the model provided by Framatome. The bottom green line, I just wanted to interject something here. It didn't show up on the slide right here, but the bottom green line shows the liquid viscosity multiplied by a factor of ten.

And for some reason it didn't show up on either my overhead plot or the plots that I printed out for handouts for you all. So this one, this bottom green line, this is actually ten times the liquid viscosity.

CHAIRMAN WALLIS: That's interesting, because ten times viscosity in the turbulent region looks as if it's almost like four times the wall drag, which doesn't seem quite right for the usual exponent on Reynolds number.

But maybe it's changing something else like the bubble rise velocity or something. So it's hard to tell.

DR. BANERJEE: What are the other lines?

MS. COLPO: The other lines there are these -- I'm not sure if I'm pointing correctly. This is two times the wall drag, one time the wall drag, or basically no multiplier, and 0.1 times the wall drag. And those are the different peak clad temperatures that you get running those cases.

DR. BANERJEE: And the lowest one is ten times the wall drag?

MS. COLPO: The lowest one is the ten times.

DR. BANERJEE: So in this case the peak clad temperature went down with increasing order?

MS. COLPO: Yes, which is interestingly enough, just the opposite of what we saw in the PWR case.

DR. RANSOM: The pressure boundary condition, although that's what you'd expect because you're reducing the flow rate, apparently.

CHAIRMAN WALLIS: Well, then you expect it to get hotter if you reduce the flow rate.

MS. COLPO: Well, FLECHT SEASET, this test had a constant velocity input, .972 inches per second reflood rate constant.

DR. RANSOM: Oh, the velocity.

MS. COLPO: Right.

CHAIRMAN WALLIS: Constant velocity.

MS. COLPO: Right.

CHAIRMAN WALLIS: Oh.

MS. COLPO: They're putting it in as a constant velocity, constant reflood rate.

DR. RANSOM: Oh, I see.

CHAIRMAN WALLIS: And that's reducing the Reynolds number.

DR. BANERJEE: Reducing it, okay.

CHAIRMAN WALLIS: So the best thing we could do is call this run molasses.

(Laughter)

DR. RANSOM: This viscosity, I guess, would go into the heat transfer coefficient calculation, as well?

MS. COLPO: Pardon me?

DR. RANSOM: Well, it would go into the heat transfer coefficient calculation, as well as the wall drag?

MS. COLPO: The --

DR. BANERJEE: This is the wall drag.

DR. RANSOM: If this is the property routine.

MS. COLPO: This is the wall drag.

CHAIRMAN WALLIS: Except for one case.

MS. COLPO: Except for the one case.

DR. RANSOM: The one case, right.

MS. COLPO: The one bottom green line case that I just put on there for comparison, say, to show that the wall drag multiplied by ten had more of a significant affect than --

DR. RANSOM: Oh, yes.

MS. COLPO: -- ten times the water viscosity.

CHAIRMAN WALLIS: It's very strange, because if you had an enormous wall drag you wouldn't have any flow, presumably, or what would happen?

DR. BANERJEE: If she's injecting --

CHAIRMAN WALLIS: You're forcing the flow.

MS. COLPO: Right.

CHAIRMAN WALLIS: Right.

MR. LANDRY: I think if you let Sarah go through an explanation.

CHAIRMAN WALLIS: Okay.

MR. LANDRY: Because we did a lot of head-scratching on what was going on in this.

CHAIRMAN WALLIS: Yes, we can do that. Maybe we should move on and then --

MS. COLPO: Okay.

CHAIRMAN WALLIS: -- see what we learn at the end, yes.

MS. COLPO: Well, this plot definitely prompted me to dig in and figure out what was going on. I wanted to show you a nodalization diagram. I promise I'm not going to try and draw any control volumes on this thing.

And I wanted to let you know also that this was generated by SNAP. So SNAP does work. As I mentioned before, there's -- the lower plenum had 40 psi and 123 degrees F, and in this junction here into the heated portion, the heated core region, there's a constant reflood rate of .972 inches per second.

So that was just constant throughout the test. Then you have the unheated core region and then the upper plenum, which also had I believe it's -- what is it -- 40 psi and 400 degrees F.

MR. SCHROCK: The injected water is always 123?

MS. COLPO: Yes.

CHAIRMAN WALLIS: Well, it keeps the liquid in there, right?

MS. COLPO: This is just a picture of the core, the heated core region with its 20 axial nodes and the elevations that correspond to each of the nodes or the volumes. The integral mass flow in, mass flow out and carry out fraction was the same for all of the runs.

There was no change when we changed the multipliers. Integral mass flow in. I'm not sure that you all have these plots in.

MR. BOEHNERT: We don't have those.

MS. COLPO: Right. Because there wasn't really anything to show except for everything was the same.

DR. BANERJEE: Well, you were forcing the in flows -- forcing velocity.

MS. COLPO: Right. So okay. Here, I get to use my two overheads. Okay. What you can see on these clads is of the steam output rate and the liquid outflow rate is that a higher wall drag produces a higher steam outflow, and a lower liquid outflow.

The one-tenth of the base case wall drag produced the highest liquid outflow and the lowest steam outflow rate. What we're seeing is more water is being held in the lower core section, which boils and produces more steam, and that's substantiated further by the next few slides.

CHAIRMAN WALLIS: I guess these steam flow oscillations are why Larry Hochreiter has his damping vessel in his new experiment.

DR. BANERJEE: It's also due to Unow's (phonetic) boiling calculation.

MS. COLPO: This slide shows a plot of the differential pressure at the lower one foot of the core. And as you can see, the larger wall drag again is seen to retain the most liquid in the lower core region. Okay. You're going to love these.

(Laughter)

MS. COLPO: All right. What you're seeing here --

CHAIRMAN WALLIS: Modern art, it looks like.

MS. COLPO: Yes. It is something more beautiful than modern art, even. What they show is the void faction in the core, and what these -- these different lines show the void fraction as you move up in the core, so.

DR. RANSOM: In this particular case does it start out full of liquid and then it's boiling off or?

MS. COLPO: No. No. It's getting filled up.

CHAIRMAN WALLIS: It's full of steam.

DR. RANSOM: Oh, is this void fraction or is this liquid fraction?

MS. COLPO: It's a void fraction. If you can see --

CHAIRMAN WALLIS: They all start at the top. They all start at the top and come down.

MS. COLPO: Well, I think the very first one started at the bottom and went up.

CHAIRMAN WALLIS: No.

MS. COLPO: No?

CHAIRMAN WALLIS: Looks to me that they all start --

MS. COLPO: Start at the top and go down to the bottom.

CHAIRMAN WALLIS: They all start at the top, yes.

MS. COLPO: Right.

CHAIRMAN WALLIS: It's dry at the start.

MS. COLPO: Right.

DR. RANSOM: Oh, I see. Right. Okay. It's filling up with liquid, yes.

CHAIRMAN WALLIS: Oh, I think we're going to have to ask Ralph Landry what he concludes from this, too.

DR. BANERJEE: And you said we've love them, so is it the art we love or is there something we should know from here?

MS. COLPO: Well, it's partially the art. The thing that you should notice from this is that in the point -- 0.1 multiplier case you see the -- in the upper regions of the core, which is as we move this direction, in the upper regions of the core you're getting a lower void fraction than you do with ten times the wall drag.

It's staying -- the void fraction is staying higher. So that's what I saw in comparing these two.

MR. SCHROCK: How do you interpret where you are in the core on this?

MS. COLPO: So and now -- now, this is why I said you'd love it, because basically it's just -- each of these lines represents a level in the core. So as you --

MR. SCHROCK: We don't have the legend yet. Okay.

MS. COLPO: As you kind of progress this direction --

MR. SCHROCK: Go across. I see.

MS. COLPO: -- through the plat, it's lines representing higher levels in the core. Did that make sense?

MR. SCHROCK: Yes, liquid is eventually getting up there.

MS. COLPO: Right.

MR. SCHROCK: Yes. The blue one sort of in the middle, is it the same location?

MS. COLPO: Yes. All of them, I made sure that the same color lines would match the same locations, which meant I had to do it over once or twice, actually. Okay. Let's see. This is another indication of less water being carried out of the core in the high wall drag case.

And these are my last sets of slides, more lovely ones I'm sure you'll appreciate. Okay. So these show the flow regimes for the .1 and the ten multiplier cases. And what I tried to do is point -- see, when you ask the code for the flow regimes it basically pops out numbers which correspond to meeting some flow regime.

So that's what's plotted out, is the numbers. And then I tried to indicate by pointing arrows to like, say, this one right here and --

CHAIRMAN WALLIS: Excuse me. This is at some particular point, because presumably the --

DR. BANERJEE: Different colors are different locations.

MS. COLPO: Yes.

CHAIRMAN WALLIS: The colors are different locations?

MS. COLPO: Just the same as in the void fraction that we were just looking at.

CHAIRMAN WALLIS: So how do we know -- oh, different levels being different flow regime, different heights on the flow regime. Okay. Okay.

MS. COLPO: So yes.

CHAIRMAN WALLIS: So four is a bubbly and five is a slug and --

MS. COLPO: Right. I believe so, yes. Right. Four is a bubbly. Five is a slug.

CHAIRMAN WALLIS: I didn't know they had such a sophisticated flow regime map, FLECHT SEASET.

DR. BANERJEE: No. They inverted and uninverted, the same fluid uses.

MR. BOEHNERT: Isn't some of this a matter of judgment, though, about what regime to use?

DR. BANERJEE: There's a map.

MR. BOEHNERT: Yes.

DR. BANERJEE: What else?

MR. BOEHNERT: Those lines were to be fuzzy.

MR. SCHROCK: Let's see. What is an inverted slug?

MS. COLPO: The slug inverted?

DR. BANERJEE: Big chunks of liquid flying upwards.

(Laughter)

MR. LANDRY: What we see the code doing is selecting the flow regime. And in fact, we're seeing the affect here of inverted annular flow and that's for the low flooding rate, or the low wall drag case that Sarah ran, a very strong affect of going into an inverted annular flow regime.

Where we're blanketing the rods with steam we're getting a high mass flow through, but the mass flow is not penetrating the annular region and cooling the rods. So even though we supposedly have a lower flooding rate for carrying out a lot of liquid, we're not doing it effectively.

CHAIRMAN WALLIS: I'm a little concerned about it, having so much inverted slug in this, which is a strange flow regime anyway.

DR. BANERJEE: It's very oscillatory. You see it in reality.

CHAIRMAN WALLIS: You've got hunks of liquid.

DR. BANERJEE: It's not like real slug, so, but boy, just up and down.

MR. LANDRY: Now, you have to put what Sarah was doing into perspective, that we were not doing it to verify the code or assess the code. We were trying to understand what the code was doing. And by doing this calculation set for a big plant calculation we had -- we confirmed our feeling that wall drag is going to be a strong affect.

Viscosity is going to be a lower order affect. When we looked at the FLECHT SEASET, though, we were very surprised because it seemed to be going the opposite of what we thought should be happening.

And what began as just doing some calculations ended up a pretty in-depth analysis that Sarah had to do, because she had to then go back and figure out why is this inverting what I expect to see.

And it's only by tracing through what the code was doing with selecting the flow regime map, matching up with the flow conditions, that we're able to see that, well, this thing is going into a flow regime that seems to be carrying out fluid or liquid, but it's doing it inefficiently as far as heat transfer is concerned.

And then when we started to think about it, okay, yeah, that is reasonable and it does fit, because we're fixing the flooding rate.

DR. BANERJEE: The pressure was higher at the bottom, right?

MR. LANDRY: Right. Right.

CHAIRMAN WALLIS: So this --

DR. BANERJEE: How much higher was it, do you know? Do you remember?

MR. LANDRY: Sarah had that.

CHAIRMAN WALLIS: Well, it's varied, depending on the wall drag.

DR. BANERJEE: Yes, that's what I mean.

MR. LANDRY: Right. The wall drag alters the pressure distribution throughout the channel, alters the flow regime.

MS. COLPO: Is this the one you were looking for?

DR. BANERJEE: Right. Right.

CHAIRMAN WALLIS: So what you're gaining from this is -- because of some confidence that the code is giving results which make some sense physically when you vary some things and you explain why it's doing what it's doing? Is that what you gain from this?

MR. LANDRY: Correct. As I said, this was not done to confirm the validity. We were trying to understand what the code is doing.

CHAIRMAN WALLIS: Sort of exploring, exploring.

MR. LANDRY: It was more exploratory.

CHAIRMAN WALLIS: Maybe in the future when you do more of this you could focus on some key areas where something might be -- have some significance relative to code assessment or a safety evaluation or something?

MR. LANDRY: Right. That's what Ralph Caruso indicated earlier in the presentation, that this is all a part of the staff's own learning curve. We've learned something from this and when we get another code, which we have another code coming in for review already, we've learned something here and we can carry through and we can explore a couple other areas now and have some ideas for our next code review.

DR. BANERJEE: Wall drag is a pretty good thing to try because you can get fuel with very different roughness and crud formation and all sorts of stuff, you know, and the fact that it's so sensitive to it is quite interesting, I would say.

MR. LANDRY: So our -- from this, our conclusion is that this was a very good exercise. It was a good exercise in understanding the code. It was a very good exercise for us in working with the code. We've been able to get into the code and fix our own minds, has the coding been done correctly, or the spot-checking we did.

Do we see which -- a couple of parameters that we feel are important, are they important? Does this confirm in our minds the importance of a couple of key parameters as indicated by the PIRT? So overall, we're quite pleased with this work.

It is a beginning for us and we hope to have the opportunity to continue with additional codes in this manner.

MS. COLPO: I'll say I definitely learned a lot in going through this exercise through the studies.

MR. LANDRY: Okay.

DR. RANSOM: I was going to ask you, did the code fail at all?

MS. COLPO: Not at all.

DR. RANSOM: No problems?

MS. COLPO: No problems.

DR. RANSOM: Robust.

CHAIRMAN WALLIS: Are we ready to move on to Ralph's summing up? Thank you very much.

MR. LANDRY: I don't know if all that last remark, if Vic meant to say rats, or yea. I don't know if he was disappointed or happy.

CHAIRMAN WALLIS: He seemed somehow surprised that the code didn't fail.

(Laughter)

MR. LANDRY: Okay. Some of the

conclusions that we arrived at in our SER, we concluded that the review of the documentation code and input models submitted by Framatome ANP, that the S-RELAP5, realistic large break LOCA methodology is structured consistent with the guidelines of the CSAU methodology, methodological process.

It addresses the licensing requirements for a variety of similarly designed nuclear power plants. And in particular, we concluded that this applies to the three-loop and four-loop Westinghouse designs and the two by four combustion engineering design with bottom-up quench, bottom -- or lower plenum injection plants.

Methodology, the model applies to bottom reflood plants only. In other words, we do not believe that this applies to the upper head injection, upper plenum injection plants, plants for which a top-down quench occurs.

If that occurs we feel that there has to be further review of the methodology and the modeling. The modeling does not determine whether long-term cooling has been satisfied, as this is determined by individual licensees as part of the application of a methodology, or as part of a design basis established by the licensee.

If the licensee's design basis has already addressed long-term cooling, this is primarily a hardware issue. Unless there is some reason that this methodology would change the conclusions already in place, we do not see a need for talking about long-term cooling with respect to realistic large break LOCA methodology.

CHAIRMAN WALLIS: So you think --

DR. RANSOM: Ralph, is there any interest in BMW plants or is that --

MR. LANDRY: This has not been assessed for application to the once-through steam generator design.

DR. RANSOM: So that's not being considered now?

MR. LANDRY: No. That may be -- Framatome now -- what was seen -- or it was Exxon, then it was Advanced Nuclear Fuels. Then it was Siemens, it's now Framatome also owns what used to be BMW. So at some point in time Framatome may very well want to apply this to the rest of the fleet of Framatome hardware.

At that point this would have to be re-reviewed for application to once-through steam generators. That's why we've been very specific. What they've asked for is applicability to three- and four-loop Westinghouse design; in other words, U-tube seeded (phonetic) generator, a recirculating steam generator, and to the combustion engineering two by four design.

MR. MALLAY: This is Jim Mallay. As I mentioned this morning, our next effort over the next five to eight years will be apply the realistic -- well, I should say the S-RELAP5 platform to BWR analysis. We currently do not have plans to apply it to the BMW units.

CHAIRMAN WALLIS: Can we look at your first bullet? You conclude from review of the documentation, code and input models submitted by Framatome is structured consistent with the guidelines. That's a very weak sort of statement.

That simply says they tried to follow the rules. Doesn't say it's good. It doesn't say --

MR. LANDRY: Well, they've concluded that they satisfied the requirements.

CHAIRMAN WALLIS: Well, of the process, but it doesn't mean to say that they met -- they went -- they took the exam, but did they pass?

MR. LANDRY: This morning and earlier today we've discussed an issue which we are now examining.

CHAIRMAN WALLIS: They passed everything else?

MR. LANDRY: They passed until we decided -- until we understood fully what was being said with regard to selection of the worst (phonetic) break size.

CHAIRMAN WALLIS: I'm very surprised, see. You've said the documentation, code and input models is what led you to your conclusion. I would have turned it around completely and said, in spite of the documentation --

(Laughter)

CHAIRMAN WALLIS: -- code and input models, our assessment -- our assessment -- of the way the overall code works when compared with the data leads us to conclude that it's a good code. I don't think you can conclude anything from what's claimed in approximate equation in some documentation. That doesn't tell you if it works or not at all.

MR. LANDRY: Well, what we're saying, Dr. Wallis, is everything combined --

CHAIRMAN WALLIS: Yes.

MR. LANDRY: -- leads us to the conclusion.

CHAIRMAN WALLIS: But please, please state that your assessment -- isn't it your assessment that has to be the key decision driving process? The documentation -- because so many assumptions are made in it and because it's so much -- so many ad hoc methods are introduced to make things happen, it is a hodge-podge.

And the only real test of its usefulness and its acceptability has to be that it works as a package. Isn't that -- would you disagree with that statement?

MR. LANDRY: I don't want to be so negative about any aspect. When we assess -- when we determine acceptability or for approval, we look at the entire package and consider the entire package. The documentation, maybe it's poor; maybe it's not.

We look at the code itself. We look at the input models that work. We look at the code and the input models to work with them. And that's what I think you're referring to as our assessment. By working with the code, the input models and the documentation we get an overall feel and we look at what is required by the regulations.

CHAIRMAN WALLIS: So when you read the --

MR. LANDRY: When we put it all together we say, yes or no. We don't say, in spite of crummy documentation we have run a case that says it's okay. We have to say when we take the entire package combined --

CHAIRMAN WALLIS: Well.

MR. LANDRY: -- we are satisfied that they meet the regulatory requirements.

CHAIRMAN WALLIS: Let me say it another way, and then I'll come back to the other one. Even if the documentation looked good, I mean, even if you couldn't question the derivation of the equations and the assumptions made and so on, even if the documentation looked really fantastic and good, I think you'd still say, you know, that's all very well, that's theory; you've got to show that it works.

And I would think that showing that it works has to be the key part of it all.

MR. LANDRY: That's an important part.

CHAIRMAN WALLIS: Isn't that really the case?

MR. LANDRY: Yes. And that -- today we're mixing what the applicant has shown with what we have learned, the code itself. The old days we would simply base it on the documentation and what the applicant or the vendor would show to us, because we would not work with the code.

So today, we have the advantage of not only having to look at the documentation and say, well, the documentation leaves a lot to be desired. We don't -- we've looked at all the assessment. We've worked with the code. We've looked at the internals of the code.

When we take the whole package together we make a judgment as to acceptability. So I prefer to not be negative about any one aspect. I prefer to take the position that because of the whole package we're able to draw a conclusion.

CHAIRMAN WALLIS: So when you saw the momentum equations you cheered and said, wonderful.

MR. LANDRY: Don't put words in my mouth.

CHAIRMAN WALLIS: But you're saying, you know, the documentation and the input model submitted seem to be put up here as being the key thing.

MR. LANDRY: No. When we take it all --

CHAIRMAN WALLIS: I can't believe that's the case.

MR. LANDRY: No. I keep trying to say, when we take everything together -- I could have put that in the reverse order and this order was just that. Writing these items down, it wasn't intended to infer --

CHAIRMAN WALLIS: Right.

MR. LANDRY: -- this is key, this is lesser, this is least.

CHAIRMAN WALLIS: I think my view is that the assessment has to be key, and maybe -- I don't know if you've got time to tell us a bit more about how you were satisfied with the assessment.

It seems to me that your manipulating the code was interesting, but it didn't really address the question of whether or not this code's adequate for a large break LOCA. It showed that you can run the code.

You can do parametric studies, but it didn't really address the key issue of having to do with nuclear safety or adequacy of the code. And I suppose if the code hadn't worked, you know, it taught you something.

The assessment has to be at a deeper level than that. So what was this deeper level of assessment that really convinced you to give this an okay?

MR. LANDRY: Well, when we look at the assessment cases we look at the breadth of the assessment that's been performed. Has the code been assessed against separate affects test? Has the code been assessed against integral systems test?

Has the code been assessed over a range of sizes? What -- just ignore the whole question of scale right now. Has there been a sufficient range of sizes or tests that are used for comparison and for -- ranging from separate effects through integral systems to big full-size, if it was available?

When we look at the entire package of assessment we can say, okay, for this test the results are not as good as we would like to see. There's something here that is happening that the results are not real good.

But when we look at the overall proponents for all of the assessments together, we get a nice feeling that the code is performing well against this whole range.

Today, the assessments that are being done are trying to cover -- this isn't talking just about Framatome -- today the assessments that are being done are trying to cover an adequate range from separate affects that model or emphasize particular phenomena, to full -- to integral systems to full-size, where possible.

When we look at those assessments we want to insure that there is as complete a coverage as possible. We're also now becoming more and more aware of the difficulties that applicants, vendors are having in obtaining good quality data over the entire range.

And we're becoming maybe a little more sensitive to this degradation that I talked about earlier, that the entire code community around the world is complaining about now.

It's going to be very interesting in the future to see results from tests like those that Larry Hochreiter is doing, to see more data in a more prototypic condition, fluid, hardware-wise, et cetera, for use against -- use with modeling and testing of the codes.

CHAIRMAN WALLIS: Does this statistical and uncertainty approach help a great deal in assessing whether a code should be accepted or not?

MR. LANDRY: Personally, I think it does help because by doing comparisons with separate affects tests, phenomenological tests, a code can -- a code-user or a code-developer can determine, are the correlations and models in the code predicting the phenomena correctly?

If not, what are the biases plus and minus? Do enough of those add the biases to enough of the phenomenological models and then go back -- or before doing that, go out and calculate a large medical assistance test, now see where the differences are, add your biases in and see what the code does as a calculation against the integral systems test?

Does the code -- did the biases now come in and give a very good prediction of the integral systems test? It does? Okay. Now, we can go out and we can understand more about the uncertainty in the code and do a prediction in a more realistic nature for a full-size plant.

I think this helps a great deal. I think this helps understand what the code is doing and say, yes, this code is calculating phenomena correctly, or we understand where there are biases in the phenomenological calculation, so that when we get a result we have more faith in the result than a methodology that's so deterministic that we say, well, we don't understand a lot of these things in the code so we're going to slap on something that's incredibly conservative to guarantee that our result is conservative, no matter what's wrong in the code.

CHAIRMAN WALLIS: Well, most things you hear about assessment of the old way of assessment seemed to be you make some runs and you look at some data and you draw some wiggles and squiggles and see if the data are somewhere near the wiggles and squiggles.

And that's a very qualitative sort of expert judgment approach. I thought that we were trying to replace that with something more logical, mathematical, statistical by saying, let's take this bunch of data, let's see what that tells us about the uncertainties in the code.

Let's use the data to establish some numerical assessment in the form of probabilities and so on with those uncertainties, and then let's synthesize this together and relate in some way to full-scale -- full -- what do you call it -- system tests.

And presumably, you need to get some uncertainty assessments out of the system tests. But now, you've got a quantitative way of saying how good the code is because you've got some statistical way of evaluating it.

The old way of just looking at data points and curves have always made me nervous, because I wasn't sure of what I was really learning from that. But if you can extract some meaningful statistical information and use it, that seemed to me a tremendous step forward.

MR. LANDRY: And that's what the bias analysis does.

CHAIRMAN WALLIS: I mean, I'm surprised you didn't emphasize that more in your conclusion. You didn't say anything about it at all. You just said documentation code input models. I would think it's the assessment and the statistics and the logical evaluation of uncertainties which is the key to evaluating the code.

MR. LANDRY: This SER is a draft. We do intend to go back and modify it. You received some time ago another draft SER --

CHAIRMAN WALLIS: Which was different.

MR. LANDRY: -- which was different because at that point we were involved in some very difficult discussions, and following those discussions we resolved problem areas that we had. So we were able to go back and rewrite the SER.

We sat down and completely rewrote the SER to try to get closer to the methodology that was being used in support of the methodology, and explain what was being done with the realistic large break LOCA methodology and why it was acceptable.

This is going to be fine-tuned. Through

the discussions of the last two days we know there's some other areas to go back and tune what we've written and explain further where we need to do further explanation in our SER.

As I said earlier at the outset, what we have done in the review is a snapshot look at the methodology, and out of that we've written an SER that's a snapshot. If we had written down everything that we did it would be massive.

So we -- but seeing where the questions are that experts on the subcommittee identified, we can see where we can go back now and further explain in the SER what we have done and why we believe it's acceptable.

CHAIRMAN WALLIS: Which will be done before the full committee meets in December?

MR. LANDRY: It will be done in -- we have an issue that we need to resolve.

CHAIRMAN WALLIS: So what the full committee sees in December is not going to change significantly afterwards?

MR. LANDRY: We would hope not. We're going to go back and work on the SER some more.

DR. KRESS: Your remaining issues, the statistical variation in the pipe size. Other than that you would have said this thing is ready to go.

MR. LANDRY: Right.

DR. KRESS: It's okay.

MR. SCHROCK: But you have a statement about that in the current draft which is puzzling to me, because as I read it I get the impression that you are ready to disapprove what they are saying about probability. And then you turn around and say that they're consistent with the CSAU approach.

MR. LANDRY: No. Go through to the top of that section on uncertainty analysis and you'll see in bold letters a statement that that entire section is being replaced. That's because that -- there's an old section from when we were having discussion, which Yuri talked about earlier, of the former approach and the current approach of Framatome.

We have a new writeup for that that will be substituted for that writeup. So that -- what I was trying to indicate on there without just leaving a big hole was, here's the writeup we had, but ignore it because we're going to change that entire section. It's going to be pulled and a whole new section put in.

DR. RANSOM: I've got a couple questions.

DR. KRESS: Do you want us to express our opinion on this statistical variation in the pipe size in a letter or something or did you --

MR. CARUSO: Not yet.

DR. KRESS: You'd rather look into the policy issues first?

MR. CARUSO: We would rather discuss this before we ask for your advice.

DR. KRESS: Okay.

DR. RANSOM: I've got a couple questions. One, did you rerun any of the assessment calculations that Siemens provided that you could then assure yourselves, I guess, that those are what they say they are?

MR. LANDRY: Well, Sarah rerun the FLECHT SEASET test --

DR. RANSOM: Right. But what about like LOFT and --

MR. LANDRY: -- 31504, I believe it was, and reran the three-loop --

DR. RANSOM: PWR, right.

MR. LANDRY: -- PWR. Those gave us the base cases for the further work that she did. But as far as going back and rerunning the other cases, no.

DR. RANSOM: The other question is, did you run some of those cases with your code, you know, with the licensing audit code that the NRC has? It would be interesting to see what the comparison is between the NRC's version and the Siemens version.

MR. LANDRY: Well, the version that we have right now is RELAP5 lot 3.2.2 or 3.3 gamma. I don't --

DR. RANSOM: Yes, whatever. I mean --

MR. LANDRY: -- I'm not sure exactly which modeling.

DR. RANSOM: -- would that be of interest? That presumably is your audit tool, right?

MR. LANDRY: Yes. But we did not go back and run it for comparative purposes.

MR. CARUSO: Actually, what I'd like to do, I think, next time around is use TRAC-M.

DR. RANSOM: Well, or use TRAC-M. It's whatever you want to use.

MR. CARUSO: And I believe we just received a copy of a SNAP tool which will do a translation between a RELAP deck and a TRAC-M. So that may make it a lot easier to do these in the future. There's a lot of effort to putting together a deck from scratch.

And we don't have any -- I'm not sure if there are any TRAC-M decks available for some of these facilities. I'm not sure what's available at this point.

MR. LANDRY: You have to keep in mind also that RELAP5 MOD 2 deck is now running RELAP5 MOD 3 without conversion.

MR. CARUSO: Right.

MR. LANDRY: So we would have to do the conversion and then we would have to do some final checking or Q/A to make sure that their MOD 2 point whatever it is deck was converted properly to run on MOD 3.

DR. RANSOM: Well, you surely have LOFT decks, don't you? I mean, because I think the thing of most interest would be to compare it to something where you do have it done.

MR. LANDRY: Yes.

DR. RANSOM: But I guess you haven't done that yet. But the other thing is, Bill Nutt showed me a curve that I don't know if you're willing or can show it here, but it seemed to me it's the kind of thing that would be very much of interest to this committee, as well as, you know, the full ACRS committee. And I'm not sure what their status is. Is that possible or?

CHAIRMAN WALLIS: Can we see it after

we've finished questioning Ralph? I wanted to go back to the first of Sarah's -- you said that she --

MR. LANDRY: You can.

CHAIRMAN WALLIS: -- she did do these 3D-2D-3D tests and then she seemed to be uncertain about what to conclude. She sort of said, well, they're mostly conservative. This is -- could something -- couldn't something more be wrung out of that by running those -- she actually ran the code on these tests, right?

MS. COLPO: No.

MR. LANDRY: No, she examined.

CHAIRMAN WALLIS: Oh, she examined how they had run the code on these.

MR. LANDRY: Yes.

CHAIRMAN WALLIS: I'm sorry. I thought she had -- so she hadn't run the codes.

MR. LANDRY: Right.

CHAIRMAN WALLIS: Okay.

DR. KRESS: The key to this whole thing, Ralph, is how they properly assess the uncertainties. And then everything there is in terms of assessment of statistical method and then what follows from it. Were you very well satisfied with the way they assessed the uncertainty in the code?

Did you -- in terms of the distributions, the inputs and the various parameters and in terms of determining the biases, by looking at the relationships between it and various separate effects and stuff?

You looked pretty careful at that stuff, and to me, if they got that right then, you know, this other stuff about comparing with the codes really doesn't matter a whole lot, as long as they got that part right and then did the statistics right to end up getting their 95/95.

MR. LANDRY: That's right. And Yuri's not here to speak for himself, but in the review that Yuri did a number of questions were asked others, specific points. And through the RAIs and responses Yuri's conclusion was, what they're doing is right.

And where he had the difference with them was when he got down to the bottom and was saying whether 59 cases constituted a uni-variate analysis or a tri-variate analysis. And that was where the disagreement arose.

He throughout his review and at the conclusion was not indicating any problem with what they had done in assessing bias, whether the distributions were proper or improper. Getting down to the bottom line, yes, they've done an assessment of overall uncertainty, and overall uncertainty and PCT correctly.

His difference with them was over whether this applied as a tri-variate analysis or not. So that's the long answer to your question. I think that we did not have problems with what they did in their overall uncertainty analysis, including the biases, this looking at scaling, looking at assessment of uncertainty.

It was only at the very bottom end, bottom line that they had a disagreement. So we find the rest of it acceptable.

DR. KRESS: Okay.

DR. RANSOM: Did you agree to show that one? Why don't you show it? I think it really, to me, summed up.

CHAIRMAN WALLIS: I think we need to have you back, then, Ralph, when we see this plot and don't go away. I have no idea what I'm going to be seeing.

DR. NUTT: And that's good, since I did it for you.

DR. RANSOM: Well, there's a 50/50 probability that you'll like it or you won't like it.

CHAIRMAN WALLIS: Well, I'm glad you give me credit for being fair.

DR. NUTT: Be nice if these were 95/95.

DR. KRESS: Now, that's an interesting curve.

CHAIRMAN WALLIS: Oh, a LOFT comparison. Good.

DR. NUTT: This is LOFT comparisons.

CHAIRMAN WALLIS: All right.

DR. NUTT: And this is actually an old slide, but I put this together, and I'm going to put some disclaimers on the front of it. It's never been QA'd. So somebody may find that it isn't exactly what I put up there.

.And so I wouldn't take copies and show it around too much. And in fact, really, it's an extension of the issue of checking the separate affects results versus the integral effects. Remember, we said everything using separate effects.

All the uncertainties were set. All the biases were set. We had originally run cases against some integral effects test for the purposes of finding out what models we should be using. So then we went back to the integral effects test.

We ran the cases that, you know, the deck -- the code to compare it, and then we stuck these unbiases in and took the biases out of the code and ran it against it and got good averages. Well, what it got -- it got me started thinking about what you could do that maybe was a little bit more.

And I think I've discussed this before with you, so it's not new. I said, you know, you could basically go in and say, oh, I'm going to do -- I'm going to treat that test just like a large break LOCA calculation.

I'm going to go in and I'm going to run 59 cases. I'm going to randomly input them, you know, all the uncertainties in the and I'm going to get a scatter on it.

CHAIRMAN WALLIS: I've seen this from some other source. So maybe it's the same as yours.

DR. NUTT: It could be the same.

CHAIRMAN WALLIS: Someone else got hold of your slide, maybe.

DR. NUTT: Oh, you know, I'm --

CHAIRMAN WALLIS: Maybe someone else --

DR. RANSOM: Let's take a look at it and see.

CHAIRMAN WALLIS: Someone else independently.

DR. NUTT: IN case, you know --

DR. BANERJEE: Bill, can you just --

DR. NUTT: Yes, I could, as a matter of fact. So the other slide was not -- you didn't see that too well, but this one is -- I picked ultimate light (phonetic) and FPLB one. The other two have that quench in them and we don't come close to that.

So I wasn't going to use that as a statistical basis because I think there's just -- there's -- I don't like the cases. I took the bias and uncertainties that were present in the separate text -- separate affects test assessments.

I included the power and peaking uncertainties. I plotted the maximum, the minimum and the average of all these 59 cases that I got, of the PCT node temperatures at each time step, and I plotted the maximum, minimum average of the measured temperatures.

And this is where I went through and got this 20 some degree windage effect, but I think it was something like a 21-degree F adjustment they wanted to put on them, plus a 29-degree uncertainty on it. So I stuck that into the data.

And the S-RELAP5 -- okay. The conclusion is that measurements with the uncertainties are bounded over most of the range. I haven't looked at this in some time. I'm not sure what that next to the bullet was. But here's some of the bottom line on it.

If I take the data and I put the range on it and I plot the data -- watch this here -- now, I take the code and of these 59 cases that we run here's the minimum temperature that would have traced through this and here's the maximum temperature.

The model now has completely encompassed the data, and that's what you'd like to see, right? You'd like to see that your prediction that you claim, you know, has the range to cover everything, should very clearly cover the test cases.

And this is -- this particular one I think we showed you something very equivalent to this, which was the -- this is the mean value.

DR. RANSOM: That would be the realistic calculation.

DR. NUTT: That would sort of be the realistic calculation.

CHAIRMAN WALLIS: So you have a kind of two-sided --

DR. NUTT: It's not truly two-sided because it's --

CHAIRMAN WALLIS: It's sawed off. It's sawed off.

DR. NUTT: Right, because there's 59 cases, right.

CHAIRMAN WALLIS: But it is up to some level, I mean, it's not like 95/95.

DR. NUTT: Right.

CHAIRMAN WALLIS: It's 92 --

DR. NUTT: It's 9X, 9X, right, something like that.

DR. RANSOM: Why is that?

CHAIRMAN WALLIS: Because there are two outputs, not one.

DR. NUTT: Right.

DR. RANSOM: Two what?

CHAIRMAN WALLIS: There are two outputs, not one. There's a top and a bottom.

DR. KRESS: Each end; each end; top and bottom.

DR. RANSOM: Oh.

DR. NUTT: The upper and lower limit. I'm taking both in upper and lower limit. I can't claim that -- I could claim this is an upper, but I can't claim that's a lower.

CHAIRMAN WALLIS: We saw something almost identical to this in Germany three weeks ago.

DR. NUTT: Well, I did this a long time

ago, so I --

CHAIRMAN WALLIS: I'm not saying that. I'm just saying, probably at some point --

DR. NUTT: No, I'm being --

CHAIRMAN WALLIS: -- probably somebody else who did it. I'm not sure that it's you.

DR. RANSOM: But that to me would tell a lot more about what a code is capable of doing, I think.

CHAIRMAN WALLIS: That's much better than just looking at the wiggles and the dots.

DR. RANSOM: Yes.

CHAIRMAN WALLIS: Yes.

DR. NUTT: And this is part of the tilt. Show is another one again. This is LP-LB-1. And you get a very similar result. As you see, this is coming close to having a problem with the code. I mean, if one were to find -- if you were to go through this whole process, set up a range of uncertainties that you claimed would characterize the whole thing and if that range somehow or another found data points when you stretched them both setting outside; that is, if you were to get one of these cute little shapes, hourglasses, sitting completely outside the bounds of this, I think that would be a good indication that you had a problem. You really weren't doing the job.

But what we've done here is we've very -- you know -- except for the quench again, which we don't quench as well --

DR. KRESS: That's quench right there.

DR. NUTT: Right. We do not quench as well. I think that's true, you know, we don't quench as well. But over all this range, it's all inside that.

CHAIRMAN WALLIS: Except for the very first point.

DR. NUTT: Yes. And I'm not sure why that doesn't fit in, but it's closer, see. See, even so, if we --

CHAIRMAN WALLIS: If you're down below it could be that you got some conservatism in there somewhere or something. Is that what it would be?

DR. NUTT: And if I'd worked on this. I simply ran these cases just as an example.

CHAIRMAN WALLIS: Yes.

DR. NUTT: So if I were to work on them longer, the assumption is, of course, the longer you work on things the closer you get. You know that, especially when you know the answer, right.

CHAIRMAN WALLIS: That's not necessarily

true.

DR. KRESS: No. No.

DR. NUTT: No, I know that. I've had that happen.

DR. BANERJEE: The uncertainty on the LOFT, was that as constant as -- sorry. Was it like this narrow band or did it change?

DR. NUTT: They just -- all I got was 21 degrees -- plus 21 degrees with a 20 -- plus-minus 29 degree on it. So basically, this is -- these points are loft measurements with 21 added and then 29 down, 20 -- so it's --

DR. BANERJEE: I seem to remember that the early trial part was uncertain, but maybe we never quantified it and made it different.

DR. NUTT: I took -- and it's probably true. There's probably a detailed write-up of how these things happen. And what I took was essentially the synopsis, sort of like the executive summary, 21 degrees, plus-minus 29 is what we were using.

And I'm sure that I've seen in there that there were different conditions or transient effects. There's a transient uncertainty, for instance, you apply to these things because of response times, and there are sheer steady state measurement uncertainties, and I think all I took was the steady state measurement uncertainty.

DR. KRESS: Now, why would you conclude that the spread on the measurements right outside your bounds? That the test should have been better?

DR. NUTT: I think so, yes. And I would also -- it actually wouldn't be a problem here. It's more of a problem if I manage to conclude the answer more accurately than the original set of data, right, it'd be a bigger problem, because this could be a back -- you know -- this could be a much more difficult case to do and the uncertainty on the data might even be higher here.

DR. KRESS: What would you call the peak clad picture on that? One way out there on the 80-second time frame?

DR. NUTT: Yes. I think it does. It does.

DR. KRESS: But you know there's something wrong with the code out there, so you could discount it.

DR. NUTT: It didn't quench.

DR. KRESS: You could discount that because you know it would never get there because your quench model's not very good.

DR. NUTT: On this, yes. On this particular one it does -- you don't usually get this -- this kind of bothered me a little. And this is not a single node. This is -- there's a node out here that was wandering along that I think it was fairly high up in the core and it managed to not quench, and pretty soon it crossed over and it became a dominant curve, so.

CHAIRMAN WALLIS: It's disconcerting if that were actually the number which you calculated for a LOCA and that's what you've quoted as your peak clad temperature out there.

DR. KRESS: Yes, but you wouldn't --

DR. NUTT: Yes. I think that one of the caveats that we haven't stuck in this whole thing is that when you're done you should probably look at your cases and see if they all make some sense.

And I think there is a plotting requirement so that one wants to see that they make some sense when you're done.

DR. RANSOM: Let me clarify one thing. You're saying those curves are not for a single point.

DR. NUTT: No, they're not.

DR. RANSOM: Or the envelope. Is that right?

DR. NUTT: At each time point I picked the maximum temperature and the minimum temperature.

DR. RANSOM: What about the mean? Is it a again --

DR. NUTT: It's just the middle temperature in the range.

DR. RANSOM: Okay. And data is a single thermocouple?

DR. NUTT: Data is a single thermocouple. It's not -- you know -- it's representative. I mean, we would call this PCT, right?

DR. RANSOM: Right.

DR. NUTT: And this was the node that had PCT.

CHAIRMAN WALLIS: If you employed other nodes they would be in there somewhere, too?

DR. NUTT: Yes.

CHAIRMAN WALLIS: Have you see it plot the other nodes?

DR. RANSOM: Well, his curves are a combination of nodes, apparently. They can switch the nodes, you know, as you're going along, because it's always going to follow the --

DR. NUTT: And to tell you the -- I hadn't thought of that point. And to make a comparable plot

I should have gone through and I should have found here -- here I should have found the highest temperature. I shouldn't have accepted this.

CHAIRMAN WALLIS: Right.

DR. NUTT: I should have asked for the very highest temperature right here.

CHAIRMAN WALLIS: Right.

DR. NUTT: And I should have asked for the highest temperature here.

CHAIRMAN WALLIS: Right. I thought that's what you did, but you didn't do that.

DR. NUTT: No, I didn't. I just took the PCT node and did this. So it doesn't quite compare. But the interesting thing is -- and I have sort of discussed this with us presenting this particular approach -- but we did conclude that what you finally get when you look at it is the answer that, yes, there is enough uncertainty in the code, and yes, it does fit around the whole thing.

And these nominal ones are the -- you know -- the nominal biases actually did a pretty good job of fitting the --

CHAIRMAN WALLIS: And also, this uncertainty is figured into the decision-making by the NRC. So it's not as if you're just showing that you can envelop the data. It's just that it's actually that that statistic -- that their choice of that 59 runs is actually what the NRC's going to use.

DR. NUTT: That's the one that we're actually going to see.

CHAIRMAN WALLIS: Right.

MR. CARUSO: Yes.

CHAIRMAN WALLIS: This seems much better, I think, than the old days where this stuff wasn't quantified, and I would think you'd want to make more of that and maybe show that that's the kind of level, at least, that you expect in the future.

DR. KRESS: Do you have any ideas on how to approach this question of some sort of formal determination of looking at the oxidation levels and saying there's some -- what the probability is, as you've found the 95 percent on them, based on just the 59 runs? Do you have any ideas on how to approach that?

DR. NUTT: Oh, in terms of getting -- quantifying it?

DR. KRESS: In terms of satisfying Yuri's principle, you know. Clearly, the distance away is a good indicator.

DR. NUTT: And we could quantify it, and

we can quantify the distance away. I think the one thing we can do is go in and quantify the distance away based on -- and we haven't discussed it so this is purely a thought, okay, on this.

I think we can quantify the distance away and we can do that by looking at the samples, doing standard statistical tests using T-tests or --

DR. KRESS: Yes, that's what I --

DR. NUTT: -- I guess it's actually a chi-squared test on it. What I'll basically do is go use a DBO and one-sided upper tolerance and run out and find out when it goes. And basically, I'll take the DBO intolerance out until we find that it's gone to one. I mean, there's a 100 percent probability --

CHAIRMAN WALLIS: I think you can do that.

DR. NUTT: And we haven't reached it yet. And if we're -- and I think we can do that before we're a third of the way the limit.

CHAIRMAN WALLIS: Yes.

DR. NUTT: Which then says that given standard -- if I just looked at this -- these data, right, without referring to anything else, if I had just looked at these data and did the standard classical processing of this data, and I pulled out what the probability was that you would have violated it, I'd conclude that with 100 percent confidence, you know, it's virtually --

DR. KRESS: And you haven't --

DR. NUTT: -- you can get very close to 100/100 on that particular one, and that sight, that number. And I'm thinking about that approach and I'm going to discuss it, you know, but that's the first one.

CHAIRMAN WALLIS: If everyone was showing you --

DR. KRESS: That would be -- that would, you know, it would put on documentation what Yuri's problem is, what is meant by forced statistical analysis. And it would only be important for people who might get closer.

DR. NUTT: Right. Yes.

DR. KRESS: But those things might have been closer to somebody else.

DR. NUTT: And I think it -- and it doesn't address Yuri's concern about the fact that we take the 59 points and then we take sort of the blind approach, and then we take -- we look at what we got as an answer and then change our conclusions a little. I think that's legitimate to do it.

I think it's formalistically may be

offensive to him, but I think in terms of reality, I think the reality is there and I think it's --

CHAIRMAN WALLIS: You know -- so how you evaluate sort of three out, but it's going to be influenced by how these are related to each other.

DR. NUTT: Yes.

CHAIRMAN WALLIS: If you can learn something about that, then you've got -- I think you've got a -- at the outset you -- say you run 824, but as you learn that these things are correlated and that these are way away from the limit.

DR. NUTT: You're actually at the --

CHAIRMAN WALLIS: You can do some more analysis, which then, you know, can reduce the amount of information you need in terms of number of runs.

DR. NUTT: I think that --

DR. BANERJEE: It's called Bayesian on the fly.

DR. NUTT: Right, but it wouldn't --

CHAIRMAN WALLIS: But let's make it more consistent.

DR. NUTT: But even in the worst case it wouldn't be as high as 20/24, so.

MR. HOLM: Yes, this is Jerry Holm. I guess I'd have to say we continue to disagree with

Yuri that we aren't allowed to use all available information to support our conclusion.

Yuri, from my perspective, wants to argue that if I decide to use non-parametric statistics I can't bring any other information to play to help me reach my conclusion.

We actually did go off and do the non-parametric 95 percent or 10 percent, and then go off and make a response surface fit for the oxidation and show that we met the criteria with 100 probability, basically doing more of a mathematical statement of that visual picture that we had showing we were, you know, 20 standard deviations away.

If you do a -- just take the 59 points and do a fifth --

DR. BANERJEE: Standard deviations.

DR. NUTT: No, it's actually -- it ranges -- I think it depends on the case, but we were up to 74 standard deviations away on one particular case. On the cold weight observation --

CHAIRMAN WALLIS: That's the probability of ten to the minus --

DR. NUTT: Oh, it's much smaller than that. I knew you were going to -- I wanted to get to 100 percent.

DR. KRESS: You have to smooth out those 59 runs in order to do this, though.

MR. HOLM: You have to make an assumption for distribution.

DR. KRESS: Yes.

DR. NUTT: And it's approximate, but we were adding additional information.

DR. KRESS: But I think that's legitimate.

CHAIRMAN WALLIS: To make an upper bound and a lower bound, it's ten to the minus 70 or ten to the minus 55 or something, but it's still pretty darn small.

DR. NUTT: Yes.

CHAIRMAN WALLIS: So figure out what the low bound is.

DR. NUTT: Oh, I don't think I'd necessarily challenge --

CHAIRMAN WALLIS: But do something logical and numerical instead of just looking at it.

DR. KRESS: Yes, and that would close the loop on it.

DR. NUTT: And I haven't proceeded with that at the time, but we -- but I think this -- you know -- as long as that's agreeable with Framatome, we'll, you know.

CHAIRMAN WALLIS: Yes.

MR. HOLM: Again, if the staff would want that, we'd provide it. We did provide that in an informal fashion and that wasn't satisfactory at the time, I have to say.

CHAIRMAN WALLIS: Okay. Are we finished with this now?

DR. NUTT: Yes, I think the slides.

CHAIRMAN WALLIS: I'd like to go back to the committee and Ralph, and I'm sure that other members or anyone probably have some questions for Ralph that I didn't cover and we haven't covered up to now.

Sanjoy, you have questions for Ralph?

DR. BANERJEE: Well, I think, you know, the overall picture is somewhat like Ralph summarized, but there are individual bits and pieces of this that I still need to feel comfortable with. One of these issues which I looked into, this issue of sensitivity to the nodalization things.

And the second, I think, is heat transfer is obviously very important in this and I don't know if there has been any adjustment made to the heat transfer correlations or not, or how they are. I assume that Forslund-Rohsenow or whatever is being used as is.

Nothing has been adjusted. Nothing has been tuned. Lahey's take on Unow's correlation is being used as is. I haven't had a chance to look at exactly what's in the code, but I would like to know if you have had a chance and you're satisfied that these are not being adjusted or tuned or whatever between runs.

And the same goes for the drag correlations, you know, things like that.

MR. LANDRY: We have not looked at every single correlation in the code. As I said earlier, what we've done is a snapshot look. And from those that we have examined, we're satisfied that they are in their proper form.

The question that we had earlier with Forslund-Rohsenow is, is it proper to use it under certain conditions. But after looking at the plots that we asked for of a heat transfer correlation at the PCT mesh point, throughout a transient, looking at the range of parameters for which the correlations were valid and the range of parameters for which the correlations were being used, and further assessments which were done to say, yeah, those correlations are valid for these conditions, yes, we were satisfied with the heat transfer package overall.

DR. BANERJEE: They interact strongly with the drag model, as well, because I guess it depends on how many drops are formed and, you know, entrainment and there's sort of an interaction in the whole package.

MR. LANDRY: Well, if you go back and read all the RAIs. I don't know if Paul has provided all the RAIs to you and responses. If you go through those RAIs there are a number of questions which we raised with regard to some of the heat transfer modeling, the droplet modeling.

We raised a number of questions with regard to droplet size and whether the appropriate sizes are being used. Yes, we've raised a lot of questions. I think when we look overall at the performance we're able to say, okay, the performance overall is reasonable, the biases are understood, uncertainties are understood.

DR. BANERJEE: Right. The feeling I get looking at this is that this is what -- maybe it was -- a lot of the things are historical that were put in RELAP5 way back.

MR. LANDRY: It's not only RELAP5, it's --

DR. BANERJEE: Maybe RELAP4.

MR. LANDRY: It's not only the RELAP family. A lot of these correlations and models are industry-wide.

DR. BANERJEE: Right. I remember that when LOFT got the early rewetting and we didn't know it was the thermocouples, the codes were almost immediately able to predict simply by changing the correlation slightly, making it L2-2 or something.

MR. LANDRY: Well --

DR. BANERJEE: So I worry about these things a little bit. And of course, it wasn't true.

MR. LANDRY: No, it wasn't. And in fact, when we ran L22, in fact, you and I were sitting next to each other --

DR. BANERJEE: Right.

MR. LANDRY: -- when that test was run.

DR. BANERJEE: Right.

MR. LANDRY: And both moved to the monitor pretty fast, what in the world is going on; who screwed up the test.

DR. BANERJEE: Right.

MR. LANDRY: Within two days we had a calculation done with another code by another laboratory than the laboratory that ran the test, which --

DR. BANERJEE: Shall remain nameless.

MR. LANDRY: -- remain nameless, which 'lo and behold predicted the quench right down to the right amount.

DR. BANERJEE: Exactly.

MR. LANDRY: And we said, what in the world did they do. Well, overnight they had installed another heat transfer correlation and model and fluid flow model into the code and got the prediction.

In the weeks that followed when we started looking at what they were doing we came to the conclusion that they were getting the right answer for the wrong reason. They had fluid flow conditions that weren't even on the same sheet of paper as were occurring.

It was by jerry-rigging a model that they could get the answer. So we knew that it's not a matter of getting the right answer. It's a matter of getting the right answer for the right reason.

DR. BANERJEE: Right.

MR. LANDRY: And that's when we started looking very heavily at the thermocouple effect and started looking at the fin effect and realized that the uncertainty due to the fin effect on the thermocouples was over 20 degrees K in our first assessment.

And there have been various and various numbers quote since then of what the real effect is. So there's really not much purpose in trying to predict that quench, because it's not a quench that's phenomenological to the transient.

It's phenomenological to the design of the thermocouples and the fuel elements for that test.

DR. BANERJEE: Yes. That's right. The point I was trying to make was more that, you know, predicting peak clad temperature, the right one, is important, but it's also important to get a bunch of other things right, you know.

But this may be one output; it's an output which may not be particularly sensitive, though it's what you need for licensing. And it's one also that can be -- you know -- correlations can be easily adjusted to try to give you whatever answer you want.

In fact, it's relatively easy to do it. It doesn't mean that it'll scale to full-scale properly or whatever. So I feel more comfortable when a whole lot of things go sort of right in the test. If they're getting, say, the carryover awfully right, which is not measured, of course, in most of the tests, unfortunately, or some other parameters like the inlet flow is right, whatever has been made.

MR. LANDRY: Or the level.

DR. BANERJEE: Or the levels, something, you know.

MR. LANDRY: That would be -- sensitive.

DR. BANERJEE: I don't know. If I really sat down, I could probably think of it, and I'm sure you guys have. But if they get a bunch of things right with the code then it just gives you a higher comfort level, I think, than just a single parameter like the peak clad temperature.

And really, that's one of the questions I'm asking you, whether there were other parameters they got right in these experiments.

MR. LANDRY: That's correct. And that's why earlier when I was talking about the assessment and individual assessment cases, I was trying to indicate that it's not only getting a whole bunch of things right in a particular test.

It's getting a whole bunch of things right in a lot of different assessments so that you can say, well, this one particular assessment, they got this thing wrong, but they got a lot of other things right. And when you look at this whole spectrum of assessments, overall, they got a lot of things very well predicted.

Then we get a feel for overall the code is performing correctly. We might be able to find one test where we can go in and look atone parameter and say, no matter what code we're looking at, it's lousy. We can find -- for any code we pull in we can find something that's lousy or a particular test.

What we have to look at is overall, do we get a bunch of the parameters right? Do we get important parameters right and do we get them right for a lot of tests over a spectrum of sizes and a spectrum of conditions.

And that's where we're coming down to in the conclusion in saying, yes, overall we believe that the code is performing well.

DR. BANERJEE: Well, if that's the case then, you know, the fact that they all heat transferred back here or dragged back here or whatever is used is not very relevant. I mean, if a whole bunch of different things are got right over a whole bunch of scales and, you know, different parameters, that's pretty reassuring.

And you've really looked at this in some detail, and have you written some sort of assessment of this or put this all together or even in your own minds, case for it?

MR. LANDRY: I think we've pretty well put it together in our own minds. We may not have put it together on paper adequately, and as I indicated earlier, we are going to be revising the SER draft, trying to incorporate more of our thoughts and more of our experience and conclusions.

And in particular, based on the discussion of the last two days where we've seen a number of the questions that have been raised and items which the members of the subcommittee and the subcommittee's consultants have raised as important issues and concerns, so that we can pull together a stronger, more cohesive --

CHAIRMAN WALLIS: I think it's one thing we miss in an SER, is enough explanation for the outside or of the -- you know -- someone who's reading it for the first time about why you reach these conclusions, some of the breadth of the (coughing) and all that.

You may need to dig into it and you can remember things you've forgotten, and say, yes, we actually did that, and tell us because it gives us much more confidence in your final conclusion.

MR. LANDRY: It's a matter of being too

close to it.

MR. SCHROCK: There seemed to be quite a few places where you are struggling with some particular feature of the code, and finally, you give in and say, well, you think it's okay because it's conservative, you believe it's conservative.

And this seems strange to me, I guess. It's a realistic code and yet, the judgment of the acceptability still in many levels appears to hinge on whether you believe it's conservative or not conservative.

MR. LANDRY: Well, that doesn't bother me quite so much when I consider that the code is not a perfect tool and has a number of assumptions in it, in addition to being basically a one-dimensional code. It has some 2D capability in places, or even pseudo-2D capability.

It's not a pure three-dimensional, first principles capable tool in every respect. There have to be assumptions made and you're dealing with assessment against data, which have uncertainty in data which are not pure, either.

So in cases where we have to come back and say, it's conservative and that's good enough, really doesn't bother me too much. It's when we get to a conclusion where we've got to say over and over and over and over and over again, it's conservative and that's good enough, then we have to say, wait a minute, I thought we were realistic.

When we can back up and say in a lot of these parameters and a lot of these tests, yes, it is very close to reality that we can say, okay, it's realistic but it does still have some conservatisms.

DR. RANSOM: Well, certainly, if I put my hat on as the general public it disturbs me a great deal that after all these years of work the NRC does not have a standard that they can compare the results of a code to this and be in the position of an auditor and say, that's good enough or not. And what I'm hearing right now is that you people do not have this.

MR. CARUSO: But we do have RELAP5, the conversion. We do have TRAC-M coming along.

DR. RANSOM: Whatever you want to use.

MR. CARUSO: In this case it's a little bit different code to apply a -- you know -- our codes to do the audits because they're developing a methodology that they're going to apply to a particular plant.

If we -- I mean, I guess we could model the three-loop and the four-loop plants.

DR. RANSOM: I'm not sure whether they're attracting --

MR. CARUSO: Right.

DR. RANSOM: -- a fine line that you could use.

MR. CARUSO: Right.

DR. RANSOM: To do a calculation and compare where the results of this go. And I always thought that was the mission of the NRC, to be the auditor.

MR. CARUSO: And we do --

DR. RANSOM: To set the standard.

MR. CARUSO: And we do that. We do use our codes to audit other people's calculations, but I guess in this case what we're looking at is we're looking at their code calculations against data. They have actual assessment data, and at this phase in the process we're assessing -- we're looking at the assessment of their code against data.

At some point if they -- when it gets applied to a plant, then we'll get a chance to use our code against their code for the plant. But right now I'm not sure how much -- how valuable it would be for us to do our code calculations, because what we're really looking at right now is how well do they predict the data from the test facilities.

CHAIRMAN WALLIS: The ACRS is very much encouraged in development of your own code.

MR. CARUSO: We think that's a good thing.

CHAIRMAN WALLIS: We've been impatient to see it used and would like -- love to see it used for some of these auditing calculations. And also, when you use it you learn thing about codes which you can then use in assessing how other people have used codes, which is very, very valuable.

MR. CARUSO: We agree, and that's why you'll find that the people who are doing these assessments are the same people that use our codes.

CHAIRMAN WALLIS: And we'd like you to put this TRAC-M on platforms which label it to run rapidly and give a lot of results and be transportable to other platforms and all those good things.

MR. CARUSO: Here, here.

CHAIRMAN WALLIS: And we're dying to have this happen. We've been waiting for this and --

DR. KRESS: On this issue of the noding that's been brought up a few times, I'm sure by virtue of precedent and the way the regulations are written that it will be an issue here. But it seems to me like there is a -- what I'd call a confirmatory research issue here.

It seems to me like there ought to be something referred over to research to say, okay, do something to assure us that the way we do this noding is a proper way to do it. Now, I don't know what that is that they should do, but it seems to me like there is an issue of confirmatory research.

You've assumed it's all right and that it's mostly based on precedent and other things, but.

MR. LANDRY: Well, that's a part of what the CSAU team was trying to get at also, that at the time the NUREG CR-5249 was written virtually every modeler with every code for every different application used their own --

DR. KRESS: Their own nodes.

MR. LANDRY: -- idea of what nodalization was to be.

DR. KRESS: Right.

MR. LANDRY: And a big part of the approach in CSAU was to try to come to some standard or some consistency in an approach to nodalization.

DR. KRESS: Yes. But it's this question of, I'll take the noding I did for the full size integral test and map it one to one, basically, on the full scale. That's the issue I'm dealing with.

Is that the proper way to do it and that's the assumption that we've had and that's what the directions are and by precedent, the way it's been done. But you know, I think there's a legitimate question there that something, research could look in a mostly analytical sense and decide whether that's the right way to do it.

You know, it has some basis in what Vic said about the noding, but you know, I think there's a confirmatory research issue here.

DR. BANERJEE: I think with the CSAU, though, we clearly understood at that point that you could get -- were almost very wide range of answers by adjusting the noding, and that's the idea of freezing the noding as much as possible to remove this degree of, you know, ability to tune the results by tuning the noding.

MR. LANDRY: And also, in modeling nodalization for a plant analysis with nodalization from the assessment analyses through a different scale.

DR. BANERJEE: Right.

MR. LANDRY: To the plant so that you use a consistent philosophy; not necessarily a consistent nodalization, but a consistent philosophy in determining the nodalization as you move from one to another.

DR. BANERJEE: Right. And I think that's the idea. The problem is I don't know, you know, in retrospect after many years, whether -- how that is to be actually applied in practice, you know.

I mean, I can -- I think the idea's clear that you have to do this in a very consistent way. But within that consistent way there's a certain degree of freedom, you know. You can node the lower plenum in a certain way.

You can nose it in a different way, and you can get different results, I'm sure, because at the end it determines what the reflood will do, you know. So it's still a subject I'm concerned about and we are going to take a look and see what you've got already, and maybe it's fine, you know, at the moment.

But what Tom was saying, is -- I think it might be worth taking -- you know -- having research take another look and sort of giving us some feedback on that.

CHAIRMAN WALLIS: We could if we feel strongly enough about it put it in an ACRS letter.

DR. KRESS: Yes, indeed.

DR. RANSOM: Well, you know, there is some evidence on it. I think back in the days of Charlton at the INEL they ran some from ten nodes, you know. I don't know what the exact numbers were, but they increased the number of nodes, you know.

MR. LANDRY: Sure.

DR. RANSOM: And there is a study that's documented, I'm sure.

DR. BANERJEE: Well, they did it on a straight five --

DR. RANSOM: And it told you roughly what the nodalization sensitivity was for a PWR application.

DR. BANERJEE: Well, did they do it on a PWR?

DR. RANSOM: It was either a PWR or a LOFT, you know. It was -- and I am sure that can be found. But these kinds of studies have been made. It's not like no one has ever studied that.

DR. BANERJEE: No. I'm sure that it's been studied enormously, that there's been --

DR. RANSOM: Well, I mean, just repeated calculations to see what is the affect of increasing the density of nodes.

DR. BANERJEE: It's been studied but I don't think there has been some sort of definitive set of conclusions that have come out of it.

DR. RANSOM: Well, the conclusion that came out of that one is the relatively -- you know -- 100 or so nodes I think they concluded was adequate. There was no real improvement beyond that. Really, we should go back and look for some of that.

MR. BOEHNERT: But that doesn't -- you know -- maybe you ought to repeat it, though. I mean, it's --

DR. BANERJEE: Well, I don't know if it affects Ralph here, but in a general sense this is something that needs to be brought together and the experience polymerized (phonetic) in some concrete --

MR. BOEHNERT: Because Vic's right. I mean, I remember some of this stuff.

DR. RANSOM: Yes.

MR. BOEHNERT: We had discussions here where people talked about looking at the noding and so forth, but you're right. No one ever really pulled it all together and sat down and thought about it from the idea of -- standard criteria.

DR. BANERJEE: It would be really nice to have a white paper put this thing to bed at least temporarily, for awhile.

CHAIRMAN WALLIS: Sure, and while we're

looking at sensitivity to noding it seems to me we could also say, let's put this momentum thing to bed by saying let's look at sensitivity to the terms you put in your momentum equation and multiply the inertia terms all by two and by a half and maybe nothing happens at all.

And then we would stop worrying about the fact that you've made guesses and estimates in evaluating those terms. And if it turns out that the answers are quite sensitive to how well you evaluate what the terms -- you know -- that are approximate, then we need to know that.

That would change the way in which you consider whether or not this is a problem or what needs to be done about that. So rather than arguing about it every time we meet I think it'd be good if someone would do a sensitivity study to some of the terms and finish -- wrap up the answer to the problem.

DR. BANERJEE: It looks like it would take some tracking --

CHAIRMAN WALLIS: Yes, that's right. When we get this thing, you know, it's like waiting for Godot or something.

(Laughter)

DR. BANERJEE: Waiting for God.

CHAIRMAN WALLIS: Now, we have to make some decision, and the first thing we have to -- this subcommittee has to do is say, is this thing far enough along that it should go to the full committee next month.

DR. KRESS: I think we'd better. I think we should.

DR. RANSOM: Did you say you think it does not?

DR. KRESS: I think it does. In fact, I think the staff is close to saying this thing's ready to be blessed and I think it's -- if we got a difference of opinion I think it'd be timely to express it.

CHAIRMAN WALLIS: And if they need to do more, we need to know what it is they need to do.

DR. KRESS: Yes. So I think definitely we need to come back to the full --

DR. BANERJEE: Well, you know, as I said, tomorrow I'm going to spend time looking at something. Ralph is very kindly making available some information, and I'll write it down. But my sense of it from talking to Ralph is that it would be okay to go forward.

DR. KRESS: So given that, we have to

decide what to do.

CHAIRMAN WALLIS: Which I would -- well, I did -- yes, what do you think, Vic? Is this ready to go?

DR. RANSOM: Well, you know, my aspect is I'd like to see some hard evidence at how it behaves. And the only thing I've seen so far is that one curve that Bill Nutt showed that gives me any feeling of satisfaction at all.

And I know there's a lot of assessment in the document. I don't know if it's my job to dig through that and come to some judgment or whether the staff should summarize that. And you know, I'm sure the ACRS would like to see at least a few examples of how good it is.

CHAIRMAN WALLIS: Why is it --

DR. RANSOM: Otherwise, I don't know how you'd make any kind of conclusion.

DR. KRESS: Are you saying we need another subcommittee meeting to see that or --

DR. RANSOM: Well, I'm just telling you what my feelings are. I don't know what I'm recommending.

CHAIRMAN WALLIS: Well, you can certainly, between now and December, dig into the documentation.

DR. KRESS: Yes, that would be one approach.

DR. RANSOM: Well, that'll satisfy me, I guess, and all that.

CHAIRMAN WALLIS: And then I think we can -- if we agree that this should go to the full committee, then we can give Ralph advice about what he needs to show. And I think it's much more impressive to show the full committee the kind of thing that Bill Nutt showed than to show a lot of stuff about the chronology of the -- how the regulations were satisfied or something.

I mean, that's -- we assume that's happened. We don't really care about the year 2001 something happened and something else happened. We can go through that very, very quickly.

The thing is, what's the real hard evidence on which you base your conclusions and how can you put that across to the full committee so they say, yeah, he's made the right decision for the right reason. That I think is what we need to think about in giving you some advice.

MR. SCHROCK: Well, there's a loose end of this question of whether the break size should be included in this --

DR. KRESS: Yes, I think definitely that ought to be discussed.

MR. SCHROCK: And so do you think you want to take it to the full committee before you get that resolved? That's a question I have. I don't know.

CHAIRMAN WALLIS: Ralph, would you take it to the full committee if you had not resolved that?

MR. CARUSO: I would be reluctant. I think we have to have that resolved before we go.

MR. LANDRY: I agree.

MR. CARUSO: To the full committee.

CHAIRMAN WALLIS: And you are going to -- is there a high probability, since we're in that sort of a world, a high probability --

MR. CARUSO: You're asking for a 95/95 or?

CHAIRMAN WALLIS: Well, you know.

DR. BANERJEE: How many test cases do you have to do, 59?

MR. BOEHNERT: Joking aside, we need some definitive answer fairly soon, because we have this scheduled, and if I'm going to knock it off the agenda I should know very quickly.

MR. CARUSO: I understand. We will be trying very hard to resolve this.

CHAIRMAN WALLIS: We need to -- if we're

going to look at the next version of the SER we need that two weeks before the committee meeting or something. I mean, the committee doesn't like to evaluate things that it hasn't seen for long enough to evaluate.

MR. BOEHNERT: Yes. That's the question. Are you going to give us another draft on the SER before the ACRS meeting? Is that a goal or --

MR. LANDRY: Yes.

MR. BOEHNERT: -- is that -- yes?

MR. LANDRY: Yes.

MR. BOEHNERT: Oh, dear.

CHAIRMAN WALLIS: It should be two weeks before.

MR. BOEHNERT: It should be now.

CHAIRMAN WALLIS: Isn't that what we just -- it should be now; it should be now.

MR. LANDRY: Okay. Make it tomorrow.

MR. CARUSO: W#hat, we have three weeks before the committee?

MR. BOEHNERT: Yes.

MR. CARUSO: So we have a week.

MR. BOEHNERT: Yes.

MR. CARUSO: Okay.

MR. BOEHNERT: And when we -- well, yes.

CHAIRMAN WALLIS: Now, so it looks as if this is going to go before the committee in December and this is where they will make a decision, write a letter and praise or castigate or whatever they want to do.

DR. KRESS: If we don't go before the committee in December then the next point we could do it in would be February.

CHAIRMAN WALLIS: That's correct.

DR. KRESS: And you know, that's getting down the line.

MR. CARUSO: We understand.

CHAIRMAN WALLIS: So this means that we need very concise and relevant and persuasive presentations by both Framatome and the staff at the committee meeting.

MR. BOEHNERT: Right now, we have a total of an hour and a half dedicated to this. I'm trying to get it to two hours. I think we really need two hours, given what I'm hearing here.

CHAIRMAN WALLIS: Normally, we give Framatome a bit longer?

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: We give them, say, an hour and we're off half an hour?

MR. BOEHNERT: That probably -- that's what I was thinking, if we could pull that off.

CHAIRMAN WALLIS: I think then we have to advise Framatome on what it is that they should emphasize in their presentation.

MR. BOEHNERT: Absolutely.

DR. KRESS: Well, I think the key to me is how you assess the biases and the uncertainties in the plan. I mean, that's what all this assessment is all about. So you know, I would focus on that part of it somewhat.

CHAIRMAN WALLIS: And then show us some data,

DR. KRESS: Yes, data wouldn't hurt at all.

MR. CARUSO: Data is correct.

DR. KRESS: Curves or whatever.

MR. SCHROCK: Well, Framatome has promised to improve the documentation. I don't know what impact that would have on conclusions --

DR. KRESS: I'd -- yes, I --

MR. SCHROCK: -- made by the full committee, but --

DR. KRESS: Yes. I don't think we need to go through that whole presentation where they showed all the equations.

MR. SCHROCK: Yes.

DR. KRESS: And the control volumes. I think we can probably not --

MR. SCHROCK: What I'm wondering is if you don't want something more specific regarding that --

CHAIRMAN WALLIS: Or something which would close the loop.

MR. SCHROCK: -- obligation that you can present to the full committee.

CHAIRMAN WALLIS: Close the loop so that it insures that this is actually done. Well, we will all see this again. We will see this again in connection with BWRs and BWR LOCAs and realistic LOCAs and so on.

And I would think that although we're extraordinarily patient people, you might try that patience if you were to come back with something which was not in good shape. So we do have a check on it at that time.

DR. BANERJEE: I guess Virgil's critical flow questions need to get answered.

MR. SCHROCK: What's that?

DR. BANERJEE: Your critical flow questions need to get answered.

MR. SCHROCK: Seems to be lost in the noise, but I'd sure like to see them take those comments a little more to heart and address them in the revision of the documentation.

CHAIRMAN WALLIS: Well, there's another thing that this full committee will get, is that I expect you will all submit a report, particularly the consultants, which they won't be at the full committee meeting.

And this will be available to the full committee and it may have some significant influence on what they do.

DR. RANSOM: Whatever we write up from this meeting you're going to put together, then?

CHAIRMAN WALLIS: Paul will put it together for the full committee, and I will put together a draft letter of what I think the full committee might consider deciding, and it will be influenced by what I hear from you folks. And the other --

MR. BOEHNERT: I need more guidance for Framatome. All I've got right now is assessment of bias and uncertainty, and maybe some discussion of documentation.

CHAIRMAN WALLIS: Not -- just a really

convincing statement that they're fixing the documentation, isn't it?

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: You don't need to go into the details of the documentation.

MR. BOEHNERT: No.

MR. MALLAY: Yes, this is Jim Mallay. I'd certainly be prepared to make a statement there that's similar to what I did this morning.

MR. BOEHNERT: Yes. Okay.

CHAIRMAN WALLIS: Right.

MR. MALLAY: I think, you know, that's two minutes' worth. I think what we would like to do is address the uncertainties, the validation process.

CHAIRMAN WALLIS: Well, we have Larry's presentation. It took --

MR. MALLAY: Yes.

MR. BOEHNERT: Yes. I was going to say something along the lines --

CHAIRMAN WALLIS: Something like what Larry said. It took, what, a couple of hours?

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: It didn't take all that long.

MR. MALLAY: No.

CHAIRMAN WALLIS: And I think you should step through the CSAU, which I think the committee probably needs to be reminded about. I think saying about half as much or less than you did say, when we figure out what really, really is important in it.

MR. MALLAY: Yes, I believe we can do that.

DR. RANSOM: Well, along that line, I think I would find it much more insightful if you would list the parameters, you know, that you're including or have you found, you know, just a summary.

CHAIRMAN WALLIS: They did that in the past.

DR. RANSOM: The uncertainties.

CHAIRMAN WALLIS: Yes.

MR. MALLAY: Yes, I think we could be more explicit in that regard and I think we can also show some validation specifically similar to what we did with Dr. Nutt.

DR. RANSOM: And I think some -- a few examples of validation.

CHAIRMAN WALLIS: Excuse me. The subcommittee or at least some members have seen these things before, but not all the members. And then as far as the full committee goes, I don't think they have seen these.

MR. BOEHNERT: No, because we don't --

MR. MALLAY: No.

CHAIRMAN WALLIS: Evaluation. So I think the plan where you actually found the effect of all these various things on PCT and what was it most sensitive to. I think that was very, very useful.

You didn't present that here at all, but that sort of thing is going to be useful to the full committee. What other advice can we give them?

MR. BOEHNERT: Well, that's probably going to more than fill an hour right there.

MR. MALLAY: Yes, that's -- yes, we'll be challenged.

CHAIRMAN WALLIS: I would cut down the words in the slides and show more figures or some such, which sum up something.

MR. MALLAY: Right. I think we understand the thrust.

CHAIRMAN WALLIS: And you know, you have to put your really best foot forward. This is the real show. This isn't a rehearsal or anything. This is it.

MR. MALLAY: We understand.

CHAIRMAN WALLIS: There's no replay.

MR. MALLAY: This is extremely important to us and I've given dozens of presentations to the full ACRS. So I'm familiar with the drill.

MR. BOEHNERT: And I'll be allotting about 60 plus minutes for ANP Framatome and about 30 for the staff.

CHAIRMAN WALLIS: Right.

MR. BOEHNERT: And then the rest for you and wrap-up, so.

CHAIRMAN WALLIS: I think we've told Ralph what he needs to do, and the main thing is that, why do you make these decisions that you make and what are the reasons and why should we have confidence that you've done it right. Okay. We're doing certainly well on time. It's because it's a subcommittee.

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: Right.

MR. MALLAY: Would it be appropriate for me to make a comment or two on the --

CHAIRMAN WALLIS: Yes, I think it's be very appropriate. I'd really love you to do that.

MR. MALLAY: Okay.

CHAIRMAN WALLIS: Please do.

MR. MALLAY: First of all, we appreciate very much the confidence the subcommittee has apparently shown today. We take your comments very seriously, specifically, the critical flow that Virgil continues to remind us of, the documentation situation and so on.

But I think generally we appreciate very much the confidence that you've shown in our model and the presentations that we've made. There's a couple things I think I need to point out. First of all, we feel very, very confident about our statistical approach.

We feel it's appropriate, that it's well-founded. And as Jerry Holm mentioned a few minutes ago, we feel it's important to exercise a reasonable level of engineering judgment, specifically on the case of the three parameters that we're looking at, peak cladding temperature, local oxidation and total oxidation.

We all know that they're very closely correlated. You don't get significant oxidation without elevated temperatures, for example. So we know they're correlated and we appreciate your understanding of that.

Secondly, in the application of our statistical approach we are also very confident that it's appropriate to look at the break size as part of those parameters. We feel it's not only appropriate, but I think we meet the regulation, namely that we examine the full spectrum of break sizes.

And even though I'm not prepared to quote specific numbers, it's pretty clear that the large break size dominates the results from the 59 cases. We typically get anywhere from .8 to .9 or so break size that will dominate. So I'm hopeful and I'm confident, frankly, that the staff and us can reach a resolution of this.

Dr. Wallis, you had mentioned also about the conclusion here in the draft SER, the fact that at the end of that first bullet it said something to the effect that addresses the regulation. I would certainly second your comment that the conclusion should be very clear that the model is acceptable, for whatever reasons.

Addresses certainly doesn't do that in our opinion. We also recognize at this point that unfortunately the SER is sort of in a dynamic situation. It's our commitment to work very closely with the staff over this next week to reach resolution on a -- there's probably a half a dozen minor items of what I'll call clearing up the language in the SER itself.

And so in addition to reaching resolution on the break size situation, we'll be working with them to clean up these other pieces of language. I think that's all I had. I don't know whether Jerry Holm has anything to add. But again, we appreciate your time.

CHAIRMAN WALLIS: Jim, you mentioned that you felt very strongly about your statistical approach.

MR. MALLAY: Yes.

CHAIRMAN WALLIS: I think you should come armed with the best evidence you have. I mean, that business of showing the statistical distribution of the O2 versus -- if you're not going to show it directly, you certainly should have it ready to show.

And I think you really need to show that evidence in the best form you can, not in lots of different forms, but if you've got a certain plot that shows the message most clearly, please show it. Don't be bashful about it.

MR. MALLAY: We will certainly do that.

CHAIRMAN WALLIS: Thank you. Anything else? Well, I would thank Framatome. Thank you, Jim, and all your folks for coming here and making your presentations and submitting to our interrogation and everything, and the same for the staff.

Thank you very much for coming here and making a presentation. And we will then look forward to seeing you in about three weeks. If there's not anymore we have to do, I will close this meeting. Thank you.

(Whereupon, the Open Session was concluded at 5:15 p.m.)