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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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OPEN SESSION

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THURSDAY, JANUARY 19, 2006

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The meeting came to order at 8:30 a.m. in room T2B3 of Building Two, White Flint North, Rockville, MD.

PRESENT:

GRAHAM WALLIS	CHAIR
VICTOR RANSOM	ACTING CHAIR
RICHARD DENNING	
THOMAS KRESS	
RALPH CARUSO	DESIGNATED FEDERAL OFFICIAL
SANJOY BANERJEE	CONSULTANT

A-G-E-N-D-A

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

8:34 a.m.

ACTING CHAIR RANSOM: The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguard Subcommittee on Thermal-Hydraulic Phenomena.

I'm Victor Ransom, Acting for Chairman Wallis, who is Chairman of this Committee.

Subcommittee members in attendance are Tom Kress, Richard Denning, Dr. Graham Wallis. He's been delayed by bad weather but apparently he'll join us later. And the consultant in attendance is Professor Sanjoy Banerjee.

The purpose of this meeting today is twofold. First we will review the analytical methods to be used to evaluate stability scenarios for the economic and simplified boiling water reactor, ESBWR. Then we'll hear from the NRC staff about their plans to revise Regulatory Guide 1.82 Revision 3 to reflect some comments that the Committee provided in the fall of 2005 concerning the proposed revision 4 to the Regulatory Guide.

The Subcommittee will hear presentations by and hold discussions with representatives of the

NRC Staff, General Electric Nuclear Energy and other interested persons regarding this matter.

The Subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full Committee.

Ralph Caruso is the Designated Federal Official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the *Federal Register* on December 23, 2005.

Portions of this meeting will be closed for the discussion of proprietary information.

A transcript of the meeting is being kept and will be made available as stated in the *Federal Register* notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard by the hearing impaired as well as the normal people.

We have received one request from GE to make a presentation related to the revised Regulatory Guide, and that presentation will be heard after the Staff discussion this afternoon.

I'm looking forward to some of the discussion. I'm sorry that Chairman Wallis is not present. I think he has a lot of things that he would go into. He's provided some comments to us in writing. And we'll try to reflect those as much as we can.

We'll now proceed with the meeting. And I call upon Ms. Amy Cubbage of the Office of Nuclear Regulations to begin.

MS. CUBBAGE: Thank you.

I'm just going to provide a few opening remarks this morning. The senior project manager in charge of the ESBWR review for NRR.

This is just a background on the design certification status before we get into the topic for today.

After a roughly three year preapplication review, GE submitted an application for final design approval and standard design certification for ESBWR in August, 2005. That application was accepted for docketing on December 1, 2005. We're currently in the process of issuing RAIs and we'll be issuing RAIs through October '06. We're scheduled to issue an SER with open items in October '07. And then we're assuming it'll take us about 15 months to close the open items identified in that safety evaluation and issue a final design approval. And then an additional 12 months is required for the rulemaking process. So we have a total nominal duration of 42 to 60 months for design certification review.

Just listed some of the previous occasions that the Committee has heard about ESBWR. In 2003 and 2004 there were meetings on TRACG LOCA application. And then this fall GE provided an overview of the ESBWR design to the full Committee.

And on December 2004 GE submitted a typical report TRACG application for ESBWR stability analysis. So that was submitted during the preapplication phase. We have now completed our review of that typical report and have provided you with a draft safety evaluation report. So this morning GE will be presenting the content of that typical report. And then this afternoon the Staff will present their review efforts and evaluation of that report.

So I'd like to introduce David Hinds from General Electric.

Would you have any questions?

MEMBER KRESS: When is the full Committee supposed to review this? Do you recall that?

MS. CUBBAGE: We had some discussion about that this morning as the Staff was under the assumption that we had been bumped to March. But Ralph has indicated to me this morning that we may be on in February.

DESIGNATED OFFICIAL CARUSO: You're currently scheduled to look at it in February, February 9th, in three weeks. And we'll be talking to the Staff about this.

MEMBER KRESS: And we'll write a letter then on this application?

DESIGNATED OFFICIAL CARUSO: That's the intention. That's what the Staff and GE hope that we will do is write a letter.

MS. CUBBAGE: Right. And this review is limited to the applicability of this code.

MEMBER KRESS: Just you have to build your stability?

MS. CUBBAGE: Right. And we're not making any judgments at this time on the design or the stability of the design, just rather the applicability of the method.

MEMBER KRESS: And it doesn't included ATWS.

MS. CUBBAGE: It does not include ATWS. GE submitted a typical report on ATWS just this week and we met with them yesterday as a kickoff. We have not started the review of the TRACG ATWS.

MEMBER KRESS: Will that be a separate review and letter?

MS. CUBBAGE: It's a separate typical report certainly from this effort. It's going to be rolled into the design certification review process.

David?

MR. HINDS: Hello. Good morning. I am David Hinds. I'm with General Electric, the ESBWR Engineering Manager.

Glad to be here today to meet with the ACRS to discuss stability, our methods related to evaluating stability.

Just like to briefly introduce our team. We have Bharat Shiralkar, who will be leading the discussion today for at least the General Electric portion and supported by Jim Shame, Wayne Marquino. We also have Allen Beard and Louie Quintana from our licensing department.

Again, Bharat will have the lead on the presentation and, of course, the rest of the team's here to answer as many questions as we can during the presentation.

Again, thanks for having us here. And I can turn it over to Bharat.

MR. SHIRALKAR: Good morning. Glad to be back here again. The last time I think I met this Committee was in January of 2004, years ago when we talking low containment.

Today's topic is stability, ESBWR stability and the methodology we're using. We try at GE to analyze ESBWR stability.

MS. CUBBAGE: I'm going to get a lapel mike. Is it there?

MR. SHIRALKAR: Should I sit down next to the mike?

MS. CUBBAGE: Yes, and I'll get a mike for you.

MEMBER KRESS: What does the E stand for?

MR. SHIRALKAR: Economic. But we go prefer to go just by ESBWR.

This is the outline for my presentation. I'll try to give us a little bit of background to start with. And this is just to establish some common terminology. At GE we trend to use certain terminology that may not be universal, so I'd like to kind of tell you a little bit about our terminology. And also talk to you a little bit about starting up as to why the ESBWR stability is so much more stable than an operating plant in natural circulation. Just as a kind of a lead in because I think that question has come up in the past and I'd like to sort of give you a little background early of that before I get into the main body of the report and start stepping through the report. Okay.

After that I'll get through the LTR licensing topical report purpose and scope, the licensing requirements, the application methodology which we'll try to be compliment with the CSAU approach, phenomena identification and ranking, model applicability. And at this point I have a few topics that are proprietary. I would prefer to keep the flow to make this part proprietary and then move back again to a non-proprietary session if that's okay with you.

ACTING CHAIR RANSOM: It's okay.

MR. SHIRALKAR: Is that right? Okay.

ACTING CHAIR RANSOM: One comment I have is that I think that there are quite a few questions about how the Chan component and the TRACG vessel nodalization is applicable or representative of the ESBWR geometry. And maybe if somebody would go into a little bit of that at the appropriate time, that would be helpful.

MR. SHIRALKAR: Sure. You mean how we represent the vessel and the channels and how they're coupled together and so on?

ACTING CHAIR RANSOM: Right, and particularly the module dimensional aspects of the thermal-hydraulics as related to the neutronic feedback.

MR. SHIRALKAR: Okay.

ACTING CHAIR RANSOM: And also cross flow between the bypass regions.

MR. SHIRALKAR: All right.

ACTING CHAIR RANSOM: And the chimney.

MR. SHIRALKAR: I'll try to do that. And we have Jim Shome who is more of an expert on the details of TRACG so we can get him here to talk to you about it in more detail if you need to.

So the proprietary session then I would like to cover model biases and uncertainties, plant parameters, initial conditions and how we combine the uncertainties using a Monte Carlo process.

And then I have a final nonproprietary session on plant startup. So if it's okay, I'll do the proprietary section session in the middle and then come back and nonproprietary session on the end.

Just to remind you again, the general layout of the vessel. The flow comes down through a downcomer and goes up through a fairly short core. The core is shorter than the normal BWR core. It's three meters high instead of 3.5 meters. And then a tall chimney region, which is about 9 meters high including the upper plenum here at the top and goes through the separators. The steam exists through the dryers and the flow returns from the separator spillover back down to the downcomer.

So in concept it's not very different from a regular BWR. The main difference is that we have natural circulation driving the flow and we have this large chimney region which doesn't exist in the BWR.

We'd want to make the point that the chimney region itself is each chimney cell encompasses 16 bundle, a 4x4 array. So it gets the flow from 16 bundles and the bypass interstational region in between those bundles. So that's all flowing into like one chimney cell.

And then the chimney cells look like this. They're partitioned and they're about 9 meters high. Okay.

ACTING CHAIR RANSOM: What provision exists at the bottom of the chimney for cross flow?

MR. SHIRALKAR: The chimney essentially rests on the top guide.

ACTING CHAIR RANSOM: Is that sealed or is it --

MR. SHIRALKAR: No, it is not sealed. It sits on it. But we expect very little cross flow leakage between those regions as compared to the resistance -- I mean, it's an open region. If you look at the--

ACTING CHAIR RANSOM: What are the dimensions of the open region?

MR. SHIRALKAR: The gap you mean?

ACTING CHAIR RANSOM: Right.

MR. SHIRALKAR: It just sits on top of it so I don't know what the gap would be. I mean, it would be like millimeter. Whatever the unevenness of that surface is, I guess.

ACTING CHAIR RANSOM: Well otherwise it basically is sealed but it's just sitting on top then?

MR. SHIRALKAR: Yes, it's sitting there on top of it.

ACTING CHAIR RANSOM: So each 16 unit definitely feeds that chimney section then?

MR. SHIRALKAR: Yes. Yes.

I mean if you look at the resistance in cross flow over here versus the opening over here, I mean it's orders of magnitude different. I mean, so the flow is going to go straight up here. There's very little gap at the top of this guide.

DR. BANERJEE: Have you done any experiments with multiple chimney cells to see what this --

MR. SHIRALKAR: No, we have not. No. So this is just based on relative resistances in the lateral and the vertical directions.

DR. BANERJEE: So you have done experiments with single chimneys?

MR. SHIRALKAR: We have not exactly with single chimney, but we have done experiments with circular pipe with about the same hydraulic diameter of the chimney cell to look at void fraction in that region.

DR. BANERJEE: And how was this pipe fed, by sort of a bunch of channels?

MR. SHIRALKAR: No, it was fed by -- actually it was fed by a single pipe. It was done in Ontario, Ontario Hydro. That's a facility for testing pumps. And they used one of the risers in that leg and fed that with the flashing mixture and then they measured the void fraction in that length.

DR. BANERJEE: What diameter was the pipe?

MR. SHIRALKAR: Fifty-one centimeters.

DR. BANERJEE: And what's the diameter of the chimney?

MR. SHIRALKAR: This is about 60 centimeters in terms of the divot.

Okay. In terms of background, really briefly the terminology and the types of instability analyzed. I'd like to tell you a little bit about the natural circulation performance of ESBWR and why you get a much larger flow in natural circulation compared to operating reactors. And then some comparisons with operating plants. Now there are a lot of similarities and there are some differences. And I'll touch upon those.

We basically look at three types of instability mechanisms. One is the simple channel hydrodynamic oscillations in which you keep the power. The power is constant. No power oscillations on the channel. The pressure drop in the channel is constant. So this is, if you will, a single channel that is being driven by a CF channel, it's maintaining constant pressure drop and power. And you're looking at the possible hydrodynamic instability in this channel.

This is not going to happen in a reactor, instability of this kind, because the channels are tightly coupled. But it's a very useful way for us to get a measure of the channel stability and in the design process when you're designing a fuel in terms of what the channel stability is.

Core-wide oscillations depend on the neutronics fundamental mode and the flows and flux oscillate in-phase. So if you really have flows all across the core going up and down at the same time, which means that the power is also going up and down in-phase across the core and the ⌂p is oscillating across the core because the flow is oscillating, total flow is oscillating. And this is exciting, what we call the fundamental mode of the kinetics, the neutronics. You have basically the normal power shape of the reactor, then you have a critical reactor.

The regional oscillations, we call them regional oscillations. They're also referred to as out-of-phase oscillations. And these depend on the channel hydrodynamics exciting higher modes of neutronics. And this was first proposed by Jose March-Leuba in about 981. And we have very good validation that that in fact is what is happening for all these regional oscillations.

And fluxes and flows in regions you oscillate out-of-phase.

ACTING CHAIR RANSOM: Since each one of

these chimney regions and the corresponding core parts are essentially coupled as a unit, you can also have channel-to-channel oscillations, right?

MR. SHIRALKAR: That's right.

ACTING CHAIR RANSOM: Is that what you're saying?

MR. SHIRALKAR: I mean that's what I call the first that I referred to as individual channel oscillations.

And you're absolutely right. We've also looked at a possible mode where you have a group of 16 channels together with the chimney cell above it whether that would oscillate. Okay.

So in a regional oscillation different parts of the core oscillate out-of-phase. And the total core in flow will be almost constant, which is why we're more concerned about this kind of oscillation because you get cancellation effects. You could get local power peaks but which wouldn't be seen on an average powering monitor because they'll be canceling out in different regions. And this excites higher or neutronic modes. And to give you an example, higher modes of the --

ACTING CHAIR RANSOM: One question that bothered me a little bit in a looking at this is the core is divided into six regions, somewhat uniformly. And --

MR. SHIRALKAR: Six regions? You mean nodalization. You mean TRACG nodalization?

ACTING CHAIR RANSOM: In terms of the thermal-hydraulics you have an azimuthal nodalization of the six angular sectors.

MR. SHIRALKAR: We'll talk about that later, but yes go ahead and ask your question.

ACTING CHAIR RANSOM: All right. Well, I guess my concern would be that's a rather course from the standpoint of channel-to-channel oscillation?

MR. SHIRALKAR: Oh, yes. I mean, normally--

ACTING CHAIR RANSOM: Compared to, say, the neutronics.

MR. SHIRALKAR: The sectors are not really there for the core at all. The sectors are actually there for the chimney. I'll talk about that later.

ACTING CHAIR RANSOM: All right.

MR. SHIRALKAR: I mean we can model individual channels thermal-hydraulically. The vessel is modeling basically the bypass region.

ACTING CHAIR RANSOM: Right.

MR. SHIRALKAR: So that just determines

how the channels provide energy to that bypass. But the channels can be located anyway you want as long as they're coupled.

ACTING CHAIR RANSOM: Well, the only problem with that is the boundary conditions for the channel are the same within that sector, I believe.

MR. SHIRALKAR: That's true. Yes.

This is what a call a first harmonic, azimuthal harmonic power distribution. What I've shown is this is upper from a PANACEA code which looks at every single channel in the core. Every box here represents the axial shape of a given channel. Okay. And so from the top to the bottom.

And you can see that this is a -- the first harmonic is actually perturbation on top of the fundamental. So you see here that some -- this region is negative, this region is positive, this perturbation. The negative and positive. The one I have darkened are the maximum amplitude channels. So you can see that these two guys are out-of-phase and have about the same amplitude. So this is symmetrical about this diagonal.

DR. BANERJEE: Is that the perturbations you're showing?

MR. SHIRALKAR: Yes. The harmonic is

basically perturbations that aren't fundamental.

And this mode is not critical so you won't get it in steady state operation. Okay. But it has subcriticality, in this case about .0058 in Eigenvalue. But this mode can be excited by the thermal-hydraulics during an oscillation, which means that you have overcome the subcriticality of that harmonic. The subcriticality then is like a damping of the system. So that has to be overcome. The neutronic damping has to be overcome by the thermal-hydraulic to produce the oscillation.

ACTING CHAIR RANSOM: What does diagonal correspond to?

MR. SHIRALKAR: It's one of the line symmetry in the core. The core is typically loaded symmetrically like octan symmetry. This is what we calculate to be the line of symmetry for that particular mode. There are a number of octagonal modes to show you --

ACTING CHAIR RANSOM: I mean, can it be anywhere?

MR. SHIRALKAR: It's what's calculated. Yes. It could be -- generally it turns out to be for a typical BWR turns out to be like this and that way, octagonal.

ACTING CHAIR RANSOM: What does that correspond to, the line of symmetry of the core loading pattern or --

MR. SHIRALKAR: No. It's the symmetry of the harmonic, the harmonic mode that you calculate from the neutronics.

ACTING CHAIR RANSOM: So does it correspond to one of the sector lines in the vessel nodalization?

MR. SHIRALKAR: It doesn't have to, but we've chosen it to be the line of symmetry. I'll show you that later on as we go along as to how that corresponds with the nodalization.

ACTING CHAIR RANSOM: How does the real reactor know where this line is?

MR. SHIRALKAR: The real reactor knows because that's how the solution of the neutronic fueling equation tells it what the line of symmetry is.

MEMBER DENNING: But as far as your loading pattern is concerned, that is a line of symmetry, isn't it?

MR. SHIRALKAR: Yes. Yes.

MEMBER DENNING: Yes. I mean --

MR. SHIRALKAR: It's one of the line of

symmetry in the loading pattern.

MEMBER DENNING: It is a line of symmetry. I think you said that it wasn't. But I think that it is a line of symmetry.

ACTING CHAIR RANSOM: But if you look at the core pattern, it looks like it's almost anti-symmetric.

MR. SHIRALKAR: No, the core pattern wouldn't be symmetric. It's likely mostly octan symmetric. And this is one of the lines of symmetry.

So if I show you what the other harmonics look like, now these are octagonal harmonics. So you see the first two harmonics are azimuthal harmonics and they're about the same subcriticality. Okay. So this one is octagonal to that one. They're all octagonal.

And then you get to -- you have an axial harmonics where you get plus or minus in axial direction. You can get a higher order as you move to harmonic. You can get radial harmonic. But as you see as you go down this progression, the subcriticality gets larger and larger. It gets more and more hard, excited harmonics. So you always excite these harmonics first. So all the data we have in operating reactors, which are out of -- fall in this category.

In fact, you can have -- so the question is whether it go about this one or about that one. And it's equally likely. And you can have a combination of the two actually happening.

And I remember being in a control room in a European plant where we were doing a stability test, and grant, it first went unstable around this diagonal. And then as we watched, it precessed, the line symmetry precessed until it become symmetrical about this diagonal, and then it went back.

So either of those diagonals was a possible mode for the oscillation.

But the point here is that you're not going to get any local harmonics or any much higher order harmonics happening because they have much higher subcriticality. So what you're going to see is basically these harmonics.

DR. BANERJEE: So when you say often symmetry, let's say with the higher order mode, those two lines then correspond to -- that's divided into four now.

MR. SHIRALKAR: Yes.

DR. BANERJEE: So you'd have to divide that into eight?

MR. SHIRALKAR: Well, but these are the--

DR. BANERJEE: They're the most unstable, right?

MR. SHIRALKAR: Yes. No. Yes.

DR. BANERJEE: So but if you had octan symmetry, let's take the other one and draw two lines between the pluses and minuses, so why does it select these two diagonal lines rather than the vertical and the horizontal line?

MR. SHIRALKAR: That's the solution that you'd get from the kinetics. Now, I don't have a good physical explanation to give you. Maybe Jim or Jose can speculate on that. But it's a physically solid fusion equation for the reactor, we extract the harmonic solutions. Then we find that they are typically these kinds of symmetry.

MR. JAN: And that's strictly either a loading pattern or -- Jim Jan.

The choice of that line of symmetry would be a function of the loading patterns so that, you know, the bundle peaking, the composition will impact that choice as well as any control rods that are in the core at the time of the evaluation will dictate where that line of symmetry or the lowest subcriticality line of symmetry will exist.

DR. BANERJEE: But in fact he says that

there is often symmetry in the loading pattern, right? So that means that the vertical and the horizontal lines could also be selected. It just depends on how your control rods are at that time or --

MR. JAN: That would be the primary difference.

DR. BANERJEE: So there will be some detail that drives you to one?

MR. JAN: Yes. And again, if you use octan symmetric, then it really doesn't matter. I mean, it's just a calculational variation and the impact on the calculation is the same.

DR. BANERJEE: So in other words it selects a line so that in fact everything as symmetric as possible about it, right?

MR. JAN: Yes. It's going to set up that line of symmetry such that the Eigenvalue is the lowest.

DR. BANERJEE: Right. Could we go back to the other slide just to fix this. Yes. So those two hottest channels are symmetric in some way, that's what I say.

MR. JAN: Yes. That the distribution has the lowest possible Eigenvalue.

DR. BANERJEE: That depends on your

control rod pattern?

MR. SHIRALKAR: Yes. In fact, we also find that the oscillation is driven, the fundamental mode oscillation is driven typically by the highest power channels or actually the square of the power in the channels is what's the driving. For the harmonic mode it's the product of the fundamental times the harmonic. Okay. So in some way that product has to be uniform or has to be symmetrical as well. So that may be another consideration that comes into why -- symmetric about a particular line.

ACTING CHAIR RANSOM: These harmonics are defined without feedback from the thermal-hydraulic--

MR. SHIRALKAR: This is calculated purely from a steady state 3-D code, neutronics code.

ACTING CHAIR RANSOM: So it would be uniform thermal-hydraulic conditions, right?

MR. SHIRALKAR: No, no, no. It has thermal-hydraulic conditions in it, but it's not a transient computer code. It's just a steady state--

ACTING CHAIR RANSOM: Right. Not changing with time?

MR. SHIRALKAR: Right.

ACTING CHAIR RANSOM: You may have core wide variation like this?

MR. SHIRALKAR: It does. Yes.

And this is -- I like this slide because to me this is a validation of the hypothesis, if you will, that Jose proposed many years ago and that is that the harmonics in fact drive the out-of-phase, the regional oscillations. And you can see that what we have here is a flux contour from a plant in Europe which actually was having out-of-phase oscillations. Okay. This is a three dimensional power plant, given a snapshot in time. So half a cycle later it will be reversed. So this probably high and that probably low. So it is a snapshot in time. It shows the contour.

And it shows this contour is calculated now with our 3-D computer code. Okay. On top of that are actual test data from the local powering monitor oscillation. So you can see that they follow the contour very well. And you can see clearly in this one, which is a cross sectional cut across the diagonal, which shows the normalized flux contour of the oscillation versus distance.

DR. BANERJEE: So please orient us with the coordinates. Where are these -- I mean, where is the center of the core, let's say?

MR. SHIRALKAR: In here. So it's

oscillating around some line like this. And so this part is now on one side of the line of symmetry and this part is on the other -- on the opposite part.

DR. BANERJEE: Why doesn't that side go to fall off like you do on the other side?

MR. SHIRALKAR: Well, it's reasonably symmetric. I mean, it's hard to see. This side is falling off and coming up here on this side. You can probably see better here where it's a cut through, cut through here up to the center line.

MEMBER DENNING: On that one there that you're showing right now, we're not seeing the reduced side, are we? We're just seeing --

MR. SHIRALKAR: No. You're just seeing half of it.

MEMBER DENNING: We're just seeing half of it.

MR. SHIRALKAR: Half of it. From the center. So this is a distance from the center line, okay? And you can see the flux contour calculated by a steady state three dimensional code, this PANACEA code, and these are now the test points which show the fall of the contour. And this is a validation that the harmonic shape in fact is what's driving the oscillations.

DR. BANERJEE: But there was some thermal-hydraulic perturbation that occurred to this, or was this very small?

MR. SHIRALKAR: There's no perturbation in this case. This is just a calculation of the harmonic shape.

DR. BANERJEE: I'm saying the real situation.

MR. SHIRALKAR: The real situation, yes. The real situation half a cycle later it would be reversed. That shape would be reversed.

DR. BANERJEE: Right. But now if you look at this real curve there.

MR. SHIRALKAR: Yes.

DR. BANERJEE: Associated with that there are some thermal-hydraulic perturbations, right, about some steady state?

MR. SHIRALKAR: Yes. Yes.

DR. BANERJEE: That's not taken into account in PANACEA.

MR. SHIRALKAR: No.

DR. BANERJEE: So how does PANACEA do so well?

MR. SHIRALKAR: Because it's driven completely by -- the shape is driven by the loading and the shape of the harmonic. This is just a normalized shape.

DR. BANERJEE: Okay. This is not an actual--

MR. SHIRALKAR: It's a normalized shape, okay?

DR. BANERJEE: Now, but there are effects which are due to feedback of thermal-hydraulics? Let's say where the power goes up, your void fraction goes up, right?

MR. SHIRALKAR: Yes. Yes.

DR. BANERJEE: If the void fraction goes up, presumably that would tend to make the power go down some. Without accounting for that, how does PANACEA get this?

MR. SHIRALKAR: The PANACEA accounts for wide feedbacks.

DR. BANERJEE: Oh, it does?

MR. SHIRALKAR: Yes, of course. But it doesn't calculate transient. I mean, it's calculating the effect in terms of -- it's a coupled thermal-hydraulic kinetic code, but the steady state calculation of the harmonic.

DR. BANERJEE: You mean it is very slow, the transient so that --

MR. SHIRALKAR: Yes. This is a snapshot in time.

DR. BANERJEE: Does it do a succession of thermal-hydraulic steady states?

MR. SHIRALKAR: No.

DR. BANERJEE: So assuming the steady state that was there before the perturbation started?

MR. SHIRALKAR: Yes.

MEMBER DENNING: How rapid does this flip back and forth?

MR. SHIRALKAR: The typical period would be of the order of like two seconds.

MEMBER DENNING: A couple of seconds.

DR. BANERJEE: So it's quite rapid.

MEMBER DENNING: Yes.

DR. BANERJEE: Maybe you can explain what is in PANACEA?

MR. SHIRALKAR: PANACEA is basically a diffusion theory code that calculates coupling between thermal-hydraulics and neutronics to give you what the shape is, you know the power shape in steady operation. It also calculates the shape for harmonic shape.

DR. BANERJEE: But does it have a module within it that corrects the thermal-hydraulics based on the increased power? Not in a transient sense.

MR. SHIRALKAR: In the steady state, yes.

DR. BANERJEE: It does have?

MR. SHIRALKAR: Yes.

DR. BANERJEE: Some sort of a module which corrects the void?

MR. SHIRALKAR: Yes. I mean, it can register a solution, a steady state solution based on the neutronics and thermal-hydraulics.

Now, Jim, you wanted to say something to clarify?

MR. JAN: Well, the only point I was going to make was that what PANACEA is calculating are the possible harmonic states for that particular reactor condition. And it tells you that, like say this particular shape is possible and what the subcriticality is for that particular mode. So PANACEA will say, you know, here's the fundamental shape. Its Eigenvalue is one. Here's this harmonic. Its Eigenvalue is, you know, 1005.

Now as far as PANACEA is concerned the harmonic is subcritical so it will not show up in the steady state solution. Now what happens when you couple it to a thermal-hydraulics is that the feedback that you were questioning, the hydraulic feedback, supplies enough reactivity so that that particular harmonic mode is now a critical mode. So it overcomes that subcriticality and is actually present in the calculation. But that's something separate from PANACEA. PANACEA just identifies what modes are possible in the neutronic.

DR. BANERJEE: I understand that. I'm just trying to understand how we've arrived at those points which are data and that solid line. Because that presumably now is a critical mode, right?

MR. HAN: No. In terms of a solid line, that's a subcritical mode predicted by PANACEA that has a particular level of subcriticality.

DR. BANERJEE: But the data are --

MR. HAN: Data is actual based on the LPRM readings across the board.

DR. BANERJEE: Right. So there's actually something happening there?

MR. HAN: And it's happening because the hydraulic feedback that existed at the time of the test was enough to overcome the subcriticality of that particular mode.

DR. BANERJEE: So what I still don't understand is really how that shape can be similar to what is excited when there is some of hydraulic feedback. Because the feedback changes the neutronics, doesn't it? I mean --

MR. HAN: Well, I think --

DR. BANERJEE: Or am I getting mixed up somewhere? I can't understand --

MR. HAN: Well I don't think you're getting mixed up. But I guess my way of thinking about it is that that particular harmonic becomes critical and observable in the plant when the conditions of the hydraulics are consistent with that particular mode. In other words, it's only when the conditions are right that the hydraulic response is providing the appropriate feedback to overcome the subcriticality that you observe the oscillation. In other words, the things must occur in tandem. You know, the harmonic is present in the nuclear evaluation or nuclear condition and the channels are, and state -- hydraulic state are such that they can provide this reactivity effect to overcome that subcriticality. So it's two things they have to line up together. Otherwise that mode is still subcritical and you won't observe it.

MEMBER DENNING: I don't understand that. You're not implying this wouldn't have a decay rate, would you? I mean, I can see how one can stimulate within a transient manner, you can stimulate these and then they die out. Is that correct? I mean, it sounds like you're implying --

MR. SHIRALKAR: In this case, it's steady limit cycle oscillation.

MEMBER DENNING: What's that?

MR. SHIRALKAR: In this case it's a steady limit cycle oscillation.

MEMBER DENNING: This is a steady limit cycle oscillation.

ACTING CHAIR RANSOM: But isn't it true that what you're describing with PANACEA is actually a linear small perturbation type analysis so it only indicates whether or not it could exist? In other words, the tendency is there for these different modes. But if it moves beyond a small change, nonlinear effects are going to come into play like thermal-hydraulic feedback, which will actually change?

MR. SHIRALKAR: It's not linear. It's not perturbation. It's a steady state calculation --

ACTING CHAIR RANSOM: In fact, one thing I'd like to --

MR. SHIRALKAR: -- of a possible condition that can exist. And you're not saying that PANACEA will calculate the amplitude of the transient behavior. But it's saying it's calculating the shape that is consistent --

ACTING CHAIR RANSOM: Right.

MR. SHIRALKAR: -- with the shape that it's seeing for the oscillation.

ACTING CHAIR RANSOM: In fact, isn't it correct to say that what PANACEA calculates is a tendency to oscillate, nothing about instability? When you talk about instability --

MR. SHIRALKAR: No, PANACEA doesn't calculate tendency to oscillate.

ACTING CHAIR RANSOM: -- it means growing?

MR. SHIRALKAR: No. PANACEA does not calculate a tendency to oscillate. It tells you nothing about whether it's going to oscillate or not.

ACTING CHAIR RANSOM: Right. Right. So all the --

MR. SHIRALKAR: It just tells you one of the possible modes of the harmonics.

ACTING CHAIR RANSOM: Right.

MR. SHIRALKAR: And in this case what data is saying is that the oscillation corresponds to this mode.

MEMBER DENNING: And you've normalized

this?

MR. SHIRALKAR: It's a normalized function.

MEMBER DENNING: This is normalized?

MR. SHIRALKAR: Yes.

MEMBER DENNING: I mean somehow you've adjusted --

MR. SHIRALKAR: PANACEA won't give you the amplitude, but it is normalized function, should function.

MEMBER KRESS: To make this calculation did you first fix the -- I'm here. Did you first fix the void fraction distribution through the core and then make the neutronic calculation test? That's how you did it?

MR. SHIRALKAR: Yes.

MEMBER KRESS: So you can speculate on what the void fraction might have been?

MR. HAN: Well, the void fraction distribution is calculated as the steady state distribution for the fundamental.

MEMBER KRESS: You have to assume a sort of power distribution for that fundamental to get that?

MR. HAN: Well, you calculate --

MEMBER KRESS: It's an iterative thing it seems to me like.

MR. HAN: Yes. The way the code will work is first it does a fundamental calculation and gets a very exact solution of the fundamental equation.

MEMBER KRESS: That depends on the core loading and the rod positions --

MR. HAN: Yes. Yes, it does.

MEMBER KRESS: -- and the flows up?

MR. HAN: And then it disturbs that distribution and begins the power iteration and the calculation on the steady state where at the end of each iteration it removes that highly converged steady state.

MEMBER KRESS: Yes.

MR. HAN: And so as a result what it's converging to is the next mode with the lowest Eigenvalue. And so you can successively do that to find the first harmonic, second harmonic and so forth.

MEMBER KRESS: Okay.

DR. BANERJEE: But does it perturb the void distribution when it does that or does it maintain the steady state void distribution?

MR. HAN: Steady state void distribution.

DR. BANERJEE: Okay. So that's what my

issue really is here, that of course you assign this your void distribution is going to be perturbed. So why is it so accurate that all this suspecting would look that good?

MR. HAN: Well, again, you know the value of this is that is so good in indicating that the mode, that the oscillation and the contours are exciting the harmonic solutions to the steady state power distribution.

DR. BANERJEE: So putting it another way, maybe we should do this offline. But what you're really saying is the perturbation and the void distributions associated with this power profile or whatever that is does not have much of an effect?

MR. HAN: The good agreement with the plant data would indicate that that effect is quite small.

DR. BANERJEE: Yes. Either that or the code is wrong? This is just luck, is it?

MR. SHIRALKAR: No, it's not luck.

DR. BANERJEE: Well, okay. I mean that sort of makes you suspect that the void effect is so small --

MR. SHIRALKAR: Because these are now perturbations that are in the fundamental. Okay.

DR. BANERJEE: But what you're really saying is that the feedback from the void was negligible, otherwise you shouldn't --

MR. SHIRALKAR: For this perturbation and for the harmonic perturbation.

MR. HAN: And again, these are perturbations that start off at very small variations from that steady state and then for the appropriate conditions grow from that point. And so the conditions that exist when the oscillation first starts are very close to that steady state distribution.

MR. SHIRALKAR: We can talk about it maybe separately later on. But --

MEMBER KRESS: The reason the power is high on one end and low on the other is because of the void difference, right, mostly?

MR. HAN: I mean, it's low on one end --

MEMBER KRESS: You're voided in the low end and not voided as much in the high end.

MR. HAN: No. I think we --

MR. SHIRALKAR: No. What I'm showing is not actual, it's radial power distribution. This is one side --

MEMBER KRESS: Sure. Yes. That's what I

meant.

ACTING CHAIR RANSOM: The thing that would be helpful, you've normalized the power and in terms of percent of power what kind of variations are you talking about there? Are these a fraction of a percent?

MR. SHIRALKAR: Yes. This is normalized of the percentage of the oscillation magnitude.

ACTING CHAIR RANSOM: Right. So how big is the --

MR. SHIRALKAR: So the maximum oscillation magnitude is one here and then the fraction.

ACTING CHAIR RANSOM: Right. And how big is that?

MR. SHIRALKAR: In this particular case, do you remember?

MR. HAN: It's 10 to 15 percent peak over average.

MR. SHIRALKAR: That's a maximum oscillation. Locally.

DR. BANERJEE: So it is having quite an effect on the void --

MR. SHIRALKAR: But the total power, the total area's power change is like one percent of this due to cancellation effects. But where we're going to use this mostly is we're going to -- the TRACG calculations that we do for visual oscillations are going to use this information in the way we group our channels. In other words, we're taking advantage of the harmonic shape to do our calculation. Effectively, that calculation -- if you will, for the fundamental for the first harmonic and for second harmonic and so on.

DR. BANERJEE: So then, Bharat, it's correct that you can see that this is the mode that's being oscillated, this is the line of symmetry and certainly if you're only using it for that, it's one thing. But I'm still concerned about the fact that you get such good agreement without what appears to be a void feedback.

MR. SHIRALKAR: That's what we get. Now, we're not going to -- we're not going to use PANACEA to calculate oscillations.

DR. BANERJEE: Oh, okay.

MR. SHIRALKAR: Okay. We're using PANACEA to guide us on where the line of symmetry is and so that we can look what channel is appropriate.

MEMBER KRESS: There is a void difference between those, and the difference is across the radial thing. That's --

DR. BANERJEE: But it still doesn't make it counterfeit and yet that's such a good agreement --

MEMBER KRESS: Well, I think it has to take account of it to get that distribution. In the steady state. It's a steady state void difference.

DR. BANERJEE: They're not changing the void distribution. In spite of the fact that the bar is changing by 15 percent. As I understand it.

MEMBER DENNING: No. Wait a second. I think that they must be --

DR. BANERJEE: That's what I was saying.

MEMBER DENNING: If you look at the positive, how far it goes up positive versus how far it down goes negative. If there were no changers you would expect that to be purely symmetric, the up and the down, right?

If you look here, you see it only goes down to a -- it's hard to see on that figure, but the minimum that it goes to on this side of the core is like minus .5.

MR. SHIRALKAR: Yes. Around that.

MEMBER DENNING: Whereas the maximum on the positive side is one, twice as high, right? That difference has to be --

MR. SHIRALKAR: Yes, there is a

difference. Yes. The perturbation that we're looking at is not necessarily 15 percent. Okay. I mean we're looking basically, the harmonic shape is basically a fairly small perturbation. We're talking about normalized shape functions here, that's all. This is not -- it corresponds to the oscillation shape and I think we probably ought to leave it at that for the time being and tell you that the thing that we're going to use from here is primarily the fact that we believe the harmonics do drive regional oscillations and we're going to use that information in how we use TRACG to calculate regional oscillations.

DR. BANERJEE: Is this the only evidence or do you have more evidence to support that harmonics drive regional oscillations?

MR. SHIRALKAR: Well, we have made calculations of I think at least two plants I know that we get good agreement with TRACG in terms of predicting the onset of instability with the regional mode and finding the axis of symmetry and so on.

DR. BANERJEE: So it's crucial in grouping the channels?

MR. SHIRALKAR: Yes, that's where we use it. It's crucial in the sense if you want to use the core intelligently. I mean, you could do a group force and have every channel represented, like 500 channels, 600 channels. But to do it intelligently we can group them so that they're grouped -- with a group based on, say, the product of the fundamental and the harmonic power distributions and groups across the line of symmetry.

DR. BANERJEE: So you select, say, six groups or something?

MR. SHIRALKAR: Well, probably about 32 groups.

DR. BANERJEE: Thirty-two groups. Okay. And so these are 16 on each side or something like that?

MR. SHIRALKAR: Right.

DR. BANERJEE: What happens if you make those eight or 32 on each side? You get the same answer?

MR. SHIRALKAR: No, eight or 32 it doesn't really matter too much. I mean, we've done those kind of sensitivity studies. The more interesting question might be let's say, you know, you're grouping it for a particular mode and let's say that the reactor is actually unstable in, say, the fundamental mode and if you disturb it in that way, it would attempt to go back to fundamental mode oscillation. And I think we've done some studies like that, that we've done some joint, you know, kind of grouping to see what would happen.

DR. BANERJEE: So this is not crucial, actually?

MR. SHIRALKAR: It helps us, yes. It should find a solution, but it helps us in terms of reducing the number of groups we use and give a more constance in how we do the calculation.

I think I'm done with my background. Oh, no, I'm not.

I wanted to share this with you. This is ESBWR natural circulation. The question always comes up how well do you know the natural circulation flow in this plant. And first of all, it's not really that different from an operating BWR. Okay. I mean, it's the same principle. We have the downcomer density that's driving the flow through the core which is at a high void fraction. What we've got now is a chimney to augment that flow. Okay.

And so if you calculate the flow through here, it's dependent on the difference in the static heads inside and outside the shroud and then the loop losses that are controlling the flow rate.

The downcomer in the ESBWR is fairly open, so there is no jet pumps, there are no internal pumps. It's a fairly open downcomer and so we have hardly any resistance in that area. We can calculate a single phase pressure drop reasonably well.

The dominant pressure drops are in the core region, the site empty orifice and the two phased flow in the core and for that we have very good data.

DR. BANERJEE: What's the velocity in the downcomer?

MR. SHIRALKAR: Velocity in the downcomer?

DR. BANERJEE: Typically?

MR. SHIRALKAR: Yes.

DR. BANERJEE: Well, velocity versus bubble rise time.

MR. SHIRALKAR: Oh, okay. It's much higher than that.

DR. BANERJEE: Much higher than that?

MR. SHIRALKAR: Yes. Yes, and you're not going to get bubbles carried under from there, especially you have feedwater coming in over here.

DR. BANERJEE: So bubble rise time or velocity is much higher than the downward velocity?

MR. SHIRALKAR: Is much lower.

MEMBER KRESS: Lower.

DR. BANERJEE: So then why wouldn't you

get carry under if you get --

MR. SHIRALKAR: Well, because you get condensation right here when the feedwater control comes in.

DR. BANERJEE: Oh, okay. The feedwater is coming in at the top.

MR. SHIRALKAR: Yes.

DR. BANERJEE: Okay.

MR. SHIRALKAR: I don't know, it's 10,000 kilograms per second divided by the area. I'm not sure.

DR. BANERJEE: And the feedwater is coming through spogs?

MR. SHIRALKAR: Yes.

DR. BANERJEE: On each side?

MR. SHIRALKAR: Right.

DR. BANERJEE: Thousands of little holes.

MR. SHIRALKAR: That's right.

So the dominate losses are inside in the orifice in two phase, pressure up in the core for which you have a lot of data, okay? Good data in terms of those losses.

The chimney has hardly any frictional losses being very open.

The separator has two phase pressure drop that we have again full scale for typical data for.

So generally, you know, these losses are well known and well calibrated. There's not much uncertainty in these frictional losses.

The drawing here is proportional to the core and chimney height and the void fraction in those regions. So it turns out that the dominate factor in controlling this is actually the void fraction in the chimney, for which we estimate the uncertainty to be about 5 percent based on our comparisons with data --

DR. BANERJEE: Five percent in what?

MR. SHIRALKAR: In void fraction.

DR. BANERJEE: Five percent in absolute void fraction?

MR. SHIRALKAR: Yes.

DR. BANERJEE: What's the average void fraction then?

MR. SHIRALKAR: It's about 60 to 70 percent.

DR. BANERJEE: And the quality is roughly?

MR. SHIRALKAR: The quality leaving the core is about 20 percent, 25 percent. And when it mixes with the flow from the bypass and it reduces to, say, about 15 percent or thereabouts.

DR. BANERJEE: And the flow regime is

what, roughly?

MR. SHIRALKAR: In the -- no the chimney is -- turbulent.

DR. BANERJEE: -- turbulent. So why do you think data from a pipe would work in a square duct?

MR. SHIRALKAR: I don't see -- yes, it's--

DR. BANERJEE: There are edges here.

MR. SHIRALKAR: There are edges, but they are similar diameter. And this would be reasonably good.

DR. BANERJEE: But wouldn't water accumulate at the edges, I mean the corners? You don't think so?

MR. SHIRALKAR: We don't think so. Not at the velocities that we have in the pipe. So you'd probably get a distribution that would be slightly different than circular pipe. On the average I think is dominated by the central region. I mean, it's a fairly large -- very large region.

DR. BANERJEE: So you say there is an uncertainty in the void fraction. So if it was 60 percent, it might be 55 percent or something like that?

MR. SHIRALKAR: Right.

DR. BANERJEE: Five percent in void fraction.

MR. SHIRALKAR: Yes. And so based on that we calculate the core for uncertainty of about 3 to 4 percent, one sigma. This is by doing a Monte Carlo analysis where we really always -- randomly, which is not too bad.

DR. BANERJEE: But you don't have any test with a chimney and a channel, 16 channels?

MR. SHIRALKAR: No, not at full scale. No, we don't.

There is some question asked about developing lengths and so on inside that chimney. I think Graham asked about it last time. And there is some data in which you have -- there is a pipe geometry in which the flow is injected toward to what they call the bubbler, which is 37 tubes injecting steam inside that, which is not unlike what we have here. And they found that they reached fully developed flow within about one to 200 times.

DR. BANERJEE: Where was this?

MR. SHIRALKAR: Russian data.

DR. BANERJEE: And what diameter was the pipe?

MR. SHIRALKAR: One was .6 meters and one was .75 meters.

DR. BANERJEE: Can you source that for me?

MR. SHIRALKAR: Yes, sure.

DR. BANERJEE: Thank you.

MR. SHIRALKAR: So if you look at the natural circulation characteristics of this plant, this is a plant at an average power per bundle, which is an average flow per bundle. And this -- I don't know why it's doing that.

DR. BANERJEE: Oscillation.

MR. SHIRALKAR: Yes. Anyway, this is the characteristic for the ABSBWR. This is the characteristic for BWR 6. And here is the flow characteristic and natural circulation for an ESBWR. And you can see how much larger the flow is compared to the operating plants. And, of course, this is by design.

We have a tall chimney that's driving the buoyancy head. We have an open downcomer, which is a big factor of just moving the jet pumps and the internal pumps from this region. And we also have a shorter core and it'll reduce two phrase pressure drop. And all of them combine to give you this large flow.

DR. BANERJEE: Why does it move back like that?

MR. SHIRALKAR: That's the point where, you know, this competition between friction and buoyancy. So what happens at that point is that the friction starts to dominate when you get to high qualities. And then the character changes. So what you get is actually a reduction in flow as the power goes up. Because buoyancy reduction is not compensating enough for the friction increase.

DR. BANERJEE: Are these mainly the returning losses or is it actual frictional drop through the --

MR. SHIRALKAR: This is friction, yes.

DR. BANERJEE: It's friction?

MR. SHIRALKAR: It's friction to the core.

DR. BANERJEE: It's not the turning and the--

MR. SHIRALKAR: No, no. It's dominated by the core, friction.

ACTING CHAIR RANSOM: Well, some of it must be acceleration. I mean, as you're changing the void and while you're accelerating the flow --

MR. SHIRALKAR: Sure. Yes. I mean, it's a total pressure drop in the core that's dominating it.

So just to make a point that this -- and we've had stability, we looked at stability in BWRs for many years. So stability, per se, is not a new thing in terms of BWR analysis. And we've been doing this for many umpteen years. And the important factor of stability are fairly well understood. So the power flow ratio or the Zuber number, if you will, the fuel thermal time consistent, neutronic parameters, actual and aerial peaking, ratio of single phase/two phrase pressure drop and for regional oscillation the subcriticality of the higher order harmonic mode.

So if compare those with operating reactors, I hope you can see these things. But if you look at wide coefficient we're in the range of operating plants. If you look at the core average exist quality, we are around -- we're at natural circulation conditions in operating plants versus ESBWR. Our exit qualities are a little bit lower than the operated plants that we have operating.

The bundle average, the bundle exit quality also is a little bit lower than natural circulations in operating plants.

DR. BANERJEE: Why do you call that favorable?

MR. SHIRALKAR: It's favorable because the larger the power to flow ratio, the more adverse --

DR. BANERJEE: So --

MR. SHIRALKAR: Favorable with respect to an operating BWR natural circulation.

DR. BANERJEE: Right. Now having a lower quality means you get a lower flow, right, because your buoyancy head is going to be lower?

MR. SHIRALKAR: No. This is -- I'm comparing for a given bundle what is the exit, what is the average condition in that bundle in terms of power and flow. And the higher you make that ratio, the worse it gets in terms of -- the higher two phase pressure drops, you know, and so on.

DR. BANERJEE: Okay.

MR. SHIRALKAR: Okay. And the ratio and the fuel time constant to the flow transit time, and this governs the attenuation of the power coming back as heat flux. Okay. So the larger the fuel time constant, the more attenuation in terms of the heat flux. And that ratio is for an operating plant it ranges from 3.5 to 6. And for the ESBWR it's larger, mainly because the transit time is faster. So that is favorable for the ESBWR.

The harmonics of criticality is unfavorable because the larger the core size, the smaller the subcriticality. And so that is one factor that is unfavorable for the ESBWR because of the core size. But the dominant one is the ratio of single phase/two phase pressure drop, and that is substantially favorable for the ESBWR because the shorter core length and also the smaller -- we have -- rods in these bundles to improve the two phased pressure drop characteristics in the top part of the bundle. And we have a larger relative length above the -- rods in the shorter fuel bundle. And so that gives you a more favorable two phrase to single phase pressure drop ratio.

DR. BANERJEE: Do you take into account the exit loss here from the top of the bundle?

MR. SHIRALKAR: Yes. Yes. The dominant losses are the friction and the local losses; the spacers and the upper -- plate and so on.

MEMBER DENNING: What's the significance of the fact that in the second line you relate it to in natural circulation? The others relate to under pumped conditions? Is that right? See, that one.

MR. SHIRALKAR: This one?

MEMBER DENNING: You quality it.

MR. SHIRALKAR: I qualified it because it's a --

MEMBER DENNING: Had natural circulation.

MR. SHIRALKAR: Yes.

MEMBER DENNING: Yes.

MR. SHIRALKAR: Because the -- well, we got a big -- so that's not a factor. But this one, yes, if you look at the rated conditions in operating plant, the qualities are going to be quite a bit lower. And the rated conditions, you know, at operating plants the -- are quite low. So I'm comparing with the natural circulation.

MEMBER DENNING: So it would have been unfavorable?

MR. SHIRALKAR: Compared to rated conditions it might be unfavorable.

MEMBER DENNING: Yes.

MR. SHIRALKAR: Yes.

MEMBER DENNING: Whereas these others are favorable --

MR. SHIRALKAR: Are favorable or in the same range.

MEMBER DENNING: Okay.

DR. BANERJEE: And you take into account the chimney pressure loss as well?

MR. SHIRALKAR: Yes. The chimney does not -- pressure loss, frictional pressure loss. Then of course the static head. But the chimney does not contribute a whole lot to the stability transfer function. It gives you a larger flow, but it doesn't have a big effect on the whole transfer function.

DR. BANERJEE: But doesn't it have -- I mean, if you could avoid perturbation in the chimney, it will effect the head so it will feed back, won't it?

MR. SHIRALKAR: Well, it turns out that the wide perturbations are mostly around subcooled -- bounding the core. And by the time you get to the edges of the bundle -- exit of the core, the changes of perturbation, wide perturbation is very small.

DR. BANERJEE: But in some turbulent flow you get these sort of void waves traveling, quite significant ones.

MR. SHIRALKAR: Yes. You're talking about something that's independent of -- well, you're talking density of the perturbation.

DR. BANERJEE: Right. It's not density waves.

MR. SHIRALKAR: It depends on the frequency of those things versus what we're talking about here.

DR. BANERJEE: What's your frequency?

MR. SHIRALKAR: Particularly they are one second. One hertz, thereabouts. And as long as those perturbations are occurring within the chimney, I mean they don't effect the outer wall --

DR. BANERJEE: These Ontario Hydro experiments, did you just measure average void or did you also measure the fluctuating void?

MR. SHIRALKAR: We measured --

DR. BANERJEE: Measured with densitometers?

MR. SHIRALKAR: Yes. We measured the average void as well as core void fractions.

DR. BANERJEE: And how many densitometers did they use?

MR. SHIRALKAR: I think they had like about six beams across, but I'll have to look it up. It's been like ten years ago.

Okay. So that finishes my background. Okay.

I'd like to step through the LTR now and kind of give you a preview of what's in the LTR. Basically we're using TRACG04 is our code that we use evaluating stability. And we use it for both normal operation and -- I need to correct my terminology here. We actually evaluated not during transients, but we evaluate conditions that might result as a the effect of a transient. In other words, if you, say, had a loss of feedwater heating event, your power would go up to some value and you would get to a point which is worse than your normal operating condition. So we would evaluate the stability at the worse condition that we can get as the effect of this transient.

We're also using it to analyze plant startup trajectories to show that there is no issue with respect to internal margins in startup. And we requested NRC approval of TRACG for ESBWR stability application.

The general licensing requirements, there are two. The more important one is particular disabilities, GDC 12, which requires that power oscillations could either be not possible or they should be suppressed. It turns out that in our case for the ESBWR the most limiting case is at the rated condition That's the highest power to flow point. So it's imperative that we maintain a very large stability margin at rated conditions. So our approach is to basically make sure that oscillations are not possible by maintaining a very large margin on the decade ratio. And if you do that then we would automatically satisfy this criterion through normal analysis and anticipated transients.

So licensing basis says to establish a high degree of confidence that oscillations will not occur by imposing great conservative design criteria on the channel core wide and regional oscillation modes. And as a backup, we will implement a detect and suppress solution that the operating plants are using as an defense-in-depth. But we hope we will never get to use that because we want to maintain large margins here.

DR. BANERJEE: Can you just give me a brief idea on what this detect and suppress solution is?

MR. SHIRALKAR: The detect and suppress solution is basically using a group of local power range monitors to detect a likelihood, the presence of an oscillation. And then taking an action to either insert rods or scram depending on the magnitude of that oscillation.

Now, the exact algorithm with which you do this is still under debate. And I think the operating plants are looking at improved solutions to this. And we will implement whatever that final solution is, algorithm wise. But for us it's just a backup. It's a defense-in-depth. Okay.

We've used what is called conventional stability map of core decay ratio versus channel decay ratio and we've calculated uncertainties and statistical limits for these parameters. Let me explain what that means.

Historically we've been using a map like this where we represent the core decay ratio and the channel decay ratio. And we basically limit them to be less than .8. The .8 allows for some margin here for uncertainty in the calculation methods. And this is historical based on our old code that was used in the old days.

Then we found in the '80s the occurrence of these regional oscillations. And the regional oscillations could occur even if you were inside the boundary of the .8. So we had to chop off this corner of the map to account for regional oscillations. And that was based on empirical data as well as calculations.

So we proposed to use this map for the ESBWR as well, but we recognized that because the core site is larger, that this line could move inwards because the subcriticality of the harmonic would be small and therefore there will be less damaging --

neutronic damaging to that mode. And so we calculated a boundary that is inside this to be our criteria.

ACTING CHAIR RANSOM: The narrowing of the region is due to feedback effects?

MR. SHIRALKAR: No. The smaller region is just due to the fact that the core is larger. When the core is larger then your subcriticality of the harmonic decreases. It becomes more excite in the larger, easier to excite -- if you will, the core is more decoupled and so easier to excite these modes on opposite sides of the core.

ACTING CHAIR RANSOM: But this boundary means that they are possible there, I guess, right?

MR. SHIRALKAR: Yes. This boundary means that you could have regional oscillations in this core in a hole here, outside this region.

ACTING CHAIR RANSOM: Like on the core decay ratio on channel decay ratio you've put an eight tenths value in. Does that curved line represent one then in terms of tendency for them to exist?

MR. SHIRALKAR: Well, that was never very clearly established, but there was some margin in establishing that line. Okay. I mean, we drew it inside of all known data and of calculations, to draw a line was inside all of the data. So it represents some conservatism to the occurrence of regional oscillations. Okay.

DR. BANERJEE: But not necessarily .8?

MR. SHIRALKAR: Not necessarily .8. But I don't want to belittle that because we've gone away from that criteria for the ESBWR.

MEMBER KRESS: You used .8 instead of one because of uncertainties, perhaps? You could have used one in --

MR. SHIRALKAR: That's right. The .8 is because of uncertainties in our methods, primarily. And from the old code that was being used, the one sigma uncertainty is relatively about .1. So the 2 sigma 11.2. And so we set it at .8.

MEMBER KRESS: In principle anything below one would have been stable?

MR. SHIRALKAR: Exactly. Right. Yes.

As a result of the NRC review we revised our approach and we have gone now to a direct calculation of the regional decay ratio and a quantification of uncertainty in the regional decay ration, just like the channel and the core decay ratios.

And so now we have a comparison with a regional decay ratio of < 0.8 rather than that map that I showed you. So the new map looks like this. It's a box. So we want to keep the core decay ratio < 0.8, channel decay ration < 0.8 and the regional decay ratio also < 0.8.

And internally we started to impose a design goal on ourselves that at a nominal best estimate basis we want to be in a smaller box about half that, 0.4. So at a 95/95 level, for example, we want to meet the 0.8 criteria. That would be our design limit. But as a design goal internally we would like to have the nominal calculation stay about half of that. And the rationale is roughly like this: That for operating plants in the flow control range you typically operate with decay ratios that are half what the limiting decay ratios are.

So in normal operation we want to keep more margin to this design limit. We don't want to be too near the design limit during normal operation.

MEMBER KRESS: What design parameters are under your control that allows you to get in that middle box?

MR. SHIRALKAR: Well, primarily, you know, we want to make sure that we have enough flow. The power to flow ratio --

MEMBER KRESS: So the size of the chimney perhaps.

MR. SHIRALKAR: The size of the chimney, the distance in the downcomer in the core, for example.

MEMBER KRESS: Yes.

MR. SHIRALKAR: You can also play with the y coefficient. You can play with the core design in terms of the phase to single-phase.

MEMBER KRESS: You can effect the void coefficient by the fuel enrichment?

MR. SHIRALKAR: No, not a whole lot. Yes.

MEMBER KRESS: Okay. So you got two things to play with.

MEMBER DENNING: No. The third thing probably more important is the ratio of the single-phase/two-phase pressure drop in the fuel.

MEMBER KRESS: Okay. And you've done that as best you can.

MR. SHIRALKAR: Right. So we've got shorter fuel.

MEMBER KRESS: Yes.

MR. SHIRALKAR: And a larger region above the -- fuel rod, which is open, more open to control that.

MEMBER KRESS: Yes, as you made your fuel even shorter, you run the risk of enhancing the regional fluctuation.

MR. SHIRALKAR: No, but we're losing fuel economy. You'd have time to take more fuel out of the core. Yes. So fuel people always want to put more.

MEMBER KRESS: So you'd made it short enough that you cut down on the pressure draw but you still have good economy?

MR. SHIRALKAR: Right. Yes. I mean, the fuels people would like us to make the rods even longer because --

MEMBER KRESS: Yes.

MR. SHIRALKAR: -- obviously they want to put more -- load more uranium in there.

MEMBER KRESS: Right.

MR. SHIRALKAR: But it's going to be a compromise between the stability and --

MEMBER KRESS: And so playing with those parameters you're able to get into that middle box?

MR. SHIRALKAR: Right.

MEMBER KRESS: Okay.

DR. BANERJEE: The regional decay ratio is 10 by numerical experiments using TRACG and coupled with--

MR. SHIRALKAR: All three of them.

DR. BANERJEE: All of them?

MR. SHIRALKAR: Yes.

DR. BANERJEE: But for others you got other codes that have done it in the past, right?

MR. SHIRALKAR: Well, we can also use a frequency to main code to do the regional.

DR. BANERJEE: Right.

MR. SHIRALKAR: And we have done that and NRC Staff consultants have done that also.

DR. BANERJEE: If it's frequency domain, it has to be a linearized system?

MR. SHIRALKAR: Small perturbation.

DR. BANERJEE: Yes.

MR. SHIRALKAR: But here we're talking about decay ratio. We're not talking about light oscillations. The linearized codes are perfectly acceptable here because we are not looking at magnitude of oscillation. We're looking at small decay ratios. We're talking about decay ratios 0.4

DR. BANERJEE: You're talking of perturbations which are very small?

MR. SHIRALKAR: Yes.

MEMBER KRESS: Does the size of the perturbation influence your decay ratio?

MR. SHIRALKAR: I'm sorry.

MEMBER KRESS: The size of the perturbation you impose, does that influence your decay ratio?

MR. SHIRALKAR: It can to some degree, and I'll show you some results. That's one of our parameters to look at different perturbations to see what effect it has on the --

DR. BANERJEE: Well, it's a highly nonlinear system --

MR. SHIRALKAR: Nonlinear system and when you calculate the decay ratio, you know, it depends on whether you use the initial part of the transient or the later part of the transient. You can get some small differences. But manageable differences.

MEMBER KRESS: When you use the initial part of the transient --

MR. SHIRALKAR: Yes, because then they're small.

MEMBER KRESS: -- you assume that's a conservative use?

MR. SHIRALKAR: We basically what we do is we neglect the first initial rebound, the bound based on the perturbation and then we use the second, third peaks to calculate.

MEMBER KRESS: Yes. And normally those

decay ratios would be higher than if you choose some other part of the decay scheme?

MR. SHIRALKAR: Yes and no. Because eventually they get to be so small that you cannot distinguish the --

MEMBER KRESS: Yes.

MR. SHIRALKAR: Very small. But, yes.

Frequency domain codes do not have that issue.

So methodologies to calculate these decay ratios at normal conditions and then do a statistical calculation to show that we can meet the 95/95 -- meet the design criteria at the 95/95 level.

And the uncertainties and biases include the model uncertainties, experimental uncertainties that are inherent in data comparisons. We don't separate them out.

We look at plant parameter variability that includes range of operation and process measurement errors.

This is just a table that shows that we tried to address the steps of the CSAU process and which sections of the report address these various steps.

I'm sorry. It's too small. I hope you

have -- it's even worse in the handout.

DR. BANERJEE: These are right, right? All perturbations?

MR. SHIRALKAR: Yes. Eye test.

The point I wanted to make was rather than go through this in a whole lot of detail is these are very similar to the PIRT for operating plants to do it. Okay. I mean, they are virtually -- there are very few differences between the stability phenomena that you get in operating BWR versus any ESBWR. The phenomena are the same. Okay. The parameters are a little different in terms of the flow rates and so on.

DR. BANERJEE: But now you have a chimney and presumably there are density waves which move, and you're saying that those density waves are totally uncoupled from the density waves in the core.

MR. SHIRALKAR: You're talking flow regime kind of --

DR. BANERJEE: Yes. Yes.

MR. SHIRALKAR: Yes.

DR. BANERJEE: So if they are at the same frequency, let's say the transient time of the density wave or the disturbance wave in the chimney is on the order of one second, they would couple.

MR. SHIRALKAR: Yes, but you don't have

nuclear feedback in that region. So the dominant region is the core where you get the feedback and you get the gain in the transient function when you go from, say, perturbation in the power, pressure to power.

DR. BANERJEE: Yes, but the feedback comes through the change in the flow, right?

MR. SHIRALKAR: In the change in the flow and -- fraction.

DR. BANERJEE: Yes.

ACTING CHAIR RANSOM: So on the driving force is related to the void and the -- in the chimney.

DR. BANERJEE: Yes, so it has to be. I found that statement very strange, the chimney had no effect on stability. That assumes, of course, that the chimney does not have -- it's sort of an assumption that you don't have density fluctuations in this chimney which coupled with the core.

MR. SHIRALKAR: On an average basis --

DR. BANERJEE: On an average, of course.

MR. SHIRALKAR: On an average basis -- and don't play any role in the stability process.

DR. BANERJEE: Right. But at every --

MR. SHIRALKAR: Now we're talking about

flow regime transitions or, say, of slugs going by or something like that that we have some frequency.

DR. BANERJEE: With a fraction of 60 to 70 percent you're bound to have that.

MR. SHIRALKAR: No, you don't have slugs because --

ACTING CHAIR RANSOM: No, now to the core you have a boundary condition that's periodic in terms of void fraction so that what's in the chimney you would think would be periodic as well.

MR. SHIRALKAR: Well, I think what you have is sort of a -- and flow. I mean, you can calculate the length it would take for this flow to develop into slugs.

DR. BANERJEE: That be long.

MR. SHIRALKAR: Yes, that would be very long.

DR. BANERJEE: The turbulent flow itself has density waves which are very strong going through it.

MR. SHIRALKAR: And those we have not accounted for in terms of whether you -- they might be of exact same frequency as the -- core and might have some influence.

DR. BANERJEE: Well, maybe the way to

handle that would be to simply say that until proven otherwise you would have to take that into consideration and say if you took it into consideration, showed that they were completely different frequencies or something, then define. But lots of data exists on the density waves in terms of--

MR. SHIRALKAR: Well, we can look at typical frequency to get that kind of flow.

DR. BANERJEE: Yes. Yes.

MR. SHIRALKAR: I think it would be extremely fortuitous if they are in exactly the same--

DR. BANERJEE: It doesn't have to be exactly the same. They have to be in the general region.

ACTING CHAIR RANSOM: Somewhere.

DR. BANERJEE: You know, if these are of the order of seconds and the fluctuations in the core that are excited are of the order of seconds, then the potential for coupling exists.

MEMBER DENNING: How does that coupling get back to the core, though, is the question?

DR. BANERJEE: Through the flow.

MEMBER KRESS: Yes, through the flow. It changes the driving force.

DR. BANERJEE: It changes the driving

force.

MR. SHIRALKAR: Well, I'm not sure it does because you've got these fluctuations within the chimney, they're traveling upwards. So whether they change the total pressure drop in the chimney is to me very --

DR. BANERJEE: Very unlikely.

MR. SHIRALKAR: Yes. It's just very unlikely to me. But you change between the total pressure drop in the core to create flow oscillations.

DR. BANERJEE: So how tall is the chimney?

MR. SHIRALKAR: Nine meters.

DR. BANERJEE: Okay.

MR. SHIRALKAR: So you got these things traveling through. And, yes, you may have local oscillation variation, but you got to change the whole pressure up in the whole thing.

DR. BANERJEE: Right. Well, how tall was on Ontario Hydro pipe?

MR. SHIRALKAR: I think where we measured the void fraction, I think it was about 6 meters or thereabouts. I'll have a check. It's been ten years.

DR. BANERJEE: So the issue really is what did they find with the pressure drops. Did the pressure drops, the hydraulic head fluctuate and by how much?

MR. SHIRALKAR: No. There was not much variation in the pressure drops.

DR. BANERJEE: And the void fraction was about 60 percent?

MR. SHIRALKAR: We took a whole range of data from low wide fractions right up to about 80 percent.

DR. BANERJEE: Including dynamic data?

MR. SHIRALKAR: Yes. Dynamic meaning?

DR. BANERJEE: I mean you took the time traces of the pressure --

MR. SHIRALKAR: Yes.

DR. BANERJEE: -- and the voids.

MR. SHIRALKAR: Yes.

DR. BANERJEE: That would reveal something?

MR. SHIRALKAR: Yes, it could.

MEMBER KRESS: Well, what was the makeup of your PIRT panel? Was that internal?

MR. SHIRALKAR: I'm sorry?

MEMBER KRESS: Your PIRT panel? Who?

MR. SHIRALKAR: PIRT panel is internal, yes. We relied heavily on the PIRT for the operating plants and we looked at the differences. And actually, I think I said, you know, we're seeing the main differences come about really only because of the chimney. But I tried to show here that these two green things that are different, there's only two things that are different from an operating plant and they are the chimney void fractions and possibly interactions between chimney cells. And by that I mean is it possible for a chimney cell along with its group of 16 bundles to have some kind of a mode of oscillation by itself. And we looked at that. That perturbing a whole group of 16 bundles inside a cell.

DR. BANERJEE: I'm simply saying there should be another entry there which says the dynamics should --

MR. SHIRALKAR: Pressure --

DR. BANERJEE: Yes. Yes.

MR. SHIRALKAR: -- to flow regime changes are inside --

DR. BANERJEE: Something like that. Now you may dismiss it at some point, but it has to be looked at.

MR. SHIRALKAR: Agreed.

MEMBER DENNING: How are we doing time wise, incidentally? Are we running into trouble time wise?

MR. SHIRALKAR: Probably.

MEMBER DENNING: Probably?

MR. SHIRALKAR: Yes, Professor Banerjee has been asking too many questions.

DR. BANERJEE: That's what they always say.

ACTING CHAIR RANSOM: I think we're already in trouble.

MR. SHIRALKAR: Let's see, I'm on -- yes. Slide 29 out of say, about 90. One-third of the way through, I think, in an hour and a half.

All right. I'll try to go through a little bit faster.

MEMBER DENNING: You may not have any options.

MR. SHIRALKAR: I'll try.

We had a comparison where we make a comparison in a matrix of the important phenomena versus the models in TRACG which I've established that we have the models required for the analysis. And we've done that in section 4.

We've got an extensive database for internal hydraulic effects in general and stability in operating plants in particular. And I didn't want to belabor that here, but if there is an interest in looking at that qualification of TRACG versus other BWR data, we can do that either now or maybe later.

So as a result of our evaluations we found that we had enough data on BWR stability, but it is mostly at the conditions close to the inception of oscillations because the primary interest for BWRs is when you actually get oscillations and you're looking at decay ratios close to one.

Now here we're looking at decay ratios in the order of .04 or .03. So we wanted to make sure that TRACG would do a good job at these low decay ratios as well, because other considerations come in like numerical dampening and so on in the code to make sure that you're not way off somewhere.

So we supplemented that data with a few points at low decay ratios. Now by this is by no means the extent of our qualification base because we've got a lot of data that is in the overall qualifications report, but I'd like to highlight just these low decay ratio points.

And I think at this point I was going to ask if we could close the session for proprietary information.

MEMBER DENNING: Well, there's the question of when we're going to take our break.

ACTING CHAIR RANSOM: Ten after 10:00 it was scheduled. Well, why don't we do it now then.

DR. BANERJEE: And close it after?

MEMBER DENNING: And close it after. We'll be back at quarter after --

ACTING CHAIR RANSOM: Five after. Quarter after 10:00. Okay.

(Whereupon, at 9:59 a.m. a recess until 10:18 a.m. at which point the proceedings went into Closed Session.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

12:49 p.m.

ACTING CHAIR RANSOM: We're back in open session. Okay. We're back in open session.

MR. SHIRALKAR: I am back. The last topic I have is on the startup of the plant and natural circulation. And I have a few charts I'd like to step through.

The natural circulation startup is something that has been done in Dodewaard, but it also been done at a whole lot of coal fired plants where the natural water has been around for a long time. So it's not something that's necessarily been very unique at this point. And provided that you take the proper precautions, shouldn't pose any problems in getting the reactor to fire. But looking at the Dodewaard procedure, which the Dodewaard is a plant which is much smaller than the ESBWR but shared some of the features; the chimney, the core regions, similar range of void fractions and qualities. And the way they started up the plant was that typically after the first cycle you always have enough decay heat to start up the plant without external heaters. But initial cycle you need an external heater to aid the startup.

You heat up the reactor coolant to 80 to

90 degrees C with an auxiliary heater and decay heat. And then they dearated the reactor coolant by pulling a vacuum on the main condenser with the steam drain line open. They pull the rods to criticality. And then slowly start pressurizing the system by pulling the rods and creating vapor at the top of the chimney region.

And as the power pressure increases, open the turbine bypass valves to control pressure.

So we intend to follow a similar process to start up the ESBWR.

ACTING CHAIR RANSOM: As you deaerate the reactor do you take it down to subatmospheric pressures?

MR. SHIRALKAR: Yes, you can --on the vessel.

ACTING CHAIR RANSOM: Yes. Okay.

MR. SHIRALKAR: You can pull pressure.

To give you a brief idea of what I'm talking about, at atmospheric pressure or low pressure we have a significant amount of static head in the system. So when we have, say, one bar pressure at the top in the steam dome we have about three bars or thereabouts at the bottom. And so a significant difference in the saturation temperature as well. So as we start heating up the fluid from coming through the core and heat it up in a slow and controlled manner so to maintain kind of a steady temperature distribution, then you get a heat up in the core, it's adiabatic in the chimney and eventually you start getting flashing in the top because the resaturated conditions at the top. And the core is significantly subcooled at the time.

And what happens at that point is that as you produce the first vapor in this situation in the top of the chimney, you reduce the static head, you cause an increase in the flow that then collapses the voids and you're back to the no wide situation. So --

CHAIRMAN WALLIS: This is a bit like what we were thinking happens in the power situation; that the voids in the chimney --

MR. SHIRALKAR: I think --

CHAIRMAN WALLIS: -- hence the circulation flow rate?

MR. SHIRALKAR: No. Because I think the wides in the chimney, I mean you're already in a situation, you already have significant voiding there. And you have a steady situation as far as the wides are concerned.

Now this is at the inception of voiding

where you really get these fluctuations happening.

CHAIRMAN WALLIS: So the change can be very big?

MR. SHIRALKAR: The change can be big. And because you're at a situation where you have a high density ratio and a large amount of void fraction generation.

So if you look at a conceptual stability map in, say, the subcooling Zuber kind of plot, a force circulator will have a stability map that looks like this. But an idle circulation has this part that bends back in and this region is called in literature the type 1 instability region. That's between the boiling boundary and until you establish some steady void fraction.

And this region here is the conventional density wave region where you're at much high qualities and pressure drops. Okay.

So as you start producing voids somewhere in the system you have to traverse this region one, there's no way around it. But what you can do is to make sure that when you start getting the initial small amount of percolation, that you are single phasing the core. And so any velocity oscillations you have are small perturbations in the single phase region without any reactivity issues. Okay.

CHAIRMAN WALLIS: So that red trajectory is where you go, is it?

MR. SHIRALKAR: Yes. The red trajectory is where we would calculate and took the trajectory. And at this point then you become stable.

CHAIRMAN WALLIS: And the reason it starts way up there is because of the density ratio, is it?

MR. SHIRALKAR: Yes. And also because you have a large amount of subcooling when you start.

So if you look at different profiles for the heat up, now what we want is a profile that heats up like this trajectory A. So you start off slowly, you establish fairly steady conditions and you have the chimney that's heating up and the highest temperature and the lowest subcooling is at the top.

Now if you were to heat it up very rapidly, along trajectory C, then what happens is that you can actually start getting voids at the top of the core when you're still subcooling the chimney. And you don't want to do that because that's where you get start getting these condensation oscillations that you want to avoid. Okay. But if you do it carefully and as a controlled heat operate, then you can get a situation like A and then progresses to B as the boiling boundary progresses down into the chimney.

CHAIRMAN WALLIS: Or the flashing boundary.

MR. SHIRALKAR: I'm sorry.

CHAIRMAN WALLIS: Which is the flashing boundary?

MR. SHIRALKAR: Yes, that's right. That's the flashing boundary. And this is the margin to flashing and it's characterized by what we call a flashing number. That's basically the difference in the saturation enthalpy at this pressure and that pressure.

CHAIRMAN WALLIS: Now there is a cause of kind of geysering where as you get more voids, you decrease the static head and it --

MR. SHIRALKAR: No, you don't really get that because the feedback from the downcomer is stronger. So what happens is that as you produce a static head you get increased flow from the downcomer. And that's a much stronger mechanism than geysering is.

ACTING CHAIR RANSOM: Well, actually you're trajectory B is a flashing trajectory. I mean, as you show, you go up to the point of saturation and then presumably flashing begins and you have two-phase on that.

MR. SHIRALKAR: Yes. B is actually following up on A and it's progressed later on and come down at some point as it's propagated further into the chimney.

So the startup procedures we're proposing is similar to what Dodewaard used. Use a mechanical pump and the vacuum pumps and the condenser to pull a vacuum to deaerate in the deaeration period.

CHAIRMAN WALLIS: Those are presumably steam injectors, are they, rather than mechanical pumps?

MR. SHIRALKAR: Yes. They're mechanical pumps and --

CHAIRMAN WALLIS: They're not steam injectors? They're actually mechanical pumps?

MR. HINDS: Sorry. This is David Hinds.

Mechanical vacuum pumps are used for the initial portion of the startup and then beyond that after we have a steam environment in the plant started up, then we use the steam generator injectors.

MR. SHIRALKAR: When we finish the deaerate, we go to the next chart here and show you. So this is the deaerate period in the beginning. And then we're starting up on this trajectory where we're heating up.

In the startup period we had to isolate the vessel. Now you can close it -- you can close the MSIVs or preferably you can close the turbine and stop -- the control valves and the bypass valves to get the system bottled up so that you can start building vapor pressure.

And then start with bearing control rods. Use efficient power to heat the water. Maintain the water level below the main steamline elevation. You pressure the RPV with vapor generation at the top of the stack and not in the core. And then the core remained subcooled due to the large static head.

And you can use the RWCU system, cleanup system can be used to enhance the coolant flow and reduce thermal stratification in the lower plenum.

CHAIRMAN WALLIS: Now, do you have some sort of guidance for the operators about how the increasing pressure and power are related? Presumably, you don't just increase the power or you have to increase the pressure in some way along with it or something?

MR. SHIRALKAR: Yes. And then the specific guidelines for the ESBWR I don't think are written yet. But they would have to be written to provide that kind of guidance.

And when the system is pressurized to 63 bars or thereabouts, then you start controlling the pressure with the turbine control rods and the bypass valves and prepare to roll the turbine.

So that's the same -- what I talked about depicted here in terms of pressure versus time.

Now we have made calculations with the TRACG of ESBWR startup. We made these calculations without nuclear feedback, without neutronics feedback. The rationale being that we want to achieve this first part of the transient before we get any voiding in the core, so no reactivity feedback at all.

We started up with three different rates of heatup; 15 megawatts which corresponds to an increase in temperature of about 30 degree C per hour. At 85 megawatts you get about 55 degrees C per hour and that is typically our tech spec on how fast you can heat up in operating BWRs because of limitations and thermal stresses and other issues.

And then just for the heck of it we tried a much larger, a 125 megawatt, which would be like 82 C per hour.

DR. BANERJEE: Well what other temps?

MR. SHIRALKAR: These are steps that you

take after you've gotten to the high pressure, that's 63 bars. And then they start increasing the power faster so that you can get up to rated power.

This is the corresponding pressure responses heating up to -- pressurizing to about 63 bars in each case and then starting -- then opening the control valves and increasing the power. At that point you are well passed any concerns about the low pressure oscillations.

This is the inlet subcooling and the inlet to the hard burn. It starts out very high, which is the reason for the high subcooling number initially, and then decreases as the plant heats up and pressurizes.

This is the one that probably is of interest. That is the calculated core inlet flow at these three different heatup rates. And you can see start getting a little noisier at the lowest heatup rate, there's a little more noise here at 85 megawatts. And you're getting more noise now you get to the higher flow rate.

CHAIRMAN WALLIS: Now what's the decay ratio when you have green noise?

MR. SHIRALKAR: Didn't try to calculate decay ratios here.

CHAIRMAN WALLIS: Well, presumably it's growing some of the time and decaying other times?

MR. SHIRALKAR: Yes. But it's inconsequential in terms of the overall progression of the transient. It's not picking up. It's not going to a situation where it's explosive kind of a situation. All we care about here is to make sure that that's nowhere near any thermal limits.

This is the corresponding oscillation and flow and the part bundle exit. And you can see some noise here in the flow as you're heating up. And then eventually you establish with a steady boiling conditions and the noise stops. And the reason for this noise, as you can see it here, this is the void fraction in the separator. The top, the very top of the separator. So when you first start getting these voiding happening in the top of the separators, you get that kind of oscillation that we talked about where you get this increased void, it increases the flow, it quenches the voids and then that cycle repeats.

Typically, the cycle has a period of about 15 to 25 seconds. So it's very slow. It's an enthalpy wave propagation rather than an density wave propagation. Enthalpy has to propagate all the way down the core up to the chimney. Now here the propagation time in the chimney is important because you're talking about enthalpy propagating all the way tot he top. And so time period is more like 15 or 45 seconds.

DESIGNATED OFFICIAL CARUSO: What's the oscillation that occurs at about 23,000 seconds?

MR. SHIRALKAR: Say that again.

DESIGNATED OFFICIAL CARUSO: What's the oscillation there that's occurring at about 23,000 seconds?

MR. SHIRALKAR: Here?

DESIGNATED OFFICIAL CARUSO: Yes.

MR. SHIRALKAR: These are small changes in the void fraction and in the separator as well. But these are -- the main concern was generally over here where you have a much lower pressure. By this time you run to fairly high pressure.

DR. BANERJEE: But there are some of those oscillations which seems quite large, right?

MR. SHIRALKAR: This is an oscillation in void fraction.

DR. BANERJEE: I mean to the 23,000 seconds. Keep going. Yes, right -- there's one big one past there.

MR. SHIRALKAR: This one here? Yes. That's when you start -- you open up the control valves and you get some depressurization when you do that.

ACTING CHAIR RANSOM: The separators, these are void fractions in the separator component?

MR. SHIRALKAR: Separators. Separators. Not in the core.

ACTING CHAIR RANSOM: And so they start out flooded, I guess, right?

MR. SHIRALKAR: Yes. And this is the transient for the higher power rate. Now the interesting thing that happened here was that initially we got a fairly high spike in the void fraction in the separators and that produced an increase in the flow such that it stopped the voiding until quite a bit later, and then it started voiding again at that point in the separator resulting in a higher flow rate.

ACTING CHAIR RANSOM: Do you have any feel for how much of this might be noise in the calculations as opposed to physical effects?

MR. SHIRALKAR: I think that physical effects in the sense, and you know that when you first put this void in the separator you're going to produce this kind of oscillation. How much of it is physical versus what is calculational, I'd have to guess and say I think it's mostly physical.

DR. BANERJEE: They're actually pretty long times, right?

MR. SHIRALKAR: Yes, these are long times. So the period here is like 15 seconds or 25 seconds. And this is the powerful rate which we are going to get to, but this shows sort of prolonged period here where the separator is trying to make up its mind whether to have voids or not. But it's flashing and then quenching and then flashing and quenching and then eventually starts building up more of a steady void fraction.

At this point now the middle part of the separator is also beginning to develop some voids. All this was only in the very top part of the separator.

Now this show the void fractions in the core. This is the top cell in the highest power bundle in the core.

CHAIRMAN WALLIS: Well, these voids aren't collapsing in the separator, are they? They don't condense?

MR. SHIRALKAR: No. It won't condense.

CHAIRMAN WALLIS: They just pass through?

MR. SHIRALKAR: They just pass through.

DR. BANERJEE: Now do you have observations of this nature in Dodewaard?

MR. SHIRALKAR: We don't. In Dodewaard they never saw any oscillations on the APRNs. So ten years ago when we are interested in the ESBWR we said, look, look harder. See what you can find. And the final startup they -- you know Dodewaard shut down many years ago. But the final startup they did a special slow startup just look at various points and see if they could see anything.

There was no indications on the APRMs, but then they did some oracle relation functions, they could surmise that there must be some slow damp velocity variations.

DR. BANERJEE: Well, the APRMs are seeing all liquid, right?

MR. SHIRALKAR: Yes.

DR. BANERJEE: There's no way, but they had no flow rates measurements, nothing?

MR. SHIRALKAR: No. It didn't show up in the flow rate measurements. The only way that they surmised it was by doing all the correlation function of APRM and -- so whatever it was, it was more like noise in the flow than anything else.

DR. BANERJEE: So if you simulated that with TRACG --

MR. SHIRALKAR: WE did.

DR. BANERJEE: Did you see any oscillations?

MR. SHIRALKAR: We got some. We got oscillations that were noticeable in the velocity but not in anything else. In the single phase region.

So the core is basically on void. And this is the top still at the hot channel that's showing small amount of subcool voids.

This is the higher power level. And then this one is the highest power level. But now we're beginning to see some voiding in the top of the core in the hot bundles. And that's probably getting down to, say, you have 36 cells, it's probably getting down about 8 to 10 cells into the core. And I think this is leading to somewhat more noisy behavior than we would like. This is the pickup rate that is beyond what we would be allowed by tech specs.

DESIGNATED OFFICIAL CARUSO: Could you go back to the previous slide? No, the one before that. I'm sorry.

At 24,000 seconds you raise the void

fraction. There's something that occurs there, right?

MR. SHIRALKAR: Yes. Yes. Then we go into the normal startup. We raise -- we've gone up to 63 bars so now we're raising the power level.

DESIGNATED OFFICIAL CARUSO: Okay. Now you have the oscillations that are occurring there at the exit. If the plant had just been allowed to sit there at that power level, how much would those oscillations grow?

MR. SHIRALKAR: These oscillations here?

DESIGNATED OFFICIAL CARUSO: Yes.

MR. SHIRALKAR: What is the magnitude of oscillation? I mean in void faction?

DESIGNATED OFFICIAL CARUSO: Well, I'm just saying, you seem to have terminated those oscillations by doing something in the plant.

MR. SHIRALKAR: Yes.

DESIGNATED OFFICIAL CARUSO: And if the plant had just sat there, what would have happened to those oscillations? Would they have damped out or would they have continued to grow? Because they look like they're growing.

MR. SHIRALKAR: I think -- I can't answer that question because we didn't the simulation longer. But my guess is they probably would have grown somewhat before they -- before they settled down to some mean value.

CHAIRMAN WALLIS: So it's sort of convenient that you raised it.

MR. SHIRALKAR: Well, we sat there for -- we're getting to about 63 bars at that point and then we start depressurizing or opening up the valves and raising the power level.

See, the next one.

CHAIRMAN WALLIS: It goes back to the beginning.

MR. SHIRALKAR: It's the wrong button.

CHAIRMAN WALLIS: Maybe you can go backwards from the end.

See, in this case you got this noise and then it died out. This is at higher power level. Same thing happened here. So my guess is if you had waited long enough, it probably would begin at similar characteristic.

DR. BANERJEE: What causes the oscillations?

MR. SHIRALKAR: Down here? I'm not sure. I haven't looked at it very hard. Our main interest was up here where we're looking at possibility of large scale oscillations when you first start the voiding process. We can look at that.

And the last one is we calculated -- I mean, so we have this small oscillation in the velocity while heating up, but what is the impact of that. And my bottom line is that it really don't have any impact. This is the calculated critical power ratio and we are used to looking at critical power ration to the order of one. Because heat fluxes here are so low, we're talking about critical power ratios on the order of 40. There's absolutely no impact on thermal limits -- heat fluxes are extremely low and we got basically single phase flow in the core.

CHAIRMAN WALLIS: Now is there a boiling boundary that's moving up and down with these oscillations?

MR. SHIRALKAR: Yes, but we're talking there about the core being essentially single phase. You know, there's a small amount of void at the very top of the hot bundle.

CHAIRMAN WALLIS: The core is subcooled?

MR. SHIRALKAR: It's subcooled, yes.

And TRACG calculates small oscillations but they're inconsequential because the core flow is single phase, no oscillation in neutron flux and large thermal margins. But if you raise the power of the heatup fast enough you can probably get into trouble. And those heatup rates would not be allowed to occur beyond by tech specs.

And we go beyond this initial phase to establish the table void fraction in separator and chimney and then you get a small extension to raise power.

Now these calculations are done without neutronics feedback with the assumption that we wouldn't have -- we were preventing void from forming in the core. Well, the Staff asked us to go back and repeat this calculation with neutronics feedback. And we have done that and the results are very similar to the situation. So essentially it confirms the point that neutronics feedback is not important when you have basically a single phase core situation.

ACTING CHAIR RANSOM: Neutronic feedback in this case was just you moderate your temperature?

MR. SHIRALKAR: Yes.

ACTING CHAIR RANSOM: Primarily I guess fuel temperature maybe enter into it.

MR. SHIRALKAR: Right. We did a simulation where we started out with some rod -- pull rods kind of in the startup mode. And then looked at the power, the full responses.

But that is my final slide unless you have any questions.

Thank you.

CHAIRMAN WALLIS: Let's see, now TRACG predicts all these interesting things. What's the check that they're right? Is there a check on the validity of these calculations or you just look at them and say that TRACG's predicting something and we've got to believe it.

MR. SHIRALKAR: Well, we've got some experimental data of this startup kind of phenomena in the -- in Japan.

CHAIRMAN WALLIS: And you have the comparisons with data then that showed support of this?

MR. SHIRALKAR: And we've compared with the Dodewaard startups and with the --

CHAIRMAN WALLIS: Because I thought we were supposed to get to sort of validate TRACG. And you're just simply showing us predictions of TRACG for ESBWR. That's no validation of anything.

MR. SHIRALKAR: Well, that's included in the validation report.

CHAIRMAN WALLIS: Which is something we're supposed to have read?

ACTING CHAIR RANSOM: Is that a GE report you're referring to?

MR. SHIRALKAR: Yes. It's a GE report.

CHAIRMAN WALLIS: Is that -- that we've never seen.

MR. SHIRALKAR: In fact, it's called TRACG Qualification for ESBWR. I think the Staff has the report.

ACTING CHAIR RANSOM: When was that published?

MR. SHIRALKAR: It was first -- first edition in the ESBWR days ten years ago and then revised maybe five years ago.

MS. CUBBAGE: This is Amy Cubbage.

That was the one now this morning I said it had been sent to the Committee earlier, but I could get you another copy of that immediately. We have all that electronically at the office. It's all in ADAMS also.

CHAIRMAN WALLIS: Well, don't give us anything in ADAMS. It's hopeless. I'm not sure any member of the Committee has ever used ADAMS.

ACTING CHAIR RANSOM: Yes.

CHAIRMAN WALLIS: Does it work?

MEMBER KRESS: Yes.

CHAIRMAN WALLIS: This is a proprietary presentation.

MS. CUBBAGE: Graham, we are going to be open for the beginning and then we're going to close, then we're going to reopen. The slide packet you have in front of you has all the slides. The audience just has the open slides at this time.

MR. LANDRY: Okay. My name is Ralph Landry from the Staff. And as Amy said, the presentation which we have today is going to have open material followed by proprietary material.

We put the statement on the cover slide just to indicate that this master set that we're using does contain proprietary information.

As we go through the presentations we'll get through my part and through Veronica Klein's part and then we get into the remainder, we'll be in close session.

The members of the review team that are with me this morning are Veronica Klein, Peter Yarsky from the Staff and our consultants, contractors, Jose March-Leuba and Jay Spore. And I'll go through in a few minutes the individual responsibilities during this review.

As an overview, I'd like to first go

through a little recap of some of the history that we've had with ACRS of looking at TRACG. And then we'll go into the scope of the review, the objective of the review as far as what we are presenting today.

And we'd like to remind everybody, as has been said several times this morning and again we're going to have to repeat it this afternoon, that this review is limited to the TRACG code and its application. It is a the methodology and the procedure for using that methodology to predict stability in the ESBWR. This is not a review of the ESBWR.

We had to use models for the ESBWR to conduct the review. However, we are not passing judgment or making any statements regarding the acceptability of the ESBWR design. We are simply coming to the bottom line of the acceptability of the TRACG code.

CHAIRMAN WALLIS: Well, the predictions of the ESBWR design that we have seen don't tell us anything about how good TRACG is. They just are predictions of ESBWR.

MR. LANDRY: We will go through our presentations and tell you some of our conclusions regarding TRACG and how good it is.

We're going to discuss a little bit about the approach that was taken in the review and who the reviewers are. I'll go through again the reviewers and what their responsibilities were.

And then we're going to talk about the results of the review. And this is where we will breakout into the individual members of the team. They will present the parts of the review which they have primary responsibility for.

And then we'll go back and go to conclusions.

Now, during the presentations we are going to make three conclusions at the end of each presentation that are regarding the parts that we reviewed. But then when we get to the end we're going to pull it altogether and give a complete sort of conclusions regarding the code.

DR. BANERJEE: What is SNPB?

MR. LANDRY: That is the abbreviation for the Nuclear Performance and Code Review Branch. And obviously that statement begins with an S.

DR. BANERJEE: Yes. Where does that come from?

MR. LANDRY: It took about three weeks before we figured out where the S came from. When the reorganization was put into place in NRR where it was broken down into three associate directorships. And under each associate directorship there were a number of branches. Or excuse me, a number of divisions and then a number of branches. Our division was the division of safety systems. So all the branches under our division begin with an S, safety.

Previously we've been the ACRS on TRACG on two occasions. In August/September with the Thermal-Hydraulic Subcommittee and then the full Committee in 2001 talking about the application of TRACG to anticipated operational occurrences in BWRs, the operating BWR fleet.

We were back again talking about TRACG in January/February when we talked about the application of TRACG to the LOCA in the ESBWR.

So we've been here on two occasions talking about TRACG and its applicability; once to the operating fleet and once to the ESBWR. And today we're here to talk about the applicability of TRACG to the ESBWR again, but for analysis of the stability.

As I've said and others have said so far, the objective of this review was to determine the acceptability of TRACG for prediction of stability in the ESBWR advanced reactor design. This review is limited to the ESBWR design. We are making no statements about the applicability of TRACG to analyzing stability in any other operating plant. This is limited to prediction of oscillation in the ESBWR. It's limited applicability to anticipated operational occurrence. And we'll get into a further definition. Bharat went through a definition this morning of what is meant by that, and we'll go through it again and explain what we mean by applicability to calculation of stability for AOOs.

And it is limited to the early phases of the startup, as Bharat previously explained.

The approach that was taken in the review follows the CSAU approach. This is the approach that was taken by the applicant, by General Electric. They have followed the CSAU approach in determining the acceptability of the code when determining the uncertainties in the code. This is involved review of the PIRT and on the identification and ranking table. We've reviewed some specific models within the code in great depth. We've reviewed the assessment cases that were run. We've reviewed the numerics used in the code and in the methodology. And we performed independent calculations --

CHAIRMAN WALLIS: Excuse me. All that stuff about explicit and implicit methods and how that seems to be a funny mix of them when they did it --

MR. LANDRY: That is proprietary and we have to go into closed session to discuss that.

CHAIRMAN WALLIS: We're not allowed to talk about that?

MR. LANDRY: No.

CHAIRMAN WALLIS: And did you understand it?

MR. LANDRY: Well, I know -- did. I won't make any plans to it, but --

DR. BANERJEE: Wait until we get closed session.

MR. LANDRY: We've performed independent calculations using the TRACG code. We've performed independent calculations using the LAPUR code and using independent void modeling methods for that was done by Jay Spore. And Jay will go through those.

The reviewers that were involved in the review include Veronica Klein. Ms. Klein has been with us for three -- three years.

MS. KLEIN: Three and a half.

MR. LANDRY: Three and a half years. She is an excellent reviewer as --

CHAIRMAN WALLIS: Excuse me. Were these independent calculations, GE seems to regard these transients as being very close to a second order down system. It would seem that you ought to be able to develop a simple model which would represent that. And this would be very helpful in convincing us that the physics are being captured. It's just a simple result, it ought to have a simple explanation.

MR. LANDRY: Why don't you wait, Graham, and let us get through the --

CHAIRMAN WALLIS: Rather than using TRACG for everything.

MR. LANDRY: Jay has pulled out particular models and generated -- grades that particular models that look at specific points. So let us get through some of those explanations.

CHAIRMAN WALLIS: You were talking about what you've done for independent calculations. There's no bullet that says simplified model which captures the physics, right?

MR. LANDRY: Not of the entire transient, no.

As I was saying, Veronica Klein has been with us for a few years now. And has been given the lead responsibility for this review.

During the past year or two years I've been very heavily involved in the 55.6A work and felt that it's time to start transferring knowledge and make sure that some of our younger staff members come along and can take over in a lot of the lead responsibilities.

Veronica has done an excellent job with leading this review. She went out to San Jose and lead an audit of GE. And she and Peter Yarsky went down to Wilmington a time with the GE/GF code developers and code modelers studying the way the code is used and the procedures for using the TRACG code.

Now Peter Yarsky came to us in September after finishing his Ph.D at MIT in reactor physics. He's one of our reactor physics experts and has done a lot of modeling for his work today.

Jose March-Leuba is the world renowned Dr. March-Leuba, world traveler also.

Jose, as you heard Bharat refer to this morning, 25 years ago was one of the originators of one of the thought on how to phase or regional oscillations may occur. Jose has had a long history of reviewing stability and is one of the leading authorities on stability analysis.

Jay Spore started this work with us when he was at Los Alamos National Laboratory. He has since left LANL and is now with Information System

Laboratories. He is a numerist and a code developer and modeler in his own right and has been the responsible person for reviewing the numerics in the code.

As I said, we're each going to give some brief conclusions. Some of the brief conclusions as a lead in. I know I haven't given you any of the basis for these, but we will give you the basis for these conclusions as we move along through the presentations.

CHAIRMAN WALLIS: Well, the first bullet, I mean TRACG can be wrong and can give results. We know that. How do you measure its capability?

MR. LANDRY: And that we are going -- as I said, that we are going to give out as we go through the presentations. I'm simply leading off with some statements of conclusion.

To give you an idea of where we're going with this discussion this afternoon, first is that TRACG is capable of calculating stability in the ESBWR.

CHAIRMAN WALLIS: Well, it's a Graham Wallis back of the envelop calculation can calculate stability, too. But I don't say it's much good.

MR. LANDRY: We didn't consider the model calculation --

CHAIRMAN WALLIS: I know, but you see what I'm getting at. And there has to be some tests of ability.

MR. LANDRY: Well, we're going to explain some of this, Graham, why we believe the code to be capable.

MEMBER DENNING: But when you say it's capable you mean within the accuracy required for this specific application?

MR. LANDRY: Yes, correct.

CHAIRMAN WALLIS: Well, with some specifications?

MR. LANDRY: Correct.

CHAIRMAN WALLIS: You have specifications of what it has to be able to do?

MR. LANDRY: This will come out in our discussion. And this comes up further down the list of these conclusions. That goes to the bottom; that it's not only the specification, it's the procedure that is described. It's not just the code that is reviewed. It's the procedure in using the code also that we have stated as our conclusion that not only is the code capable and acceptable, but that you must use the procedure that has been defined also. You can't go off and use a different procedure for using the code and our conclusion would still apply.

CHAIRMAN WALLIS: Now that means noting and all those boxes and everything?

MR. LANDRY: We'll get into a lot of these discussions.

CHAIRMAN WALLIS: Is that what you mean by procedures?

MR. LANDRY: Yes.

We also concluded that TRACG's stability procedure can be applied to an ALO once a new steady state condition has been achieved.

CHAIRMAN WALLIS: Are you going to conclude that TRACG gives results correct within certain acceptable limits?

MR. LANDRY: That is within the uncertainty which has been defined. An uncertainty analysis has been performed and we will conclude that that is an appropriate uncertainty.

CHAIRMAN WALLIS: Well, again, it went through the motions.

MR. LANDRY: We'll make some more conclusions about this. You have to look at the assessment and we'll talk more about the assessment cases and the conclusions in a little bit. We have to get into closed sessions to go into that, Graham.

CHAIRMAN WALLIS: Okay.

MR. LANDRY: And as Bharat has described, we are also going to make some statements regarding TRACG's ability to predict the startup trajectory stability for the first four startup phases.

And with that, I'd like to turn the presentation to Veronica Klein.

MS. KLEIN: Hi. My name is Veronica Klein. And as Ralph mentioned, I'm a member of the Nuclear Performance and Code Review Branch. And my role in this review was I did the overall coordination between the review of our contractors and the staff. And so today I'm going to give you just a brief overview of where our review our was focused and perhaps a preview of some of the reviews that are to follow.

Now as Ralph mentioned, the applicant followed a CSAU approach and we reviewed -- some of the main areas in which we reviewed were the code applicability, the PIRT, the assessment, the bias and the nodalization. In addition, we reviewed the calculation procedure, the xenon assumptions and TRACG's capability of modeling oscillations during startup.

And as Ralph has also mentioned, we had

some assistance from some of our experts. Dr. March-Leuba who is an expert in stability, he took the lead in reviewing the PIRT and also the nodalization and the startup. And he was also just our resource for any sort of ESBWR stability features in which we needed to understand.

And Jay Spore was our expert on numerics and TRACG models. And he took the lead on reviewing the code applicability and the assessment and the bias and uncertainty and made contributions to the review of the PIRT. And he and Jose will be presenting their results following the presentations of the NRC Staff. And the Staff has reviewed the contractor's report on this topical, and we have found it to be acceptable.

Now, there are several considerations in which -- well, there are several items in which we found to be important for predicting stability in ESBWR but were not part of the scope of this review. And the first four items on this list we are considering as inputs into the model, and they're all handled in their own topical reports and will be reviewed separately. And these are the dynamic back conductants input, the critical power correlations, the cross section generations and the ESBWR fuel.

The next two bullets were not submitted as part of this application and are considered outside of the scope and will be reviewed at a later time. And that's stability during ATWS events and stability with transient xenon conditions.

Now, our review of TRACG we tried to expand upon the previous reviews that were performed in the past. And so for when we reviewed the code applicability we tried to keep our focus on the models that were important for predicting stability, such as void fraction. And we also reviewed in detail the explicit integration scheme. And in our review of the PIRT we only reviewed the phenomena which we considered important for the prediction of stability events.

And in the review of the assessment of TRACG we did look at the current assessment base, which has been mentioned by GE, that is in these preceding documents. And we have reviewed that information, plus we've also reviewed the information which was contained in the topical that had tests that were specific or that were done more specifically to address ESBWR stability.

And we've also reviewed in detail the bias and uncertainty. GE has used a previously approved statistical methodology which we have also reviewed and have approved that in the past.

We approved their nodalization scheme and in particular, GE uses a fine axial and radial nodalization scheme for performing their calculations.

The range of our application of this review only covers steady state conditions. We also cover the range of off normal steady state conditions, and this has also mentioned twice. What this really means, but this is for the AOO condition that are just steady state conditions which may seen during an AOO. And we also reviewed the applicability of TRACG for startup.

In support of this review we had performed a number of calculations. We have performed audit of GE calculations. We have visited the San Jose offices and looked at their calculations, as well as visiting the Wilmington offices and looked at their calculational procedures in detail and have used TRACG the same way in which GE does to get a better idea of how they do their calculations.

We've also performed independent calculations using TRACG. We've performed independent calculation using LAPUR, which is a frequency domain code. And we have performed independent calculations of the void profile in the hot channel using TRACE, RELAP5 and an independent drift flux model --

CHAIRMAN WALLIS: Now these are steady state voids, they're not perturbed voids?

MS. KLEIN: Yes.

ACTING CHAIR RANSOM: When you said steady state on the previous slide.

MS. KLEIN: Yes.

ACTING CHAIR RANSOM: Did that include oscillations without a steady state, you know, for stability?

MS. KLEIN: I'm sorry, what do you mean?

ACTING CHAIR RANSOM: Well, you're saying the range of application of steady state conditions. But we're talking about stability here.

MS. KLEIN: Basically what we're saying is, and this has been repeated in the past, is that there is some confusion when you talk about application of the methodology for a transient. And all we're saying is that methodology itself does not allow for calculating things as decayed ratios during a transient. So what we are saying is that everything that we have reviewed requires that GE have a steady state condition and then perturb it. But what we're saying is it has to be a steady state before there's any perturbation, otherwise it was not covered in this topical report.

ACTING CHAIR RANSOM: But that is a transient. You're saying the range of application includes steady state but also perturbations around the steady state?

MS. KLEIN: I guess basically it's just that we're saying that we're not trying to evaluate the decay ratios during the actual transient while power, while flow, while things are actually changing. That was just not part of the methodology. The methodology was just you have to perform a steady state to begin with. And that's all that we're trying to say, that that starting point, it has to be a steady state in order for this methodology to be applicable.

MEMBER DENNING: Is that a deficiency in the analysis? I mean, I understand what you did. Have you evaluated that and said that's okay, all we have to really do is look at these steady state or quasi-steady state initiating conditions.

MS. KLEIN: Yes. Dr. March-Leuba has a lot of good slides on this where he talks about how it encompasses some of the limiting conditions that may be seen. So if you can wait until his presentation, I believe he'll be able to really illustrate that for you.

CHAIRMAN WALLIS: Well, presumably, if the system were unstable, TRACG would be unable to calculate a steady state condition.

DR. MARCH-LEUBA: This is Jose March-Leuba.

What we're going to say, and you are 100 percent correct, that you have to perform transient to measure the decay ratio with TRACG. That's what the procedure says.

Now, the decay ratio, and when it's my time I'll give a little bit of the math behind it. The decay ratio is a property of the core like its mass or its temperature. It's a parameter that exists even if you don't run the transient.

TRACG chooses to run a transient with TRACG to calculate that parameter or that property of the core. Okay. But it is a property of a steady state condition of the core. The core or the steady state operating condition has a decay ratio whether a transient or -- imposed or not. The same way it has a mass --

CHAIRMAN WALLIS: Well, I think you're right, but I think it's true that it's the core plus the downcomer and the chimney and the circuit which has the decay ratio. It's not the core by itself.

DR. MARCH-LEUBA: I mean the whole.

DR. BANERJEE: But the decay ratio assumes a certain model first, doesn't it? I mean, it doesn't have to be a decay ratio in the sense that --

DR. MARCH-LEUBA: You are 100 percent correct. We have already -- we want to develop on my slides and we want to go to the blackboard because I hear there are so many misconceptions about what we're talking about.

There are many things that we can't measure. And it's not relevant and I'll tell you why if you're willing to wait another 20 minutes.

CHAIRMAN WALLIS: We have to wait until it's your turn.

DR. MARCH-LEUBA: Yes.

CHAIRMAN WALLIS: You're going to tell us that there's a decay ratio no matter what the form of the signal?

DR. MARCH-LEUBA: Yes.

CHAIRMAN WALLIS: Okay.

MS. KLEIN: Okay. That ends my presentation. Next is to Dr. Peter Yarsky of the NRC Staff.

CHAIRMAN WALLIS: So your role in this was a manager, was it, or did you do calculations?

MS. KLEIN: Pretty much. I assisted Pete a little bit when he did his calcs. And I went with Jose when we went to GE and he taught me a little LAPUR. But I didn't do any main calculations.

CHAIRMAN WALLIS: But you didn't say gee wiz, I don't believe that. How about this and that and show me this.

MS. KLEIN: Well, I mean I read through the topical. And there were things that, you know, what I didn't know. And since I'm not an expert in a lot of the areas that were involved, that was why we have the assistant. And so I would had to call up Jay and say could you look at this. And call up with Jose and say could you look at this.

CHAIRMAN WALLIS: Whoever managed the SER seems to have avoided asking a lot of questions. And maybe they got answered and they were thought to be not important. It doesn't seem to be full of a lot of questions being raised and answered.

MR. LANDRY: The Staff did ask a number of questions. We had a number of interactions with the applicant. We did not provide a listing of all the RAIs and all the responses in the SER, but yes we did ask quite a few questions.

DR. BANERJEE: We got all the RAIs and responses, didn't we?

MR. LANDRY: Yes.

DR. BANERJEE: I mean there were four of these that were sent to us.

I had a related question. Were there no codes which could actually do a time domain integration other than TRACG?

MS. KLEIN: No. TRACE is currently still being developed for that capability. We don't expect that it would be completed until 2008.

CHAIRMAN WALLIS: I thought TRACE was based on TRACG to be able to do the same thing.

MS. KLEIN: It is, but it doesn't have the right numerical schemes in it to perform these types of calculations.

DR. BANERJEE: So it can handle a blowdown but it cannot handle a stability problem?

MS. KLEIN: It's mostly within the numerics when we talk about the inputs, explicit integration schemes that has not been fully implemented in TRACE yet and it's not been tested. And we hear rumors that some people do, but it's not been fully benchmarked, it's not been tested. And Research has told us that it won't be ready until 2008.

DR. BANERJEE: For stability?

MS. KLEIN: Yes.

DR. BANERJEE: What about other codes like RELAP and so forth?

MS. KLEIN: We don't know of a time domain code that has ever, other than TRACG that has performed--

DR. MARCH-LEUBA: We do know of other codes. For example RAMONA code is used by -- it was licensed to ADD and it is their primary stability mission into analysis. And they've been using it for 6 years -- it's been licensed for 5 or 6 years already.

DR. BANERJEE: Is that a drift flux type model?

DR. MARCH-LEUBA: RAMONA? Some expert on thermal-hydraulics will have to tell you that. It is an internal momentum equation so it doesn't follow the speed of sound. That what I know. And that's why it's able to model each and every one of the channels which is a really good advantage. I believe it's a five equation model, but don't -- I don't know those details.

So RAMONA is widely used in Europe. It's used a lot in Sweden and Switzerland. Some codes in tandem may have been used in Japan for stability, too. But I'm not that familiar with those. So definitely RAMONA is licensed by the NRC to be used for stability equations since the year 2000.

DR. BANERJEE: So could it have been used or could not have been used?

DR. MARCH-LEUBA: RAMONA could have been used, but it's a proprietary code that GE does not have access to.

DR. BANERJEE: No, but NRC does or does it not?

DR. MARCH-LEUBA: Is the RAMONA 5- the ADD version of RAMONA. It's not the public version of RAMONA. When you start talking about RAMONA, there are many versions. It was the ADD RAMONA version that was qualified, not the public version.

DESIGNATED OFFICIAL CARUSO: Could the NRC version be used to evaluate the stability?

DR. MARCH-LEUBA: Conceivably, but you will have to benchmark it first. And honestly, if you want to know what the decay ratio is, you really should use a frequency domain code, that's what they're designed for. And they're much cheaper, easier to use and, frankly, much more accurate.

The only problem and the reason why GE

decided to TRACG for ESBWR was the concern with the chimney that the code that they have licensed does not have a chimney and therefore -- and also cannot handle the startup, cannot handle the -- the other disabilities. So that was, in my opinion, that's the reason why ESBWR decided to use TRACG. It's a lot more expensive. I can run a pool in about a minute and a half of CPU time and it has 400 times it has full -- imagery and it's accurate.

MS. CUBBAGE: All right. Dr. Peter --

CHAIRMAN WALLIS: So why can't TRACG do something similar?

DR. MARCH-LEUBA: Sorry?

CHAIRMAN WALLIS: Why is so expensive and complicated to do a lot of TRACG runs?

DR. MARCH-LEUBA: It's an expensive--

CHAIRMAN WALLIS: It's just a code.

DR. MARCH-LEUBA: Yes.

DR. BANERJEE: Well, it's not paralyzed or what is the problem?

DR. MARCH-LEUBA: CPU time is not a concern anymore.

DR. BANERJEE: And it is a 1-D calculation.

DR. MARCH-LEUBA: Yes. But -- it has to

go in with a -- and it's all on the code -- on the input development, evaluation and recommendation. That's where your cost is. It's not the CPU time anymore.

CHAIRMAN WALLIS: Did anybody do a simple thing with simply an average channel and an average chimney and circulation loop which you can run in about two seconds?

DR. MARCH-LEUBA: Things like that have been done.

CHAIRMAN WALLIS: To explore it, to explore what happens when you change things?

DR. MARCH-LEUBA: You can Google Jose --

CHAIRMAN WALLIS: I think that would be very helpful.

DR. MARCH-LEUBA: Jose's five equation model, you can do a Google search, and you'll find lots of hits. I mean, I develop it 20 years ago and people still use it. The problem is accuracy.

MEMBER DENNING: On the frequency domain codes, how do they treat the core the thermal-hydraulics in detail? I mean, are you just saying that you don't have to treat those in the detail the TRACG does?

DR. MARCH-LEUBA: No, you do.

MEMBER DENNING: You do?

DR. MARCH-LEUBA: Yes. Only the solution is in the -- instead of having to step through the time. So you have --

MEMBER DENNING: You have the same level of detail, a description of the neutronics in the--

DR. MARCH-LEUBA: Considerably. Considerably you can do it. In the particular case of the LAPUR code, which is the one that the Staff uses because it is the one that we own, it's not. It doesn't have that much detail. It was developed in the late 1970s and it was developed by a graduate student for $40,000.

It's a slip model on kinetics, so it does have some limitations. All the code you see is 1-d kinetics and it has better thermal-hydraulics. That Staff that Areva uses is an excellent code and it has all the detail you would ever want.

There are some frequency domain codes, including LAPUR, that have been upgraded to 3-V. There is a LAPUR version 6 which has been developed in Spain which has built the 3-D capability neutronics.

MEMBER DENNING: And somebody is going to explain to us why one has to use the explicit version of this analysis rather than implicit even though it's solution of the same equation?

DR. MARCH-LEUBA: Yes. That's the first thing I want to do on the blackboard.

DR. BANERJEE: But now when you say a frequency domain code with thermal-hydraulics, the behavior will depend on the thermal-hydraulics model, right?

DR. MARCH-LEUBA: Correct.

DR. BANERJEE: So if you have a six equation model of the type that TRACG has with whatever the interfacial friction or whatever, basically you're linearizing that system in some way.

DR. MARCH-LEUBA: Correct.

DR. BANERJEE: And that's all that you're doing in a frequency domain code. So if you have the same models as TRACG and you did a linearized analysis of this, would you get the same answer as TRACG gets?

DR. MARCH-LEUBA: You should if TRACG is using the displaced interfacial method correctly. The frequency domain analysis will give you the correct decay ratio because it's integrated analytically and there is no numerical diffusion.

DR. BANERJEE: Based on a linearized-

DR. MARCH-LEUBA: Decay ratios are linear parameter.

DR. BANERJEE: Yes. So if it was finite amplitude oscillation, you would not capture that correctly, right?

DR. MARCH-LEUBA: The decay ratio is a characteristic of the reactor that exists even if there are no oscillations.

DR. BANERJEE: Sure.

DR. MARCH-LEUBA: And it's defined only for oscillations of 10 to the minus 5 in the linear region. So therefore, the decay ratio is a linear parameter.

DR. BANERJEE: Yes. So if you'd start with the linearized thermal-hydraulics model --

DR. MARCH-LEUBA: Yes.

DR. BANERJEE: -- you'd start with the linearized neutronics model?

DR. MARCH-LEUBA: Yes.

DR. BANERJEE: And you'd look at this whatever the hell you get.

DR. MARCH-LEUBA: Write down the equations and invert the methods.

DR. BANERJEE: Yes. So that's all you do?

DR. MARCH-LEUBA: Correct.

DR. BANERJEE: Right. But it doesn't tell you anything about finite amplitude perturbations?

DR. MARCH-LEUBA: No. No. And that's the beauty of the time domain codes. You use time domain codes whenever you want to see what happens when the decay ratio goes over one. The moment the decay ratio is greater than one, the oscillations start to go. Frequency domain code doesn't tell you absolutely nothing about how those are going to grow; are they going to be ten percent, 1,000 percent, 10,000 percent.

DR. BANERJEE: Yes, but I want to give analogy which may not work here but it has some meaning. If you look at pipe flow --

DR. MARCH-LEUBA: Yes.

DR. BANERJEE: -- if you give it infinitesimal perturbations, it will stay stable until the 100,000 Reynolds number. On the other hand if you give it a finite amplitude perturbation, it'll go unstable at 2,000. So the stability of pipe flow, however, is not determined by these little frequency domain things of the Navier-Stokes equation. If you do hydrodynamic stability analysis you get nothing of usefulness.

DR. MARCH-LEUBA: Yes.

DR. BANERJEE: So why do we expect that to work for reactors?

DR. MARCH-LEUBA: Because it has benchmark over the last 50 years of experience. If you do a search, you will hear the term subcritical Hopf bifurcation. That's what you're talking about is when you can make a linearly stable reactor go unstable by having a large enough perturbation. And there are some publications by -

CHAIRMAN WALLIS: We talked about that this morning.

DR. MARCH-LEUBA: Yes. Basically what you're doing is if you perturb the power sufficiently enough so you make it unstable, then it's unstable. Okay. So if you put a perturbation that is large enough, you will make anything unstable. But you require a very, very large perturbation.

We use decay ratios of .02. It's not because we're noisy, it's not because it is the final goal. We want to know how much margin this piece that they're proposing to build has to stability. And the decay ratio is a nice figure of merit that tells you you have decay ratio of .02, you have lots of margin. You have a decay ratio .08, you don't. And you have to worry. We use decay ratio of .02. I mean, you have to keep that in mind.

MR. LANDRY: At this point, we're going to go to closed session.

Mr. Chairman, the rest of the presentations are going to be closed.

ACTING CHAIR RANSOM: Now is this presentation closed?

MR. LANDRY: This presentation is closed.

ACTING CHAIR RANSOM: Okay. We'll go into closed session then.

(Whereupon, the proceedings went into Closed Session.)

ACTING CHAIRMAN RANSOM: We're in open session then.

MR. LANDRY: Well, I had prepared a number of slides which you have in the handouts which restate all of the conclusions that we've arrived at for each of the presentations. Rather than go through that whole list again, which we talked about considerably as we've been going through the presentations, I'd like to come point and state at this time where the Staff would like to go next.

We would propose to provide to the Committee as soon as possible, which means that I'll probably try to burn a CD for you tomorrow, with three documents. The TRACG qualification report from January of 2000. The TRACG qualification for SBWR report from August of 2002. And the TRACG qualification for ESBWR report from August of 2002.

CHAIRMAN WALLIS: How long are these?

MR. LANDRY: Pardon me?

CHAIRMAN WALLIS: How long are these documents?

MR. LANDRY: I don't know how many pages off the top of my head. But they're several binders.

CHAIRMAN WALLIS: So they're substantial documents?

MR. LANDRY: Yes. These are the documents--

CHAIRMAN WALLIS: And we have a lot to do next week. We can't do anything on this, it seems to me. And you're asking us to review these before we meet next in the February meeting?

MR. LANDRY: No. Our proposal is to get these documents to you which will contain within them the comparisons with the CRIEPI, SIRIUS information so that you will have that information at hand before we meet again.

CHAIRMAN WALLIS: Are you talking about the February meeting or the full Committee?

MR. LANDRY: We haven't said anything about the next meeting yet.

CHAIRMAN WALLIS: No, but we are meeting. We have two hours scheduled in February to discuss this in full Committee meeting. For some reason someone has decided that this can all be done in a couple of weeks and we'll be ready to write a letter saying everything's fine.

MS. CUBBAGE: Graham?

CHAIRMAN WALLIS: And I think that's a mistake.

MS. CUBBAGE: Graham, this is Amy Cubbage.

CHAIRMAN WALLIS: Yes.

MS. CUBBAGE: Actually, I just found out this morning that we were scheduled for February.

CHAIRMAN WALLIS: You didn't know that?

MS. CUBBAGE: I did not know that. We had been bumped to March and I was informed this morning that we were on the schedule for February. So if everyone's in agreement, we don't have any problem with going to the March meeting. But as I understand from Ralph telling me that they may not have room for us.

MR. LANDRY: It's going to be a problem going to March.

CHAIRMAN WALLIS: Well, I think we need to spend some time discussing this. Because if you go to the February meeting, you'll probably get a letter or at least you'll get comments from me saying we need to see more before we can really say this is okay.

MS. CUBBAGE: Okay. Well, some of the information we think you need to see would be in this qualification reports.

CHAIRMAN WALLIS: But we wouldn't have time to read it.

MS. CUBBAGE: No, I understand. And that's why we're suggesting -- I agree with you that

February is aggressive. So we'll have to work with ralph if we can get back on the schedule at a later time.

You want to continue, Ralph.

CHAIRMAN WALLIS: But we're in the *Federal Register* for 2 hours of your presentation on --

MS. CUBBAGE: Okay. Well, it's unfortunate that no one told me that.

MR. LANDRY: Right. In NRR we've been operating under the instruction that we've been given for the schedule.

CHAIRMAN WALLIS: It seems to me strange. Usually you guys are pressing us to do things quicker.

MR. LANDRY: And we had planned on being here in February. But we were told through the scheduling process that we had been bumped from February to March. So that's been the target date that we've been shooting for. And our proposal is get these three documents to you as soon as possible.

We will also be revising the SER to include further description which Jose has presented. Something along the lines of what Jose has presented today. And we will also be getting from General Electric additional calculations of looking at nodalization in the chimney. When we get that additional demonstration of the effect of nodalization in the chimney and the description of stability from Jose, and you will have the documents on SIRIUS, CRIEPI comparison --

CHAIRMAN WALLIS: So the logical thing would be to have another Subcommittee at which we go through all this stuff, which is now going to answer some of the questions we had today. And then at the end of that we agree that the case has been properly made, then we say we go to the full Committee, which we would be a process which I would think would take, you know, several months. It's not something you do tomorrow. And this is a very important issue for a very important new reactor design. It's some new features we haven't seen before. I don't think you just brush it off in a couple of weeks.

MR. LANDRY: No, we weren't trying to brush it off in a couple of weeks.

CHAIRMAN WALLIS: Well, you're asking us to make a decision by -- well, it appears that we're being asked.

MS. CUBBAGE: No, we're not.

MR. LANDRY: We're not.

CHAIRMAN WALLIS: It would appear from our schedule that we're being asked to make a decision.

MR. LANDRY: Right. And what we're proposing is to update the SER appropriately and get that to you quickly so that you --

CHAIRMAN WALLIS: So that's more of this evidence you've been asking for?

MR. LANDRY: So that you can have it to look over and we can prepare for our March meeting.

CHAIRMAN WALLIS: Don't we need another Committee meeting to look at this.

MR. LANDRY: If we can be put into the March schedule. It is the feeling of the Staff that there is sufficient information that if we supplement it with a nodalization study on the effect of the chimney and further description of the stability, that this is not a major perturbation in the information at hand, and that we should be able to go forward.

MEMBER DENNING: Ralph, can we change it from February or are we locked into February meeting anyway?

DESIGNATED OFFICIAL CARUSO: We don't have to go in February, but March is very full. We moved a bunch of things out of March into February because March was undoable for the Committee.

CHAIRMAN WALLIS: So let's move it to April.

DESIGNATED OFFICIAL CARUSO: It might be possible to move it into April.

MEMBER KRESS: Can we make a four day meeting in March?

DESIGNATED OFFICIAL CARUSO: I don't know. I have to check. I don't control that. I don't know.

MEMBER KRESS: It's the same week of the regulatory information conferences.

DESIGNATED OFFICIAL CARUSO: In March?

MEMBER KRESS: That same week.

DR. BANERJEE: Ralph, there's a Subcommittee meeting in February, right, 14th to 16th?

DESIGNATED OFFICIAL CARUSO: Yes, to talk -- and it might be possible to add on to that. That's a possibility.

DR. BANERJEE: Yes.

DESIGNATED OFFICIAL CARUSO: But that's going to be all about your favorite topic, chemical effects and --

CHAIRMAN WALLIS: Well, that's another one we're wrestling with, yes.

DESIGNATED OFFICIAL CARUSO: Right.

CHAIRMAN WALLIS: And I understand the 23rd/24th ESBWRs has moved.

MS. CUBBAGE: That's right.

CHAIRMAN WALLIS: So we can worry about this part of ESBWR perhaps in February?

DESIGNATED OFFICIAL CARUSO: 23rd/24th of February?

MS. CUBBAGE: We were scheduled to go to the PRA Subcommittee. That has been postponed.

CHAIRMAN WALLIS: Right. SO we have more time free than they have.

DESIGNATED OFFICIAL CARUSO: Oh, okay.

DR. BANERJEE: If you could add it to the debris thing, it would make some sense.

CHAIRMAN WALLIS: That would make sense.

DR. BANERJEE: Because we're going to be here for that. Save a trip.

CHAIRMAN WALLIS: And then we'd actually go to the full Committee in March.

DESIGNATED OFFICIAL CARUSO: 14th, 15th and 16th. Let me go off and check about March, the availability with the full Committee --

CHAIRMAN WALLIS: Well, Sam is not going to be happy.

DESIGNATED OFFICIAL CARUSO: I know. I know.

MEMBER DENNING: But I think we do agree we need another Subcommittee meeting before we go to the full Committee?

CHAIRMAN WALLIS: That's my impression is we need a Subcommittee meeting.

DESIGNATED OFFICIAL CARUSO: Okay.

CHAIRMAN WALLIS: There's too many loose ends.

And I thought that the whole process of operation at this ACRS was that subcommittees reviewed material. When there was general agreement that the subcommittee had seen enough, that the stuff was mature enough, then it went to the full Committee. You couldn't just sort of schedule it's going to the full Committee without having any idea how the Subcommittee is going to respond to what they see.

MEMBER DENNING: But I think we do agree with the items that you identified as those things that we'd like see more of. I think that, you know, it's clear we have heard what people have been saying here. So I think that you're going off in the right direction as far as additional information provided. Don't you agree?

CHAIRMAN WALLIS: Yes. And what we heard from Jose I thought was very valuable today. But we didn't know it until we came here today. We need to have it, perhaps, in a more organized fashion.

MS. CUBBAGE: On behalf of the staff I just want to make sure there's clarity on what you're expecting at this next Subcommittee meeting. You've already heard additional information from Jose today. So I don't expect that you'd want to hear that again.

We're going to provide you with these qualification reports. Are you asking plus --

CHAIRMAN WALLIS: Well, we asked all these questions about while -- you know, you've got this one plotted decay ratio of the power. And you've got a decay ration in some way. There's no indication of what the voids are doing in various parts of the system or how the pressure drop and the inertia terms are balanced around the loop, how important are the terms coming from the core and coming from the chimney and all that which would indicate why it is that the core is more important than the chimney. Those are the sorts of things I'd like to hear so that we get a proper perspective that shows that you understand what's going on. Not just one curve we're supposed to believe.

MS. CUBBAGE: Okay. So, obviously, this is the technical issues that the reviewers address --

CHAIRMAN WALLIS: It's a major issue.

MS. CUBBAGE: But would the calculation

with a finer nodalization in the chimney address the issue of the role of the chimney?

CHAIRMAN WALLIS: Well, you've got to show what the voids are doing in the chimney and how the pressure drop components around the loop contribute to the dynamics of what's happening.

MS. CUBBAGE: Okay. I'm just trying to avoid us coming back and then that not satisfying --

CHAIRMAN WALLIS: Well maybe we need to make a list of some of the things we're hoping to see. And I just wonder if you can do that in a short time.

DR. BANERJEE: Sorry. I was just going to say that it would be very useful to see comparisons of the code against data.

CHAIRMAN WALLIS: Yes.

DR. BANERJEE: And not just for decay ratio and frequency, but actual predictions from the code compared with data on things like pressure, void fraction, flow rates; whatever available. So these were oscillating, how well are those oscillations actually being predicted by the code. Not just the decay ratio and a table and a frequency and a table.

MS. CUBBAGE: Okay. GE, does the qualification report have that level of detail?

DR. BANERJEE: Well, if it doesn't, then

they must have that data.

CHAIRMAN WALLIS: Somewhere.

DR. BANERJEE: Somewhere. That's what we would like to see.

MR. SHIRALKAR: It does in some cases. Whatever is available was in that. And for the pre-EPRI, for example, there's comparisons of void and observations and so on.

DR. BANERJEE: Right.

MR. SHIRALKAR: For LaSalle there's comparisons of flow and power oscillations. And for other plants as well. But not in, say, obviously in the plant data I won't have detail like what action and like that.

DR. BANERJEE: Right. But he put a void propagation velocity down for Forsmark and Leibstadt. This was presumably cross correlations between NPRMs or something. Could you get that what propagation velocity and frequency or whatever.

MR. SHIRALKAR: We didn't measure any propagation velocity.

DR. BANERJEE: Was it just frequency then or --

MR. SHIRALKAR: I think it was frequency.

DR. BANERJEE: All right. And that was

just oscillations in the NPRMs.

MR. SHIRALKAR: Yes. Typically the relationship between the propagation time and the period of oscillation. I think that's probably what it will show.

CHAIRMAN WALLIS: You see now we have this argument about how -- that the voids here are out of phase with the flow rates through the orifice and so on. Why don't you show it. Show the TRACG predictions of the flow rates through the orifice and the voids and show that there is. And if there isn't, then you've made a statement that is not validated by the TRACG.

DR. BANERJEE: Yes. To answer your question, it would be nice to actually see the experimental traces of certain quantities versus the predicted traces of that. Not just a number but to see in fact are they looking somewhat similar? Are they just shifted? Is the frequency wider. You know, so to get a real feel for what's going on. Because you can't get that from a couple of numbers on a table. And the judgment as to whether to it's actually doing a good job or not is a feel for how these agree with each other. It's not just looking at two numbers at a table and then putting an uncertainty band on it.

CHAIRMAN WALLIS: I mean if I were consulting an industry on something like this for why some manufacturing process producing sausages is producing sausages with the wrong wave length and the wrong amplitude or so, I would want to do this. And this is nuclear safety. So I expect at least that quality of detail.

DR. BANERJEE: Plus, Jose is documenting your model, right? So we know you equations you've solved. And what is the matrix you inverted and all those things.

DR. MARCH-LEUBA: This is fully recommended.

DR. BANERJEE: Yes.

ACTING CHAIRMAN RANSOM: It would be interesting to interpret those result that show the separation between effects in the chimney from the core. And I think you probably can do that.

DR. MARCH-LEUBA: Yes. I'm already thinking of how to do it.

DR. BANERJEE: So if you wrote a paper for the EPS Science and Engineering and I have to review it, think of it that way.

CHAIRMAN WALLIS: Maybe he will.

DR. BANERJEE: Yes.

DESIGNATED OFFICIAL CARUSO: If you think of anything else that you would like to provide, have them provide, send me an email and I'll pass it along to the Staff.

MS. CUBBAGE: Okay. And then, Ralph, you'll get with us on schedule.

CHAIRMAN WALLIS: I'm a bit surprised that we have to ask them specifically and explicitly for all this stuff. And I would think a professional trying to present stuff to another professional to convince him that he knew what he was doing would know some of the level of detail that was appropriate.

ACTING CHAIRMAN RANSOM: I think that pretty well summarizes it. So maybe we can move on to the next topic.

Sometimes I think they're afraid data.

DR. BANERJEE: But actually every time they could have shown us data, we have gone over it actually in less time.

ACTING CHAIRMAN RANSOM: Well even in the code calculations, always parameters are there. It's very easy to generate this information.

CHAIRMAN WALLIS: And if you show that you've been as curious as we have been curious.

(Whereupon, at 4:59 p.m. off the record

until 5:01 p.m. for the evening session.)

CHAIRMAN WALLIS: This is a progress report, Rich? This isn't a finished product?

DESIGNATED OFFICIAL CARUSO: I'll give it to you.

MR. TSCHILTZ: Good afternoon. My name is Mike Tschiltz. I'm the Deputy Director of Risk Assessment in NRR. We're here today to discuss our plans to revise Regulatory Guide 1.82. The planned revisions relate to the topic of net positive suction head for the ECCS in containment heat removal pumps.

We have previously discussed revising Reg. Guide 1.82 Revision 3 with the Thermal-Hydraulics Phenomena Subcommittee on July 10, 2005 and with the full Committee on September 8,2005.

Dr. Sharon, the Associate Director for Engineering and Safety System in NRR has discussed our plans our rationale for risk-informing that positive suction head regulatory guidance with the ACRS on October 7, 2005.

The ACRS provided feedback in a letter, dated September 20. 2005. The letter recommended that the proposed Revision 4 of Reg. Guide 1.82 not be issued for public comment and should be revised to improve clarity and reflect other recommendations in the letter.

ACRS also provided feedback on the topic of credit in containment accident pressure for that positive suction head in January 4, 2006 letter on the Vermont Yankee extended power uprate request.

Today we'll share some our preliminary thoughts on the changes to address the issues you've raised in past discussions on the topic and our proposed schedule.

I will also note that a representative from GE is here to discuss their plans with respect to this subject.

I'm very thankful for the level of effort that the ACRS has devoted to obtaining an in depth understanding of the analysis and reviews performed by licensees and the Staff in contemplating allowing the credit for containment over pressure for ECCS and containment heat removal pump net positive suction head. I understand that the ACRS faces a significant challenge in capturing and communicating its concerns to the Staff in a manner that allows the Staff to make its determination on a safety basis. And by that I mean presenting concerns to the Staff in a manner that allows the Staff to focus on plant parameters of particular concern rather than plant design or subjective terms that are subject to interpretation.

With that, I will turn it over to Rich Lobel.

MR. LOBEL: Do I change the slides for this.

MR. STUTZKE: Yes.

MR. LOBEL: Up arrow and down arrow? Okay.

Good afternoon. My name is Richard Lobel. I'm a senior reactor systems engineer in the Office of Nuclear Reactor Regulation, NRR. Seated also at the table is Marty Stutzke who is a senior reliability and risk analyst, also in NRR.

Okay. As Mr. Tschiltz said, we're here today to discuss our preliminary plans to revise Reg. Guide 1.82 Revision 3, which we'll consider feedback from ACRS and NRR management. And we're really here to get your comments and try to incorporate your guidance into what we're planning to do.

I realize we're not going to give you a lot of detail today, but we'll be back again with the details. And we can have more discussion then. But we're trying to give you an idea of where we are right now.

The September 20th ACRS letter recommended that licensees should demonstrate that there's no practical alternative to crediting containment accident pressure and that credit should be granted only for robust containments for which there is a positive means for indication of containment integrity inerting or subatmospheric. And at that the time interval should be limited to a few hours.

The January 6, 2005 ACRS letter on Vermont Yankee EPU also contained some recommendations on this topic, especially on the development of a statistical approach that would quantify the uncertainty. And we're going to talk about that in a little more detail today.

Okay. Related documents. We've talked about this before. That there are some documents that reference Reg. Guide 1.82 or are connected with it.

Reg. Guide 1.1 will be revised since it's the licensing basis for some licensees. It'll be revised to just reference Reg. Guide 1.82 for any future work.

Standard Review Plan Section 6.2.2 is containment heat removal, and it has the SRP discussions of NPSH. And it will be revised also to reference the Reg. Guide. And likewise, with the NRR Review Standard for Extended Power Uprates.

This is just the list of some of the current applications that deal, among other things, with containment overpressure. We've talked to you before about Vermont Yankee extended power uprate, Browns Ferry, Units 2 and 3 already credit some amount of containment overpressure. And Unit 1 is requesting a like amount. Beaver Valley 1 has several applications in house right now that are being reviewed. Beaver Valley 1 and 2 are converting from subatmospheric containments to large dry containments and they're also requesting an extended power uprate concurrently. And Beaver Valley 1 currently and after the containment conversion in the EPU will also will need credit for containment accident pressure for NPSH. Unit 2 will not, and that's due mostly to a difference in design between the two units. I guess the licensee learned something from Unit 1 when they designed Unit 2.

ACTING CHAIRMAN RANSOM: I guess in the revision Vermont Yankee EPU will now fall within the Reg. Guide?

MR. LOBEL: Yes. Yes. I'll talk about that a little bit more.

ACTING CHAIRMAN RANSOM: So it's precedent setting, I guess, in some ways, isn't it.

MR. LOBEL: It was precedent setting in several ways. It was precedent setting certainly for the amount of attention it got and the detail to this subject. We spent a lot of time and the licensee did a lot of work trying to demonstrate the amount of conservatism that was in the calculation that we hadn't really appreciated before. And I think that lead to the idea of doing the statistical approach to get out of, I'll it a box. To get out of a situation that we got in artificially. A plant didn't really need the overpressure, but their calculations were conservative enough that they ended up in that situation.

CHAIRMAN WALLIS: I think we all have egg on our faces a bit because the previous EPUs, neither the Staff nor the ACRS paid much attention to. And the public came up and the state of Vermont came up and started asking these questions, and that's what stimulated all this hard work that you've just been talking about. It wasn't something that happened as a result of the Staff or the ACRS saying we got to look at this. It's a little bit surprising.

MR. LOBEL: In a little defense of the Staff, you're correct. But in a little defense of the Staff, what we focused on was the analysis that it was a conservative analysis, but we really didn't appreciate -- we didn't go into detail about the amount of conservatism. And Vermont Yankee raised the question of why are we in this situation. And we tried to elicit some information from the licensee, and we got quite a bit that quantified the degree of conservatism. We had not done that before.

DR. BANERJEE: Well, the one area they want is was the degree blockage. It was very nonconservative.

MR. LOBEL: Well, no. I don't know that --

DR. BANERJEE: They used a completely wrong approach velocity.

CHAIRMAN WALLIS: Well, they were conservative in some ways and not in others.

DR. BANERJEE: Yes.

CHAIRMAN WALLIS: And they used more crude or more mud or something than they actually had, but then they used the wrong --

MR. LOBEL: Yes.

CHAIRMAN WALLIS: So it was a bit hard to tell whether they were conservative or not.

DR. BANERJEE: And they ignored the paint chips as well.

CHAIRMAN WALLIS: Yes.

ACTING CHAIRMAN RANSOM: Will the revisions address some of the added comments that were in that letter that ACRS wrote on Vermont Yankee?

MR. LOBEL: The revision will address some and we'll certainly address them all with you when we present the Reg. Guide.

ACTING CHAIRMAN RANSOM: But I mean in terms of the method that would be required in the future.

MR. LOBEL: Well, that's something we still need to talk about and it would be good to get your feedback. But normally reg. guides aren't to the level of a recipe where they specify first you do this, and then you do this and then you do this. As it's written now, it tells licensees and the Staff here are the things to consider. Here are all the water sources that you should consider. Here's all the things that can effect the blockage and the drop in head across the screens, and here's the things to consider in calculating the pressure. But it doesn't put it altogether in a recipe of just how to do the calculation.

CHAIRMAN WALLIS: It was actually that Reg. Guide 1.82 is one of the guides that goes the furthest in the direction of not giving much help about how to do it.

MR. LOBEL: Well, we're going to try to do more of that and --

CHAIRMAN WALLIS: What's this overall? Do you think that by putting in these conservatisms or putting in these uncertainty estimates that you can move more to satisfy what the ACRS was asking for, which was only a little bit of overpressure for a short period of time? Are you going to ask them to show that, you know, although with a very conservative analysis you're going to say you need overpressure for three days? In fact, if you actually do the uncertainty analysis, the probability of needing it for more than two hours is very low. Isn't that the kind of thing you're looking for? So that this business of being so conservative as it looks as if you need a lot of overpressure for three days, and that absurdity is going to go away and you're going to say well realistically with uncertainty the probability of needing this overpressure is really very small and it's only for a very short time. Isn't that the way you're aiming to go?

MR. LOBEL: Well, that would be the goal of the statistical approach that we'll try.

CHAIRMAN WALLIS: And then you'll come

closer to what the ACRS was asking for which was, you know, small amounts for a short period of time and so on.

DR. BANERJEE: Sort of like a best estimate with uncertainties.

MR. LOBEL: That approach -- the representative from GE here is going to talk about some and I'm going to say some more about that. But that would be, hopefully, the approach that would get some plants out of demonstrating that they need overpressure because of too much conservatism. And that approach would be able to define the degree of conservatism. Not for all variables. It would follow, hopefully, pretty close to the guidance that's in the best estimate LOCA reg. guide in terms of recognizing that not every variable can be treated as a best estimated with an uncertainty. There were some things where the bounding approach still has to be used. But then you would have to follow the guidance of that reg. guide which says the conservatism can't mask phenomena and it can't lead to unreasonable results.

So, yes, some of that will be put into the reg. guide. We'll try to define the statistical approach some more. And I'll talk about that a little bit more.

DR. BANERJEE: So I suppose in Vermont Yankee the issue really was that you have to have a single failure as well as something like RHR train knocked out, as well as containment failure. So you wouldn't need the overpressure unless you had one RHR train out of action. So a single --

MR. LOBEL: Well, that was the conclusion for Vermont Yankee. Let me just say that not every reactor out there is going to be able to do what Vermont Yankee did and say it's all on the fault of conservatism and if I just didn't have so much conservatism, I wouldn't need overpressure. There are some licensees that are telling us that even realistically they need some credit because of the design of the plant, because of the way the plant is laid out.

So like I was trying to say when we were talking about Vermont Yankee to keep that discussion just in terms of Vermont Yankee and don't generalize it too much, because these other cases won't be exactly the same and we're going to have to come to you for some of those cases, too, and discuss those.

CHAIRMAN WALLIS: Now when you have these probabilistic statistical methods, presumably they ought to go into a realistic type PRA. And the problem I see with a PRA is PRA as it's constructed today doesn't really contain these phenomena in it. So you have to somehow do something very artificial to do a risk-informed type of --

MR. LOBEL: Which is what Vermont Yankee did and what --

CHAIRMAN WALLIS: Whereas really if you have statistical measures of the probability of ever needing this and the success of NPS -- having NPSH and so on, that could perhaps go right into a PRA itself. And then it might show that the risk contribution is very small. And it's only a small fraction of the total risk.

MR. LOBEL: Well, I think what we did with Vermont Yankee was we artificially biased the PRAs to show that there was little effect --

CHAIRMAN WALLIS: But that gave a risk which was bigger than the total risk of the plant. It doesn't make any sense.

MR. LOBEL: If we adjust -- done a realistic calculation, it never would have showed up as a consideration.

CHAIRMAN WALLIS: I understand what you did there. But it would be nice to have that PRA itself make use of this better thermal-hydraulic analysis that you're going to put into your statistics. Rather than taking some extreme bounding value of something.

DR. BANERJEE: Which plants are likely to need this credit even with a best estimate uncertainty sort of --

MR. LOBEL: I believe Beaver Valley 1 will still need the credit because of the relationship of the sump to the location of the pump. Even with realistic calculation I've been told that they'll still need some credit for that.

And the reason is that it goes to the basis design of the plant. A subatmospheric containment was designed, you know, it has the criterion that after a LOCA they have to be back to subatmospheric conditions in an hour. So what they do is they design spray systems that put a lot of water into the containment atmosphere in a very short time. The crunch spray system takes suction from the RWST, but they have an inside and an outside recirculation system that take water from the sump in a very short time after the accident starts. So in that case there isn't very much water on the floor. And so they have always since they started operating have taken credit. And the other subatmospherics have also due to --

CHAIRMAN WALLIS: Do they need the containment pressure to get the pumps to work to produce flow into the containment to reduce the --

MR. LOBEL: Right. To release the pressure, right. And so the rule is in the SRP, not the rule. But the guidance in the SRP is that they can only take credit for containment accident pressure during injection. During recirculation they don't. That's when there's enough water on the floor already. The thing is with the subatmospherics it a little blur between recirculation and injection because they're doing both at the same time with different pumps.

DR. BANERJEE: So they would need it over what period of time?

MR. LOBEL: They take credit for it during the injection phase.

DR. BANERJEE: So that would be the first--

MR. LOBEL: From the beginning of the accident --

DR. BANERJEE: Yes.

MR. LOBEL: -- until --

DR. BANERJEE: Thirty minutes?

MR. LOBEL: Roughly, I guess, when they use up the water in the RWST.

DR. BANERJEE: Which is what? Half of an

hour or what?

MR. LOBEL: Roughly, maybe a little less for that.

DR. BANERJEE: And the pressure would go how much higher than atmospheric?

MR. LOBEL: I don't have the number off the top of my head.

DR. BANERJEE: So they need a substantial credit or little credit?

MR. LOBEL: Their design pressure -- yes, I'm sorry. I do have numbers. But their design pressure is about -- I believe it's around 45 psig and with the containment conversion they're very close to the 45.

DESIGNATED OFFICIAL CARUSO: We're scheduled to have the opportunity to discuss this in detail in April.

MR. TSCHILTZ: I just wanted to point out that the main purpose of this slide was to make the Committee aware that the Staff would be coming back before then with EPUs before the regulatory guidance is revised.

MR. LOBEL: Yes, we probably should --

CHAIRMAN WALLIS: One thing that bothered me I know, I don't know about the rest, but was the idea that there are no practical alternatives. In Vermont Yankee, for example, there was never any discussion of that, any meaningful discussion.

MR. LOBEL: Well, the answer to -- one of the members asked them a question and they answered the question. But --

CHAIRMAN WALLIS: Well I asked it in private and they said it would take $20 million to put new pumps in. And actually when they talked about I think the revenue from this change, it didn't seem like it was all that great an amount.

MR. TSCHILTZ: The issue with practical alternatives I think that we get into is it's a rather subjective issue. And the Staff needs to make its decision based upon a safety case. And when you enter into that consideration, other practical alternatives, we have a very difficult time dealing with it.

ACTING CHAIRMAN RANSOM: I admit it's a weak argument to say that when there are no practical alternatives, you know, obviously they're going to take the easiest way out.

MEMBER KRESS: I think I agree with you. NRC should just look at the safety case and let them propose and you dispose.

ACTING CHAIRMAN RANSOM: Yes.

DR. BANERJEE: Yes, the problem, though, there is that clearly they are violating something otherwise we wouldn't be discussing it here, right? So they want something that requires a special dispensation. So then it's a trade-off between getting that and --

DESIGNATED OFFICIAL CARUSO: Well, in these cases it's not a matter of them violating anything. They want something.

DR. BANERJEE: Yes.

DESIGNATED OFFICIAL CARUSO: They want to change their licensing basis so they can make more power.

DR. BANERJEE: Right.

DESIGNATED OFFICIAL CARUSO: As the plant sits, it's acceptable because the Staff allows it to operate.

DR. BANERJEE: Well, sure. That's agreed. But now they want to make more power. And it's a question of how much they pay to do it, right?

ACTING CHAIRMAN RANSOM: Well, but that's not our question.

CHAIRMAN WALLIS: So in all practical terms is a bad term.

MEMBER KRESS: Yes. It really is.

MR. LOBEL: And it has no longer -- I'm sorry.

ACTING CHAIRMAN RANSOM: Well, you may as well not have that term in the reg. guide because --

MEMBER KRESS: Well, I think that's a good proposal.

MR. LOBEL: And as I'm going to say, it isn't going to be in the revision.

One of the comments that we've gotten was that the wording wasn't clear. And, hopefully, we can simplify the wording and not try to address too many things in one position. And so whether we agree or disagree, at least you'll be able to understand what it is we're trying to say.

Okay. One of the attempts to clarify things a little is this reg. guide really addresses a lot of different issues. And it's kind of thick. And what we would like to do is have a very a brief amount of body of the report and put all the different subjects into appendices. So we're not going to change the wording of these other areas. Because right now we're just talking about revising the reg. guide in terms of NPSH. But we want to try to make it easier for people to use while we're in the process of revising this thing. And the appendices that I'm talking about, these are all subjects that are already in the reg. guide. We're not adding any new subjects.

DR. BANERJEE: But they all affect in some way NPSH?

MR. LOBEL: They're all related to pump suction issues.

DR. BANERJEE: Right.

MR. LOBEL: Yes. NPSH or acceptable behavior of the pump.

Okay. Like I was just saying, we're going to delete the words about NRP practicably altered. We're going to use the position that we presented to you several times now about the approach will be acceptable if it's acceptably conservative, and the reg. guide will specify that to some extent. And an acceptable risk evaluation.

DR. BANERJEE: But you also specify the methodology or what would be an acceptable assessment?

MR. LOBEL: For the risk or for the --

DR. BANERJEE: You said "acceptable risk evaluation," right?

MR. LOBEL: Right.

DR. BANERJEE: Now --

MR. LOBEL: And Marty's going to talk about that. But, yes, that will be specified in the reg. guide. Now that will be added to the reg. guide. That will be the big new edition.

We're considering now that there doesn't need to be a limitation on the time pressure as credited since the argument goes that the most likely containment failure modes are that either a containment has already a failure and an opening somewhere or else that the other high contributor to containment integrity is loss of isolation or isolation failure. And those things occur immediately. So the big concerns are right at the beginning.

CHAIRMAN WALLIS: So you're not going to take seriously the ACRS statement that it should only be allowed for a short time? You're going to allow for as long as they want it?

MR. LOBEL: Well, as long as it's necessary. But hopefully going to this other approach, the statistical approach, the times will be demonstrably shorter.

CHAIRMAN WALLIS: Well, why should they do it if they're going to be allowed it for an indefinite time anyway?

MR. LOBEL: Well --

CHAIRMAN WALLIS: By just being conservative?

DR. BANERJEE: You have to show the risk is small, right?

MR. LOBEL: It's another thing that's hard to define. I mean, how do you say what's short. If you say -- we have this problem in --

CHAIRMAN WALLIS: What if you said a few hours?

MR. LOBEL: You say two hours and then somebody comes in, but I only need 2 hours and five minutes or I only need 2 hours and 15 minutes.

CHAIRMAN WALLIS: Well, except when they need 40 hours. That's different from 2 hours.

MR. LOBEL: You know, I guess the 40 -- the 56 hours for Vermont Yankee never bothered me too much because I always thought it was in the conservatism in the calculation --

CHAIRMAN WALLIS: Well, I brought it up because we've written a letter saying 2 hours.

MR. DENNIG: Excuse me, Rich. Rich, this is Bob Dennig. I'm Chief of the Containment and Ventilation Branch and work with Rich.

In the spirit of what Rich said at the beginning of his discussion, we're going to be coming back to you and in the spirit of getting your input, your guidance, your thoughts. And so what he's telling you is that you've given us sort of this challenge of how to deal with this time. And we're starting out from the hypothesis that we don't have to put limitation on the time. And we're going to find out in further meetings whether or not we do or do not convince you that you agree with that position or we find something about that that allows us to change our position.

CHAIRMAN WALLIS: So, for instance, when you look at this time you're going to say during this period of 56 hours what's the probability that some seals will fail on the containment or that something will happen to prevent the pressure being there? Is that the kind of thing you have in mind?

MR. LOBEL: That would be part of the likely --

CHAIRMAN WALLIS: Because the time has some consequences.

MR. LOBEL: Right.

CHAIRMAN WALLIS: I mean, you'll look at the risk of longer times or something?

MR. LOBEL: That's right. But there's other considerations, too, that is going back to the conservatism. I think in the case of Vermont Yankee they were assuming that the pumps were at ground flooded for the whole time of the accident. And by the time you get out to 56 hours they didn't need much pressure. And just altering that one assumption to a more realistic calculation, more realistic assumption of the operator taking some action, the minimum NPSH was somewhere around 6 to 8 hours. And the idea that the operator couldn't do anything to control the pump for 6 to 8 hours that it would still be a runout condition is another large conservatism. And probably the pump has a considerable effect on a NPSH in two ways: In terms of the losses and in terms of the required NPSH goes down substantially with lower --

CHAIRMAN WALLIS: But you have no limitation on time and you think you're having no limitation on pressure as long as it's below a conservative recalculated value or something?

MR. LOBEL: That's the idea that --

CHAIRMAN WALLIS: You're going to relax everything very much.

MR. LOBEL: Well, the way things were done before was you would have this minimum pressure and then the licensee would say well I don't need all of that. You calculate 10 psi. "I don't need all the 10 psi, I only need 5." And we fussed a lot about the 5. Well, you can have 5, but you can't have more than

5.2 because you can have a little margin. That didn't seem to make a lot of sense either.

So we're trying to come up with criteria and a way of doing this that's more reasonable and really makes more sense.

MR. DENNIG: Well, clearly, we're going to have to take this on. And in the brief time we've been talking about it, I've heard 3 or 4 different ideas about how one could interpret or address that particular parameter. So the process of getting some feedback and getting some ideas how to deal with this has already begun.

MR. LOBEL: Let me move a little faster through some of this.

CHAIRMAN WALLIS: Well, why never discuss estimate calculation if you're allowed an indefinite time and pressure up to the conservative value? You only use your statistical approach if you wanted more than the conservative pressure, wouldn't you, which would --

MR. LOBEL: Well, but the goal of the statistical approach was to approach this in a way that we wouldn't be in the position of defending something that didn't need to be defended.

CHAIRMAN WALLIS: That was part of the

goal of our early comments was to say, look, if you can show that you don't need this anyway, then that's a really conclusive argument.

MR. TSCHILTZ: I think the reality of the situation is that given the alternative, licensees will choose to do the statistical approach which will cause them not, in most cases, to have to credit --

CHAIRMAN WALLIS: And that would then caution -- the critics are saying, look, we don't like it for a long time and all that stuff.

MR. LOBEL: Right. Right.

CHAIRMAN WALLIS: And then there's some people who don't want to give up defense-in-depth under any circumstances no matter what the risk arguments may be. Well then you can then use the statistical approach to show that there's almost very little probability you'll ever need this pressure and so on, and that would help to convince them that you're not really giving up defense-in-depth. So that was, I think, part of our hope. And it doesn't seem to be the thrust of what you're telling us.

MR. LOBEL: Well, I haven't gotten to the statistical part yet.

CHAIRMAN WALLIS: I just wondered about that.

MR. DENNIG: I think part of what Rich is saying is that we have a difficult time dictating any particular specific approach as long as we can make a safety decision. And it's our hope that the licensee will take the most effective and efficient approach to make that safety case and, hopefully, it aligns with the best estimate with uncertainties approach than with some value approach. But we have a hard time, again, saying you have to do it this way. That's why we call the reg. guides and not reg. requirements.

So I think he was just trying to be very honest about not being about to tell a licensee particularly how to do something.

MR. LOBEL: This approach is going to a little while to implement, too. And we'll talk about that a little bit in one case a little later. But there are licensees that have analyses already in the pipeline coming in and they can't go back and in the time frame that they need to get their licensing approved, they can't go to a completely new method of analysis. But, hopefully, when the reg. guide is out or even before the reg. guide out some licensees will start taking this new approach.

Let me just finish.

CHAIRMAN WALLIS: My impression is you're going to be just as lenient as you were in the past.

MR. LOBEL: Well --

MR. TSCHILTZ: I think the impetus is to go towards the new calculation method. And once that topical report is issued that provides that, I think it's going to benefit the licensees. I think they will see it as a benefit.

CHAIRMAN WALLIS: Well, what's the benefit though?

MR. TSCHILTZ: Well because it will be a lot easier for them to gain Staff approval.

CHAIRMAN WALLIS: Well, if they can get Staff approval already simply saying that you've granted it to Vermont Yankee for 56 hours and for so many psi, give it to us, too --

MR. TSCHILTZ: It was not easy, sir. It was a difficult process for both us and the licensee. So I --

MR. LOBEL: Yes, it was mutual.

MR. TSCHILTZ: There are gains to be had there, I think.

MR. DENNIG: And the picture that you just described is usually the one that's held in the mind of the licensing managers someplace. That if you give it to X, so give it to us. And it never really ever quite works that way. There's all these little devilish details. And licensees pick up on how they can provide those details up front and get through the process instead of playing 20 questions every couple of months. And that's just the licensing process.

So, again, that's the idea is that folks will see that indeed not only do you get a better view of what's going on, a better answer, not only do you not paint yourself into corners but this does provide a more efficient way to have Staff perform it's review. They come back to us with fewer RAIs. And our cost go down for the license.

MR. LOBEL: Okay. We're also thinking of changing a position that had to do with a credit for pumps operating in cavitation. And it was something that we reviewed and approved for Vermont Yankee where they had some data from their pumps, but their pump vendor also used some data from similar pumps that were identical to the Vermont Yankee pumps and the parameters that affected NPSH; specific speed, suction specific speed, blade inlet angle and things like that that affect NPSH at the pump.

The pump vendor used data from the pumps of similar design but where those parameters were identical. And we're considering maybe changing the position from the position now, which is that you have to use an identical pump and you have to run that pump for the amount of time -- at least the amount of time you were credited for in your accident analysis.

ACTING CHAIRMAN RANSOM: Now, would this change the definition of the NPSH required --

MR. LOBEL: Yes. That's what --

ACTING CHAIRMAN RANSOM: In other words, you could operate beyond the 3 percent drop in --

MR. LOBEL: Yes. They have a slightly larger head drop and that was compensated for by the pump vendor limiting the amount of time that they could operate with that reduced required NPSH.

ACTING CHAIRMAN RANSOM: Well, do you require data or experience with that kind of pump?

MR. LOBEL: It was data. It was data.

Okay. Oh, the last thing is the most important. Like we've been talking about we'll provide more detailed guidance on the statistical approach. And we've been having some very preliminary discussions with GE about them preparing a topical report that would describe a method that would go through this method and actually define the criteria and the distributions for the different parameters, decide which parameters are the significant ones to consider. And you're going to hear more about that when I get done.

Now we're in risk-informed. Marty Stutzke will address the risk-informed aspects. And then we're prepared -- General Electric is prepared to give you their presentation on their thoughts so far on the statistical approach.

MR. STUTZKE: Hi. Marty Stutzke from NRR Division of Risk Assessment.

I want to talk to you about the risk-informed guidelines we intend to put in Reg. Guide 1.82.

Before talking about the technical details of how that risk assessment should or should not be done, I've tried to lay out the regulatory thinking as to why we want to go down this pathway.

I will remind you that risk-informed license amendment requests are voluntary. We can't demand a license risk-informed license amendment request unless we have belief that adequate protection is questionable. That guidance is in Standard Review Plan 19 Appendix D. Basically the burden is upon the Staff to demonstrate that special circumstances may exist that rebut a presumption of adequate protection. And these are typed examples of special circumstances that I think are particularly appropriate for the containment accident pressure credit.

Now the way Appendix D leads me is when I reach this decision, I have to convince my management that such circumstances exist. We can then request the licensees to provide information. If they don't provide it, we can elevate it to higher levels of management, all the way up to the Commission like this. So what we're trying to do in the revision of Reg. Guide 1.82 is very -- in my mind it's similar to what we've done on the extended power uprate where we've already made a finding that EPU, in this case, containment overpressure credit does question or raise special circumstances. And so from that we can request the information up front rather than going through the process here.

That's kind of the regulatory, I guess, perspective from them.

Changing to the next slide, I'll remind that you've written a letter back in the end of the 1997 that said decisions to grant overpressure credit should be risk-informed and consider a broad range of accident sequences. And we intend to do that as was done at Vermont Yankee.

The other thing is that risk informing

the overpressure credit we need to be consistent with various NRR office instructions. LIC-101 is our procedure for reviewing license amendment requests. And there's guidance in there basically is abstracted from the various risk-informed reg. guides like 1.174, the SPR 19, Appendix D that tell us the process by which we view these things.

A more recent NRR for this instruction is LIC-504. It talks about risk-informed decision making for emergent issues. And the reason why I mention that is that it talks about when you reach an impasse in the implementation of Reg. Guide 1.74 we're supposed to use an integrated decision making process. We're not risk-based, we're risk-informed. We have to consider other aspects. And when we have trouble reaching that decision, LIC-504 tells us what we should do.

So going to the next slide, once we've reached a conclusion that the special circumstances may exist, Appendix D of SRP 19 refers us directly to Reg. Guide 1.74. Specifically it says evaluate the credit against the five key principles of risk-informed decision making. That's what we did at Vermont Yankee,

Okay. Some of those key principles are

risk-informed; in other words we actually calculate a change in core damage frequency or LERF. Others have been more deterministic; the consideration of defense-in-depth, adequate safety margins, compliance with regulation like this.

What I want to point out, and in fact to ensure you based on some discussion we've had with the ACRS in the past, there's a distinction between the concept of adequate protection and risk numbers. Just a meeting numerical risk acceptance guidelines doesn't mean you have adequate protection. There are other features that need to be considered like this.

It was stated before by one member of the Committee that the PRA argument always trumps the defense-in-depth argument. We don't agree with that. We've never operated that way. The risk assessment is not a trump card, but it may be the ace in the hole, if you want to look at it that way.

So given that, we will devise appropriate guidance in Reg. Guide 1.82 that will refer to this SRP chapter 10 and perhaps onward to Reg. Guide 1.174 like that.

MEMBER KRESS: When you did the Vermont Yankee thing, you did not use LERF, you used LRF. I presume you intend to continue in that direction? If you went to 1.174, you wouldn't find that anywhere. That bothers me because this is a late --

MR. STUTZKE: Well, it's true that large release frequency is not one of our current risk matrix. Large early release frequency is directly in 174. At Vermont Yankee I looked at conditional containment failure probability.

MEMBER KRESS: Yes, that's right. Yes.

MR. STUTZKE: And the reason is that gave me some measure of much defense-in-depth was being changed.

MEMBER KRESS: Yes.

MR. STUTZKE: This notion of balance between accident function and --

MEMBER KRESS: And I would hope you would continue along that same line, even though --

MR. STUTZKE: Well, that's --

MEMBER KRESS: Something has to be said in the guide that somewhere --

MR. STUTZKE: That's right. That's right. That's the sort of detail that I may add. And, of course, it's an uphill battle. I have to convince other people of the Staff. I personally --

MR. TSCHILTZ: It's a policy issue of the Commission.

MEMBER KRESS: Certainly. I understand.

MR. TSCHILTZ: You would have to go back to the Commission and --

MEMBER KRESS: I understand. But it's the right thing to do. And you'd probably get a lot of support from the ACRS.

MR. TSCHILTZ: We're having discussions with Mr. -- they contain the failure next week in a meeting.

MEMBER KRESS: Okay.

MR. TSCHILTZ: We are discussing it.

MR. STUTZKE: Rest assured, I won't write a one liner that says go look at 1.174 and do all that. We will have to amplify that.

That's about all I have to say.

MR. LOBEL: I'd just like to add the schedule. We had -- it's in the handout.

This is a tentative schedule, but hopefully we'll be able to meet. To come back to you-- well, to the full Committee in June. Because of the amount of changes from the previous version of the reg. guide, although we did a waiver the first time around adding the risk by component and then statistical approach, we need to consider whether we need to go to CRGR or, as we always, as ACRS.

Then we'll put out the draft for comment and roughly a year later issue the final version. Okay.

And now I'd like to just have a representative from GE discuss the statistical approach in a little more detail. And I don't think you have handouts for that, do you?

CHAIRMAN WALLIS: No.

ACTING CHAIRMAN RANSOM: This is also discussion of Reg. Guide 1.82?

MR. TSCHILTZ: This is in support of the Staff's effort on the statistical approach.

MR. LOBEL: This is Richard Lobel again from the from the Staff. We've discussed this situation with GE some because a lot of the plants that -- at this point a majority of the plants that we're taking for containment pressure are Mark 1 BWRs. And we've had some preliminary discussions and GE has offered to try to produce a topical report that would go through the method and define the method in a way that at least would be applicable to BWRs. So it won't be done in time to reference in the draft version of the reg. guide, but hopefully by having continued discussions between GE and ourselves, a lot of what they find out in the process of developing the topical report will help us write the reg. guide.

DR. BANERJEE: Do we have a copy of this? So you're anonymous.

MR. QUINTANA: I apologize for that. My name is Lou Quintana. I'm the licensing manager at GE Nuclear Energy. And in discussions with the staff, Rich in particular, he asked if we could make a small presentation on essentially a joint effort at working on a statistical uncertainty based approach for, in this case, limited to BWRs, special temperature calculations which are the predominant inputs from the GE perspective of our portion of the analysis on the NPSH calculations.

So this will be a, as you'll see obviously, a very high level discussion because we are, again, in the process of starting this detailed methodology development, to call it that, working with Staff and ultimately intending to be consistent with the changes to Reg. Guide 1.82 are those are developed.

The main goals, obviously, are to better define the uncertainties and the degree of conservatism. Certainly in the VY experience that was very important to the ACRS as well as to the other stakeholders. And so that's the goal.

The derivative result is that ultimately the NPSH calculation basis will be clear.

CHAIRMAN WALLIS: This will include everything, including for instance the pressure drop across screens and that sort of thing as well?

MR. QUINTANA: I'll discuss that briefly.

CHAIRMAN WALLIS: That's part of --

MR. QUINTANA: We're trying to determine what ultimately the key parameters will be. And you'd all, obviously, voiced an input on what some of those are. Ultimately it will be a balance between what can be done with significant enough data to justify the variation in that parameter or whether it just be able -- or be only forced to pick a conservative number that we all agree is conservative, which is where we are today. But the theory here is that for as many parameters as we can, and certainly the ones that are key, we try to develop --

CHAIRMAN WALLIS: Will you do something about the uncertainties in the debris, for instance?

MR. QUINTANA: Dr. Wallis, I can't answer that one right now. I don't have an answer to that one. I think that's a developing situation in particular in BWR space. Obviously, we talked about it for Yankee. But I couldn't tell you -- I'm not even sure -- well, maybe the ACRS could tell us what uncertainties you think we should use. But I don't know that those are established. So --

MR. LOBEL: Could I just make a comment.

MR. QUINTANA: Certainly.

MR. LOBEL: I think as far as debris goes, the whole program to design the suction strainers for the BWRs and quantify all those things was done on the basis of trying to bound. And to try to derive distributions from the tests that were done is probably not possible. And so that would probably have to fall into the category that's in the best estimate LOCA reg. guide of you're allowed to keep something conservative as long as it doesn't mask the behavior of other variables or bias the final result. And I just know how it would be possible to derive a meaningful distribution from that. We could make an assumption. GE could make an assumption. But I'm not sure that we could ever defend it very well.

DR. BANERJEE: So these are what pressure requirements are usually for Mark 1s, right?

MR. LOBEL: Yes. BWRs Mark 2s and Mark 3 containments don't need containment over pressure. Just the Mark 1s.

DR. BANERJEE: And they have this, the

same as --

MR. QUINTANA: It's the Torus wet well.

DR. BANERJEE: So they would come up with solutions somewhat similar in terms of strainers and things like that?

MR. LOBEL: Well, all the Mark 1s have installed final design strainers in response to a bulletin from 1996. Bulletin 9603. And most of that work was done before 1999 or in 1999.

DR. BANERJEE: But do they look a lot like the Vermont Yankee stack screens or --

MR. LOBEL: That was one design.

DR. BANERJEE: Okay.

CHAIRMAN WALLIS: And one of our problems was that was they used tests on single disks to predict the performance of stack disks. And that seemed to be inappropriate. So we I think sort of stuck an uncertainty factor on that, a factor of 10 or something, or some of us did in our thinking.

MR. QUINTANA: Right. That was discussed in the --

CHAIRMAN WALLIS: Are you going to go into that sort of thing?

MR. LOBEL: Well, hopefully not. But if there really is a question about the accuracy of the design --

CHAIRMAN WALLIS: Well, the model --

MR. LOBEL: -- I guess we'll have to go back and address it.

MR. DENNIG: Rich, I think if we -- not to jump too far, but jump to the second page and go to the last bullet 7. It says "Utilizing downstream NPSH evaluations." Our discussions so far have been along the lines well what can we generalize from the major vendor and what they can provide that would be a general tool that everybody that's everybody got a similar design containment could use. And then there's always going to be a plant specific portion to this.

And so, please, let's just see if we can get something we can plug in where we can now take on a plant specific issue and see if we can continue with the same kind of methodology or we default to some bounding approach. But all GE can do in their topical way is to take on what can be genderized in a reasonable fashion.

MR. QUINTANA: Right. And maybe to clarify--

CHAIRMAN WALLIS: Well, maybe the representative lead plant should be Vermont Yankee since we know such a lot about it now.

MR. QUINTANA: I don't know that it'll be Yankee. Certainly there's a lot of information, as you said, Dr. Wallis, on Yankee. Ultimately we need to look at all the other key parameters and figure out where we have data where a model is already reflective of it and so forth. And that will be some work that we'll have to work through. But the obvious goal is to make it representative calculation. We're not trying to come up with a bounding calculation that would apply to everybody. We're trying to come up with an approach, a methodology of looking at the containment analysis parameters.

DR. BANERJEE: So you want to come up with SPT basically, right?

MR. QUINTANA: Yes. Or the effect of variation in parameters on SPT which ultimately then can be utilized in the downstream NPSH calculations.

DR. BANERJEE: But this depends on the flow rate, right, in some way to record? I mean, in the long term this --

MR. QUINTANA: Yes. I believe that's correct.

DR. BANERJEE: So how can you separate that from things like strainers?

MR. QUINTANA: Well, I'll go back to what

Rich said and he probably said it better than I can. But on some parameters we may need to say we're going to take a conservative number. Now we all have to then decide if that number is conservative, because we probably won't be able to come up with statistical distributions that, without test data to prove that they're close enough to reality --

DR. BANERJEE: So your approach would be a mixed approach to the SPT? Certain parameters where you have uncertainties and so forth would go in some normal distribution or abnormal distribution of whatever things are? And some you just wouldn't have a clue in this chain. And you'd just say I'm putting the bounding value.

MR. QUINTANA: I think that's a fair statement. It will be similar I think to the LOCA collocation, the best estimate LOCA where at some point we may -- we declare an over conservative by -- I'll use the word consensus. And then we look at more representative, a better estimate type numbers for all the others. And then provide statistical treatment of those to come up with a --

DR. BANERJEE: But isn't SPT just a part of a recirc and a LOCA calculation anyway? Can't you just use the methodology that you would use for LOCA?

MR. LOBEL: This is Rich Lobel from the Staff.

You have to realize how these calculations are done and who does the different parts. And what we're telling you is still in the really preliminary stages and all the planning hasn't been worked out, let alone the analytical methods yet. But General Electric or General Electric in general does the containment calculations. They calculate the containment conditions. And then typically a licensee may have another engineering organization or they themselves may do the rest of the calculation. They may take the temperature of the suppression pool of water and then take that water out of the Torus. And then after that it's a different calculation.

And one of the things we're still talking about is how far we're going to be able to go with this method in bringing other parties in so we can do not only the containment part, but also the rest of the downstream part that you're talking about.

DR. BANERJEE: Well, they're coupled to some extent.

MR. LOBEL: They're definitely coupled, yes. Yes. They're definitely coupled. And my wish is that we could do the whole thing. But we have to work with different organizations and try to put something together. And like I say, what we're telling you is very preliminary yet and all that plans haven't been done.

DR. BANERJEE: So let me get this sort of fairly clear in my mind. A utility might use some other organization to do LOCA type locations in the long term cooling.

MR. QUINTANA: Typically, those are done by GE. The LOCA containment and then the downstream part, in this case the NPSH, that typically is done by a licensee or --

DR. BANERJEE: But the NPSH depends on flow through the core and boiling int he core and all sorts of -- pressure loss through that circuit.

MR. LOBEL: Well, not in the core.

This is Rich Lobel from the Staff again.

DR. BANERJEE: Yes.

MR. LOBEL: It depends on what -- I'm sure that's part of it, but the temperature you're concerned when you do all that calculation is the temperature in the Torus.

DR. BANERJEE: Right. So you decouple that. Okay.

MR. LOBEL: So you've done the containment calculation, mass of energy into the containment. Some of it goes to the suppression pool. And you calculate the temperature in the suppression pool. That's one part of the calculation. And then the utility can either ask General Electric to do the rest of the calculation or they can ask somebody else, or they can do it themselves.

DR. BANERJEE: Right.

MR. LOBEL: But you're correct, it's all connected. That's organizational. Technically it's all --

DR. BANERJEE: Right. So it's loosely enough coupled that you can see another organization doing that, starting with the SPT temperature.

MR. LOBEL: Right.

DR. BANERJEE: Okay.

MR. LOBEL: Because each utility has it's own pumps and it's own piping designs --

CHAIRMAN WALLIS: Well, at Vermont Yankee the service water temperature came into this, though, didn't it? Was that because that was used to cool the suppression pool? Was that what it was?

MR. LOBEL: Yes. That is --

CHAIRMAN WALLIS: And that's going to be tremendously seasonal.

MR. LOBEL: That was the only source of --

CHAIRMAN WALLIS: I mean that was sort of 40 something for six months of the year.

MR. LOBEL: Right.

CHAIRMAN WALLIS: And then occasionally in the summer it might go up to 80. That must be plant specific. There's nothing generic about surface water temperature.

MR. LOBEL: Oh, absolutely. Yes.

ACTING CHAIRMAN RANSOM: That's a very key variable.

MR. QUINTANA: Right. And we touch on it briefly as one of the variations is liquid. And for them it made a, I'll say dramatic appearance of difference. But that may not be true for somebody in the south.

CHAIRMAN WALLIS: But GE isn't going to do that.

DR. BANERJEE: They may.

MR. QUINTANA: It would be a factor, because it is ultimately an input for us.

MR. LOBEL: It would be a factor and the suppression pool temperature would have to be considered because that's the only source of cooling that's taken credit for in the analysis is the RHR heat exchanger. There are other loss mechanisms, but they're not included in the analysis. Another conservatism.

DR. BANERJEE: I think I get the picture. Okay. Yes.

MR. QUINTANA: Okay. Without belaboring it too much, the goal is to quality those as best we can with data that exits. We would develop sets of inputs essentially for statistical analysis variations in those inputs. A somewhat traditional approach.

We would do the temperature calculations. At this point we're thinking it'll be a Mark 1.

And then when we determine the response and the variation, obviously, in that response with the variations in the inputs and come up with a statistical uncertainty on it's confidence level, whether that will be something that we actually but something we're considering.

And ultimately feed that into NPSH calculations and what we have to sort of work out with industry and the staff is how you do that. But the point here is that we would look at the way we do this pressure temperature calculations and the derivative NPSH in a statistical approach. And again try to come up with a methodology that can ultimately be referenceable and useable by licensees so that they don't have to have these super-conservative analysis that ultimately if we look at it in a more best estimate manner would not even need the NPSH. Yankee being one exception. But at least it would -- and also for other plants would minimize the time that even with conservatism that you would feel you needed it.

So that's the goal.

CHAIRMAN WALLIS: So it all looks pretty preliminary.

MR. QUINTANA: That's correct.

DR. BANERJEE: So if you use a containment code then you should get this as well as -- I mean, the early stages of LOCA you probably look at the --

MR. QUINTANA: At this point in discussions with the Staff we would use a containment code that's been reviewed and approved by the Staff on a number of different applications already.

DR. BANERJEE: So your LOCA code would have to also be a best estimate code of some sort in this case, right? I mean --

MR. QUINTANA: It typically is.

DR. BANERJEE: Yes.

MR. QUINTANA: At almost every plant that I can think of right now is a safe injester plant, which is a safe injester LOCA, which is a best estimate code. I think there might be a couple that are still safe reflood, but I can swear to that.

DR. BANERJEE: Safe injester.

MR. LOBEL: This is Rich Lobel from the Staff.

That just puts the mass and the energy into the containment and then the super hex code would be used to do the suppression pool temperature calculations.

MR. QUINTANA: We're not posing as part of this to change the input, the LOCA input.

DR. BANERJEE: And super hex is a best estimate code?

MR. LOBEL: I'm not sure.

MR. QUINTANA: I think it'll be closer to one after we -- the application of it will be closer to one.

MR. LOBEL: That's one of the questions that I have that we haven't talked about the general effort. That's something that needs to be discussed. Like I say, this is in the very early stages.

MR. QUINTANA: Ultimately if a best estimate code meets every parameter and every model and everything in it is best estimate, then I suspect that we won't be by that definition. But we'll be a closer to that than we are today.

ACTING CHAIRMAN RANSOM: Any further discussions.

CHAIRMAN WALLIS: Well, let's go back to Rich's presentation. Now I didn't quite see Rich's logic so these are the present problems with the present guide and these are my ideas for resolving them. This is how I will measure that I've succeeded in resolving the problems. I think you've got some ideas of what you might do. It wasn't clear to me why they resolve the problems, some of which are some incompatibility with some of the statements by ACRS, for instance.

MR. LOBEL: We really haven't sat down yet and thought thorough all this. I think we've all been busy with other things. And with the tentative schedule we've given you, we got to get started pretty fast. But we really haven't given this much thought since the discussions with Vermont Yankee.

MR. DENNIG: Very well. While Rich was working on Vermont Yankee in parallel we were feeling people out about how can we make progress on doing things differently or at least offering alternatives to people instead of doing things the same old way ad nauseam ad infinitum. So during that period of time the past year or so we've had feelers out for how we can leverage ideas about doing best estimate plus uncertainty in this particular area. And I think Rich and Marty have been very successful in getting GE's cooperation so far to even look at the idea and to invest some of their effort in the idea since what they do is going to effect a fairly large population.

So we're going as fast as we can. We wish we could go faster. We wish you had the answers. But we've heard the message. We think everything -- a lot of other things have gone in the best estimate and uncertainty directions. So it's time to get on with it.

ACTING CHAIRMAN RANSOM: Okay. Thank you, Rich, fellas.

MR. LOBEL: Thank you.

ACTING CHAIRMAN RANSOM: Do I have anything to do for the next one?

CHAIRMAN WALLIS: Well, you might have to write a letter on TRACG application --

ACTING CHAIRMAN RANSOM: You still want a letter --

CHAIRMAN WALLIS: No. I think it depends upon the schedule.

DR. BANERJEE: I don't think you're put that today --

CHAIRMAN WALLIS: You could write a letter which says more work needs to be done and here is some of the things we'd like to see.

DESIGNATED OFFICIAL CARUSO: Or you could do it in March.

CHAIRMAN WALLIS: Or we could just pull it into March. Well, I think Sam would be very upset.

DESIGNATED OFFICIAL CARUSO: They're not happy at all with March right now.

DR. BANERJEE: Who is not. Oh, you mean for the main Committee.

(Whereupon, at 6:11 p.m. off the record until 6:13 p.m.)

ACTING CHAIRMAN RANSOM: We're still on the record.

DR. BANERJEE: You know, my sense of it is they're going to get the approval. It's a question of making sure that we're happy --

ACTING CHAIRMAN RANSOM: Right.

MEMBER KRESS: I think we just need some more assurance.

DESIGNATED OFFICIAL CARUSO: I think it's a good idea that we're happy with the reactor, but we're not happy with the methodology. And we just need to express that better.

MEMBER KRESS: Well, I think TRACG looks pretty good. You know the PIRT is good.

DESIGNATED OFFICIAL CARUSO: But the explanation sucks.

MEMBER KRESS: Well, yes, the explanation sucked and we didn't see a good database for calibration of TRACG. But, you know, I think when we see it, we'll probably like it.

DR. BANERJEE: Yes. We like it --

ACTING CHAIRMAN RANSOM: Yes, we might as well go off the record.

We can go off the record at this point.

(Whereupon, at 6:14 p.m., the meeting was adjourned.)