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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON POWER UPRATES

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THURSDAY,

APRIL 27, 2006

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 8:30 a.m., Richard S. Denning, Chairman, presiding.

COMMITTEE MEMBERS:

RICHARD S. DENNING, Chairman

THOMAS S. KRESS, Member

OTTO L. MAYNARD, Member

JOHN D. SIEBER, Member

GRAHAM B. WALLIS, Member

ACRS/ACNW STAFF:

RALPH CARUSO, Designated Federal Official

NRC STAFF:

PATRICK D. MILANO, Division of Operating Reactor Licensing

SAMUEL MIRANDA, NRR

LEONARD W. WARD, NRR

PANELISTS:

JIM DUNNE, Constellation Energy

DAVID FINK, Westinghouse

MARK FINLEY, Constellation Energy

MARK FLAHERTY, Constellation Energy

ROY GILLON, Constellation Energy

JOSH HARTZ, Westinghouse

DAVE HUEGEL, Westinghouse

JOHN KILLIMAYER, Westinghouse

CHRIS McHUGH, Westinghouse

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8:32 a.m.

CHAIRMAN DENNING: (presiding) The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Power Uprates. I am Richard Denning, Chairman of the Subcommittee.

Subcommittee members in attendance are Tom Kress, Otto Maynard, Jack Sieber, and Graham Wallis.

The purpose of this meeting is to discuss the extended power uprate application for the R.E. Ginna Nuclear Power Plant. The Subcommittee will hear presentations by and hold discussions with representatives of the NRC staff and the Ginna licensee, Constellation Energy, regarding these matters.

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 the meeting previously published in The Federal Register on April 12th, 2006.

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.

We have not received any requests from members of the public to make oral statements or written comments.

I would make some comments. We are kind of experimenting with some revisions to this room, and some of these speakers do not transmit very well. So when you are making your presentations, please make sure you are up very close to them and speak directly into the microphone.

We will now proceed with the meeting, and I will call upon Mr. Milano of the NRC staff to begin.

MR. MILANO: Good morning. Again, my name is Patrick Milano. I am the Licensing Project Manager with responsibility for Ginna.

This morning we are going to have presentations by Mr. Sam Miranda and Dr. Len Ward of the PWR Systems Branch in the Division of Safety

Systems.

On the agenda this morning I am going to give you a brief introduction as to where things stand with the uprate application itself, and then we will cover the items that came out of the March 15th and 16th Subcommittee meeting and then go into those open items that were not in the first draft safety evaluation that was provided to you. The subsequent safety evaluation that you received on or about April 4th does have the remaining open items evaluated in it.

Just as background again, the EPU application that came in on July the 7th was preceded by three license amendment requests that are all tied directly with the license application. We have made some progress in all three. Those were the relaxed x axial offset. As you see on the slide, it is complete. The main feedwater isolation valve one we have issued and it is complete.

The revised LOCA analysis amendment, the staff's safety evaluation is complete. You will be hearing some of the information that is in it which is in today's presentation. The safety evaluation has been completed by the staff and the inputs provided, and the actual package is currently in concurrence review.

Again, we had the Subcommittee meeting on March 15th and 16th, and we are scheduled next Thursday to have the full Committee meeting with you.

Also, as part of the uprate, you recognize we have to issue an environmental assessment. That environmental assessment was published in the middle of April for comment, and the comment period ends May the 12th.

Again, the licensee plans, if we should issue the power uprate amendment and these other packages, they are planning to implement the uprate during the fall 2006 outage.

Again, in addition to hearing presentations by the licensee staff -- they are going to cover the same subject areas -- the NRC staff is going to likewise prepare presentations about what we did during the review. For the non-LOCA analysis, you are going to hear from Sam Miranda. He is basically going to talk about acceptance criteria margins and interpretation of the results of three or four different non-LOCA transients as they were reviewed for Ginna.

Dr. Ward is going to go through those items. The next two items here are those items that were not present in the first draft safety evaluation. These were the open issues or open items from the last Subcommittee meeting. He is going to go through the small break LOCA evaluation review that he did and then go into post-LOCA, long-term cooling boron precipitation.

That, basically, is all I wanted to say before turning it over to Constellation Energy for their portion of the presentation. With that, Mr. Mark Finley is the Project Manager for the uprate with Constellation, and he will be introducing his staff.

MR. FINLEY: Yes, Good morning. Mark Finley, Project Director for the power uprate at Ginna, as Mr. Milano said.

I would like to introduce Mark Flaherty, current Acting Vice President of technical areas at Constellation, to kick off the meeting for Ginna.

CHAIRMAN DENNING: Speak into that mike and let's make sure that he can hear you.

MR. FLAHERTY: Hi. I am Mark Flaherty.

CHAIRMAN DENNING: Okay, good.

MR. FLAHERTY: Here although the slide shows that I am the Acting Vice President of Technical Services, I was just transferred to the Engineering Manager of Calvert Cliffs on Monday. So with respect to the project and ACRS, whatever else, I wanted to continue supporting this project for as long as need be. So that is why I am here today.

As Pat Milano indicated, Constellation is back to discuss two topics that the Subcommittee requested further discussion from the March meeting. Those are RCS materials and non-LOCA margin. So we have presentations for both of those topics.

Secondly, there's two topics that we did not present at the last Subcommittee meeting. Those are small break LOCA and long-term cooldown. Then I will follow up with a summary conclusion once we go through the subject for presentations.

So, with that, I will turn this over to Jim Dunne who will lead us into RCS materials.

MR. DUNNE: Good morning. My name is Jim Dunne. I am an Engineering Consultant at Ginna Station. I have been at Ginna for 15 years in the Engineering Department, and for the last three years I have been the Lead Mechanical Engineer for the uprate project.

One of the open items from the meeting we had in March was a request by the ACRS to see a list of where in the reactor coolant system we have alloy 600 material or its weld equivalent, Inconel 82 or

Inconel 182, present. So the purpose of my presentation is to go over those locations.

Basically, there are four locations in the reactor coolant system where we have alloy 82 or the equivalent weld material. Three of them are in the reactor vessel. One of them is in the steam generator.

The three locations in the reactor vessel are in, basically, lower radial supports at the bottom of the reactor vessel, the bottom-mounted instrumentation welds to the reactor vessel lower head. We also have a third location which is a weld buildup on a safety injection nozzle for our upper plenum safety injection, and then in the steam generator we have alloy 600 weld material as cladding on the steam generator tube sheet.

Go back to the slide.

This is a schematic of the reactor vessel internals, showing the various components. Two of the three items in the reactor vessel are shown here. The safety injection nozzle is not shown on this schematic, but basically our safety injection nozzles are located at the same elevation as our hot and cold leg nozzles up in this area of the reactor vessel.

The other two locations, like I said

earlier, the lower radial supports, which are at the bottom of the core, basically, there are lugs welded to the reactor vessel that act as radial supports. They basically act as a keyway for keys from the core barrel that allow the core barrel to be aligned properly inside the reactor vessel.

There are four supports 90 degrees apart. The support material is alloy 600, and it is welded to the lower reactor vessel inner shell with an alloy 600 weld material.

MR. SIEBER: Have you ever examined those for cracking?

MR. DUNNE: We do a visual examination for them as part of the 10-year ISI when we do the vessel examination.

MR. SIEBER: It is hard to see though, right?

MR. DUNNE: Right. But, other than that, I don't believe there's any special inspections of that. This would be generic probably --

MR. SIEBER: It's cold.

MR. DUNNE: -- to all Westinghouse reactor vessels, would be my guess.

MR. SIEBER: It is cold down there anyway.

MR. DUNNE: Yes, the other thing is,

because it is at the downcomer, it sees cold leg temperature. Our cold leg temperature for EPU is increasing by about 8 degrees from where we are presently operating. However, the cold leg temperature at EPU will be a couple of degrees below where we operated the plant from 1970 up through 1996, when we replaced our steam generators and lowered our TF.

The second location, next slide, the second location that we have it is in the bottom-mounted instrumentation weld locations. We have 36 penetrations through the reactor vessel lower head for bottom-mounted instrumentation.

Basically, there are three areas on the bottom-mounted instrumentation where we have alloy 600 material. The nozzle itself is an alloy 600 nozzle that is machined. It is welded to the reactor vessel lower head in this area with the J-Weld, which is an Inconel 182 J-Weld material. Then the nozzle outside the reactor vessel, our nozzle, the alloy 600 nozzle is welded to a stainless steel nozzle with an Inconel 82 weld.

All three of those locations are pressure-boundary locations, and all three of them, basically, see cold leg conditions. So, as such, we don't believe they would be susceptible to any PWSCC concerns.

Next slide.

The third location in the reactor vessel where we have alloy 600 is a weld buildup on our SI nozzles. This is a plane view looking down at basically the nozzle location, the reactor vessel, the two hot and cold legs over here.

We have two SI nozzles 180 degrees apart that penetrate into the upper plenum region of the core because we are an upper plenum injection plant, like the other Westinghouse two-loop units. At the end of the SI nozzle in the reactor vessel itself internally there is a weld buildup over in this area.

Next slide, please.

So this basically shows the entire SI nozzle forging. This is the reactor vessel material here. This is the weld for the SI forging to the reactor vessel material. The SI forging itself is basically a carbon steel material with a stainless steel cladding for the nozzle itself, but at the end of it inside the reactor vessel they put in a 1-inch Inconel, I believe it is 182 weld buildup, to extend the nozzle down an inch. That was for fabrication, final fabrication, of the internals to the SI nozzle.

Then they ended up machining back on these to get the clearances they needed between the OD of the upper barrel and the SI nozzle.

MR. WALLIS: What is the SI nozzle made out of? The safe end there, what is that made out of?

MR. DUNNE: The SI nozzle is basically --

MR. WALLIS: The safe end of it.

MR. DUNNE: The safe end over here --

MR. WALLIS: Yes.

MR. DUNNE: -- is a 182 316 stainless. This weld here is not Inconel. So the only place where we have Inconel is this, which is a weld buildup. It is not pressure boundary --

MR. SIEBER: It is not load-bearing either?

MR. DUNNE: It is not load-bearing. The inside of it, basically, sees hot leg conditions or upper plenum injection conditions, which would be upper plenum pressure and upper plenum temperature. The outside portion over here and over here, because you have the upper core valve basically coming around here, basically, sees cold leg pressures and cold leg temperatures.

So there is a minimal delta P across this internal component right here because it is inside the pressure boundary. Obviously, out here this SI nozzle sees the full RCS pressure, but this portion of it is basically seeing about 30 to 40 psi delta P between the cold leg pressure and the upper plenum injection pressure. As such, it is not a highly-stressed component.

Also, because you have hot leg temperature in here and cold leg temperature out here, basically, its temperature is someplace probably close to TF. So, again, we don't believe that is susceptible to PWSCC, mainly because of the low stresses and because the temperature is relatively low and it is not really hot leg temperature.

So those are the three locations --

MR. WALLIS: It cycles in temperature a bit, doesn't it? It cycles?

MR. DUNNE: The cycles -- well, the SI nozzle for up and down, yes, that is part of the design for the reactor vessel.

MR. SIEBER: Well, ordinarily, there's no flow there, right?

MR. DUNNE: There would be no flow, yes, in here. It is a stagnant region during normal operation.

The fourth location where we have --

MR. WALLIS: Do you ever test this in some way? Do you test --

MR. DUNNE: We don't do tests to --

MR. SIEBER: Injection.

MR. DUNNE: We don't do flow tests into the reactor vessel. We do test SI flow in a recirc mode.

The fourth location where we do have cladding, basically Inconel 82 cladding, is on the steam generator tube sheet, between the bottom portion of the tube sheet. This shows the tube sheet here, and this is the primary head. Basically, the tube sheet is carbon steel. It is 25-and-a-quarter-inch thick.

The bottom portion, which has siezed the RCS conditions, basically has about a three-eighths-inch Inconel 82 clad material deposited on it. So the clad material isn't the pressure boundary material per se. It is more just to protect this carbon steel base, tube sheet base metal from the borated water.

Basically, the divider plate, in a new replacement generator this divider plate is basically a 690 material. The cladding of the primary bowl itself is a stainless steel clad material.

There's also in this little blowup here, this is the divider plate, and here is your tube sheet cladding. There is something called a seat bar buildup off the tube sheet that they use to basically build up the tube sheet so they can weld the tube sheet to the divider plate. This seat bar buildup is also Inconel 82. This weld here between the Inconel 82 material and the 690 primary divider plate is basically a 690 weld material.

During building of the replacement generators we did look at substituting a 690 clad on the tube sheet versus a 600. BNW Canada has had lots of experience with 600 clad material. They have never had any problems with it. But because of the industry concerns about 600 material in general, we evaluated going to 690 during the fabrication of the replacement generator.

There was a test program done. This cladding is basically a bead-welded material that is automatically welded to the tube sheet. So they evaluated going to a 690 wire material in lieu of the 600 material, but the testing that was done indicated that they were having problems with under-bead cracking and inter-bead cracking on the clad material. So the decision was to stay with the 600 material because of those problems with the welding.

Basically, the Ginna replacement generators and the other replacement generators that went through BNW Canada about the same time, which would be the St. Lucie replacement generators and the Duke Catawba McGuire replacement generators, all had 600 Inconel 82 clad material on their tube sheets. The Commonwealth replacement generators that BNW Canada built subsequent to ours also had 600 weld material.

After the Commonwealth, BNW was able to optimize the Inconel 690 wire chemistry and their welding process to get 690 to be an acceptable cladding material. Some of the more recent replacement generators that BNW Canada has built for U.S. utilities have gone to a 690 clad material, but at the time we were doing it they were not able to get the 690 material to work.

Basically, obviously, on the cold leg side, whichever one is the cold leg side, the cladding sees cold leg temperature; the hot leg side sees hot leg temperatures. So the cladding material will see a higher temperature than it has historically seen at Ginna. Right now we are running a T hot of around 590. Prior to replacing the steam joiners in 1996, we operated around 601-602. For a T hot with EPU we are going to be operating with around a 608-609 T hot. So we will be slightly higher there.

Historically, BNW Canada has never seen any problems with the Inconel 600 cladding in the industry. As far as we know, nobody in the industry has seen any problems with the 690 cladding on tube sheets.

The replacement generators for Commonwealth and Duke with the 600 material are operating at hot leg temperatures comparable to where Ginna will be at EPU. They have been operating for about to eight to ten years without any reported crack problems with the material. So we don't believe it is going to be an issue.

The other thing is the fabrication of the generator. Basically, the way BNW Canada fabricated the generator, they put this assembly together, welded the lower shells to the tube sheet, welded the transition cone to the lower shell, and then put that entire assembly into a heat treatment oven to do stress relieving on the pressure boundary welds. So that operation would have also acted to reduce any residual stresses from the original cladding welding on the Inconel material.

The next slide.

So, basically, in conclusion -- that's not the slide we had, but that is okay. Our conclusion is we don't believe there is any new PWSCC concerns that would arise to the Inconel alloy 600. We don't believe the alloy 600 we have in the RCS is basically going to create any new concerns due to EPU. For the lower radial support and for the bottom-mounted instrumentation, they see cold leg temperatures, so their susceptibility to PWSCC is low.

The SI nozzle weld buildup, it is not a highly-stressed component. So we don't believe it is an issue.

Then for the Inconel cladding on the tube sheet, basically, because it was stress-relieved during fabrication, it is not really a pressure boundary material. It is also the hot leg temperatures we are seeing are consistent with hot leg temperatures that other plants presently operating are seeing with the same type of cladding. Because there's been no issues in the industry on tube sheet clad problems with steam generators over the last 35 years, we believe that there are no issues with tube sheet.

MR. WALLIS: This isn't an issue for power uprate. It might be an issue for license renewal, when you are trying to extend the period of time?

MR. DUNNE: Well, this was evaluated and there is a -- basically, license renewals, which we have gone through and the NRC has approved, they looked at all the cladding material. They basically said there is no indication of cladding damage out there. Therefore, it was viewed that the uprate would not have any -- that extending the license, which would not change any conditions, just put more years on it, would not have any issue. This cladding material and tube sheet is low-flow incidency, any radiation. Again, Westinghouse's experience and BNW Canada's experience has been there have been no problems with tube sheet cladding reported in the industry.

Now for 600 material in general, the industry has a mandate to establish an alloy 600 management program, which the industry, which Ginna is part of, is going through creating an inspection program for alloy 600 going forward. So all this stuff will be reviewed as part of that program. That is how we identified, basically, the SI nozzle weld buildup, as part of just going through the weld records for the RCS just to identify where we have 600 material in the RCS.

MR. SIEBER: Do you, by any chance, know what the reactor vessel hot leg safe end to the cast piping, what the weld material is there? Is that a stainless?

MR. DUNNE: It is stainless.

MR. SIEBER: Okay. How about the pressurizer surge and spray lines?

MR. DUNNE: Stainless.

MR. SIEBER: Stainless?

MR. DUNNE: Yes.

MR. SIEBER: Okay. There are some plants where 82/182 is used.

MR. DUNNE: Right.

MR. SIEBER: But you are not one of them?

MR. DUNNE: No.

MR. SIEBER: Okay.

MR. DUNNE: And that is all I have.

MR. SIEBER: You are lucky.

MR. DUNNE: Yes.

CHAIRMAN DENNING: Do we have any other questions? Jack, are you comfortable?

Okay, thank you.

MR. SIEBER: I guess I would point out that all these cladding depositions are not pressure boundary. You can sustain a crack and have corrosion underneath, but since there's virtually oxygen in the coolant, the corrosion rate is very slow.

MR. FINLEY: Good morning. Again, Mark Finley, Project Director for the Ginna power uprate. If you recall from last time we met, in my previous life I was actually Supervisor of the Safety Analysis Group at Calvert Cliffs for several years. So I am the lucky one to present our safety analysis discussion here this morning, but I am backed up by our Westinghouse experts to help with questions.

As you recall, at the last meeting you asked about margin associated with several of the non-LOCA events. That is what we are going to talk in some detail about today, and, also, Sam Miranda, I think when I am finished, will discuss these events and perhaps others with respect to margin in the safety analysis.

I will show you the current results that are applicable now as well as the EPU results that are being reviewed by NRC. We will talk specifically about the loss of flow, loss of load, and rod withdrawal events, which were three of the more limiting events in our safety analysis.

This slide shows the current and EPU results associated with the three limiting events I just mentioned. As you can see, the EPU results in the center column there are close to the results in the righthand -- excuse me -- the acceptance criteria in the righthand column. This is the reason for the discussion today.

MR. WALLIS: These are predicted with RETRAN, is it?

MR. FINLEY: That is correct. These results, we did for the non-LOCA methodology at Ginna, we revised the methodology from LOFTRAN to RETRAN, and with respect to the core thermal-hydraulic code, changed that method from the THINC to the VIPRE code.

MR. WALLIS: Well, there's sort of two questions that are basic. One is these numbers are awfully close to the limit, and what does that mean? And the other thing is RETRAN isn't a very accurate code. You can tweak it various ways. When you get 2748.1, it would seem that the slightest tweak could make it 2749.

MR. FINLEY: Right.

MR. WALLIS: So what's implied by your saying that this is the number rather than some other number which is perhaps close to it?

MR. FINLEY: Right, right. And, actually, Gordon, temporarily go to the next slide.

We did this with the understanding of the approach that was used. We modified inputs to the analysis until we got acceptable results by the approved criteria. We didn't attempt to go any further than that and demonstrate additional margin.

That is because we understand the margins that are in our analysis and the inputs that are assumed and in the methodology, as well as margin that is above the safety limit controlled by NRC. So these results are not coincidental, as was mentioned last time.

Because of that approach --

MR. WALLIS: Deliberately tried to get to the limit, essentially?

MR. FINLEY: Well, I wouldn't term it like that. We were above the limit --

MR. WALLIS: You tested them until you got to the limit?

MR. FINLEY: We were above the limit without any changes to the inputs, and we tweaked on the --

MR. WALLIS: Pulled it down to be below though?

MR. FINLEY: That is correct.

MR. WALLIS: So it is similar. Which kind of inputs did you adjust then?

MR. FINLEY: Okay, I'll tell you what, if I can hold off on that question until I talk about the events specifically, then we can get to that.

MR. WALLIS: Sure.

MR. FINLEY: Go back one slide, Gordon. Okay, just stick with this slide.

One more comment: Current results you see in the lefthand column of the three columns there. As expected, they are somewhat higher in DNBR space than the EPU result. The trend is all, you know, it makes sense to us.

The pressure results, the same way, about eight pounds lower for the pre-EPU result, increased somewhat. We would expect that with the increased power level and decay heat.

CHAIRMAN DENNING: You're going to talk about how do you get the DNBR? What about the criterion? Where did that criterion come from?

MR. FINLEY: Yes, we will speak to where the criterion comes from here in a minute.

CHAIRMAN DENNING: Okay.

MR. FINLEY: Okay, next slide, Gordon. Actually, two slides.

With respect to the first event, this is the loss of flow and the DNBR margin, where the result was, again, close to the acceptance criteria.

Let's focus here in the middle of this slide. That is sort of the way I set up this discussion for all the events. But that is where the safety analysis limit is. Just below that you see our safety analysis result, 1.385 versus the 1.38 for the limit.

But what we are attempting to demonstrate here is sort of the range of results as you move from more realistic conditions up to the very conservative conditions.

Right underneath the safety analysis result we just modified one input to the analysis associated with the trip time delay for loss of flow. We used a conservative time in our analysis result to get the 1.385. It was 1.4 seconds.

We have done one-time testing in the past to demonstrate that result is actually less than one second, and a more typical assumption for plants in the industry is one second for other Westinghouse plants.

If you remove that margin and that trip time delay assumption, again, still using a conservative assumption that bounds actual plant performance, there's about a 3 percent change in the result, as you see, 1.42.

Now that's not a best-estimate analysis. This would still be a bounding conservative analysis. But that was one input that we could have changed even further to demonstrate additional margin.

MR. WALLIS: Now your safety analysis result is conservative in some sense? I would say that you have just mentioned one conservatism. Does it have other conservatisms in it?

MR. FINLEY: Yes, that is correct.

MR. WALLIS: You say it is a bounding result?

MR. FINLEY: That is correct, it is a bounding result. I am not going to go through all the conservatisms here.

MR. WALLIS: If there are, what do we have -- you put in some bounding assumptions. But RETRAN itself has uncertainties in it which you don't know, or you don't assess, it seems to me. So you don't really know how much uncertainty there is in the code itself. So even though you are putting in conservative assumptions, the safety analysis result is really 1.385 plus or minus something, which has to do with the inherent uncertainties in the code itself.

MR. FINLEY: Yes, to some extent, that's --

MR. WALLIS: I am curious about how big those are. If those are 5 percent, maybe it doesn't matter; you don't get beyond the design limit. But if the uncertainties in the code itself are 25 percent, then one might say, "Well, it could be that in the extreme case you could be way down to your bounding test data."

MR. FINLEY: Right, I understand.

MR. WALLIS: How to assess that?

MR. FINLEY: I understand, but our point is that these inputs are quite conservative in bounding. They more than make up for any uncertainties in the RETRAN methodology.

MR. WALLIS: That has been demonstrated somewhere?

MR. HUEGEL: In the WCAB 14882, we did -- I am sorry; this is Dave Huegel from Westinghouse.

As part of the effort to transition to RETRAN, we did do a bunch of benchmarks which compared the results to actual plant data and confirmed that the RETRAN results were consistent.

MR. WALLIS: Plus or minus what sort of --

MR. HUEGEL: The other thing is, for this event --

MR. WALLIS: Plus or minus what sort of number?

MR. HUEGEL: No, we just did comparisons to make sure that they were in line.

MR. WALLIS: Oh, you looked, you made a curve and you showed some data points that were near the curve?

MR. HUEGEL: That is right.

MR. WALLIS: There's no quantitative assessment of the uncertainty in RETRAN?

MR. HUEGEL: No, but we do know that it is conservative in terms of --

MR. WALLIS: So it is on one side of the data point? There's a bunch of data on the graph and RETRAN is above or below in some conservative way? Is that what you're saying?

MR. HUEGEL: What we are doing, what we did is we compared it to plant data and we didn't predict it on one side or the other. But what you have to do is keep in mind the transient that you are looking at.

Here we are looking at a loss-of-flow event.

MR. WALLIS: Right.

MR. HUEGEL: For the loss-of-flow event, the plant does an actual plant coast-down and confirms that the coast-down that is being predicted is conservatively bounded by what we have assumed in the safety analysis.

What is going on for this loss-of-flow event is primarily driven by the characteristics of your RCPs. The plant does confirm that the calculation of the flow coast-down is bounded by what we have assumed in the safety analysis.

Additional conservatisms that we have in the loss-of-flow event include the fact that we have skewed the reactivity that we have assumed toward the bottom of the core, so that you are not seeing any significant amount of negative reactivity until the rods are well into the core. That is another conservatism that we have within the analysis.

Another thing is, even though we have modeled the complete RCS for this particular event, as Mark is showing there, we have taken no credit for the increase in pressure, which is definitely a DNB benefit, in the calculations that we have performed.

Another thing we have assumed is frozen feedback. When you assume the effects that you have going on due to the loss of flow in the reactivity feedback, since we are modeling a point kinetics model, we get a very conservative calculation of the reactivity during this transient that is relatively quick and is over in a few seconds.

Again, as I mentioned earlier, it is primarily driven by the effects of how the RCPs are coasting down, which, again, is confirmed by the plant.

When we did a more realistic best-estimate-type calculation, we didn't do this for Ginna specifically, but we have done calculations with our RAVE methodology where we have linked the different codes, the kinetics code with our thermal-hydraulics code, and then also the VIPRE code, which does the calculations within the core. We find DNBRs that are well over two for this kind of event.

So in doing the analysis for Ginna, we have all kinds of conservatisms that we believe are backed up based upon actual test data that the plant has performed, as I mentioned, like the flow coast-down, which confirms that what we have done is conservative.

Another conservatism is in the rod drop time that we have assumed. The rod drop time is assumed based upon a very high mechanical design flow.

If you look at this particular event, what you have is a drop in the RCS flow. What you would find is your rod drop time would be much quicker, and if we were to take credit for that conservatism, we would even show a higher DNBR.

MR. WALLIS: Instead of whatever --

MR. HUEGEL: Right. You have layer upon layer upon layer of conservatism placed in the analysis.

MR. WALLIS: But say that these conservatisms somehow overwhelm the uncertainties in the thermal-hydraulic code.

MR. HUEGEL: Yes, absolutely.

MR. WALLIS: And, also, you have to put, in, to get this 1.385, you have to put in a DNB correlation --

MR. HUEGEL: Right.

MR. WALLIS: -- that has uncertainty in it as well.

MR. HUEGEL: That is correct.

MR. WALLIS: Presumably, all these things are figured into the choice of 1.38.

MR. FINLEY: And so that gets to the other side of the curve --

MR. WALLIS: There's a whole pile of stuff behind this which is difficult for us to assess without digging into it for days.

MR. HUEGEL: Understood, yes. So there's a lot of --

MR. WALLIS: If I am understanding -- I mean you're assuring us of all this stuff which sounds good, but we don't really know how to balance these things, some of which move one way and some of which move the other --

MR. HUEGEL: Understood.

MR. WALLIS: -- to be really convinced that everything you are doing is conservative. So that is the problem --

MR. FINLEY: Well, Dr. Wallis, one of the things we tried to demonstrate on this slide is the margin in the DNB testing and the data, and so forth, as well.

MR. WALLIS: Yes.

MR. FINLEY: As you see up above, up above the safety limit, there is a stackup of margin --

MR. WALLIS: Right.

MR. FINLEY: -- to address those uncertainties.

MR. WALLIS: Right.

CHAIRMAN DENNING: Are you going to

explain --

MR. FINLEY: And I will start with that.

CHAIRMAN DENNING: Go ahead. Go ahead, do that.

MR. FINLEY: I think Sam Miranda is actually going to speak more to that. But if you start sort of with the definition of critical heat flux, 1.0, of course, we have test data which is done for the particular fuel type that we are using, and there is a scatter of that data, of course.

MR. WALLIS: Well, the 1.17 reflects the DNB correlation uncertainty?

MR. FINLEY: That is correct.

MR. WALLIS: Okay.

MR. FINLEY: At a 95 percent probability with 95 percent confidence, and the applicable limit is 1.17, right?

On top of that, we have a design limit which accounts for parameter uncertainties such as temperature, pressure, flow --

MR. WALLIS: Depending on where you are on in the physical space?

MR. FINLEY: Right, some of the geometries, et cetera. So there's an additional 5 percent or so on top of that to protect for that.

MR. WALLIS: Then the thermal-hydraulic calculation uncertainties is what makes you go up to 1.38, is it?

MR. FINLEY: Help me out, if you would.

MR. WALLIS: The RETRAN uncertainties?

MR. HUEGEL: The difference between the 1.24 and 1.38 is just generic margin that we retain to account for unexpected penalties that may come up.

MR. WALLIS: There's several engineering guesses? We're not quite sure, so we'll add something on?

MR. HUEGEL: I'm not sure I would say, "guess," but --

MR. WALLIS: Well, a judgment. It is a judgment.

MR. HUEGEL: It is a judgment.

MR. WALLIS: Because other plants have different numbers.

MR. HUEGEL: Yes, that is correct.

MR. WALLIS: That is what is so mysterious about how someone arrives at 1.38 and someone else is 1.45 and --

MR. HUEGEL: Well, hopefully, it is not mysterious.

MR. WALLIS: -- someone else is 1.5, and

so on. Okay.

CHAIRMAN DENNING: A couple of other questions then.

MR. FINLEY: Yes.

CHAIRMAN DENNING: On the over-pressure, I want to make sure I understand.

MR. FINLEY: Yes.

CHAIRMAN DENNING: This is different from what -- this is primary system pressure?

MR. FINLEY: That is correct. This, of course, loss-of-flow event is a heat-up event.

CHAIRMAN DENNING: Yes.

MR. FINLEY: During the event, D average goes up, causes an insurge to the pressurizer. It compresses the bubble in the pressurizer. And even taking credit conservatively in this case for the sprays acting as they should, and so forth, the pressure goes up about 75 pounds in this transient at the time of minimum DNBR.

CHAIRMAN DENNING: And you don't take that into account in your correlation?

MR. FINLEY: That is correct.

CHAIRMAN DENNING: You just keep it at the initial pressure?

MR. FINLEY: That is correct.

CHAIRMAN DENNING: Now you could take into account or is there not a pressure dependence developed for the correlation?

MR. FINLEY: We could --

MR. HUEGEL: I think it was partly in the SER that we received, based upon how we explained the methodology, we felt that we mentioned the nominal pressure; therefore, it wouldn't be appropriate, even though it is certainly justifiable, to credit anything beyond the nominal pressure.

Certainly, as Mark explained, we see a pressure increase, and since we do see a pressure increase, we would typically assume your pressure control systems to minimize any pressure increase, like your sprays and your PORVs, but we felt, based upon what we had written up in our methodology and what was issued in the SER, we felt that we couldn't go above nominal pressure even though, again, it was perfectly justified in our minds.

CHAIRMAN DENNING: Okay. So you're saying that there are some control factors that are not allowed to be taken into account in the performance of the analysis like sprays and stuff like that?

MR. HUEGEL: No, it is just we stated we were using nominal pressure there; therefore, that's all we felt we could get away with using.

MR. FINLEY: There are items like that that we consider part of the approved methodology --

MR. HUEGEL: Right.

MR. FINLEY: -- that we would not take credit for, depending on what has been approved previously. Here I think we felt not taking credit for pressure was part of the approved method for Ginna and so we left that out.

MR. HUEGEL: Right.

MR. FINLEY: But we feel perfectly justifiable would be to take credit for that.

CHAIRMAN DENNING: Yes. Now -- I'm sorry, go ahead, Jack.

MR. SIEBER: In this particular event, though, as the coast-down is occurring, the effectiveness of sprays has gone away.

MR. HUEGEL: Sure.

MR. SIEBER: It is driven by the pump DP.

MR. FINLEY: That is correct.

MEMBER MAYNARD: But, typically --

MR. SIEBER: I mean you could actually -- well, the coast-down is what, 30 seconds or thereabouts?

MR. HUEGEL: It is a couple of seconds.

MR. SIEBER: Spray is over with before coast-down?

MR. HUEGEL: That is right.

MR. FINLEY: And we did model the spray, in determining that 75-pound increase, that was with modeling of sprays, the effect of sprayers.

CHAIRMAN DENNING: In this particular version of loss of flow is one in which, it is almost like a loss of power to the pumps where they just go into coast-down?

MR. FINLEY: Actually, this is even more severe than the typical loss of power. This, for Ginna, our limiting event is actually a grid frequency change of 5 hertz per second, which is a very, very severe grid transient, one that is worse even than the blackout that we had in 2003, where the grid actually drives the pump speed down because we are locked into the grid, okay, for a certain amount of time. It is actually a more rapid coast-down of the pumps, if you will, than the flywheel-driven coast-down would be. We actually call that a Condition 3 event for Ginna, even though we conservatively apply the Condition 2, no fuel failure criteria.

CHAIRMAN DENNING: In getting back to a point that you made about the comparisons that are made with the plant data, the plant does a similar test or has done a similar test in which it does a pump trip or something like that? And you are saying that in the prediction with RETRAN that the RETRAN results fall below the --

MR. FINLEY: Right. What we do is part of our hot functional test program. I think all plants have done this reactor coolant pump coast-down. So you get an actual data curve for --

MR. WALLIS: You don't have a back-up slide that shows that, do you?

MR. FINLEY: I don't. Sorry, Doctor.

CHAIRMAN DENNING: And that was performed a long time ago or --

MR. FINLEY: That would have been part of the initial plant startup.

CHAIRMAN DENNING: The initial plant startup?

MR. FINLEY: Hot functional testing, yes.

CHAIRMAN DENNING: But you have done the RETRAN analysis recently to demonstrate just what we heard?

MR. FINLEY: Right. But, of course, nothing really of significance would change to affect that; i.e., it is a flywheel mass really that provides the momentum and determines that coast-down rate. We have not modified --

MR. HUEGEL: But that is another conservatism, that we would reduce the inertia, even though it wouldn't apply to this event because of the frequency decay driving the pumps down, but in a complete loss of flow where the pumps are free to coast down, we reduce the inertia of the flywheel by 10 percent so that we get a conservative coast-down relative to what the plant would measure.

CHAIRMAN DENNING: And now, as far as the analysis is concerned, you start it at a slight over -- like 2 percent or 3 percent over? I mean, is this the kind of thing, over normal power?

MR. HUEGEL: Yes, all uncertainties are accounted for, but the way that we have done them is they are included in the DNB design limits. So we would have uncertainties in the power level, in pressure --

CHAIRMAN DENNING: But when you actually run it, when you run it, what power level do you use as the start?

MR. HUEGEL: It is done at nominal power.

CHAIRMAN DENNING: At nominal?

MR. HUEGEL: Yes.

CHAIRMAN DENNING: So that uncertainty was included in that --

MR. HUEGEL: Yes, that is correct.

CHAIRMAN DENNING: Now what about as things -- about during the cycle and stuff like this? Is there a point in the cycle like when the moderator coefficient is the least negative or something like that that has an impact? I am trying to get a feeling for whether it is done at the worst time in the cycle.

MR. FINLEY: Right, right. Certainly, yes. This is a heat-up event. Obviously, the least negative or positive moderator temperature coefficient would be limiting. We can't operate at full power with a positive moderator temperature coefficient. So it would be something, our most, least -- excuse me -- our least negative moderator temperature coefficient would be used early in cycle, right.

Right. So, as was said before, there are layers and layers of conservatism in each of the inputs that we take at the same time. We think that far outweighs any uncertainty in the RETRAN numerical calculation itself.

CHAIRMAN DENNING: Well, the best evidence I have heard so far is that you actually have done the work on the experiment with the plant and that the

RETRAN results fall below that level.

MR. FINLEY: Right.

MR. HUEGEL: That is correct. That is correct.

CHAIRMAN DENNING: Okay.

MR. FINLEY: We typically do that in the safety analysis for the parameters that are critical. It is done and NRC has asked to do that over time to approve the methodology.

MR. WALLIS: When you come to the full Committee I don't know if we are going to go into this again, but other Committee members may have the same curiosity that we have. So it might be good to have some back-up slides with this RETRAN compared with the real plant transient, and so on, just in case someone starts to probe.

CHAIRMAN DENNING: Well, I think let's get a little bit beyond that. I mean I would certainly like to see that.

MR. WALLIS: So we want to see it ourselves?

CHAIRMAN DENNING: Why don't we see that?

MR. WALLIS: Can we see it when, this afternoon or something, or when?

MR. HUEGEL: Do you have any of the coast-downs, Mark? I don't know.

MR. FINLEY: I will try to get it this afternoon. I don't have it at my fingertips. So we will look.

CHAIRMAN DENNING: Okay.

MR. WALLIS: Yes, maybe if we are satisfied, we can convince our colleagues to be satisfied, but that is always difficult.

MR. FINLEY: Okay, any other questions on loss of flow?

CHAIRMAN DENNING: No. Let's move on.

MR. FINLEY: Okay.

MR. WALLIS: So now we have a different issue, which is pressure.

MR. FINLEY: Okay, a different issue. This is pressure. This is a loss-of-load event. Just as the title suggests, it is a full loss of load, a turbine tripped a generator off the grid.

Again, I will start in the middle here. Our design limit or acceptance criteria for the event is 110 percent of the design pressure for the reactor coolant system. The safety analysis result was about a pound and a half below that, 2747 as compared to 2748.5.

Again, this looks close, but we need to

take it in the context of margin below and margin above, which is what this slide tries to demonstrate. For example, if we did take credit for control system functioning, i.e., steam dump operation and pressurizer spray operation, that alone would reduce the peak pressure by over 100 pounds. Similarly, if we added operation of the PORVs to that mix, that would provide another 40-pound-or-so reduction.

Probably most importantly, and why you don't see issues with these types of events in the industry, is when you get a turbine trip, we are designed, as all plants are, to get a reactor trip automatically. So there is no real delay between the time of the turbine trip and the reactor trip.

What causes the over-pressure in the analysis is a short time delay between the trip of the turbine and the trip of the reactor. There's where you have a power mismatch for a short period of time, causing additional heat and causing the pressure overshoot --

MR. WALLIS: If we were following a PRA-type analysis, you would go through this event tree and you would say, did the PORVs work or did the Pzr pressurizer spray work? And you give some probability to all those things, presumably. That would be a way you could --

MR. FINLEY: That is correct. That is correct.

MR. WALLIS: Here you are simply saying we will just assume it doesn't happen.

MR. FINLEY: Right.

MR. WALLIS: And so you give a probability of zero.

MR. FINLEY: Exactly, exactly. In fact, I discussed -- just to give a flavor for that, we have two, essentially, relays on sets of contexts which will cause a reactor trip on a turbine trip. If either one functions, you will get the reactor trip simultaneously, essentially.

I talked to our PRA folks a little about that and asked them what probability they would assign to that. He said between 99.9 and 99.99 probability of success.

So between 99.9 and 99.99 percent of the time our result is down here.

MR. HUEGEL: But it is not a safety grade function. Therefore, we can't credit in the safety analysis. So we have to rely upon the high-pressurizer pressure reactor trip to terminate the transient, even though, as Mark said, that that function, even though control grade, is highly reliable.

CHAIRMAN DENNING: At what level does the pressure trip then?

MR. FINLEY: The high-pressurizer pressure trip --

MR. HUEGEL: Yes, 2377 is the value at the plant, but the safety analysis would assume 2425 or 2435. So we have accounted for uncertainties between what the plant would be dialing in and what we were assuming in the safety analysis to account for all the instrumentation uncertainties.

MR. WALLIS: How about RETRAN here? Is RETRAN accurate to 10 percent, so we don't have to sort of add another 10 percent on this thing for some reason?

MR. HUEGEL: Well, RETRAN we found is very conservative in terms of over-predicting the pressure. Yes, it would predict a higher pressure than you would expect to see at the plant for a similar --

MR. WALLIS: It is supposed to be a realistic code.

CHAIRMAN DENNING: My experience with these codes has generally been that they predict pressure comparatively well, but what kind of evidence do you have from plant data? I mean, do you have evidence for plant data?

MR. HUEGEL: We do a lot of comparisons with these codes for load rejection tests and making sure that all the control systems are functioning as designed. We have plants out there that are full-load rejection capability plants, and in tuning the control systems we would use the LOFTRAN and RETRAN codes to make sure that we are predicting that these control systems are functioning as designed.

When we see the plant actually doing its test, we find that the results compare very favorably. But, again, that is with crediting all the different control systems, which we don't assume or credit in any of the safety analysis unless it makes the transient worse.

CHAIRMAN DENNING: Yes. As far as absolute safety is concerned here, suppose we are wrong and the pressure really is higher. Then you adjust -- you would go to the safety and the safety valves would relieve?

MR. HUEGEL: Well, the safety valves do operate in this transient.

MR. DUNNE: That is typically what terminates the transient, is when the relief valves open, but you've got to remember --

MR. HUEGEL: The reactor trip and the --

MR. DUNNE: And the reactor trip and the safety valves opening. What is happening is the peak pressure is occurring at the RCP discharge.

MR. HUEGEL: Right.

MR. DUNNE: And the pressure that the relief valves are set at is the pressurizer pressure, which is nominally around 2500. We have about a 2 to 2.5 percent uncertainty on that set point. So in the analysis base we raised the actual set point in the analysis by that 2.5 percent.

We also have a 1 percent uncertainty for loop seal drift because we have a loop seal in front of our relief valves. So you add another 1 percent on the pressure at which the safety valves will open on the pressurizer. Then there is a time delay to clear the loop seal, which is around .8 seconds or so, which there is no way to relieve --

MR. HUEGEL: Right, and there's no credit for any of the relief during that time period where the loop seal is clearing, even though you would be getting some pressure relief capability. As Jim stated, there is no credit for that in the safety analysis.

CHAIRMAN DENNING: Except if we are in an ATWS scenario which you analyze differently.

MR. DUNNE: Well, in an ATWS scenario you don't take any credit for any of that stuff. Well, you take credit for the relief valves, I think.

MR. HUEGEL: Yes, we would.

MR. WALLIS: Do you have plant data on this loss of load?

MR. FINLEY: Of course, we have experienced loss-of-load-type trips in the past.

MR. WALLIS: Yes, and you take the data and you use a realistic analysis, which would be the bottom line here using RETRAN.

MR. FINLEY: Right.

MR. WALLIS: It would be interesting to see how well you predict what really happened.

MR. FINLEY: Right. The difficulty there is you have a very benign event. This is actually the pressure at, I think, the reactor coolant pump discharge. It is low in the RCS. It is actually higher than the pressurizer pressure.

MR. WALLIS: Yes.

MR. HUEGEL: You don't even get to the point of the PORVs on the pressurizer.

MR. FINLEY: Pressurizer pressure goes up very, very little. So that data, in terms of comparison to RETRAN, wouldn't show much.

MR. WALLIS: Wouldn't show much of a challenge to RETRAN. Nothing much is happening.

MR. FINLEY: Right.

MR. WALLIS: All that is happening is in regulatory space.

MR. DUNNE: And, simplistically, you know --

MR. CARUSO: It is a challenge to RETRAN. I mean it has to calculate the physics properly.

MR. HUEGEL: That is true.

MR. CARUSO: Whatever you put in it should be able to calculate it. So if you have data for a real trip, then RETRAN should be able to calculate a real trip.

CHAIRMAN DENNING: Sure, sure.

MR. WALLIS: That would be really convincing stuff if you produced that.

MR. HUEGEL: We did have some plant comparisons in the WCAP that we submitted and was reviewed by the NRC, 14882. We chose the comparison of the RETRAN results to different plant events. I think there were some load rejections.

MR. WALLIS: Okay. Is there some key part of that that we can see at this meeting?

MR. HUEGEL: We could probably pull out the slides from that WCAP.

MR. WALLIS: Because it would be good to go away with a very convinced sort of happy feeling and not feel there are a lot of things we had better study.

MR. HUEGEL: I think the important thing to take away is that the methodology, even though we have got different DNBR limits that we are using, we still apply the same exact conservative methodology which has, as we mentioned, for example, in loss of flow, layers upon layers of conservatism. I think that is the important part.

MR. WALLIS: You sound very convincing, but then, of course, you are an advocate for your point of view.

(Laughter.)

MR. HUEGEL: Understood.

MR. FINLEY: Certainly with respect to the plant data, part of the approval process with the staff in WCAP review and approval is to provide that sort of benchmarking data.

MR. WALLIS: We have to assure ourselves that the staff at least has investigated and asked the kind of questions that occur to us.

CHAIRMAN DENNING: All right. Let's go to the next slide.

MR. FINLEY: Okay. Well, before we go to the next slide, we didn't talk, I don't think, about above the design limit, to speak to that margin.

We have for Ginna calculated, as you see here, an ASME service level C limit for hot conditions of around 3200 psig. That was done for the ATWS scenario. In fact, when we do an ATWS event, we have to meet that pressure.

That is where you would potentially start to deform components in the RCS, not likely, but potential. We wouldn't expect catastrophic failure there, but potential for bolting to stretch and that sort of thing.

So that gives you some feeling for, you know, we are not on the hairy edge in terms of this 110 percent.

MR. WALLIS: You're assuming a standard atmosphere or something when you do this? We went through this before. The difference between your psi and your psi design pressure on one of these charts is less than the variability in atmospheric pressure itself.

MR. FINLEY: Correct. We don't vary --

MR. WALLIS: You're trying to assume some kind of atmosphere --

MR. FINLEY: It's 14.7.

MR. WALLIS: Although in reality it is fluctuating up and down quite a bit.

MR. FINLEY: Okay, and the last event I wanted to speak to was the rod withdrawal at power event. This event provided results close both to the DNBR criteria --

MR. WALLIS: This is where you are even closer. This is where you are about as close as you can possibly get.

MR. FINLEY: -- and also pressure. And, again, the reason for the closeness of the result to the acceptance limit is that we reduced the -- I think in this case -- Chris, correct me if I'm wrong -- we reduced the rod speed or reactivity insertion rate, essentially, until we met this limit. That is what we established as our core design.

MR. WALLIS: How can you reduce that arbitrarily? You actually can control the insertion rate?

MR. HUEGEL: No. We make sure that we've got a conservative insertion rate. Obviously, it would bound anything that we would see at a plant.

MR. WALLIS: Make it less conservative in some way? How did you manage to change that?

MR. FINLEY: And then we incorporate that restriction into our core design.

MR. WALLIS: Make it less conservative? You justify making it less conservative? Is that what --

MR. HUEGEL: No, it is the same conservatism.

MEMBER MAYNARD: This feeds back into what your surveillance requirements would be or what set point you would have to have for certain instrumentation?

MR. HUEGEL: Exactly. The other thing is when you --

MEMBER MAYNARD: You are trying to give yourself as much of a margin --

MR. HUEGEL: When we define a safety analysis limit, keep in mind that the over temperature and over power delta T trip set points are designed to provide protection based upon the conditions that are associated with what you selected for your safety analysis limit. So it is no surprise that when you have a revised safety analysis set point, you are going to have trip set points, the OTDT and OPDT, which are designed specifically to ensure you are meeting your DNB design basis, that you are going to end up with a result that is consistent with your safety analysis limit here.

What Mark was saying is we refined the reactivity insertion rates that we looked at to make sure that we were getting the closest match to the safety analysis limit. We analyzed a whole wide range of reactivity insertion rates from like 1 pcm per second up to, say, 110 pcm per second, which covers the maximum differential rod worth you would expect to see anytime in the core design life and also associated with your maximum rod speed that you would expect to see at the plant. Combining those two, we cover the whole wide range of reactivity insertion rates.

What we just did here is refine and make sure that we picked the lowest or the exact reactivity insertion rate that gives you the closest approach to your DNBR limit. So that might have been, say, 25 pcm per second, where maybe in the previous analysis we used a more coarse comparison of reactivity insertion limits because we had more margin to the result.

MR. WALLIS: Make sure although in reality it isn't worse?

MR. FINLEY: That's correct. And then we factor that input assumption to the safety analysis into our surveillance program as well as into our core design process. So that when we design the core and we use the physics codes to validate the reactivity parameters, we do that. We do that each cycle.

MEMBER MAYNARD: They're not arbitrarily changing numbers that have no impact on something else.

MR. HUEGEL: No.

MEMBER MAYNARD: They are really defining what their surveillance requirement or their set points would be on other parameters to assure they're meeting them.

MR. WALLIS: I'm just trying to figure out if there isn't a possibility that the rod withdrawal rate somehow exceeds something that you have set to it.

MR. HUEGEL: No. The other thing is we don't limit the insertion either. I mean you have a limited amount of bank worth that you can add in terms of reactivity. What we assume in this transient is that we keep adding whatever amount of reactivity it takes us to get us up to the trip condition.

So, in reality, you may have a total bank worth say at 90 percent power of 500 pcm. That might not be enough to take you up to the trip set point that we have assumed, which is like 118 percent power. However, as part of the conservatism of the analysis, we keep adding reactivity, even though it may not truly exist, until we get to the reactor trip set point.

We do that from all different power levels, from different times in life, and for all different reactivity insertion rates. So we are analyzing hundreds and hundreds of cases to get to the reactor trip set point, when in reality for a lot of the cases you wouldn't even get there.

MR. WALLIS: Well, tell me, physically, how does this reactivity get inserted?

MR. HUEGEL: It is assumed to be inserted at a constant rate.

MR. WALLIS: It is a withdrawal of rods, right?

MR. HUEGEL: Right.

MR. FINLEY: You have to start --

MR. WALLIS: The physical withdrawal of rods? Is this something that happens inadvertently due to some glitch or is it something the operators do? Is it something that happens because of an accident or what?

MR. HUEGEL: It is considered to be a Condition 2 transient, which could be, one, a failure in your control system or, two, it could be operator error.

MR. WALLIS: So the physics limits the reactivity addition rate, doesn't it?

MR. HUEGEL: And keep in mind that --

MR. WALLIS: Doesn't it? In some way?

MR. HUEGEL: Yes.

MR. WALLIS: And so you can't so arbitrarily set it? It seems to me you are still twiddling it until you get the right number, and you can't do that. It tells you what it is going to be --

MR. FINLEY: No, no. In the core design process, by changing your core design and the worth of the rods, you can effect that reactivity addition. So we control that.

MR. WALLIS: And then you control that to be the maximum it could possibly be in the transient?

MR. FINLEY: That is correct.

MR. HUEGEL: Yes. They would have some curve. The differential rod worth varies as a function of rod position. We pick off the peak and then make sure that our --

MR. WALLIS: You make sure that it is as fast as possible then?

MR. HUEGEL: That presents an upper bound which essentially we are well beyond that differential rod worth peak in terms of the range of reactivity insertions that we would look at.

CHAIRMAN DENNING: With regard to the implied rate of withdrawal of the rod --

MR. HUEGEL: We cover a whole wide range.

CHAIRMAN DENNING: But how does that relate to the maximum, that withdrawal rate that is possible? I mean you push a button and have a rod withdrawal.

MR. HUEGEL: That's right.

CHAIRMAN DENNING: It is a certain rate of withdrawal that is implied.

MR. HUEGEL: That is right.

CHAIRMAN DENNING: And then the reactivity rate depends upon what the worth of the rod is.

What is the implied rod withdrawal rate relative to the standard? Is it --

MR. HUEGEL: Again, what this safety analysis assumes is a whole wide range of constant reactivity insertion rates in pcm per second. That implies a constant differential rod worth and a constant withdrawal rate for that given condition that we are analyzing.

Keep in mind that we analyze a whole wide range of reactivity insertion rates which conceivably would cover a whole wide range of differential rod worths and rod speeds. So we have encompassed any particular rod speed that you could have at the plant and also we have bounded any particular differential rod worth that the core design would calculate, which is confirmed on a cycle-by-cycle basis.

CHAIRMAN DENNING: What limits the rate of rod withdrawal?

MR. HUEGEL: What is the fastest -- I think it is 72 steps per minute -- or is it 66? Okay, sorry, 66 steps per minute. The maximum differential rod worth that I think we have assumed is something like 100 pcm per step.

MR. McHUGH: Yes, this is Chris McHugh from Westinghouse.

The last reload cycle, the actual calculated maximum rod worth was about 30 pcm per second. In our rod withdrawal power analyses, like Dave said, we go up over 100. So we have covered from 1 pcm per second up to 100, and on a cycle-by-cycle basis we need a maximum of about 30.

MR. HUEGEL: Thank you, Chris.

MR. CARUSO: Can you physically change the rod withdrawal speed? Or is that something that is locked into your control system design?

MR. FINLEY: Right. Not without modifying the plant and doing testing post-modification to verify the rod speed.

MR. CARUSO: But you have a current defined rod speed that is locked into the rod control logic?

MR. FINLEY: That is right. It is part and parcel to the design.

MR. WALLIS: 1.381 comes from the fastest withdrawal rate that is possible?

MR. HUEGEL: No. We have looked at a whole wide range.

MR. FINLEY: No, it is one of the intermediate --

MR. HUEGEL: Yes.

MR. WALLIS: One of the intermediate ones which is worst?

MR. HUEGEL: Yes.

MR. WALLIS: Okay. And rod ejection is something else?

MR. HUEGEL: Yes, that is a whole other beast.

MR. WALLIS: A whole other beast because, obviously, rods could go, you know, flying out under some imagined scenario.

MR. HUEGEL: Right. The other thing is I think there are also rod blocks. I think if you exceed like 3 percent, don't the rods automatically -- but that is a control grade function again, which we don't credit in the safety analysis.

CHAIRMAN DENNING: Why don't you come up to the mike? State your name, please.

MR. GILLON: I'm Roy Gillon. I am Senior Reactor Operator since 1991, current Shift Manager at Ginna.

We also have five rod stops, OT delta T, OP delta T; difference in average T, any single T average, low power, 12.8 percent, and a 20 percent drop in power also give us a rod stop.

MR. HUEGEL: And these are all well below the reactor trip set points that we are crediting on the safety analysis. We don't take credit for any of these control grade functions, which would effectively limit or make these transients very, very benign.

MR. WALLIS: I am trying to think if I'm

right now. This 1.381 comes from looking at all times in the cycle, all places where rods could be, and all rates at which they could be withdrawn? At the worst? Is that what you have done?

MR. HUEGEL: This limit is set before we even look at the transients.

MR. WALLIS: But I am just trying to make sure, are you telling me it is the worst case when you look at --

MR. HUEGEL: Yes.

MR. WALLIS: -- all times in the cycle, all places where rods could be, and all rates at which they could be withdrawn? You somehow span this whole volume of space and you look for the worst DNB situation?

MR. HUEGEL: Yes, with no credit for any of the control functions and with an infinite amount reactivity.

MR. WALLIS: So when you say 1.381, you are probably looking at the real tail-end of some probabilistic distribution of what could happen?

MR. HUEGEL: Yes.

MR. FINLEY: That's correct. Absolutely correct.

MR. WALLIS: And, in effect, you are

beyond the tail-end or you so claim to be, the real limit of the tail-end?

MR. HUEGEL: We believe that the analysis, again, is very, very conservative.

MR. DUNNE: This is Jim Dunne.

Again, what Chris McHugh said is this is the analysis that we have set up as a bounding analysis going forward for EPU. Then as part of every cycle design for the core design for that cycle, they've got to verify that their limiting condition for that cycle is, indeed, still bounded by the --

MR. WALLIS: It must be running for quite a long time to get this number.

(Laughter.)

You must be running about a third of the time you are running the reactor to predict what is going to happen next time.

MR. FINLEY: There are dozens and dozens of cases, yes.

MR. WALLIS: Right. Okay.

MR. HUEGEL: We make assumptions that, hopefully, we don't have to look at the safety analysis every cycle, but what we do confirm every cycle is that what we have assumed in the safety analysis is bounding, and as Chris McHugh stated, what we have assumed in terms of a peak reactivity insertion rate is as well above what the core designs are currently predicting.

MR. WALLIS: If you conquered some sort of fuel management program which enabled you to do this, you presumably would reduce the power or do something? You have to adjust something.

MR. HUEGEL: You would have to adjust something, but we've got so much margin here I don't think it is a problem.

MR. WALLIS: Okay.

CHAIRMAN DENNING: I think they can continue.

(Laughter.)

MR. FINLEY: Good. Next slide, Gordon.

Okay. The last slide with respect to margin here for non-LOCA events would be, again, the rod withdrawal, but this time with respect to pressure. This just demonstrates, again, if we took credit for a more realistic, yet still bounding and conservative reactivity addition rate, the peak pressure would come down nearly 200 pounds as a result, still a similar sort of bounding analysis looking at all the potential scenarios we could be in, but just taking some of the margin that is in that one assumption with respect to reactivity addition.

MR. WALLIS: So it looks as if this is what is limiting your power uprate then?

MR. FINLEY: That is correct.

MR. WALLIS: If you had a higher power uprate and you didn't twiddle a few more things, you would go over this bound?

MR. FINLEY: That is correct. These three events are the limiting events for the Ginna uprate.

CHAIRMAN DENNING: And this is actually a slightly different, it is a different -- the particular selection of input parameters that leads to this limited event is different from the selection that led to the DNB --

MR. FINLEY: That is correct. This comes from a different set of initial conditions, yes.

MR. HUEGEL: But we do cover the wide range of reactivity insertions that we talked about in the DNB space. So we still are looking at anything that we conceivably could come up with in terms of --

MR. WALLIS: When you are searching for an optimum or maximum, you have to take a lot of runs to be sure you are there?

MR. HUEGEL: It runs pretty quickly.

MR. WALLIS: So that when you take small

break LOCA, you have to take quite a lot of steps in the break size in order to get the real maximum?

MR. HUEGEL: Well, these transients are over in a few minutes. So we can run tons of cases within a half an hour. I mean this is not a problem running many, many cases. It is not a LOCA where you are looking at it for an extended period of time.

MR. WALLIS: I am just wondering if mathematically you can be sure that you are within this .4 psi in terms of having determined the maximum.

MR. HUEGEL: Well, the closer we get to the limit, obviously, the more refined we have to be in terms of what we look at in terms of reactivity insertion rate.

MR. WALLIS: But we have to get comfort from the fact that there's all this margin and all these conservative assumptions.

MR. HUEGEL: And that's what we want you to walk away with, that there is a lot of conservatism.

MR. WALLIS: About the accuracy with which you can predict this to five significant figures.

MR. HUEGEL: Exactly.

MR. FINLEY: Okay, the next slide, Gordon.

Just to summarize, once again, all of the results meet the acceptance criteria. There are various areas of margin in the methods and in the inputs. In addition, there's margin above the acceptance limits to the point of failure.

MR. WALLIS: What would make me happier, I think, in the long run would be if the margin were expressed in some quantitative way representing a measure of safety, whatever that is. Because you can talk forever about margin and say, "Well, we've got 100 psi here," but what does that really mean in terms of public safety? You have to be an engineer and you have to use judgment to say, "Well, we've got 100 psi. That sounds good."

But if you could express this margin in terms of some measure of public safety, which is 10 to the minus 10 or something, that might be much more convincing.

MR. HUEGEL: Right, and you have to also have confidence that the methodology that we are applying is robust. What we are applying here is the same that we have applied for the last 30 years.

MR. WALLIS: Then we would have to examine ASME and I would hate to get into that.

(Laughter.)

MR. FINLEY: Okay. Well, thank you. That

is all I had for the non-LOCA events.

CHAIRMAN DENNING: Very good. I think we will just go ahead.

MR. WALLIS: Very, very good. Thank you very much.

CHAIRMAN DENNING: Go ahead with the regulatory version of this.

MR. WALLIS: It's not quite a Ph.D. exam because you didn't show us equations, but we are getting there.

(Laughter.)

Now we are going to look at the staff view of all of this?

CHAIRMAN DENNING: Yes.

MR. WALLIS: To put this in perspective, I was interested enough after our last meeting on this subject, margins, to go back and read the transcript, which I very rarely do, to see what questions got answered and which questions did not. So we are really interested, at least I am very interested in this issue. I want to look at the transcript maybe from this presentation and see how well we got convinced.

MR. MIRANDA: My name is Sam Miranda. I'm a reviewer in the PWR Systems Branch. I reviewed the

Ginna power uprate application.

I have the same slides, basically, as you have seen before.

MR. WALLIS: But with now different curves on them or the same curves?

MR. MIRANDA: I have the Ginna transients I can discuss, but before that I have all the same margin and acceptance criteria slides that you have seen. Unless there are any questions, I suggest we just enter them into the record and move on.

CHAIRMAN DENNING: Okay, very good.

MR. WALLIS: Okay.

MR. MIRANDA: There is this one slide that is a little bit different. It has some different numbers on it.

MR. WALLIS: You have different numbers and then they use RETRAN instead of some other code, and so on, right.

MR. MIRANDA: So we move from seventies technology to nineties technology from LOFTRAN to RETRAN.

MR. WALLIS: So we are on the margins part here, are we?

MR. MIRANDA: Well, I am going to start with the accident analyses unless you have some questions on the margins.

CHAIRMAN DENNING: Well, I guess the only question is that change that we just had where yesterday we were looking at 1.55 and today we are looking at 1.38, and the question is, what's the smallest value that NRR will accept?

MR. WALLIS: I'm sure the industry is very interested in their answer, I'm sure.

MR. MIRANDA: That margin between the design limit and the safety analysis limit is determined by the licensee and the vendor analysis, the analysts at the vendor. It is a safety margin in the true sense. It is a contingency. It is for unexpected problems.

It is something that the staff doesn't really see. All we can judge is, do the accident analyses meet the safety analysis limit? We know there is some amount of non-zero margin between the design limit and the safety analysis limit.

MR. WALLIS: But suppose a vendor came in with 1.25 and you don't see where it came from; are you going to accept it?

MR. MIRANDA: A safety analysis limit of 1.25?

CHAIRMAN DENNING: No, the safety analysis limit is, I think, 1.2 --

MR. WALLIS: No, the safety analysis is 1.38. That is the one we are talking about.

CHAIRMAN DENNING: Oh, I thought the DNBR. Yes, let's put the margins up there again, the one that has the 1.38.

MR. WALLIS: I am a little bit puzzled. This is determined by the licensee and the vendor using methods that you don't know about?

MR. MIRANDA: We know about the correlation limit.

MR. WALLIS: Yes, that is based on a publication.

MR. MIRANDA: And we know about the design limit.

MR. WALLIS: That's based on a publication.

CHAIRMAN DENNING: Right, right.

MR. MIRANDA: Those have both been reviewed and approved by the staff.

MR. WALLIS: Right.

MR. MIRANDA: The part we don't know about is the space between the design limit and the safety analysis limit.

CHAIRMAN DENNING: Right, and Graham says, okay, suppose this is 1.25; they decide let's go for 1.25. What do you do?

MR. MIRANDA: It is a matter of judgment. If they say 1.25 and if they produce analyses that all meet that value, I don't see how we can object.

The only problem with that is if something comes up in the future, some rod bow problems or something else and they need that margin, it won't be available. Then they will have to come in and change the safety analysis limit, and that is going to require a license amendment.

MR. WALLIS: I don't understand that. I mean with 1.25, they may be predicting 1.35, and they say, well, it's a huge margin because we are predicting 1.35 and our limit is 1.25.

CHAIRMAN DENNING: Well, let me say something that I think was implied that we didn't pick up on adequately. That is this contingency element. That is, suppose during the operation of the plant there's some issue that comes up like rod bowing, and they have to then go back and say, "Oh, well, you know, we really had that extra margin in there between 1.24 and 1.38, or between 1.24 and 1.55. So we don't have to shut down the plant."

MR. WALLIS: That's what it's for?

CHAIRMAN DENNING: I have a feeling that may be what it is for?

MR. WALLIS: Is that what it is for?

CHAIRMAN DENNING: Would you respond? I wonder whether the licensee might --

MR. WALLIS: It is a very arbitrary thing.

CHAIRMAN DENNING: -- or Westinghouse might comment on that.

MR. KILLIMAYER: Hi. This is Jack Killimayer from Westinghouse, the Fuels Division.

The safety analysis limit that we use, okay, the 1.24, the design basis limit has the uncertainties rolled in and meets the 9595 criterion. When we do our analyses, we do them all to meet the higher limits, so we can build in a certain amount of margin that is shown up here.

CHAIRMAN DENNING: And the purpose of that margin is to be extra safe or is it in part or largely because you want to make sure that, if issues come up, that suddenly you're not in a position where it appears that you are beyond the design limit?

MR. KILLIMAYER: Yes to all of them. There are some known penalties that we choose to cover with DNB margins such as the rod bolt penalties. We've got a rod bolt penalty of about a percent, a percent and a half, depending on the fuel type. We cover that with the margin that we retain between the safety analysis limit and the design limit.

You do want to have some margin in all your analyses when you are going into a cycle in case something does happen when you are doing an analysis for a given reload. All our DNB analyses have an assumption on axial power shapes, and we use a bounding axial power shape, what we consider to be a bounding axial power shape, going in, and we verify that each cycle.

So if you did end up with a more limiting axial power shape, you would have margin within the safety analysis limit to address small issues like that.

MR. WALLIS: So we are talking about .14, a difference between 1.24 from 1.3, which seems to be based on something insubstantial in terms of justification. Then we quibble about the difference between 1.38 and 1.381, which is less than 1 percent of this thing which seems to be somewhat arbitrary.

CHAIRMAN DENNING: Well, you and I are quibbling; I am not sure that they are quibbling.

MR. WALLIS: Well, we are questioning, let's say.

And yet they struggle to meet this 1.38 with this huge accuracy when it seems to be itself picked out of the air, to some extent. It seems to me a strange thing, you know.

Maybe if it is 1.3 -- it really might as well be 1.37. Why not?

MEMBER MAYNARD: I didn't see that they were struggling to meet that. They were intentionally --

MR. WALLIS: Yes, they were. They deliberately tried to get right on the --

MEMBER MAYNARD: -- getting there, so that they could establish design and set point criteria.

MR. WALLIS: They deliberately tried to get to 1.381, as far as I can make out.

MR. MIRANDA: I think the difficulty there is that the safety analyses that we were looking at are not safety analyses in the strict sense. They are also sort of design analyses. They are trying to come up with, by doing these safety analyses, come up with enough operating margin, operating space, for the future as possible.

So they use, they did, for example, the rod withdrawal at power analyses over a wide range of reactivity insertion rates and other conditions such that there's no future core reload that will go outside that area. They would do that up to the very limit, up to the 1.38, to make sure that they have given themselves as much space as possible.

MR. WALLIS: But the area then doesn't set the number 1.38. They could have had a higher power uprate and done all this analysis of core reload and said, "All right, our number is 1.36 and we're happy with that."

MR. MIRANDA: Well, they could have just as easily have done that.

MR. WALLIS: Well, why don't they do that and they come in with a 10 percent power uprate?

MR. DUNNE: The power uprate, power level was picked first and then all the analyses to support it were done.

MR. WALLIS: That's right.

MR. DUNNE: We didn't do all these sets of analyses and then come say --

MR. WALLIS: Put the cart before the horse. So you assume what you want to do and then justify it.

MR. DUNNE: Well, the other thing on the power uprate is we are also limited by the balanced plant side of the plant.

(Laughter.)

So if we wanted to go higher, then we would have more modifications to make on the balanced plant side of the plant.

So, you know, you end up choosing what your power level is --

MR. WALLIS: I understand that, but we are talking about safety here. We are talking about safety.

MR. DUNNE: Right, but that's the reason why we would not have actively pursued going much higher than the number we chose.

MR. WALLIS: It seems to me there has to be a justification for 1.38 which is more than saying that the vendor and the licensee decided in some mysterious way that's what it should be.

CHAIRMAN DENNING: And that they wanted that margin.

MR. WALLIS: Right.

CHAIRMAN DENNING: I mean that seems to be the margin they want. Again, it is a value to them related to these unforeseen --

MR. WALLIS: In some unforeseen circumstances they might go down to 1.30.

CHAIRMAN DENNING: Yes, that's right.

MR. WALLIS: And then they would come to us and say, "There's no problem because it is still above 1.24."

CHAIRMAN DENNING: And then they would come up and they would say, "Well, it's no problem." I think that's what we are hearing.

MR. WALLIS: Is that what happens?

MR. MIRANDA: No, they can't -- I don't think they can do that. I mean they have set the safety analysis limit that's in the tech specs. If they come in with something less than 1.38, they would have to justify it. They would have to come in and ask for an amendment, and then the staff would review that. But anything above 1.38 --

CHAIRMAN DENNING: They're locked into that.

MR. WALLIS: There had another plant yesterday that was 1.55.

MR. MIRANDA: Yes.

MR. WALLIS: They look at this plant and they say, "Gee whiz, there's no reason we should be 1.55. Why don't we come in with 1.38 and go for a power uprate of 30 percent?" Would you let them do that?

MR. MIRANDA: Well, actually, for Beaver

Valley, that has a little bit of history behind it. They could have been below 1.55, but they had, I believe they had 1.55 in the past and they didn't need to go below 1.55. The results were acceptable at 1.55, so they just kept it. So they had more than the average margin between design limit and the safety analysis limit.

MR. WALLIS: Yes, but that's why they might use it. Why don't they use it? Why don't they capture some of that margin and go to higher power?

MR. SIEBER: Well, the higher power is limited by how many dollars you want to spend on --

MR. WALLIS: But we're talking about safety. Dollars are irrelevant.

CHAIRMAN DENNING: No, but as far as the plant is concerned, they're --

MR. WALLIS: But these numbers should have a relationship to safety. That's what we're here for, isn't it? We're not here for anything to do with dollars.

MR. FINLEY: Right, Doctor, and we meet the safety limit, right?

MR. WALLIS: Set by you, it seems to me.

MR. FINLEY: No. These limits have been reviewed by the staff and accepted. We treat them as safety limits and we demonstrate we meet them with the power level that we have chosen.

As Jim Dunne said, we chose the power level based on many parameters. These safety limits are part of that decision process.

MR. WALLIS: The 1.38 is historically what you have had in this plant, is that it?

MR. KILLIMAYER: No. This is Jack Killimayer again.

We do set the safety analysis limit. Yes, there is, in a sense, an arbitrary amount of margin that is put in. It does cover known penalties, and we do build in extra margin to cover contingencies for the future.

It is an agreed-upon number as to how much margin we retain in the DNB analysis versus where it is in operating space.

MR. HUEGEL: It is agreed upon between Westinghouse and the licensee.

MR. WALLIS: That's right.

MR. HUEGEL: We don't treat that as the license limit. The license limit would be the design limit, okay?

MR. WALLIS: The license limit is 1.24?

MR. KILLIMAYER: Right. The safety

analysis limit is essentially our -- it is like an accounting method for keeping track of DNB margin to account for penalties.

MR. WALLIS: So when the staff evaluates your submittal, do they look to see the DNB number is bigger than 1.24 or that it is bigger than 1.38?

MR. MIRANDA: We use the 1.38 value.

MR. WALLIS: You use the value, but that seems very strange because you are using something defined for the convenience of the licensee which has no relationship to public safety whatsoever.

MR. MIRANDA: Well, there is a relationship to public safety. It is a value that is greater than the design limit.

MR. WALLIS: But 1.24 has some merit in terms of a measure of public safety.

MR. MIRANDA: Yes.

MR. WALLIS: The 1.38 does not; you said, but it is bigger.

MR. SIEBER: It has more --

MR. WALLIS: But it could be 1.9. I mean it is just arbitrary.

MR. HUEGEL: But the important thing is it is greater than; the 1.38 has an important part because it was met based upon a conservative methodology. So using our conservative methodology, we are meeting the 1.38, which includes, granted, it is rather arbitrary, but some amount of DNB margin above the design limit to handle the unexpected issues that do arise, as was pointed out, the rod bow penalty, for example.

You don't want to be in a situation where you have done your safety analysis right up to the design limit; something comes up unexpected, and you're strapped and you have no room to maneuver other than telling the plant, "Well, you have to derate or something." This gives us the flexibility to address the unknown issues that we hope don't occur, but, unfortunately, do occur.

MR. WALLIS: How do you get flexibility if the staff is approving 1.38 and you go down to 1.37 because of rod bow or something?

MR. HUEGEL: Because we show that the safety analysis --

MR. WALLIS: But they wouldn't shut you down?

MR. HUEGEL: No.

MR. WALLIS: Because you're above 1.24, is that right?

MR. MIRANDA: No, they would have to

explain why they are below the safety analysis limit.

MR. HUEGEL: But we have met the design limit and the safety analysis limit, and we have said that --

MR. WALLIS: It's strange.

MR. MIRANDA: Telling us that you met the design limit does not satisfy us.

MR. WALLIS: Am I just odd? I think this is very strange.

CHAIRMAN DENNING: But it is possible they could come to you and say -- I mean it sounds like we're hearing slightly different things, but what you are saying is that is what you license them with a particular core reload, core load; that's the way they operate the plant. If they find something mid-cycle that is an issue that would say that they are in conflict with that, then the licensee comes to you and says, "We want to have some granting relaxation," right? And it would be up to NRR to say yes or no, is that right?

MR. MIRANDA: Something like that. If something comes up in the future that causes them to use up all of their 11 percent margin between the design limit and the safety analysis limit --

CHAIRMAN DENNING: Well, I'm only going to let them use up 1 percent of it. Suppose they decide that it is 1.37. You know, something has happened. Now what is the requirement on them? Do they have to now -- are they in conflict with their license and they have to either shut down the plant -- I mean they have to shut down the plant within "x" amount of time or something.

MR. SIEBER: Reduce power.

CHAIRMAN DENNING: Or reduce power? And then you would have to grant some exception to allow them to go back to power? Is that a true statement?

MR. SIEBER: They would have to justify that based on a reevaluation of the uncertainties. That is one way to do this.

CHAIRMAN DENNING: So, actually, what would probably happen --

MR. SIEBER: What they come up, the staff might or might not agree with --

CHAIRMAN DENNING: Might or might not.

MR. SIEBER: -- a new limit.

CHAIRMAN DENNING: Yes, right?

MR. SIEBER: And you would recapture some of the margin that you put in there in the first place.

MR. MIRANDA: I'm a little bit confused.

Are you talking about the safety analysis limit or the design limit?

CHAIRMAN DENNING: The safety analysis limit.

MR. SIEBER: The safety analysis limit has extra margin.

MR. MIRANDA: They need to change the safety analysis limit; they would need to come to the staff.

MR. SIEBER: You would have to agree before they could do it then?

MR. MIRANDA: Since that is in the tech specs, that is a license amendment and the staff would have to review and approve that.

MR. WALLIS: It seems to me to have nothing to do with nuclear safety. I mean if 1.24 means the public risk is 10 to the minus 5 and 1.38 means it is two times 10 to the minus 5, that is very different from its being 10 to the minus 6. Until there is some scale which tells me what we gain in public safety by having this extra margin from 1.24 to 1.38, I don't have any way to evaluate how big it should be.

MR. MIRANDA: I don't see the need for evaluating that. That is a designer's margin. That is for their use in contingencies to cover unexpected problems.

MEMBER KRESS: You are suffering under the whole problem of all the licensees in design basis space which has a relationship to safety but it is not fully quantified because you've got these design basis events that represent ranges of accidents, and they do them conservatively. You end up with margins for the design basis events.

But how to relate that to some real measure of safety, which might be a risk number, is you have to -- it is an after-the-fact thing. You can go back now and say, "We'll do a PRA and we'll see if this design is safe from the standpoint of any risk measures you have." But it is an after-the-fact calculation.

To try to relate things like how much this margin contributes to that safety is just --

MR. WALLIS: I'm really puzzled though. I mean 1.24, see, it has a basis, right? It seems to me that -- I'm trying to relate it to my experience. If we say that we are going to educate students to pass a professional engineering exam, in a professional engineering exam to be a qualified engineer, you have to get a grade of 1.24. But the student says, "Well, I want to be better than that because I want to be a better engineer. So I am going to come up and say you're going to grade me to be above 1.38," and we agree to that. But it is all just arbitrary from the student's point of view.

MEMBER KRESS: Well, sure it is.

MR. WALLIS: It is not justified by the agency.

MEMBER KRESS: It is not quite arbitrary because it is designed by space and you did it in a conservative way and you end up with a conservative --

MR. WALLIS: But the number is set by the licensee and the vendor. It is not set by the agency.

MEMBER KRESS: That's pretty much arbitrary.

MR. WALLIS: It is really peculiar to have a safety thing set by the vendor rather than the agency. But, anyway --

(Laughter.)

MEMBER MAYNARD: I think the safety thing here is the design limit. Now the closer that the safety analysis limit comes to that, the less things that they are going to be able to tolerate --

MR. WALLIS: I understand that.

MEMBER MAYNARD: -- from other things.

The higher they go, that removes operating flexibility from the plant.

It is not as much a safety issue as it is as to, how much do you want to be able to tolerate without having to go back and reanalyze and resubmit?

MR. WALLIS: They still have to resubmit though. If they come up with something which is 1.3, they have to resubmit.

MEMBER MAYNARD: But they are a lot less likely, if they started with 1.3 and that you had some rod bowing or you had some thing, they are not going to be able to absorb as much of that. So the lower they make that limit -- yes, if they do end up below that 1.38, they've got to come in.

MR. WALLIS: Right. There's a likelihood that after they come in they can go out satisfactorily?

MR. SIEBER: Yes.

MR. WALLIS: Whereas if they were closer to it, they might be more at risk of being shut down? Is that the idea?

MR. SIEBER: Well, you have to make sure that you aren't going to approach the design limit.

MEMBER MAYNARD: It is going to change other -- if they have to come in with a lower number, then it is going to change some other things in a tighter design or different set points or different limits from that aspect.

CHAIRMAN DENNING: I think another thing that we have to get perspective on, we tend to think in risk space, and these are Condition 2 and Condition 3 events. Even defeating the design limits in these cases doesn't put you in a core meltdown situation typically.

MR. WALLIS: That's right.

MEMBER KRESS: It could possibly do some fuel damage.

CHAIRMAN DENNING: It could do some fuel damage.

MEMBER KRESS: And we don't have criteria in terms of risk of fuel damage other than full core damage almost. So if we had that criteria, you might possibly be able to relate this change in the limit to how much fuel you might damage if you had a whole spectrum of events, but we don't have that, unfortunately.

MR. SIEBER: Actually, you don't do fuel damage until you hit the critical heat flux.

MEMBER KRESS: That's right. That's right. But if you did it right, these would have probability distributions. The overlap would give a probability of meeting that for all the design -- for not the design basis accident, but for the spectrum of accidents. You could end up with a probability of core damage and you could have some sort of measure. That could be a measure of safety.

We don't do that because right now it is too hard. This seems to guarantee safety this way by experience. It is a way that the staff can deal with and a way the licensee can deal with.

MR. SIEBER: It's deterministic. That is the way these things were --

MEMBER KRESS: Deterministic as opposed to --

MR. WALLIS: My problem dealing with it, because we are going to evaluate whether or not to allow a power uprate, and if one plant comes in with 1.55, this one comes in 1.38, another plant comes in with 1.3, another one comes in 1.25, and they all say, "We want the power uprate." It is clear that the one with 1.25 is probably going for a higher power uprate. So how do we decide?

MEMBER KRESS: That's a good question.

MR. WALLIS: How do we decide what is reasonable?

MEMBER KRESS: That's a good question.

MR. MIRANDA: Well, you would be putting yourself in the position of judging as to how much --

MR. WALLIS: We're asked to write a letter, right. Right.

CHAIRMAN DENNING: That's exactly where we are.

MEMBER KRESS: You had a suggestion once, Graham, that I really liked, and that is, these are calculated by some code, a thermal-hydraulics code.

MR. WALLIS: Right.

MEMBER KRESS: And if you, instead of having this number, had a distribution and you could come up with some sort of probability of exceeding your design, your actual CfA, actually correlation limit, and you have some idea --

MR. WALLIS: Where we are, yes.

MEMBER KRESS: But even there you've got a problem because, even though we have that probability, you don't know what probability is acceptable. And that is an arbitrary choice.

MR. WALLIS: But at least you know what you are doing more.

MEMBER KRESS: You know what you are doing.

MR. WALLIS: Right.

MEMBER KRESS: But not enough to base a decision on.

CHAIRMAN DENNING: Let's not redefine the whole regulatory basis.

MEMBER KRESS: No, that is not in the regulatory basis right now; that's right. So we are stuck with the judgment.

MR. SIEBER: The only way we could be certain that their number is right is for us to do these calculations, this whole series of calculations, and I don't want to do that.

(Laughter.)

CHAIRMAN DENNING: Well, thank you, Jack.

Go now to where you were going to start your presentation.

MR. MIRANDA: Okay. I was going to talk about the same three transients that Mr. Finley discussed earlier: loss of flow, which is the event that challenges that DNB ratio; the rod withdrawal at power, which, by the way, I disagree; I don't think this is a challenging analysis for the DNB ratio. Rod withdrawal at power is more of a design event in terms of testing the over temperature delta T trip to be sure it covers the --

MR. SIEBER: That's the culmination of it.

MR. MIRANDA: Yes.

And the loss of load, which is the event that is most likely to over-pressurize the RCS.

These are the results for the loss-of-flow accident. There are two cases described here. One is the frequency decay, which is the limiting event, and then there is the complete loss of flow. With both complete losses of flow, one involves tripping both reactor coolant pumps and the other is the situation where the reactor coolant flow is driven down by a frequency decay on the grid. That one produces a lower DNB ratio.

I would say that this event is governed mainly by the power-to-flow ratio. That is very important in DNB ratio. If you look at the power-to-flow ratio, if you delay the reactor trip, if you keep the power relatively high compared to the flow, which is decreasing, either because it pumps a trip or because of being driven down by frequency decay, delaying that reactor trip will cause a lower DNB ratio.

We can see, for example, here that looking at the two events, in the flow coast-down event you have the reactor trip immediately because that is the initiating event, the undervoltage condition on the power supply buses on the reactor coolant pumps. So there you have an immediate reactor trip; whereas, for the frequency decay you have to wait for the signal, for the under-frequency reactor trip signal, and that takes a little bit more than half a second.

Here we see on the bottom curve -- it is not a curve; it is a straight line. It is the flow rate responding to the frequency decay.

Then we have the under-frequency trip burn in about two seconds. Then, as the rods are falling into the core, you have reached a minimum DNB ratio about here. You see the power level is still relatively high.

This is the heat flux in the core average channel and the hot channel. This is a reminder, for one thing, that this event is analyzed with RETRAN and VIPRE. The RETRAN code will calculate the transient in terms of power level and back to coolant system pressure and temperatures and flow rate. Then that information is passed to VIPRE, which actually calculates the heat flux, and VIPRE will model a hot channel. Here we can see there is not that much difference between hot channel and average channel.

MR. WALLIS: All this is at some time in

the cycle or some extreme case or something that bounds --

MR. SIEBER: Worst.

MR. WALLIS: The worst?

MR. SIEBER: The worst. The worst time in the cycle.

MR. MIRANDA: From this curve, we see that minimum DNB ratio -- well, actually, I have another plot I can show that describes all of this.

The minimum DNB ratio will occur actually before the time that the PORVs might open. This is an illustration of that.

Here's the minimum DNB ratio occurring. If you take that up to the pressurizer pressure curve, you see that the minimum DNB ratio has been reached before the core opening set point is reached.

All of this is interesting and it is not really relevant, though, for this analysis because this pressure is information that is not passed to VIPRE as you see it here. The VIPRE code will calculate the DNB ratio based on the nominal pressure. So there is no credit taken for the pressurization.

MR. WALLIS: I think the key thing is what turns around the DNBR. It seems to be headed down and then it gets turned around rather abruptly by something.

MR. MIRANDA: The rods are fully inserted, okay.

CHAIRMAN DENNING: Heat flux. Heat flux.

MR. MIRANDA: It is the power to flow --

MR. WALLIS: It is the power that turns it around? Okay.

MR. MIRANDA: If we look at the first curve with the power levels --

MR. WALLIS: Okay, it is the power. That is where it is. The power torque falls off the cliff or it goes over -- it is not really a cliff, but it goes down the slope. Then that is what turns it around. Okay.

MR. MIRANDA: It is all a function of power-to-flow ratio.

MR. WALLIS: Okay.

MR. SIEBER: Well, the whole transient is caused because of the mismatch between the trip and seeing the actual cause, which was the loss of the coolant pump.

MR. WALLIS: So what would seem to be --

MR. SIEBER: You are producing power in a regime where the flood is decaying.

MR. WALLIS: What would seem to be

critical here would be how fast the rods drop.

MR. MIRANDA: Yes, and we had --

MR. WALLIS: Because if it is a little bit later, then this DNBR would go down below the safety analysis limit.

MR. MIRANDA: Right. That's right.

MEMBER KRESS: Why doesn't the DNBR turn around again at some longer time? Because your flow has continued to drop, but the power sort of levels off. So you expect that curve to turn over again.

MR. MIRANDA: Well, you do not produce -- you have the reactor trip. So you're not producing power anymore. The power that you see there is --

MEMBER KRESS: Decay heat.

MR. MIRANDA: Decay heat, yes. It is kind of hard to come up with --

MR. SIEBER: Well, if the flow continued going down, then even decay heat could reach the DNB.

MEMBER KRESS: The flow never really stops.

MR. SIEBER: Oh, that curve doesn't continue on down like that?

MEMBER KRESS: No, because you end up in natural circulation.

MR. MIRANDA: Natural circulation is --

MEMBER KRESS: Okay. Well, that's the explanation.

MR. WALLIS: DNBR in a close to dryout situation, high quality, the power-to-flow ratio might seem -- no, it is all liquid. It is all liquid, isn't it? It is all liquid. So it is not. No, it has nothing to do with that. Yes, it is all liquid.

I am just trying to figure out why it should be power-to-flow ratio, but that doesn't matter. It doesn't matter.

MR. MIRANDA: So this DNB ratio, the 1.385 I believe is the limiting, is the lowest DNB ratio you will find in Ginna.

MR. WALLIS: Well, you have 1.381 in another one.

MR. MIRANDA: I will talk about that when I get to the rod withdrawal at power.

MR. WALLIS: Okay.

CHAIRMAN DENNING: Okay, proceed.

MR. MIRANDA: Loss-of-load event, Ginna has done three different cases here.

MR. WALLIS: I'm sorry, I want to go back to this other one. Since everything seems to be governed very much by when the rods drop, is this a conservative analysis you are showing us about rod drop or is this a realistic analysis?

MR. MIRANDA: This is conservative.

MR. WALLIS: So the rods, where actually it says two, it is more likely to be one?

MR. FINLEY: Right. I think, Sam, if you put up your sequence of events table there?

MR. WALLIS: As rods begin to drop at two seconds; it is more likely to be one second, is that right?

MR. MIRANDA: Well, they take 2.8 seconds to drop.

MR. WALLIS: Well, they begin to drop at two. Is it more likely that they would actually drop earlier than that?

MR. FINLEY: That is correct. This is Mark Finley, Project Director for Ginna.

I mentioned in my presentation there is a 1.4-second time delay assumed between the time the frequency set point is reached --

MR. WALLIS: That is the .6 --

MR. FINLEY: -- right -- and the time the rods begin to drop. We have actually timed that in the past at less than one second. So on my slide I said, if you reduced that 1.4-second delay to one second, then you would benefit in margin.

MR. WALLIS: Yes, I was recalling what you said.

MR. FINLEY: Yes.

MR. WALLIS: And I was trying to relate it to what is being presented here.

MR. SIEBER: The rod drop speed is slow, too.

MR. FINLEY: And then the rod drop speed is tested. We have a tech spec number we have to meet for the rods to reach the bottom, and that is tested each startup.

MEMBER MAYNARD: I think I also heard Westinghouse say that they don't take much credit for the rods until they get almost to the bottom, as though all the power were being generated in the bottom there. So that is another conservatism, I believe.

MR. FINLEY: They certainly use a bounding shape in terms of the rods and the position of the rods for the negative reactivity insertion.

MR. MIRANDA: Okay, the loss-of-load case, there are actually three cases, but the important one is the RCS peak pressure case, the last one.

Ginna has looked at the loss of load in terms of DNB ratio and also in terms of secondary site over-pressurization. They are different cases.

The DNBR case is a case that is designed to produce a low DNB ratio, which means you try to keep the pressure low. To keep the pressure low, they would use the pressurizer pressure control system, pressurizer spray and PORVs. They also use the revised thermal design procedure to evaluate the DNB ratio.

For this type of an event, as a reviewer, I would look for a trip coming from the protection that is designed to protect against low thermal margin. That would be the over temperature delta T trip. That is what is happening here. The over temperature delta T trip occurs at 11.6 seconds, and then the DNB ratio reaches a minimum, again, as the rods are nearing the bottom of the core.

The case designed to look at secondary site pressure, we are not looking at DNB ratio anymore. So they are using the standard thermal design procedure, which means, for example, that they are going to use different initial conditions. They are going to use 102 percent of rated thermal power, and they are going to use temperature uncertainties on the high side.

Also, in this case they are, for peak

secondary system pressure, they are assuming no steam generator tube plugging to maximum the heat transfer from primary to secondary.

Finally, the RCS peak pressure case --

MR. WALLIS: So that's a conservative assumption?

MR. MIRANDA: Yes.

For the RCS pressure case, they are not using any pressurizer pressure control, no PORVs, no spray. They are using all the uncertainties in initial conditions in a conservative direction, high temperatures, high power, and they produce the highest pressure. For example, for a trip on the high pressurizer pressure reactor trip --

MR. WALLIS: Now, presumably, the steam generator is cooling better; the pressure is lower, isn't it? That's a different --

MR. MIRANDA: They would assume different plugging level --

MR. WALLIS: Higher secondary pressure, but what did you assume about the steam generator in the last case?

MR. MIRANDA: Maximum plugging, 10 percent plugging.

MR. WALLIS: You assume 10 percent

plugging, okay.

MR. MIRANDA: That is why in each one of these analyses you look at what parameter you are interested in --

MR. WALLIS: No, I am just interested about the steam generator in the last case because it doesn't seem to be written down here. Okay.

MR. MIRANDA: So in the first case, in the DNBR case, they have the over temperature delta T trip occurring right about here.

MR. WALLIS: We don't have that.

CHAIRMAN DENNING: It is on the third one.

MR. WALLIS: It is on the third one, okay.

MR. MIRANDA: That trip corresponds to this point. Here is your DNB ratio.

MR. WALLIS: Oh, it wiggles, unless you put the pencil mark on there.

MR. MIRANDA: Oh, the wiggle mark?

MR. WALLIS: It is your pencil mark you put on there as a wiggle, isn't it, or is it not?

MR. MIRANDA: Yes, the wiggle is due mainly to this.

MR. SIEBER: Actually, we don't have that.

MR. WALLIS: We don't have that. We don't have that, no.

MR. MIRANDA: Here we have the pressurizer pressure and you see that we have PORV opening at 2350 psi, and, in fact, it gets up to 2500, where you might begin to see the safety valves opening. Over temperature delta T trip occurs right about here.

MR. WALLIS: We don't have your first curve there for some reason.

MR. SIEBER: We don't have the last one.

MR. WALLIS: We don't have the one you just showed, the one before this.

MR. MIRANDA: The one before this? This one?

MR. WALLIS: I don't think we have that.

CHAIRMAN DENNING: No, I don't think we do.

MR. WALLIS: We don't have that.

CHAIRMAN DENNING: It is missing.

MR. WALLIS: So DNBR is sort of headed to China until the PORV opens, is it, or something? It seems to be falling off a cliff and then it levels off again.

MR. MIRANDA: Well, I don't really connect it to the PORV. It is connected to the rods providing enough negative reactivity to trip the plant.

MR. WALLIS: And that's what stops it

abruptly? Okay.

But is that wiggle something you drew on there? We don't have this figure. You drew something on there?

CHAIRMAN DENNING: Yes, that is just a marker, I think.

MR. WALLIS: It's a marker, okay. You put that on? Okay. Just don't draw on the screen, whatever you do.

(Laughter.)

Okay, so that is the figure we don't have.

CHAIRMAN DENNING: But that's okay. Proceed.

MR. WALLIS: That's okay. We have seen it.

MR. MIRANDA: So this is where the trip occurs. I mean this is where the --

MR. WALLIS: And that is, again, conservatively estimated in time and stuff?

MR. MIRANDA: The over temperature delta T trip, that is the trip that is designed to keep the DNBR above 1.3 --

MR. WALLIS: Again, you've got two second between the trip and the rods dropping? Is that this conservatism again?

MR. SIEBER: Yes.

MR. MIRANDA: Yes.

MR. WALLIS: Yes, okay.

MR. MIRANDA: That is a long time.

There is also, by the way, in the over temperature delta T trip, there is also a delay built in actually before you even reach that signal to account for loop transit time because the temperature is measured in RTDs in the hot legs and the cold legs, and it takes time to get there, something like a six-second delay.

This over temperature delta T trip is current compensated, lead line compensation to account for the time that it takes to measure the temperature versus the time to actually put the rods into the core and actually trip the plant before you reach the core limit of 1.38.

MR. WALLIS: All right. I find this extraordinarily useful. We have complained in the past many times that when you read the SER and you simply see a description of what the applicant did, and then you say the applicant meets the regulations, everything is fine, there's no indication that anything like this sort of study is behind that decision. And I think this is the first time we have really seen that this staff knows what is going on in some detail, and it has been very useful to me. So please continue.

MR. MIRANDA: This is simply the steam generator pressure, the pressurizer water volume. The limit for the steam pressure is 1209, which is right about here, 1209.

The over temperature delta T trip occurs right here.

And we also verify, since this is a Condition 2 event, that the pressurizer is not filled.

MR. WALLIS: Yes.

MR. MIRANDA: This is an 800 cubic foot pressurizer, 18.6 cubic feet for the surge line. So we see that this event would not cause any water relief for the --

MR. WALLIS: And it's getting pretty close?

MR. MIRANDA: Close, yes.

MR. WALLIS: Yes.

MR. MIRANDA: Yes, Ginna has gone about as far as they can with this uprate.

MR. SIEBER: There's still margin.

MR. WALLIS: The operator might have a little concern when he sees that headed up like that.

MR. FINLEY: Exactly, and he's got many indications that might cause him to take actions that would improve these results, but we don't take credit for that, at least not for 10 minutes.

MR. WALLIS: These are seconds on the axis here?

MR. FINLEY: Yes.

MR. WALLIS: So the 15 and 18 seconds, if this is true, this curve, he's going to be having some qualms or something. Something is going to be happening to him.

MR. MIRANDA: Well, the reactor trip takes care of that situation. As soon as you turn off the --

MR. WALLIS: If it happens, yes. Yes.

MR. MIRANDA: It starts to go down.

In this case, the steam generator peak pressure case, you see that DNB ratio is not the issue and there's lots of margin there.

MR. WALLIS: Well, as long as it turns around, right?

MR. MIRANDA: It turns around due to the trip, yes.

MR. WALLIS: Which is conservatively estimated in time.

MR. MIRANDA: This is the RCS volume for the steam side pressure case. That volume is actually much lower.

MR. WALLIS: RCS pressure?

MR. MIRANDA: RCS pressure is -- we do have core opening of 2250 --

MR. WALLIS: I guess where you said "volumes temperature," you mean the temperature increase swells up the volume? Because it is sort of related to volume, isn't it? It looks like volume.

MR. MIRANDA: This is the core opening here. Then we have safety valves opening just barely right about here, taking into account 2.5 percent pressure accumulation.

MR. WALLIS: These are all curves submitted by the applicant?

MR. MIRANDA: Yes.

MR. WALLIS: And you folks didn't do any separate predictions or running of the code or anything? I guess Westinghouse doesn't give you the code to run?

MR. MIRANDA: Actually, we ran it. We ran a case with LOFTRAN.

MR. WALLIS: They did give you LOFTRAN to run? Or you have LOFTRAN?

MR. MIRANDA: We had access to LOFTRAN at their Rockville office. We ran the loss-of-load event with LOFTRAN. LOFTRAN agrees pretty well with RETRAN.

Back in the sixties, before LOFTRAN was written, there were some tests done at some plants, including Ginna, load rejection tests. They were used to benchmark LOFTRAN. RETRAN later was used, was benchmarked against LOFTRAN, and also these tests. Those codes are available. I think they might in that RETRAN WCAP.

MR. FINLEY: They're off looking for those curves as you speak, Sam.

MR. MIRANDA: Okay. If you look at those curves, I don't think you will see a consistent conservatism where the pressure is always under-predicted or over-predicted. They are going to cross each other at several points. Probably the better measure is a statistical correlation rather than a pressure margin.

All those results were available since the sixties.

This is the last of the steam flow pressure case. We see here that the pressurizer doesn't fill and that the steam system design pressure is not exceeded, level 9 psi.

This is the peak pressure, the peak RCS pressure case. This case does not assume any operation of the pressurizer pressure control system, no PORVs, no spray. We see the DNB ratio doesn't even go below its initial value.

We were looking for peak pressure. This curve, we have the high pressure trip occurring at about five seconds, right about here.

MR. WALLIS: The rods drop later at some time, yes.

MR. MIRANDA: Yes, the rods drop, but the pressure continues to go up until the safety valves open. The safety valves are opened --

MR. WALLIS: This is stored heat in the fuel or something?

MR. MIRANDA: Yes. Yes, that's right.

MR. WALLIS: Stored heat in the fuel?

MR. MIRANDA: Yes.

CHAIRMAN DENNING: Are the PORVs still open in this one because they are not a safety grade --

MR. MIRANDA: That's right, the PORVs are considered a control system. So they are not credited to operate.

MR. WALLIS: Not allowed to open?

CHAIRMAN DENNING: Not credited, but the reality is that they would, you said? Yes.

MR. MIRANDA: This same event, the loss of load is analyzed as an ATWS event, and that is a best-estimate analysis. In that case, the PORVs would open.

MR. DUNNE: I think the point to notice on this one for peak pressure, what terminates the peak pressure is when the safety valves open. Independent of the computer program, when the safety valves on the pressurizer go open, that's when you get your peak pressure in the pressurizer and --

MR. WALLIS: So it is going to be less, so it should be less than your design because they are open?

MR. DUNNE: Right.

MR. WALLIS: And at that point it is suitable.

MR. DUNNE: Yes,

MR. WALLIS: Yes.

MR. MIRANDA: Okay, these curves verify that the pressurizer does not fill. In this case, too, the steam side pressure does not exceed its safety limit.

MR. SIEBER: What is the volume of the

pressurizer?

MR. MIRANDA: The volume of the pressurizer is 800 cubic feet.

MR. WALLIS: These maximum pressures are really determined by set point on the relief valves? Nothing else matters, does it? Or does something else matter?

CHAIRMAN DENNING: There is overshoot.

MR. WALLIS: There is overshoot?

MR. DUNNE: Yes, basically, the two things that control this one from pressure is tripping the reactor and the safety valves opening. In this event the reactor trips early, but you don't really terminate the heat up the RCS until you basically -- a little bit later in time. So you keep on pressurizing until you get to the relief valves. When the relief valve pops, they have more relief capacity than the thermal expansion of the RCS, and that terminates the transient.

MR. MIRANDA: Just to complicate things a little bit, if you were to assume the PORVs were open in this event, for example, that would delay the reactor trip because the PORVs will open at 2350 psi; the reactor trip set point is about 24-25 psi. So that PORVs opening and relieving steam at 2350 for a few seconds would delay the reactor trip for a few seconds.

MR. WALLIS: That's because they like to keep the reactor running if they possibly can?

MR. MIRANDA: Yes. They put the reactor trip between the PORVs and the safety valves. The PORVs prevent the reactor trip, and the reactor trip prevents the safety valves from opening.

CHAIRMAN DENNING: I was going to let you get through your presentation, but I think that things have gone a little bit too far for the break. So why don't we take the break now and have you come back and finish? So we will recess until 10 minutes before the hour.

(Whereupon, the foregoing matter went off the record at 10:35 a.m. and went back on the record at 10:51 a.m.)

CHAIRMAN DENNING: All right, we're going to come back in session now, please.

Proceed.

MR. MIRANDA: We had some discussion about this earlier. The licensee submittal contains three transients. The first two are examples and really are two of a series of something like 50 or 60 cases that are done for the rod withdrawal at power, basically, to map the reactor protection system area of coverage for this event in terms of reactivity insertion rates.

Now these notations that you see here are the result of some errors in the license amendment request. The first case is not a maximum case; it is a minimum reactivity feedback case.

The times of reactor trip and minimum DNBR are the times that you will see on the curve. The times were originally printed for another curve.

The same thing with the slow reactivity insertion rate, 5 pcm per second, the second case. That is a really a maximum feedback case. Those are the times of reactor trip and minimum DNBR.

These two examples of transients are taken one at a high reactivity insertion rate, one at a low reactivity insertion rate, to illustrate a transient that is protected by the high-flux trip and another one that is protected by the over temperature delta T trip.

Finally, Ginna submitted a transient to show that the rod withdrawal at power event would not violate the reactor coolant system pressure acceptance criteria.

Maybe I should mention that DNB ratio at this time. The DNBR ratio for the rod withdrawal at power that was listed at 1.381, that is not really comparable to the DNB ratio that you find from the loss-of-flow accident, the 1.385. That 1.385 value comes from VIPRE results, and the 1.381 number comes from RETRAN results. The 1.381 is really an estimate of DNB ratio based upon insensitivity of DNB ratio to changes in power, temperature, and pressure -- yes, power, temperature, and pressure all taken at a constant flow.

So that 1.381 value from RETRAN is conservatively underestimated. That value, if those same conditions of power, temperature, and pressure were to be input to VIPRE, the DNB ratio would be higher than 1.381.

MR. WALLIS: This is because RETRAN is predicting the average behavior? Is that what it is?

MR. MIRANDA: It is an estimate. RETRAN is calculating transient conditions for power, temperature, and pressure.

MR. WALLIS: But they are all average? They are all --

MR. MIRANDA: Well, no, they're not all average.

MR. WALLIS: That's total power? Okay.

MR. MIRANDA: It will calculate the

average power, but then it will also calculate pressure at various points in the reactor coolant system. It will calculate temperature --

MR. WALLIS: But it doesn't deal with hot rods and things like that?

MR. MIRANDA: Oh, no, it doesn't have that kind of resolution. That is what VIPRE is for. So it takes the average conditions and puts them into VIPRE for the DNBR evaluation.

MR. WALLIS: Why was it not put into VIPRE?

MR. MIRANDA: Why was what?

MR. WALLIS: I mean in the other case they did use VIPRE, didn't they?

MR. MIRANDA: The loss of flow, they did use VIPRE.

MR. WALLIS: Yes. So why did they not use it in this case?

MR. MIRANDA: Well, they can't do that because the DNBR estimate routine in RETRAN is all based on the core limits, and the core limits are at a constant flow rate.

MR. WALLIS: I thought last time they took the RETRAN and then fed it into VIPRE.

MR. MIRANDA: In the loss of flow they do that, yes.

MR. WALLIS: They couldn't have done it this time, too?

MR. MIRANDA: They could have done it. It would have taken longer.

MR. WALLIS: Time is of no matter when you're satisfying ACRS.

(Laughter.)

MR. MIRANDA: The limiting event is not the rod withdrawal at power; it is the loss of flow. The rod withdrawal at power has a 1.381 value.

MR. WALLIS: So you think that this is very conservative? It really should be higher than that? Okay.

MR. MIRANDA: It will be much higher than that.

Chris, did you want to say something?

MR. McHUGH: No.

MR. MIRANDA: Okay.

MR. WALLIS: Well, I think it would have been good for them to have done it and got a better number. Then we wouldn't have asked so many questions about it.

(Laughter.)

MR. MIRANDA: Well, it is a little bit

misleading because you think you are comparing apples and apples and you're not. They come from different places.

This is the rest of the sequence of events tables and the --

MR. WALLIS: Now this pressure that comes so close, is, again, this because the pressure is relieved by safety valves? Is that why?

MR. DUNNE: It's both -- the pressure is really controlled by the safety valves lifting and when the reactor trips.

MR. WALLIS: So we shouldn't be so concerned about it coming up to a limit?

MR. DUNNE: No. That's right.

MR. WALLIS: That is why the safety valves are there.

MR. DUNNE: Yes, that's why the safety valves are there, and you get full opening on the valves to get full flow and you figure out what your parameters are for --

MR. WALLIS: And you have enough valves and they are reliable and all that sort of stuff?

MR. MIRANDA: Yes, that is all conditioned on the valves relieving steam. As long as the pressurizer doesn't fill and you open the valves as designed, they release steam and they load the pressure --

MR. DUNNE: And as long as the safety valves open within the stated tolerance on them, your pressure is really limited by that, and it is not really that sensitive to the code itself.

MR. WALLIS: If this were PRA, we would be looking at the probability of those valves opening, wouldn't we? Here you just assume they do?

MR. DUNNE: Well, we actually go in and test our safety valves.

MR. WALLIS: I know that.

MR. DUNNE: We basically change out our safety valves every refueling outage. We've got two sets of safety valves.

MR. WALLIS: But for this analysis you assume they open?

MR. DUNNE: Yes.

MR. WALLIS: In this design basis accident event?

MR. SIEBER: Well, they are safety degrade, too.

CHAIRMAN DENNING: Yes, but in PRA space safety --

MR. DUNNE: They are basically the code

valves required to basically prevent over-pressurization of the --

MEMBER KRESS: Failure to open in the PRA space is like one times 10 to the minus 3.

MR. WALLIS: Okay, there is a probability though.

CHAIRMAN DENNING: I don't think on the failure to open --

MEMBER KRESS: About 10 to the minus 4 failure.

MR. WALLIS: Okay.

MR. MIRANDA: This is the transient for the first case. The high neutron flux signal is reached at about a little more than one second, and the rods begin to fall a half a second later. The rods begin to fall about here.

MR. WALLIS: Where is this?

MR. MIRANDA: The DNB ratio occurs at 2.26 seconds.

MR. WALLIS: Something we don't have, right? That's something we don't have. We don't have that upper curve.

MR. MIRANDA: You don't have this one?

CHAIRMAN DENNING: We have the lower curve but not the upper curve for some reason.

MR. MIRANDA: All right. We will copy for that.

This is the behavior in pressurizer water volume and pressure. Here we verify that the pressurizer doesn't fill. In fact, in this case the PORVs don't even open or they wouldn't open.

Since we are looking for a low DNB ratio, if the PORVs were supposed to open, if the pressure were to reach the PORV opening set point, they would open. They would be assumed to open.

This is the minimum DNB ratio occurring at 2.26 right there.

Then, as an example for low reactivity insertion rate, 5 pcm per second, this is a transient that would be protected by the over temperature delta T trip. That occurs at about 214 seconds, and you can see where that is.

MR. WALLIS: So it just slowly creeps up in power?

MR. MIRANDA: Yes. As you approach the core limit, as you approach that 1.38, the over temperature delta T trip tripped the plant.

MR. WALLIS: Would the operator do nothing all this time when it is creeping up in power?

MR. GILLON: Yes, this is Roy Gillon

again, Reactor Operator.

Yes, we are aware of 214 seconds' change in power, PPCS, our computer systems, and both observation of the control board. So this would be hard to believe that the operator wouldn't terminate this within 30 seconds.

MR. WALLIS: Before the temperature does, yes.

MR. GILLON: Right. We would see temperature increasing. We would see power increasing.

CHAIRMAN DENNING: It looks like the pressure has the water volume really increasing.

MR. WALLIS: Yes, what is this pressurizer up here?

MR. MIRANDA: The margin water level would increase since the reactor coolant system temperature is increasing, and, in fact, I have asked in the past licensees to show me a very low reactivity insertion rate because I look for this pressurizer water volume; I need to see a maximum value to be sure that it is not going to fill the pressurizer.

In real life a lot of these reactivity insertion rates are more limited than what you would see in these analyses because, on the one hand, on the high end you just don't have the differential rod worth and the rod speed to get to that 100 pcm per second. Also, on the low end or for a long transient like this, for 200-and-some seconds, chances are that you are just going to reach the end of the rod. I mean the rods are at various insertion limits. You are going to pull it out and the reactivity insertion will end, and very often without a reactor trip. You will just have a new equilibrium power level.

Here's the average temperature. You can see it looks like the pressurizer volume curve, and there's the DNB ratio slowly dropping to its minimum value where the reactor trip occurs.

These are the results. Of all of the cases that were run, something like 50 or 60 or 70 cases, at different reactivity insertion rates with maximum feedback and minimum feedback at three different power levels. So these are the results for the 100 percent power cases.

We see from this curve that the low reactivity insertion rate cases are protected by the over temperature delta T trip, and the high reactivity insertion rate cases are protected by the high flux trip. We also see what the minimum value of the DNB ratio is. These DNB ratios, again, are from RETRAN.

MR. WALLIS: So you have to have things just right to get one of these valleys? You have to have just the right reactivity insertion rate to be in the region where you get near the minimum?

MR. MIRANDA: Well, actually, these curves, there's something that is not shown on these curves. That is, when you do these cases, for example, this curve actually continues. This curve here would continue. This is the intersection. That's where they stop.

MR. WALLIS: Wait a minute. I don't understand that.

MR. MIRANDA: They do other analyses. They would do other cases. They don't know when this is going to occur, when this minimum is going to occur. They would do a whole series of cases, and there may be some cases down here that are not reported because they are covered --

MR. WALLIS: They wouldn't get there?

MR. MIRANDA: They wouldn't get there, yes.

MR. WALLIS: Okay.

MEMBER MAYNARD: But I think you're right; it takes just a very unique set of circumstances to hit one of the valleys that takes you down.

MR. McHUGH: It is Chris McHugh from Westinghouse.

We actually search for that valley. When we do our initial set of runs, we will do like 10, 20, 30, 40 pcm per second to determine where we are switching from high flux over temperature delta T, and then we do a finer mesh in between. We go down to single units, 12, 13, 14 pcm per second. So we hunt for that case.

MR. MIRANDA: That is in order to find a minimum DNB ratio.

These are the results at 60 percent power. These are not transient cases. This is a map of the minimum DNB ratio results.

MR. WALLIS: This is a lot of computation then.

MR. MIRANDA: Yes. Yes, you need a fast-running code like LOFTRAN or RETRAN. We just stack the cases one after the other, changing a single parameter like reactivity insertion rate.

MR. SIEBER: That is why you pick a number and don't do this every time. Otherwise, you would be doing it for every --

MR. FINLEY: That's right, yes.

MR. MIRANDA: And then one last case is

the pressure case. This one is at 55 pcm per second. I believe that is more realistic. That is about what you could get, right, for the Ginna?

MR. McHUGH: No, realistic value is around 30 pcm per second.

MR. MIRANDA: Thirty?

MR. McHUGH: Yes, that is the maximum that would still yield an acceptable pressurizer pressure. So we have instituted 55 pcm per second as a reload criteria and a reload limit that the core designer has to verify it is always going to be under that. The typical number is around 30.

MR. MIRANDA: So we have the reactor -- the high pressurizer pressure trip occurring in this case at about 13 seconds. Normally, if I were looking at a case of rod withdrawal at power cases, a series of cases, I would want to be sure that the protection occurs from either the high flux trip or the over temperature delta T trip because the parameter of interest is DNB ratio.

MR. WALLIS: Why does nuclear power start off so low in this plot?

MR. MIRANDA: This is an 8 percent power case.

MR. WALLIS: Oh, it's an 8 percent power?

Okay. I didn't look at it. Okay. I didn't look at the title there.

MR. MIRANDA: But since here we are looking at pressurizer pressure, the parameter of interest is pressure, and the protection comes from the high pressurizer pressure trip.

So we have the reactor trip here, and we have the PORVs opening at 2350. No, no, no. No PORVs, no PORVs in this case. This is a high pressure case; no PORVs.

So we have the reactor trip, the rods fall in two seconds later, about 15 seconds, and the safety valves open at about 2500 or a little bit higher than 2500. Then the limit is 2750, right about there.

MR. WALLIS: So the safety valves open and the pressure keeps rising for a while, and then --

MR. DUNNE: Well, I think what happens is the safety valve set pressure is actually biased up from a nominal 2500, so they really don't open up until about 2600.

MR. WALLIS: Until that peak is there.

MR. DUNNE: I think where the pressure falls is probably where the safety valves actually did open, would be my guess.

MR. WALLIS: They open pretty quickly?

MR. DUNNE: Yes.

MR. WALLIS: And they relieve pressure right away?

MR. DUNNE: They're 15 milliseconds, something like that.

MR. WALLIS: Right. So I would think the peak would be when they open.

MR. DUNNE: That's what I would expect, the peak, because, again, we biased the safety valve opening upward based tolerances on the set point and loop seal time delay and other parameters.

MR. MIRANDA: That's all I have.

CHAIRMAN DENNING: Very good. That is very helpful.

MR. WALLIS: Do you have some strange logic with all kinds of time constants in it and things that sets these response to signals and opening valves?

MR. DUNNE: I'm sorry. For the safety valves, there is no logic. It is just a spring --

MR. MIRANDA: It is spring-loaded.

MR. WALLIS: So I would think your maximum pressure would be the set pressure on the valve.

MR. DUNNE: That is correct.

MR. WALLIS: There's no control involved

at all.

MR. DUNNE: That is why there really isn't a lot of variation in what the pressure is.

MR. SIEBER: There is some uncertainty about what that set pressure --

MR. WALLIS: This is just a little bit?

MR. DUNNE: Right, yes.

MR. WALLIS: This is a little bit. But we shouldn't be surprised that the pressure is about where you set it.

MR. DUNNE: Right.

MR. SIEBER: Do you heat the loop seal at all?

MR. DUNNE: Yes, we do. We have a hot loop seal around 300 degrees.

MR. SIEBER: Keeps it from looking like a steel bullet.

MR. DUNNE: That is to protect the downstream piping from a cold water slug if the safety valves actuate.

MR. SIEBER: Three hundred degrees?

MR. DUNNE: I think it is around 300 degrees. What we have actually done is the piping from the pressurizer nozzle to the safety valve is inside the pressurizer insulation.

MR. SIEBER: Okay.

MR. WALLIS: Well, cold water slugs can be quite interesting.

MR. SIEBER: Only once.

MR. DUNNE: That's the reason why we heat them.

MR. SIEBER: Only once are they interesting.

CHAIRMAN DENNING: Okay, we are going to keep going. We are going to move ahead with the small break LOCAs now.

MR. WALLIS: I'm amazed that we're under time. We seem to have asked a lot of questions, and yet we are still within time.

CHAIRMAN DENNING: I think we got through their presentation early, quickly.

MR. FINLEY: Mark Finley again.

Two analytical areas had not yet been reviewed by NRC when we last met. So we will discuss this morning both the small break and the long-term cooling analyses, and then Len Ward from NRC will discuss the same analyses.

In terms of an agenda for this presentation, we will talk a little bit about the Ginna design and why that is helpful in the small break LOCA analysis and then shift to talk about current and EPU results for small break LOCA analysis. You will see there is a significant margin here in these results. Then delve into the long-term cooling analysis with respect to the Ginna design and then both the large break and the small break long-term cooling analysis.

First, with respect to two key aspects of the Ginna design that help in small break LOCA, we have relatively high flow, high head safety injection pumps that start to kick in around 1400 psi and capacity conservatively above 1000 gpm. In terms of the power level of Ginna, the two-loop Westinghouse-type power level, this is significant flow at high pressure, and that helps the small break result.

In addition, we have relatively high-pressure accumulators which would start to discharge at around 700 psia.

MR. WALLIS: This is injection into the upper head?

MR. FINLEY: No, the high head safety -- and I'll talk more about that -- the high head safety pumps actually inject into the cold leg.

Yes?

MR. SIEBER: You don't use them as your

normal charging pump, do you?

MR. FINLEY: No, we don't use these in our normal charging pumps.

MR. SIEBER: What do you use for charging?

MR. DUNNE: Positive displacement pumps.

MR. SIEBER: Okay, like the Navy.

MR. FINLEY: Right. And we don't take credit here in this analysis for the charging flow.

MR. HARTZ: This is Josh Hartz of Westinghouse. I'm in charge of NOTRUMP.

Westinghouse basically has two different ECCS categories, high- and low-pressure plants. The Beaver Valley cases that you saw the other day would be what we would consider a high-pressure plant because they had safety grade charging plants. The two-loop plants do not have that capability. They've got dedicated SI pumps instead.

MR. DUNNE: This is Jim Dunne.

I think the big difference is that Beaver Valley's high head safety injection pumps can pump in against RCS pressure whereas our high head pumps can't. But it gives us more flow capability at the lower pressures.

MR. SIEBER: So you have to wait. Before you can inject at all, you have to have some blowdown?

MR. DUNNE: Pressurization of the RCS, yes.

MR. HARTZ: This is true, but the SI set point is typically around 1700. So even with the very small breaks, they depressurize quite quickly and go past that. So these pumps inject very quickly into the transient.

MR. FINLEY: Okay, on this slide you see the current results and the EPU results for small break LOCA Pclad temperature. Two key points to take away from this slide:

One is the EPU result, 1167, for the limiting break size, which I believe is two inches, right, Josh? --

MR. HARTZ: That is correct.

MR. FINLEY: -- is very low, 1167, quite a bit less than the 2200.

MR. WALLIS: Using a different method than the current method, is it?

MR. FINLEY: The method is the same. Both analyses use NOTRUMP methodology.

The second key point to take away, as you already allude to, Dr. Wallis, is that the current result is actually a little higher than the EPU result. That is unexpected, but it is due to a physical phenomenon in the NOTRUMP analysis that relates to loop seal clearing, which at the time in 1994 the analysis chose to leave alone because it was still an acceptable result by far.

MR. WALLIS: The prediction using this 9595 method or is this some other sort of conservative approach? What is the method that is used?

MR. HARTZ: This is Josh Hartz.

This is not a best-estimate approach. It is an Appendix K model.

MR. WALLIS: This is an Appendix K run? Okay.

MR. HARTZ: That's correct.

MR. WALLIS: So it is pretty low for Appendix K, isn't it?

MR. FINLEY: Yes, that's the point. Very low for Appendix K. A good deal of margin on small break LOCA.

I will also point out that you see the maximum transient oxidation there, .07 for EPU, well below the limit. We also add in the pre-transient oxidation level and we control that in the reload analysis to make sure the total stays below the 17 percent.

MR. SIEBER: Now this is for the worst-case small break? What size is this?

MR. FINLEY: That's correct. This is a 2-inch break, is the worst case for Ginna.

MR. SIEBER: Did you model in quarter-inch increments or?

MR. FINLEY: We did a spectrum of analyses using the standard Westinghouse method. I believe it was the 1.5-inch, a 2-inch, and a 3-inch break.

MR. SIEBER: That's pretty gross.

MR. FINLEY: We didn't go to the quarter-inch level. I think you saw Beaver Valley did that. The reason is because we have so much margin here. Because that Pclad temperature is so low, Westinghouse hasn't seen a large variation in the Pclad temperature at this low level.

Josh, you might be able to speak to that?

MR. HARTZ: Yes. Actually, in this case we did go off and look at quarter-inch intervals just to assure ourselves that that wouldn't be the case. Because when the whole issue of break spectrum up in the Beaver Valley analysis review, we wanted to make sure that everybody was captured in that regard. So we used Ginna as a test case to kind of confirm that, and it did not show much variation in the results.

That is mainly because this is not a

boiloff -- the boiloff turbine PCT plants are the ones that are sensitive to that. Beaver Valley would fit into that category.

MR. SIEBER: So you actually did do the work?

MR. HARTZ: Yes, we did. It would not be in Ginna's SER though.

MR. FINLEY: Yes, it was not a part of the licensing report, but they did that after the fact in response to requests for additional information.

MR. SIEBER: Basically, what you are saying is you didn't find much sensitivity with regard to break size?

MR. HARTZ: No. No, not for a plant of this type.

MR. SIEBER: Okay.

MR. WALLIS: Assuming a zero break size, though, is --

MR. SIEBER: That is one of the better breaks.

MR. WALLIS: Better points, right.

(Laughter.)

When you did the large break, you did use the 9595 method?

MR. FINLEY: That's correct. The large

break was the best estimate --

MR. WALLIS: Because you got better results, presumably, than using Appendix K?

MR. FINLEY: The large break for Ginna is the limiting LOCA, and we did need the --

MR. WALLIS: Here Appendix K is okay, and it's simplest, so you just did it?

MR. SIEBER: Was your accumulator pressure always 700 or is that a change?

MR. HARTZ: No, that's -- the two-loop plants have 100 psi higher design limit than the three- and four-loop plants.

MR. SIEBER: Okay, but that is all for large break protection?

MR. HARTZ: They do give you benefit in small break space, and that is one reason why the small break results are so good in this case, is because they are jumping into the transient even sooner. Because you go into a depressurization phase --

MR. SIEBER: Right.

MR. HARTZ: And once you hit the set point of the accumulators, they deliver enough water to terminate your heatup. So, yes, in small break space they do tend to help you out, especially more in the three-loop plants where we have the safety grade charging, and the flows to mitigate the accident aren't as marginal here.

MEMBER KRESS: I don't know if you can answer this or not. If you used the transition break size, could you have a substantial increase in power and still meet the rules?

MR. HARTZ: Are you referring to the 5046(a)?

MEMBER KRESS: Yes. I know you may not be prepared to answer that, but I was just curious.

MR. HARTZ: I guess in my judgment there would probably be some other accidents waiting to get into the way of that.

MEMBER KRESS: Waiting to catch you before --

MR. HARTZ: Yes. So in LOCA space they tend to do pretty well, the two-loop plants.

MR. WALLIS: This plant is large break LOCA-limited. So if you back off a bit on the large break LOCA criteria, you might gain a bit.

MR. HARTZ: It would open some things up. It is a possibility, but I think their large break results were pretty good to begin with compared to what some other plants would be.

MR. FINLEY: Right.

Okay, so just to summarize quickly, small break, a significant amount of margin to the acceptance criteria.

MR. WALLIS: In this case the safety analysis limit is a legal one, not one specified by the vendor and the licensee.

MR. FINLEY: That is correct. That is correct.

With respect to long-term cooling, some of the key aspects of the Ginna design that come into play: again, the high head safety injection pumps. These pumps are aligned to the cold leg.

We also have low head safety injection pumps. We call them residual heat removal pumps, RHR pumps. They are aligned to the upper plenum. I will show you a diagram in a second, the same nozzles that I think Jim Dunne had on his slide earlier.

But these inject directly into the upper plenum.

MR. WALLIS: Do you understand how the water gets down into the core from there? It is a counter-current-flow situation.

MR. FINLEY: Yes, actually --

MR. WALLIS: Because it has to be lopsided or something with flow down on the outside and steam coming up in the middle or something?

MR. FINLEY: Right. In fact, in a couple of slides I will show you physically where the nozzles are with respect to the core.

MR. WALLIS: Well, you've got water up there and it has to come down here.

MR. FINLEY: That's correct. That's correct.

MR. WALLIS: It is cold water, so the steam rushing up to condense on it, and so conceivably you have a CCFL-type situation.

MR. FINLEY: Right.

Gordon, click on that slide there and let's see what we've got.

All right, this just shows --

MR. WALLIS: We can see the hole.

MR. FINLEY: -- the elevation of the nozzle there in between the hot and the cold nozzle on the reactor vessel.

Next slide, Gordon.

MR. WALLIS: Yes, as far as into the --

MR. FINLEY: And here, the plan view shows where the nozzles would inject.

MR. WALLIS: I think it makes a pool up

there, as I remember. Doesn't it make a pool up in there? It fills up. Doesn't it fill up that plenum to some extent and then it somehow drains down in preferred locations?

MR. HARTZ: Dr. Wallis, you're probably referring to the early phases of a large break transient where you could be CCFL-limited in upper plenum, yes. Yes, but in the long-term cooling situation, the steaming rates --

MR. WALLIS: Okay, yes, I'm referring to a different situation.

MR. HARTZ: Yes.

MR. FINLEY: And I'll actually in a future slide --

MR. WALLIS: Do you understand that fully, do you?

MR. HARTZ: Yes.

MR. WALLIS: Of course you're going to say yes, I know.

(Laughter.)

It was a concern of mine at one time.

MR. HARTZ: Yes, with the UPI plants and with the licensing of SECY originally, that was a big concern, to mitigate the large break transient because of the water holdup in the upper plenum.

MR. FINLEY: And I will actually speak to this mixing assumption that we make with respect to long-term cooling in this UPI injection here in a couple of slides.

MR. WALLIS: You'll come to that?

MR. FINLEY: Yes.

MR. WALLIS: Okay.

MR. FINLEY: So the point here would be we have the high head SI pumps to the cold legs, the low head SI pumps to the upper plenum, and when they are both injecting simultaneously --

MR. WALLIS: These look like hot leg injection.

MR. FINLEY: That's correct. That's correct.

MR. WALLIS: You don't have to switch it on? It just happens?

MR. FINLEY: That is correct. It just happens. They are aligned permanently this way. We verify valve lineups and locked valves, and so forth, to make sure they inject in this manner.

Okay. And just fundamentally -- and I'm sure you talked about this some with Beaver Valley -- if you have the break on the hot side, you need the injection on the cold side to get the flush through the core, and the converse.

MR. WALLIS: You've got both of them.

MR. FINLEY: Say it again?

MR. WALLIS: You've got both of them here?

MR. FINLEY: That's correct.

MR. WALLIS: You're coming from both sides?

MR. FINLEY: That's correct.

Okay. Just to walk through the large break sequence here, of course, by definition, essentially, for the break size, the RCS rapidly depressurizes to below both the high head SI and the low head SI injection points. So you get the simultaneous injection early on, and that prevents any buildup early on of boron.

As the refueling water storage tank lowers, the level lowers, at that point we switch to the recirculation mode manually. At that point we actually turn off the high head safety injection pumps.

I am sure you would ask why, but fundamentally Ginna was not designed for simultaneous injection throughout the recirculation process. In fact, early on in the large break LOCA scenario the sump temperature is higher than would support the required NPSH that is needed to run simultaneous injection for the whole course of the recirculation.

So we turn off the high head SI pumps and then turn them back on. What we have verified through this long-term cooling analysis is that we turn them back on prior to the point that we would have concentrated then to the saturation point for boron.

MR. SIEBER: How much time is that?

MR. FINLEY: And I'll get to that in the next slide.

The other point to make here -- and I will show it on the next slide in terms of a better view -- but, conservatively, we don't take credit for the upper plenum injection essentially mixing with the core volume region to prevent concentration of the boron. That is a very, very conservative assumption.

Then the operators procedurally will restart those high head safety injection pumps to again restore simultaneous injection.

Gordon, if you will go to the next slide?

In terms of the analysis that was done -- and this was in response to the NRC's staff questions. As you probably are aware, they questioned, how are we determining what the void fraction in that water in the core region is and exactly how are we calculating the two-phased level and the volume, the mixing volume. Those were good questions that we really had simplified in the past.

But in response to those questions, this time we did an analysis using the Westinghouse COBRA/TRAC method to determine what the void fraction was and take account for that, as well as what the dynamic pressures are around the loop and how that affects the two-phase level. So all that is accounted for in this concentration analysis that was done.

Gordon, why don't you click on the first one?

Here is the void fraction versus time for a large break. You can see it starts up on the order of .75, .8, and down to just under .55 for the void fraction.

And next slide, Gordon.

Sort of the converse of that is the mixing volume. This is how, with that void fraction, the volume of water changes over time for the large break. So that now is calculated explicitly with the COBRA/TRAC code.

MR. WALLIS: It is throwing away all the upper plenum injection water.

MR. FINLEY: I'll tell you what, let's

hold that thought. I will show you the control volume that we use.

MR. WALLIS: You are not taking credit for it in this volume?

MR. FINLEY: Right, we are not taking credit for any of the water coming in from the UPI up above after this point.

MR. WALLIS: So where does it go then? You just ignore it? Just ignore it?

MR. FINLEY: I will show you in a second, Doctor.

Next slide. Maybe the slide before there. There we go.

Here is a depiction of the mixing volume that is used. This is the expected condition. Actually, this was not what was used in the analysis but what would be expected would be that you would get some upper plenum injection that would then mix with this entire region, both in the core region and in the upper plenum. Because this is obviously a very turbulent region, there is a lot of boiling go on, we would expect significant mixing here. Then, of course, some amount of that is out the break.

Gordon, go to the next.

MR. WALLIS: So you are assuming the SI

flow just gets washed out in the break?

MR. FINLEY: Right, correct.

So next slide, Gordon.

What we do, very conservatively, is take this mixing volume right at the bottom of the hot leg here, and then we assume the only upper plenum injection flow that crosses the boundary is enough flow to replace the boiloff, the steam that boils off. Obviously, very conservative.

The rest of the upper plenum injection flow is assumed to go out the break, carried out the break with the steam.

MR. WALLIS: In reality, it is intercepted by all those control rod tubes and things?

MR. FINLEY: Right.

MR. WALLIS: And it drains down on them?

MR. FINLEY: The guide tubes, the rods, and so forth.

MR. WALLIS: The guide tubes and things.

MR. FINLEY: All that interference is going to cause; plus, this is not a uniform, these assemblies are not producing uniform decay heat. So you will get some hot assemblies with more steaming and cooler assemblies with less steaming. All that would tend to drive mixing across this boundary, a significant amount of mixing. But we don't take credit for that, haven't taken credit for that.

CHAIRMAN DENNING: Now I am missing some element of that, and that is, so that the amount that is going from the upper plenum injection down is matching exactly the steaming rate? Is that what is going on? Does that mean that you have no water in that period coming from the annulus? From the downcomer?

MR. FINLEY: Right, right. This particular break, this is a hot side break. This is prior to the SI pumps being started, restarted. So we have no flow coming in from the cold legs at this point in time.

MR. WALLIS: Well, you might have negative flow, wouldn't you? If you have enough pressure drop out the break, you might actually depress the level in the core, wouldn't you?

MR. FINLEY: Right. We have adequate flow here from upper plenum injection to replace the boiloff. Again, the level is calculated dynamically with that COBRA/TRAC code, so that we know exactly what the pressure drops and the manometer effect around the loop is doing to the two-phased level.

MR. WALLIS: I was just concerned about

taking too much of this safety injection out the break and produce a back pressure that actually depresses the level in the core.

MR. FINLEY: Essentially, we maintain a two-phased level in the core region, which just reflects that the pressure drops due to steam flow out the break, yes.

MR. WALLIS: All right. And SI flow?

MR. FINLEY: That is all calculated dynamically now.

MR. WALLIS: And SI flow, too, isn't it?

MR. FINLEY: Well, right now we don't have the SI flow. This is the period of time while the SI is turned off and we are calculating an increase in boron with the SI --

MR. WALLIS: So the figure doesn't apply then?

MR. FINLEY: Right. As soon as we kick the SI pumps on and then we get flow --

MR. WALLIS: Oh, I'm sorry, SI is a different thing. I mean the UPI, the UPI.

MR. DUNNE: Between low head and high head SI.

MR. FINLEY: I'm sorry. We don't have the high head SI pumps on yet in this particular diagram.

Once they are turned on, you would get the flow in the cold leg and then up through the core.

MR. WALLIS: It is the UPI flow I mean. That produces pressure to drop out at the break --

MR. FINLEY: Right.

MR. WALLIS: -- which can depress the core level, can't it?

MR. FINLEY: The steam flow and the UPI flow together would produce --

MR. WALLIS: That would depress the core level?

MR. FINLEY: Right, that produces a --

MR. WALLIS: So it reduces your mixing volume?

MR. FINLEY: That is correct. We have taken that effect into account. That is correct, yes. Yes.

CHAIRMAN DENNING: Now, as you are talking about this, this is merely the calculation of how much boron is concentrating in this period? This is not something that you are doing with a dynamic code, computer code?

MR. FINLEY: I showed you previously the input that was taken from the dynamic code COBRA/TRAC --

CHAIRMAN DENNING: Yes.

MR. FINLEY: -- that related both to void fraction and mixing volume.

CHAIRMAN DENNING: Yes.

MR. FINLEY: That was then fed into, essentially, a hand-calculation methodology that conservatively bounded that input from the COBRA/TRAC calculation.

CHAIRMAN DENNING: Yes. So you ran the COBRA/TRAC through the entire scenario?

MR. FINLEY: Yes.

CHAIRMAN DENNING: And when you did that, you had some different behavior; that is, the amount of flow that was occurring from the upper plenum injection was probably not exactly matching what is going -- I mean, isn't it possible you had some flow coming down the downcomer at that stage, even though you had UPI injection and not SI injection or is that impossible? Or was there even negative flow through the lower plenum?

MR. FINLEY: Maybe you can help me out. I'm not sure if we had any flow in the SI -- excuse me -- in the cold leg or not.

MR. FINK: This is Dave Fink from Westinghouse.

Yes, what we did was we used a dynamic code simply to adjust our mixing volume, our control volume, to account for core voiding.

CHAIRMAN DENNING: But you ran your system code through the whole scenario, right? Forgetting about what is happening with boron, you ran it through the whole --

MR. FINK: Right.

CHAIRMAN DENNING: And so, as a function of time, you have temperatures in the core; you have void fraction in the core, and this kind of stuff? Right?

MR. FINK: That is correct. Correct.

CHAIRMAN DENNING: During this period we are talking about, was there any flow in the positive direction? I mean, was there any flow in the normal direction of water coming down the downcomer and up through the core or how was it --

MR. FINK: We didn't look at --

CHAIRMAN DENNING: How did you treat it?

MR. FINK: -- those particular regions. The problem as we have it outlined here is the stagnation, the stagnant pot. So under the classic three-loop/four-loop design, the stagnant pot has always been a cold leg break with overflow out the break.

For a UPI plant for the longest time we said there is no real stagnant pot scenario, but if you look at the way we conservatively outline the control volume, you would say, yes, there could be a stagnant pot scenario. That scenario is where the UPI flow crosses the upper plenum and goes out the break.

So in our dynamic code we didn't really look at what was happening in the downcomer.

MR. WALLIS: What we are concerned with here is not when it is stagnant but when it is in reverse flow, that the flow actually comes out into the downcomer, depresses the level in the core, and decreases your mixing volume.

Is that precluded by your analysis?

MR. FINK: Well, we are looking at an equilibrium condition clearly.

MR. WALLIS: It has to go all the way around the loop?

MR. FINK: That is correct. We did spend most of the time, most of the inspection of the COBRA/TRAC runs actually looking at what happens in the core region.

I see Mark put the slide up there.

MR. FINLEY: Yes, I just pulled this from

-- actually, it is an RAI response that we haven't formally sent in yet, but we have shown it in preliminary form to the staff, to document the flow the COBRA/TRAC would calculate over what we'll call the cold sections versus the hot sections in the core, where you actually see some downward flow over the cold sections of the core and upper flow over the hot sections, as you would expect.

MR. WALLIS: Average flow rate --

MR. FINLEY: So the average flow would be --

MR. WALLIS: Is the average flow zero or is it positive or negative?

MR. FINLEY: The average flow would be negative to replace -- correct me if I'm wrong -- would be negative to replace the steam flow, the boiloff.

MR. FINK: I think the answer to the original question, we would expect virtually no flow in the downcomer and up through the lower plenum because the flow would have to -- there is nowhere for anything to go. The equilibrium level --

MR. WALLIS: Yes, but if there was a pressure drop on it, it could be pushed one way or the other, couldn't it?

MR. FINLEY: Yes, but then it is just all water head laying on top of the core region, and it will tend to communicate that effect into the cold legs, but that water will quickly fill up and seek an equilibrium throughout the whole rest of the reactor coolant system.

MR. FINK: Yes, the problem statement is an equilibrium condition.

MR. FINLEY: Right. So we don't think there would be any significant flow in that cold leg without the SI pumps, the high head SI pumps running.

MR. FINK: I think on this slide here the thing that we are most interested in is, what happens in the COBRA/TRAC models, a hot core channel, and then peripheral channels. Clearly, what we see, as evidenced in this plot here, is you get significant upward flow in the center hot channels and significant downward flow in the outer channels.

The flow that actually crosses the upper plenum in the top of the core is like an order of magnitude more than the boiloff. So that shows that you have significant circulation within the core region.

MR. WALLIS: Completely independent of the effects of the boron density, and so on?

MR. FINK: That is correct.

MR. WALLIS: Which would enhance this perhaps.

MR. FINK: Perhaps.

One other thing to take into account here, the UPI flows are very high relative to the safety injection flow rates. I mean you are down at real low pressures at this point when these pumps are injecting. The volume flow rate is very high being delivered in this situation.

We are only assuming a little fraction of it for makeup, and then everything else is just getting discarded.

MR. FINLEY: Okay, so to carry on with the analysis, we do take credit for mixing of one-half of the lower plenum. We take credit for some of that volume, and that is based on testing that has been done previously. We think that is a conservative estimate of the amount of contribution you would get from the lower plenum.

We have calculated -- click on that slide there, Gordon --

CHAIRMAN DENNING: And you base that on the BACCHUS tests?

MR. FINLEY: That's correct.

CHAIRMAN DENNING: Is that what you meant?

MR. FINLEY: That's correct. We have calculated, based on that mixing volume assumption, the time to concentrate the boron, again, using the saturation limit that is associated with atmospheric pressure, a time to reach the saturation limit of approximately six hours and 13 minutes.

MR. WALLIS: But it is really unrealistic to assume that all that upper head injection, upper plenum injection, goes out the break and doesn't -- some of it doesn't go down to the core, especially since you've got this circulation pattern and everything going on.

MR. FINLEY: That is correct.

MR. SIEBER: If you don't know what the mixing really is, you are sort of forced to make that assumption.

MR. FINLEY: Right, right. And this we will say: We have enhanced this methodology greatly in response to some of the staff's recent questions. So I am sure down the road we are going to look at taking credit for those sorts of things. But because we were resolving this on the EPU schedule, we wanted to do it conservatively.

MR. WALLIS: Well, because it can be

resolved without allowing any of the water to come down, you don't worry about it?

MR. FINLEY: Right.

MR. WALLIS: But if it couldn't be resolved, then you might do a more realistic analysis?

MR. FINLEY: That is correct. That is correct.

Now I mentioned to you with respect to sump temperature we need to have the sump temperature come down somewhat in order for the operators to restart those safety injection pumps.

If you will look at this one slide here, we have calculated that at 190 degrees we have adequate NPSH, which occurs about four hours. Again, this is for the type of an accident that would maximize sump temperature.

CHAIRMAN DENNING: In this plant how are you getting your long-term cooling for containment in the sump? Is it through sprays and a heat exchange or on sprays or what is it?

MR. FINLEY: It is RHR pumps on recirculation.

MR. DUNNE: And containment is containment air coolers.

CHAIRMAN DENNING: You have safety grade

containment in those coolers?

MR. DUNNE: Yes, we do. Basically, we have a containment spray system and a containment air cooler system. We use both of them during the injection phase of LOCA. When we go into recirc, we basically terminate containment spray, when we transition to recirc, and we just use containment air coolers to do long-term cooling containment.

CHAIRMAN DENNING: Cooling the sump is occurring by cooling through the --

MR. DUNNE: Well, the sumps basically are low head SI pumps take their suction off the sump; they pump through a heat exchanger, and then that heat exchanger then delivers low head back to the RCS. We can also piggyback our SI pumps off the low head discharge coming out of basically mobile heat exchanges.

MR. FINLEY: Right. So the point of this slide is to show that at four hours we would be able to turn back on those SI, high head SI pumps, and procedurally we are going to set that time at four-and-a-half hours to make sure we have some margin here. Even at that four-and-a-half hours, that should be well before the time to conservatively saturate the core region with boron.

Next slide.

Okay, now we will shift gears to small break, a different scenario.

CHAIRMAN DENNING: A quick question, and that is, is it possible that for this plant we are overcomplicating things? I mean, as I look at the configuration here in this scenario, I mean the feeling is it is probably not a real scenario in terms of boron concentration. I don't know what reality is.

Here we are now requiring you to turn on SI at a particular point, but maybe that is not a big issue anyway, since you're not going to need the SI.

MR. FINLEY: Right.

CHAIRMAN DENNING: For it to go on too early and you lose the SI --

MR. FINLEY: This is conservative. We have made some changes to the analysis method here that we want to cautious about. We are doing it on a constrained schedule to support the EPU.

So it does not impact safe operation in terms of doing something that is not smart. So we felt that this was the right conservative approach.

Okay, with respect to small break, here the difference, the key difference is that the RCS will depressurize below the high head SI pressure but not below the upper plenum injection pressure. Remember, I said that that is around 140 psia for the upper plenum injection point.

So there are many small break sizes which won't cause you to rapidly depressurize below that 140 psi point. So the significant difference here is we need to take credit for operator action to help that depressurization process, which is really a part of our normal LOCA response procedures. That is nothing new. Operators are going to want to depressurize to stop an unisolatable lead regardless of the boron situation. So we are just taking credit for that in the boron scenario, as I will discuss.

So for the period of time that the low head SI pumps are not injecting to the upper plenum, we do expect there will be some concentration of the boron in the core region, where you have boiloff occurring and leaving behind boron. So we would expect some concentration there.

But the operators would depressurize the plant. Again, once you depressurize to below that upper plenum injection pressure, you would get a simultaneous injection setup, both from the upper plenum and the cold legs. That would flush the core for a break on either side.

Okay, next slide.

With respect to the analysis that was done, again, we used the dynamic, in this case, NOTRUMP analysis methodology to calculate the core voiding and the mixing level, et cetera, to feed into the concentration study.

A 4-inch break was conservatively used to bound all of the small breaks in this particular study. We didn't take credit for any beneficial effect of sump additives. We have sodium hydroxide added, and that would have a beneficial effect. We did not take credit for that.

We calculated a time to reach the boric acid solubility limit of six hours and 48 minutes, assuming that the solubility limit is established based on atmospheric pressure conditions.

Gordon, if you would click on that one slide?

So here a similar curve that you saw for large break; this is for small break. As long as we initiate the upper plenum injection prior to six hours and 48 minutes, we would stop the concentration process at about 29 weight percent, and that's the limit that corresponds to the atmospheric pressure condition.

MR. WALLIS: Stopped because the UPI now flows through the core?

MR. FINLEY: That's correct. That is correct.

Okay, click on this one here, Gordon.

So it is important now for the operators to depressurize the plant prior to that six-hour-and-48 timeframe. So what we did is, again using the NOTRUMP analysis methodology and taking credit for the operator actions, conservatively taking credit for the operator actions that would occur in the EOP response, we would get below the upper plenum injection point within about five, five-and-a-half hours.

So at that point, without any further action, essentially, the upper plenum injection would kick in based on the RHR pump shutoff head.

MR. SIEBER: How do the operators depressurize the plant? What do they do?

MR. FINLEY: The first choice for the operators would be to use the steam dump system. That is not what we used here. Of course, steam dumps would require offsite power availability and condenser vacuum.

MR. SIEBER: Right.

MR. FINLEY: So what we model here is

atmospheric dump valves. So they would use the atmospheric dump valves next after the steam dumps, and if they were to fail, then we would revert to use of PORVs.

Next slide, please.

So to summarize, we feel the Ginna design is robust with respect to having the upper plenum injection point as part of the two-loop Westinghouse design.

We have significantly upgraded the analysis to address the staff concerns with respect to void fraction, mixing volume, and decay heat. I didn't mention the fact that the staff questioned the uncertainty value used on decay heat. Essentially, we used the Appendix K uncertainty for decay heat, and that will prevent boric acid precipitation based on the design and the operator response in the LOCA procedures.

Any questions?

(No response.)

Then I will turn it over to Len Ward.

CHAIRMAN DENNING: I think we will probably take our break now. Instead of doing that, we will take our break. We will take our lunch break right now, and we will pick up at 10 minutes before

1:00.

(Whereupon, the foregoing matter went off the record at 11:50 a.m. for lunch and went back on the record at 12:51 p.m.)

CHAIRMAN DENNING: I think we are ready to restart. So you can just go right ahead, please.

MR. WARD: I am basically going to talk about the same items, subjects, I did on Beaver Valley. It is just the equipment has changed; the objectives are still the same though.

So I am going to talk about, first, just quickly the ECCS design, show you a little picture on why the limiting break for a large break is different from the cold break. You know that, but I think it just helps to set up what I am going to say.

Then I will talk about large break LOCA. I am only going to talk about long-term cooling, and, of course, that is boron precipitation. You need to be able to remove decay heat for an extended period of time. It is criteria five. In order to do that, you've got to put in more water than you are boiling. Then you have to make sure the boron, the boric acid doesn't precipitate.

For small breaks, I will talk about short-term behavior. Again, that is PCT, clad oxidation.

Then I will also talk about boron precipitation for that because it is an issue for small breaks as well.

Then we can summarize with some conclusions.

Ginna is a two-loop plant. This plant is different from all the other plants in that it has an upper plenum injection system that delivers low-pressure flow through two ports into the upper plenum. Then it has cold leg injection. They call it high head safety injection. That is delivered to the cold legs.

So the operators don't have to realign HHSI. All they've got to do is make sure the pressure is low enough to get that low pressure pump on, and then they will have a flushing situation.

Now they mentioned in the large break LOCA when the RWST drains, and that takes 24 minutes for the limiting large break, they turn off the high head pump. You've got low pressure injection going in.

So for the purposes of a boron precipitation calculation, that break is going to be worse because we are going to make the assumption that it doesn't flush the core. There is water going in that keeps it covered, but we are going to assume it concentrates. We are not going to take credit for any of the circulation, if that exists. So we are going to try to do a bounding calculation there.

Before I get into the picture, I think you saw this. Here's the high head safety injection pump. It has a shutoff head of around 1400 pounds.

This is the important one. It is the low pressure. I guess they call it RHR.

This is the curve and this is how I received it. So this is what I put in the code. I think the flow really would behave this way, but we are assuming that there is no flow -- you've got to get the pressure below 140 pounds to get the system on. So for the small break where you've got to cool the plant down, that is the item we are going to be concerned with.

I think my analysis shows you are up in this range where I've got at six hours, I mean you are at 60 to 80 pounds per second. The boiloff is like 23. Remember this is a small plant. So just remember that is a key ingredient.

My cartoon here is not to scale. I am sure Sanjoy wouldn't like it, but it is simple.

This is at the wrong location, but I want to show that the UPI comes in the center line to the hot leg through two connections, and then you have hot side and high head safety injection coming into the cold legs.

So after 24 minutes in the large break, if you turn this off, the hot leg break would become limiting because there is no flow from the cold to the hot side. We are going to assume that any of the ECC coming in from the UPI doesn't flow in and mix and flush it out. We are just going to assume that it just replaces -- just keeps the core covered in concentrates. So that is why the hot leg break is going to be limiting for this plant.

MR. WALLIS: Now would you explain why the core is stagnant?

MR. WARD: Well, I can show you, explain why. The core is not really stagnant. It is boiling. Steam is rising and water is flowing down counter to it to replace the boiloff.

MR. WALLIS: Where is that flow coming in, though?

MR. WARD: If you will recall, they showed, the Ginna people showed a WCOBRA/TRAC calculation. That is their best-estimate calculation. I asked them to run that.

I will get to the reasons why. I mean

when you see when the boron starts to build up, but that is a few slides later.

What that calculation shows, the water going down the peripheral assemblies and rising up the center. So it is just sitting there circulating, replacing the water that is boiling off.

So the flow in the central part of the core is upflow, and the flow down is really cold peripheral bundles --

MR. WALLIS: If you look at the whole loop, conceivably, you could have this UPI coming in and the flow actually going up the downcomer and around.

CHAIRMAN DENNING: Well, actually, you can't.

MR. WARD: I don't see how you could get --

CHAIRMAN DENNING: We've got a hot leg break.

MR. WARD: Yes, it is a hot leg break.

CHAIRMAN DENNING: A hot leg break, right, and we are looking at large --

MR. WARD: Here's a 2-foot hole. There is a 2-foot hole right here. This is 14.7.

MR. WALLIS: Everything is the same

pressure?

MR. WARD: You've got cold side injection, and the first 24 minutes you've got forward flow. I mean everything is going to be pushed out.

MR. WALLIS: Well, that was my question. Everywhere at a certain level you get atmospheric pressure.

CHAIRMAN DENNING: Yes, and it can't go around the loops.

MR. WARD: In other words, what's on, just the UPI?

CHAIRMAN DENNING: Just the UPI is on.

MR. WARD: Okay. Well, the accumulators and HHSI pump have filled the system up. So any more water that I had in excess of the water is going to spill out the break.

MR. WALLIS: It can't push through the loop seal or something?

CHAIRMAN DENNING: No.

MR. WARD: No.

CHAIRMAN DENNING: Because you can't get over the steam generators --

MR. WARD: There's a steam generator here. It has got to flow over the steam generator to get to the loop seal. There is just a water level, there is a weir here. So it is going to sit.

CHAIRMAN DENNING: So it is really stagnant there in this case where --

MR. WARD: Unless you boil off the water -- maybe if you've got some wall heat on that side and you boil off a little bit, I think you could get some oscillations, and then that would probably promote mixing. But I don't want -- they are not going to take credit for that. I just want it to buildup -- let's try to make this the worst -- let's beat it to death. That is what I am trying to do.

These are all good questions.

MR. WALLIS: So there is no way the water can go up and spill over that loop seal until that loop seal -- is the loop seal full of water, too? Does the water level --

MR. WARD: Remember we've got a hot leg break. There's no steam binding problem here. The steam that is building up in the core, where does it go? It goes out this huge hole.

MR. WALLIS: So everything there is at atmospheric pressure?

MR. WARD: Yes, I am assuming we are at 14.7 in this guy right here, 14.7 everywhere.

MR. WALLIS: How about the other way? The other way is --

CHAIRMAN DENNING: You mean the other hot leg?

MR. WARD: Well, the other hot leg -- I mean you've got two hot legs. I mean the steam is going out that hole in the hot leg.

MR. WALLIS: So I suppose as long as it is a big break this is okay?

MR. WARD: This is a double-ended break, yes.

MR. WALLIS: Okay.

CHAIRMAN DENNING: Well, actually, we did miss the possibility of steaming going up into the steam generator, condensing in the steam generator.

MR. WARD: The path of least resistance is probably right out the side and then just flow down a hot leg, go up a bend, and then contract and get into those tubes. I think it is going to go out the hole.

CHAIRMAN DENNING: But you absolutely rely on water recirculating back into the core? Otherwise, there is no way to keep the core cool.

MR. WARD: Right. The key ingredient here is the LPSI pump, this UPI pump is putting in far more water than you are boiling.

CHAIRMAN DENNING: Yes. It can flow down

some way to get into the core.

MR. WARD: It is going to spill out that hole.

MR. WALLIS: It will fill up the vessel, won't it?

MR. WARD: Yes, sure.

MR. WALLIS: So just lower the curtain and end the play.

MR. WARD: Right. That's right.

CHAIRMAN DENNING: That is a good question.

MR. WARD: So for large breaks, what do they need to do since you turn off the high pressure pump once the RWST drains? They've got to turn it back on, and you've got to turn it back on before you would predict precipitation. It is simple.

They don't have to split the --

MR. WALLIS: But you are foolishly throwing away the other water, aren't we?

MR. WARD: Yes. But now for small breaks, the pressure -- you have to remember in the large break it gets down below 140 pounds, but for a small break you can be above 140 pounds for a long time. So what do you want it to flush the core in order to get both systems working? Remember the HPSI pumps work or that high pressure pump is working in the beginning. We need to get the pressure down so we can get that other pump from the hot side, so that if the break is on the cold or the hot side, it will just flush.

So the key ingredient there is to cool the plant down, and that is where the operator actions come in. Long-term cooling is different than short-term behavior PCT. The ECC is designed to keep the temperatures low. The operators should just verify everything is on and diagnosis. They shouldn't have to take any action.

In the long-term cooling they've got to do things. So to control boric acid, that is on the operators' shoulders. It is up to them to make it work. That is why we are focusing on this.

This being a particularly different plant, we had them do a lot of calculations. Let me talk about the large break model.

You've seen the same model in the original submittal that went back, the long-term cooling -- the large break LOCA analysis was very crude. They used a decay heat multiplier of one. They assumed the whole mixing line was full of liquid.

We didn't like that. So we said, hey, let's step back and let's do a little bit better calculation.

So they went and they did the calculation where they justified their mixing volume, took credit for the void fraction, so it is not solid liquid.

Now we are also using the same precipitation limit, 29 percent, and that is 14.7.

MEMBER KRESS: How good do we know that number?

MR. WARD: What, that?

MEMBER KRESS: Yes.

MR. WARD: How good do you know that?

CHAIRMAN DENNING: Well, for pure boric acid you know it well.

MR. WARD: I've got a curve from the boric -- from the borax company. I will just show you what it looks like.

They have measured the precipitation limit as a function of temperature. We are down here around 29 percent, 212. If you've got additives, it is up here.

So we are essentially using this. We are using the data from this.

MR. WALLIS: Is this the same borax I can buy in the supermarket?

MR. WARD: It probably is.

MEMBER KRESS: Twenty Mule Team.

MR. WARD: I think it is.

MR. WALLIS: Twenty Mule Team, yes.

MR. WARD: It is.

So you will recall this is the calculation I did, and it says, "delay" on it. You will notice that it doesn't start until 24 minutes. I will show you another curve, but if you assume the boron builds up from time zero, you are going to precipitate in four-and-a-half, 4.8 hours.

I was really confused: How are they getting this six hours and 13 minutes? I couldn't figure it out until we finally talked enough and finally he says, "Oh, wait a minute. We're not letting buildup until 24 minutes."

The reason, the logic for that is during the initial portion of the large break LOCA I have high pressure pumps on; I have a hot leg break. There's a lot of forward flow. You are depressurizing in that upper plenum. It fills up. It is probably going to concentrate within maybe the first several hundred seconds.

But once you fill that vessel up, you've got 80 pounds per second going on in one side and of the order of 80 or 90 pounds going out the other side.

So you are not going to build up boron in the first 24 minutes.

I asked them to do a calculation to prove that. They went and exercised their best estimate LOCA model, the large break LOCA code. That code has UPI models that were reviewed. It has de-entrainment on the guide tubes. It has entrainment phenomena that sweeps out drops. The droplet size distribution is based on data for spraying horizontal jet of UPI into a vertical column of guide tubes. Those models are all in there, and it's got CCFL limits. If the steam is too high, it won't let liquid go down.

So they ran that. They ran that code in an Appendix K mode.

MR. WALLIS: Let's put this in perspective. It starts off at 2400 parts per million, is that right?

MR. WARD: It starts off around, it is 3050 parts per million.

MR. WALLIS: What's that? So that's point --

MR. WARD: It is like 1.5, something like that, 1.7.

MR. WALLIS: One point five percent. It is not .3 percent.

MR. WARD: Yes, it is something like that.

MR. WALLIS: So I can't take parts per million and get percent directly.

MR. WARD: Divide by 1748. Take the ppm --

MR. WALLIS: Okay, so it is 1.5 percent or something?

MR. WARD: Right.

MR. WALLIS: And I'm going to concentrate it to 30 percent. So I've got to drive off 20 times as much water as I leave behind?

MR. WARD: Well, no, it is going to concentrate at the rate it is boiling.

MR. WALLIS: Yes, but I mean to get 29 percent, I've got to drive off 19 parts in 20 of the water. For 20 gallons, I've got to boil it down to one gallon.

MR. WARD: Yes, something like that.

MR. WALLIS: It is a humongous amount of water I've got to boil off.

MR. WARD: Sure, there is.

MR. WALLIS: I've got to start with an enormous amount of water in order to finish up with something which is the amount of water you're ending up with in the vessel, which is concentrated to this.

MR. WARD: Right, and don't forget, you know, there's a high --

MR. WALLIS: So where does all of that water come from that I've driven off?

MR. WARD: The initial water that is there, the ECC injection.

MR. WALLIS: That's nowhere near enough.

MEMBER MAYNARD: Accumulators.

MR. WARD: You are putting in 80 pounds per second in the cold side, and what's the LPSI flow?

MR. WALLIS: It is all accumulating all that time?

MR. WARD: I mean, you've got a 700-pound accumulator in there.

MR. WALLIS: And you are boiling all that off?

MR. WARD: Right. I mean you've got two huge accumulators and they just --

MR. WALLIS: So you've got plenty of water in there?

MR. WARD: -- dump tons of water in there.

MEMBER MAYNARD: You're putting a lot of water in it.

MR. WARD: I'll show you when I get to the --

MR. WALLIS: Not as much water as you finish up with that you boiled away. That is a huge amount.

MR. SIEBER: A couple of hundred thousand gallons.

MEMBER KRESS: When you boil off at atmospheric pressure --

MR. WARD: Yes.

MEMBER KRESS: -- doesn't the steam take the boron with it?

MR. WARD: It does, but we're not --

MEMBER KRESS: You are not even going to account for that?

MR. WARD: That is not credited.

MEMBER KRESS: That might take your time way out.

MR. WARD: That is right, and there's entrainment, too, that is taking that liquid and --

MEMBER KRESS: Yes, not even counting the entrainment, no.

MR. WARD: No, I'm not counting that either. I'm not. Zero.

MEMBER KRESS: Okay, so that is another conservatism there?

MR. WARD: Right, and there's 20 percent

additional power on the decay heat.

So this calculation that I did reproduces the licensee calc.

I just want to show you, well, what happens if there is no delay? This is what I was getting originally, at or around 4.8 hours. This is what was confusing me.

But look at it this way: The additives, the precipitation limit is really up here with the additives and the containment. So even if it builds up from time zero and it wasn't flushed at all, you're still going to be okay. This is still going to take, well, it is going to take a long time. This is 20 percent more decay heat. If you subtract -- if you go to 1.0, it is even going to push you out farther. That's at 14.7.

So I think it is safe to say that there is some margin in that calculation.

MR. WALLIS: As long as it doesn't boil over when it gets to about 15 percent. Suppose its properties change so that it boils over like milk boiling in a pan. At 15 percent, then you have lost it.

MR. WARD: Well, none of the tests show that. You think it is going to do that?

MR. WALLIS: You don't know that yet. I don't think anyone has done tests to that high a concentration. It is stopped at a lower concentration than that.

MR. WARD: I have seen tests that have gone up to 32 weight percent, but I can't discuss it. I've seen it. Maybe we can talk afterwards.

MR. WALLIS: Okay.

MR. WARD: So let's go to the short-term behavior and let's jump back and let's look at PCT. In the original submittal they submitted three break sizes. That is obviously not enough to identify the peak, and the peak was found to be a 2-inch break. But with a Pclad temperature of 1167, I ran that calculation and I got around 1100 degrees.

This ECC system is probably the best I have seen. I have never seen a plant with 700-pound accumulators. Those accumulators come on real early. They keep the core from uncovering.

It is really a good design in that respect. It has got very high capacity, high pressure pumps compared to the boiloff. I mean you could pump the Atlantic Ocean through this core in about 10 minutes. It is why the core doesn't uncover. If I run this at 1.0, there's going to be no uncovery for this break specter. I am going to get no heatup.

So based on the calculations that we did, and what they did, there's really no need for them to go off and spend their time looking at these non-integer break sizes when at most it might increase the PCT by what, 100 degrees. I mean they are well below 1500.

So we said, "You don't need to submit that." They went and did it anyway. But we really didn't need it.

As a mater of fact, we had them look at some larger breaks because -- and I am going to show you this in a minute -- you turn the HPSI pump off during a small break. There is no injection. Here you've boiled the system down with levels in the hot and cold leg, not something that I really like, like to see, but they've done a lot of analysis.

As a matter of fact, they looked at these larger breaks and turned the pump off for 10 minutes because they have stated that they can make that switch in five minutes and certainly within ten. When you look at all these breaks, you see a drop in the level when they turn it off but the core doesn't uncover because of the fluid above the top of the core.

Even for these larger breaks, they didn't uncover and they didn't even take credit for the UPI, only the high pressure, and it still didn't uncover. So I liked that when I saw that.

Now we did calculations with Relap also, and I am going to show you one in a minute.

MR. SIEBER: So if the UPI is the break, that side of the break, you're still okay?

MR. WARD: Yes, I'm okay.

They also looked at severed ECC lines. When you have a severed ECC line, you have one line that sees 14.7 and the other one that might see 800 pounds. So you are not going to lose half the flow. You are probably going to lose more than that. Those were not limiting also.

Now we confirmed this with a Relap5 calculation, ran the 2-inch, ran a lot of breaks. Of course, we were 1811 megawatts and 17.5 kilowatts per foot.

Again, I said we confirmed that breaks on the top of the cold leg, where you can fill the loop seal out, didn't depress the level into the core, nor did severed ECC lines become more limiting.

But the key here is you've got to reinitiate that high pressure pump within 15 minutes, and I will show you why in a minute.

One of the things that you are going to see in the calculation is I got a CHF condition again. As I mentioned before, I have been talking with Josh Hartz at Westinghouse. I think it is probably a combination, as I said before, between assumptions and differences in the code. Maybe our code is more conservative. Maybe the resistance is in the hot bundle or maybe they are a little too high.

Nevertheless, I got a 1400-degree temperature. It is maybe close to 1500. But the point is the PCT still remains well below 10 CFR 5046 limits. But we really want to understand this, and if we have to pursue it further, we will.

CHAIRMAN DENNING: Now this is where you were saying you used the Relap?

MR. WARD: Yes, this is Relap, and I am going to show you this calculation.

I am looking at a 2-inch diameter break here and turn the pump off. This is about the time the RWST drains. Turn the pump off. This is a 2-inch break, cold leg break. Turned the pump off here around 7200 seconds, and in about 15 minutes the core uncovered. In about another 15 minutes it is 2200.

So they say they can perform the action in five minutes, no later than ten. This is 1.2 times ANS. They've probably got 20 minutes if you have this break in this location.

So it is very important that the EOP be emphasized and the training be emphasized with these operators to make sure that they can do that within five to ten minutes.

MR. FINLEY: Yes, this is Mark Finley again, the Project Director for the uprate.

Len is correct, and we have emphasized this in our procedures. They have the procedures set up now to emphasize to minimize the time that these pumps are off.

But I will make the point that you see we would terminate the high head SI pumps at around two hours into this event. So this is not happening five minutes after the break occurs. So there would be time here to ensure that the operators are briefed; they understand the actions that they have to take and would turn these pumps back on.

MR. WALLIS: Why do they turn off?

MR. WARD: Because not enough net positive suction head. That is for the large break. You've got to switch it to the sump.

MR. FINLEY: Right, we are shifting from

the injection phase to the --

MR. WARD: From the RWS -- they are starting from a tank and now they have got --

MR. WALLIS: You have drained that tank; now you have got to switch to the sump? So you have to realign the intake and everything?

MR. WARD: Yes.

MR. FINLEY: Right. There's three sets of valves that have to be repositioned. We feel very confident we can do that within five minutes.

MR. DUNNE: Yes, this is Jim Dunne from Ginna.

Basically, our ops procedures, urgency procedures, basically, tell our operators to basically turn off SI and then check RCS pressure. If RCS pressure is above a certain value, then they are told to restart SI pumps. In this mode for a small break LOCA that is what they would be doing. They would turn it off.

They probably at this point in time would already know what the RCS pressure is before they go into the recirc mode. So they would probably even make an assessment as to whether they really should be turning off the SI pumps or not.

But the ELPs are based upon symptoms. So they will check the RCS pressure, and if the RCS pressure is above a certain value, they are basically instructed by procedures to restarting that SI pump.

MR. WARD: And this break, bigger breaks, and I will show you what they look like --

MR. WALLIS: How is this affected by the EPU? We are talking about power uprate.

MR. WARD: Well, it is a higher power.

MR. WALLIS: Does something change? This picture is the same now. This is what they do now, isn't it?

MR. FINLEY: That's correct.

MR. WALLIS: How does it change by the EPU. Is it a shorter time period?

MR. WARD: They probably have a shorter amount of time before the core uncovers.

MR. WALLIS: Is it really a critically shorter amount of time or how does it change?

MR. WARD: You've probably got -- what's the power increase, about 20 percent? So five minutes maybe.

MR. WALLIS: So you do have a shorter time?

MR. WARD: It is decreased by five minutes.

MR. WALLIS: Which is significant.

MR. FINLEY: Like Len said, he calculates something on the order of 20 minutes, I think, before you would start to uncover again. So that time is shortened from, say, 25 minutes to 20 minutes as a result of the EPU, something on that order. But, again, we can make these actions within about five minutes.

MR. WALLIS: And has the net positive suction head changed as well because of the EPU?

MR. WARD: I think the containment, the sprays for this have been operating for this period. You've got cold water in there. You've filled it up.

MR. FINLEY: Right. That really only applies to the large break scenario.

MR. WARD: That is the large break where you're early, you're hot, and it is probably not a good thing to do.

MR. GILLON: This is Roy Gillon, Shift Manager.

We run a scenario multiple times a year in a simulator, and we have criteria. Typically, we can get this done in five-six minutes of time. We have never had any trouble getting it done in 10 minutes.

CHAIRMAN DENNING: And is there no option considered for depressurization to assure that your pressure is low enough to have the BPI?

MR. WARD: Well, there is. I am going to get to that.

They will initiate a depressurization with both ADVs and one out, cool the plant down now. I will show you, but this is the break. A break bigger than two inches gets the UPI on it. It is a moot point.

This is probably the biggest break where you are only going to have hot side high head injection. So if it is the biggest break, this is the earliest that it would occur with the highest of K heat. So I picked this one because this is the limiting one.

CHAIRMAN DENNING: But you are showing us a case in which they have not successfully depressurized.

MR. WARD: Yes, I will show you what happened.

MR. FINLEY: Let me just clarify. There's two independent sort of issues here. This relates to not turning the SI pumps back on in a timely fashion when you switch from the injection phase to the recirc phase.

CHAIRMAN DENNING: Yes, right.

MR. FINLEY: It really doesn't relate to the pressure in the RCS.

CHAIRMAN DENNING: Well, if you had depressurized and you had the UPI on, does it make any difference?

MR. FINLEY: Well, you are correct, if we could get down below 140 psi, but this is only about two hours in. We really can't get there for all the break sizes, right.

MR. WARD: Right, and that is why this one is limiting for that case, and you're right.

MR. DUNNE: If you did depressurizing down to below the UPI cut-in pressure, you would not see that interruption at all.

MR. WARD: Now I want to talk about long-term cooling for small breaks. The analysis shows that you can borrow for long periods of time, and because it is a small break, the pressure remains above the shutoff head of that low pressure injection pump. So what do you do?

Well, you need to reduce the pressure below 140 pounds to get the UPI on, or if you can't do that, then show that it refills. I will show you what that looks like in a minute in a slide.

Now what I asked them to do is -- there were no analyses of these breaks because of this plant. I want to know which breaks will you stay in natural circulations, which ones refill, which ones don't refill, and get UPI on, so we've covered all the bases.

So they did this detailed analysis. Below two inches the UPI comes on. So they did a pretty good job and a pretty detailed analysis, looking at all these with their -- this is their Appendix K small break NOTRUMP code.

MR. WALLIS: Below two inches or above two inches? You mean above two inches?

MR. WARD: I mean above. Yes, I'm sorry, above two inches. I'm sorry. You are right.

MR. WALLIS: That was just to test us, wasn't it?

MR. WARD: Yes, that was a test, wasn't it?

Now what our audit calculation shows is that for an 01 square foot break this is a 1.5-inch; this is about 1.3 inches. I think in terms of square feet. I don't like inches. So I have got square feet here.

But in 2.8 hours this break refills, and this little larger break refills in about four hours.

Now the other thing I looked at is when I said, gee, what if I fail one of those ADVs? Well, I've got two PORVs. What does the system look like under that condition? I will show you that in a minute.

Let me show this critical break size range that I could call for small breaks. We are looking at 2 inches, 1.5, 1.3. This is RCS pressure.

Now there is a 2000-second steady state, and I didn't subtract that off, but the break opens at 2000 seconds.

Operators open both ADVs at this point and start cooling the plant down. You can see if I have a 1.3-inch break, if I refill and resubpool the system somewhere in here -- a bigger break takes a little longer. I'm out here maybe four hours. If you look at the void fraction in the core, it goes to zero for this 1.5-inch break and it will go to zero back here for this slightly smaller break.

Now if I look at a 2-inch break, I am depressurizing, but what happens is I get down below 100 pounds. So I am right in here. So the UPI is on. So I am fine.

Bigger breaks, depressurize faster. I get more and more flow. Smaller breaks will refill earlier, and you will probably repressurize up near 1400 at some point because the break is so small. So the operator will see that response.

All breaks from roughly two inches down will refill and resubpool and disperse the boric acid. Good system response.

Now I am going to say, what happens if we only have -- I'm looking at a double failure here. I just wanted to see what this looked like. This is that 1.5-inch break. I have one ADV and I am only opening up two PORVs, and I am hanging up in pressure for a while. Let's blow that up. So I am out eight hours.

Actually, what is happening is the low pressure pump is coming on here. This is about 140 pounds. I would like to see it get down around 120 pounds because now you are getting a lot of flow in there and it is flushing. It is flushing, okay, but I am out probably eight hours.

But the point is, if I have delayed the cooldown and I am coming out here and it is a slow -- it is at a high temperature, there's a high limit. It is not 29. It is 35, 40. As a matter of fact, in this case it is probably greater than 50 percent if I look at the boric acid concentration as a function of time. I am at a higher pressure. I have a lower void fraction. So it takes a while even to get to 29, but the limit is way up off the top of this page because I am over 300 degrees.

So the point here is you don't want to be crashing the pressure down if you have been boiling for a long time. So we made a point to have some discussions about changes to the EOPs, the guidance, to make sure that in order for this to be successful, you start to cool down at one hour. Caution the operators, if you have been boiling, not to crash the pressure down if you are out there eight or nine hours.

There are strict statements that do not exceed the 100-degree-per-hour cooldown limit, and that will prevent you from, say, opening the bypass and crashing the pressure down if you get power back. We don't want that to happen.

So we basically talk about emphasizing cool-down time and the equipment and the timing and the operator actions, and their attention to this event, because it is going to be controlled by them.

There are training programs that they are running their operators through. As a matter of fact, I think we are going to verify and observe and make sure that we see these things being done by the operators and they are done very effectively and very timely.

MR. FINLEY: Yes, and this is Mark Finley, again just to interject.

Like Len says, the priority is on starting the cooldown and then finishing the depressurization prior to the boron concentrating.

This really fundamentally doesn't change the operator response to a small break LOCA, however. We are not having to make any significant logic or sequence changes in the EOPs. We are doing some streamlining to minimize these times, but fundamentally the operators are going want to cool down and depressurize the plant to stop or minimize the leak.

So what we have done is put some cautionary statements in the procedure to emphasize to the operators to get the cooldown started within an hour and then to get below the UPI injection point within about five-and-a-half hours.

MR. WARD: So I guess I can summarize the review. Initially, we asked the licensee to do some more calculations because we learned the HPSI pumps, because of their design, are terminated for small breaks. There were some omissions in their long-term cooling analysis.

They did a detailed analysis to show what breaks refill, what don't, what can be cooled down, and what can be refilled if you can't flush. There was a very detailed spectrum analysis that was done with their NOTRUMP small break LOCA code to show that.

The temperatures are low for small breaks because the ECC design is very robust. They have very high pressure accumulators, 700 pounds. That terminates, prevents, precludes, basically precludes uncovery in the real world, and even in Appendix K space we're get what, 1100-1200 degrees. Good design.

Staff calculations confirm their precipitation. As a matter of fact, by doing the calculations we have found out a lot about the plant and understood better how this thing works and what is going on in the beginning of the transient as well as at the end.

It showed that boiling can last for a long time, and equipment and timing for its use is very important and needs to be emphasized again and again. I think that is a key ingredient here.

I think by this whole analysis, the

emphasis on operator actions is a positive safety thing, and it is going to be included in their training programs for their operators. The analysis that the vendor has done is going to be able to show these operators what is the signature of this, what's it going to look like, how long do we have to get down. So there's a lot of good analyses they can use there to supplement the information the operators have.

Based on the calculations that they have done, I looked at the short-term small break LOCA behavior and the long-term cooling and feel that it is a bounding calculation. It is comprehensive and it meets 10 CFR 5046.

CHAIRMAN DENNING: I have a couple of questions that I don't consider EPU questions. That relates to the modeling assumptions associated with 50 percent of the lower plenum and this kind of stuff.

MR. WARD: Right.

CHAIRMAN DENNING: The BACCHUS experiment is the principal rationale that you have --

MR. WARD: It is one of them.

CHAIRMAN DENNING: -- that are supportive of that?

MR. WARD: It is one of them. There's a

Finnish paper, and I am not sure if you remember, Ralph, or not; I think I gave you a copy of that. That shows some lower plenum mixing as well, but they have some current concerns with scaling.

I mean we have the same concerns with the BACCHUS. There's a gradient; there's a concentration gradient in the core. We are mixing everything together.

So I took the code that I developed and I predicted that if I assumed the entire lower plenum, I am too late on the precipitation. So I cut the lower plenum volume in half, and I better predicted the timing for when the top half of the core reached the limit.

MR. WALLIS: That comes from matching the BACCHUS data within a model?

MR. WARD: Yes, the boiloff. Right. I took my model and modeled that test and compared it to the boron concentration as a function of time.

CHAIRMAN DENNING: I think that we don't understand the BACCHUS experiment well enough to really understand its direct applicability in a manner like that.

MR. WARD: Okay.

CHAIRMAN DENNING: I think that one can do more mechanistic analyses of what is really happening in attempting to predict the BACCHUS experiment.

MR. WARD: Yes.

CHAIRMAN DENNING: We would like to see some effort done there.

You know, earlier we had some recommendations related toward looking at what happens as you get closer to precipitation.

MR. WARD: I agree.

CHAIRMAN DENNING: I understand there's some work that is going to happen there.

MR. WARD: Right.

CHAIRMAN DENNING: We would like to see a little more.

MR. WARD: We will gladly share that with you. I mean, for example, what I would like to see is break the core up into 10 regions and model the gradient. That is a more sophisticated calculation, but --

CHAIRMAN DENNING: Yes, I think you can do that calculation --

MR. WARD: Yes, that can be done. That can be done.

CHAIRMAN DENNING: -- in a mechanistic way.

MR. WARD: Yes, I think it can be done. I agree with you.

This generalized letter with the concerns in it about how the vendors have been doing calculations, that is one of the issues in there.

This one, this average concentration, show me that that -- make it bounding or do a detailed calculation. Show me what it is. What does it really look like?

MR. WALLIS: Wasn't there some kind of critical thing in BACCHUS where after it got a certain difference it turned over or something?

MR. WARD: Yes. They are putting in cold water. Once the concentration in the core and upper plenum exceeded the density in the lower plenum, then it started to mix.

MR. WALLIS: And then it turned over. It is a turning-over criteria.

MR. WARD: Then it turned over, yes. You can look at the Finnish test and you will see the same thing. It occurs at a different time. It is at a different temperature.

But there are a lot of questions, and the owners' group are addressing them right now.

MR. WALLIS: You have a half. If you had

something like a third, this would change the time when they have to take action?

MR. WARD: Sure, absolutely. Sure. Lower plenum is probably worth three or four hours on pre-set time.

MR. WALLIS: I think this is a little bit tenuous, this determination of just what the time is when they have to take action.

MR. WARD: Well, remember the limit is more like 40 percent. If you threw out the lower plenum, you've got 15-16 hours.

CHAIRMAN DENNING: Well, we hear you, but we would like to see a little more to make us understand what is really going on.

MR. WARD: All I am saying is there is a margin there, and they are doing analyses to address all these issues. We don't have all the answers right now, but we are going to get them.

MR. WALLIS: There's a research program in RES that is addressing this?

MR. WARD: Well, no, but --

MR. WALLIS: Is it Westinghouse? Who is addressing it?

MR. WARD: The owners' group.

MR. WALLIS: The owners' group.

MR. WARD: The letter went out to all of the vendors and utilities who do calculations, asking them -- well, there was a list of concerns on how they do their calculations. We wanted to get them on the same page. There are a lot of questions about justification for their model; what happens when you've got debris going in there; what happens when you add cold water. That is in there, too.

There's probably two pages of issues that I see is going to require some experiments to --

MR. WALLIS: What will concern me is if, as a result of this new research, you have to radically revise your view of boron precipitation.

MR. WARD: Boy, I hope that doesn't happen.

MR. WALLIS: I know.

MR. WARD: I know. Well, I can't stand here and say, "Boy, that's not going to happen." I can't. That's why we asked the questions.

MR. WALLIS: Well, think of all the surprises you got with the sumps. Surprises do happen.

MR. WARD: That's right. Well, I suspect there's going to be a few surprises here.

MR. WALLIS: We will shine the spotlight

on you in a while. Okay.

MR. FINK: If I can say something? It is Dave Fink of Westinghouse.

I heard something up here, if you will forgive me. The WAD program has been mentioned a few times here.

Recently, the NRC sent a letter to the PWR owners' group stating the staff's principal boric acid precipitation methodology concerns. The PWR owners' group is in the process of preparing a response to this letter.

I happen to be the lead, the Westinghouse lead on that program, so I know a little about it.

It is important to emphasize that the methodology concerns raised by the NRC in their letter have been addressed for Beaver Valley and Ginna for the uprates, as we discussed over the past few days.

As suggested by the staff, in the owners' group response to the NRC letter we use insights from these analyses, that is, as performed for Waterford, Beaver Valley, and Ginna, to show that from the plants represented by the owners' group that existing calculations are conservative and that existing emergency procedures will prevent boric acid precipitation after a LOCA.

While the upcoming owners' group response to the staff's letter addresses the principal methodology concerns, there are many other tougher questions that the staff and the Committee have raised regarding mixing phenomena in the reactor vessel and regarding boric acid solutions in general.

These questions are the subject of ongoing GSI-191 programs and also a longer-term owners' group boric acid precipitation methodology program. The objective of this latter program is to answer the questions that can be answered and, probably more importantly, to show that the methodologies such as those used for Waterford and Beaver Valley and Ginna are adequate to ensure the safe operation of the plants and to demonstrate compliance with all regulations.

The owners' group intends to meet with the staff in the near future to discuss this program, the specific objectives of this program, and the long-term solutions to these questions and these problems.

CHAIRMAN DENNING: Thank you for that.

I think we are done now with the presentations, and I think we are just into some wrapups.

MR. FINLEY: Yes, Dr. Denning?

CHAIRMAN DENNING: Please.

MR. FINLEY: There is one open question from this morning. We do have some data with respect to the question about RETRAN uncertainties. So we would like to show you that data.

CHAIRMAN DENNING: Please do that.

MR. FINLEY: Okay.

MR. HUEGEL: My name is Dave Huegel. I am from Westinghouse.

One of the things that was being discussed this morning was the loss-of-flow event. What we have here is I just put together a plot where the blue line -- and I picked out points as best I could of what the flow coast-down was as measured at the Ginna plant.

This is a normalized curve and it is based upon whatever the actual flow that was being measured at the plant. Keep in mind they do have a tech spec which identifies the minimum measured flow that the plant has to meet and verify going into a cycle that they are above that flow rate.

The minimum flow rate that we assume in the safety analysis is the flow that we were doing the DNB calcs and lower than what the plant has to ensure that it is meeting.

What you have here in the purple line,

that is the complete loss-of-flow event where the coast-down that is caused by the complete loss-of-flow event, where the pumps are allowed to coast down freely.

Probably the biggest difference between these two curves is, as I mentioned this morning, the fact that in the safety analysis we take off 10 percent from the pump inertia, and we do in the safety analysis model all of the pump characteristics, the homologous curves, so that we have captured in the RETRAN model an accurate representation of what the plant or the pump models are.

Another thing that I mentioned in the loss-of-flow analysis, when we assume the rods are dropping into the core, that is based upon a confirmation that the plant performs based upon full RCS flow conditions. As you can see, during the coast-down you are going to be at a degraded flow condition, and we would expect that the rods would fall into the core even faster.

Another thing that we do is in the modeling of the reactivity that is inserted in our point kinetics model, as I mentioned, it is assumed that there was a xenon transient in effect where your reactivity is pushed towards the bottom of the core, and that is what we assume for the addition of the reactivity as the rods are falling into the core.

Yet, at the same time when we do the DNB analysis we would assume a shape that is closer to a shape that has an AFD axial flux difference closer to zero, which would be limiting for DNB-type calculations.

So, at the same time, you would have a reactivity shape where your axial power shape is skewed towards the bottom of the core. Yet, at the same time we are assuming a DNB axial power shape that is skewed more closer to the top of the core. So that is an additional conservatism that we have within the analysis.

The results that are represented this morning were for the under-frequency decay case. The way that the pumps operate is they operate off of the frequency on the grid. So if you have a change in frequency, it affects how the pumps are operating. Fluctuations in voltage typically don't affect the pump speed that much.

What we have here is a case where we have assumed a very conservative 5 hertz per second decay in the pump coast-down. Now one of the features at a typical Westinghouse plant, and it also applies to

Ginna, is that as soon as you hit the under-frequency set point, then your trip breakers would, your pump breakers would open, and the pumps would be free to coast down.

So that at some point in here the pumps in reality would begin to follow the line closer to what you would see in the purple line, actually the blue line. Yet, we have assumed in the analysis that the pumps are dragged all the way down to essentially a zero condition at 12 seconds.

So this is just to show you the comparison and to tell you that we did do a comparison of what the actual plant data would be versus what we have assumed in a safety analysis.

MR. WALLIS: There is no plant data per se here?

MR. HUEGEL: Well, the blue is the plant data.

MR. WALLIS: It is plant data?

MR. HUEGEL: Yes.

MR. WALLIS: Okay. I wasn't quite sure --

MR. HUEGEL: I'm sorry, yes.

MR. WALLIS: -- if it was your prediction from realistic or it is the plant. Oh, it is actually the data? Okay.

MR. HUEGEL: Yes, that is actually the data, yes.

MR. WALLIS: It is a line through the data or does the data have a big scallop --

MR. HUEGEL: I was just given a plot from the UFSAR, and I was picking off points as best I could. I apologize; I didn't do a super job there with the blue line.

MR. WALLIS: Which is one transient. There's no bouncing around?

MR. HUEGEL: No. If there was any bouncing around, it would probably be to detect noise. I mean we do see, if you look at, for example, your hot leg temperatures due to the RTDs being where they are, you do see noise in your hot leg signals which presents a problem for like the over temperature delta T, which has a lead lag function. If you have a spike in your T-hot which affects your TAV, you get a spurious spike on your margin of the OTDT, which isn't real, yet presents a problem in terms of ensuring a plant margin when you are just in a steady-state condition.

MR. WALLIS: This is graph paper.

(Laughter.)

MR. FINLEY: That is the curve from the

UFSAR and shows the two-pump coast-down alpha and bravo flow.

MR. WALLIS: This is measured?

MR. HUEGEL: Correct, that is measured.

MR. FINLEY: Correct. That was part of the hot functional testing when Ginna initially started up. Dave just transcribed that data to the plot you see on top, the blue.

MR. WALLIS: Oh, okay.

MR. HUEGEL: I am due for an eye exam. So I apologize.

CHAIRMAN DENNING: Now are you going to show other characteristics then of the --

MR. HUEGEL: Yes, yes.

CHAIRMAN DENNING: Go ahead.

MR. HUEGEL: Were there any questions?

CHAIRMAN DENNING: I understand that, yes.

MR. HUEGEL: This is a comparison of the RETRAN that we just recently completed. This was for the Ringhals 3 plant. It is a plant in Sweden where we did some comparisons against plant data.

We don't have any, other than what I was just showing you with the flow coast-down for Ginna, but here is a comparison, if you can see that.

CHAIRMAN DENNING: It looks like you cut

off the top. What are they?

MR. HUEGEL: I'm sorry. That is the nuclear power transient.

This is for a power load decrease, and the hash line in here is the plant data, and the red line is what the RETRAN model is doing.

MR. WALLIS: After being adjusted?

MR. HUEGEL: Yes, keep in mind that the RETRAN model, we are using a point kinetics model. So as your rod control system is moving in and out, we have some differential rod data, but the fact that we are using frozen feedback and a point kinetics model, we did have to make adjustments to that differential rod worth. Once we did, we got a close match with the nuclear power.

MR. WALLIS: Are you fitting the data or are you making a real comparison?

MR. HUEGEL: Well, this, actually, on the nuclear power, you would say it is more like fitting the data. Then the question is, how is the RCS responding to the transient once you have done a comparison or a fit of the nuclear power?

This here is your vessel TL. The plant data is the black hash line, and your red line is the RETRAN predicted --

MR. WALLIS: You have used invisible ink for the RETRAN base somehow?

(Laughter.)

MR. HUEGEL: Actually, it's in there.

CHAIRMAN DENNING: It's in there. Yes, I see it.

MR. WALLIS: It is sort of visible.

MR. HUEGEL: But this is a comparison where we have the rod control system turned on. We have the steam dumps model. We also have your pressurizer pressure control and level control all turned on. So all these kinds of different control systems that certainly we don't credit when we perform a safety analysis.

CHAIRMAN DENNING: And that is a pretty fine scale, actually. I mean things are a little bit tight --

MR. HUEGEL: Yes. Granted, it is.

Here is a plot just showing response of the RETRAN model to the pressurizer level. Again, given the scale, I think it is tracking the results rather well.

Here's the pressurizer pressure transient, again, the red being the RETRAN results and the hash line being the plant data. So it is showing a fairly good match of this transient where you are getting fairly substantially large changes in the nuclear power and other parameters.

This is the coolant flow, the RCS coolant flow, the loop steam flow, steam header pressure.

MR. WALLIS: Wait, wait.

MR. HUEGEL: Do you want to go back and look?

MR. WALLIS: So when we look at these, we see a sort of agreement, but there's a difference, too. So we don't quite know how to interpret this when you show us a plot of a prediction of a transient, how much we should allow for RETRAN uncertainties around that prediction, because we know there are some, as you can see here.

MR. HUEGEL: Sure.

MR. WALLIS: We don't quite know how to translate what you show us here to what you showed us earlier today.

MR. HUEGEL: Again, I would look at the scale and say that, yes, it looks like a big change, but if you look --

MR. WALLIS: This is a proportionate change or is it a certain error and a certain number of bars?

MR. HUEGEL: I think it is more a function of the units that were selected. I mean I only have 70 units a bar here.

The other thing, as I was mentioning this morning, the other important point is we do make very conservative assumptions in the analysis in not crediting the different control systems, which gives us what we believe a very conservative analysis.

When we do a comparison, for example, to flow coast-down, we do see that we are predicting a very conservative coast-down.

MR. WALLIS: In this case the actual pressure is significantly above the RETRAN phase. The change in pressure is also significantly bigger.

MR. HUEGEL: Keep in mind this is the steam header pressure.

MR. WALLIS: Right.

MR. HUEGEL: We are most concerned in looking at the steam pressure and the steam generator conditions, not necessarily what is going on down in the steam header. So the question is -- in most plants you do have different runs between where your steam generators are located and then your piping to where they are all headered together. So it could have been the assumption that is made in terms of what piping was selected, because I don't really care what is going on at the header.

My concern is what is going on in the steam generator and between the steam generator to where the safety valves are connected. What's the delta P between those two points? What happens down at the header is not really a big concern.

CHAIRMAN DENNING: Why don't you go find another curve that is more appropriate than on the pressure.

MR. HUEGEL: Well, the good plot I thought was on the pressurizer pressure where we did actually have a good comparison of what the plant was indicating in terms of a pressure versus what RETRAN was showing the pressure was.

CHAIRMAN DENNING: Yes.

MR. HUEGEL: Obviously, the peak pressure is one of the parameters of concern in the non-LOCA analysis that we do look at.

MR. SIEBER: Probably if you started your scale at zero, it would appear to have much greater correlation.

MR. HUEGEL: Yes. There's all different ways of manipulating the data. That would be one of them.

(Laughter.)

MR. SIEBER: And it is apparent.

CHAIRMAN DENNING: Is there anything else you were going to show us then?

MR. HUEGEL: If that is good enough --

CHAIRMAN DENNING: Yes, excellent.

MR. WALLIS: It is very interesting. It is, however, qualitative, isn't it? So we don't quite know how to look at its effect in some sense.

MR. HUEGEL: Well, I still feel very strongly that the methodology that we are using for performing the analysis is very conservative and does a good job of ensuring that the plant is safe.

If I look back, like I was talking about with the rod withdrawal at power, we analyze a whole wide range of cases and go all the way to the condition of trip. I know from my discussions with plants that they have problems just at normal operating conditions because of the noise in the channels and the hot legs, of having margin to the trip, and that is without any transient going out at all.

Yet, here I am running my transients and going up to power levels of 120-130 percent, which is where I have the trip set points because I have accounted for all the safety analysis uncertainties. In the case of an OTDT K-1, the uncertainty is on the order of 15 percent. So I've got my safety analysis that is showing I've got a nice, smooth plot of here's what TAV is doing. Yet, at the plant it is bouncing all around, and with the lead lag compensation, it is trying to compensate for the difference between indicated and actual conditions. I am running into problems trying to ensure the plants have adequate margin just for normal operating conditions.

Then if you go out, say, for example, a loss of loss in feedwater event, that is a heat-up event. Well, if you were to ask a plant when they have a loss in feedwater event, it is a problem in terms of maintaining shutdown margin because they get so much cooling because of the aux. feedwater.

Yet, we would assume a turbine-driven failure. We assume one of the two motor-driven has failed and is at a minimum condition. So that we would analyze it in safety space; it is heatup event long term. But if you look at the plan, it is a cool-down problem.

So I feel very comfortable that the methodology that we are applying in these different events is conservative and robust and ensures that the plants are operating in a safe manner.

CHAIRMAN DENNING: Thank you.

MR. HUEGEL: Thank you.

CHAIRMAN DENNING: Okay, let us now move into our wrapup.

MR. FINLEY: If we perhaps could summarize, Mark Flaherty would just give a conclusion from our side.

MR. WALLIS: Well, I have a question. I was just looking here at this solubility of borax versus temperature. Do you have also some sort of a curve of the boiling point versus the degree of borax dissolved in the concentration? Is there a boiling point elevation due to concentration as well, a curve like that you could give us to take away?

CHAIRMAN DENNING: Yes, also if you have density, too, because --

MR. WALLIS: Density, too, because all those things are related, yes.

CHAIRMAN DENNING: -- I had some trouble getting the density's function on concentration.

MR. WALLIS: If we want to look at BACCHUS with some intelligence, we need to have that sort of stuff.

MR. FINLEY: I'm not sure this is what you are looking for.

MR. WALLIS: That is solubility. I was looking for boiling point. Presumably, as you dissolve more borax, the point goes up, does it?

MR. FINLEY: I don't have the boron point.

MR. FINK: This is Dave Fink.

Mark, go back to that plot you just had up there.

MR. WALLIS: There is a boiling point. It says, "Boric acid solution boiling point, 218," but that must be at some concentration.

MR. FINK: That is at the atmospheric solubility limit, that is correct.

MR. WALLIS: That is at 30.

MR. FINK: Correct.

MR. WALLIS: So it hasn't changed very much then. I presume it is coming up from 212 to 218, as you have added up to 30 percent by weight.

MR. FINK: That is correct.

MR. WALLIS: So it hasn't changed that much. Okay, thank you.

MR. FLAHERTY: In conclusion, Constellation came back today really to discuss four topics. Two of them were bring-backs.

For the first one, dealing with alloy 600 material and PWSCC, we believe that we proved that it is not a concern with respect to uprate.

The other bring-back item dealt with the margin. Obviously, we have had lots of discussion about margin. I believe that what we attempted to show you today was that there's margin in many different aspects with how we do things. This includes inputs, assumptions of keeping RCS pressure at nominal value even though it increases, and not crediting that for DNB; looking at reactor trip at 1.4 seconds versus less than 1 second; doing some analysis at 102 percent power; looking at steam generator plugging from 0 to 10 percent, depending on which is worse case. So that is one aspect for inputs.

We just discussed again some of the code. The code has been benchmarked somewhat against real plant data.

There's also margin and safety analysis limits where we do assume penalties in looking at margin with that.

Finally, even the design limits, even though there's, for instance, a limit of 3200 pounds for RCS pressure from ASME code, that is just at the point at which you have an increased probability of causing additional damage. So there is additional margin even beyond that.

So, in sum, there's lots of different sources of margin within the analysis.

With respect to the two new topics we discussed today for small break LOCA and long-term cooling, we did demonstrate that we do have acceptable results. I would like to reiterate that the analyses that were done were very conservative from the standpoint of looking at things even from decay heat of 120 percent. This decay heat, that adds -- that affects the analysis in many ways with respect to what we believe would actually occur during a real event.

To put this in perspective somewhat, with the higher decay heat, you are going to have increased steaming and, therefore, increased pressure inside containment. So this will increase the need for containment spray.

But, in all honesty, if you look at just normal decay heat with reduced, relatively reduced steaming effects, so, therefore, containment pressure would be reduced; hence, containment spray by procedure would be looked to be terminated in an earlier standpoint, extending out the period of time in which operators would look to go on to recirculation.

So a lot of these conservative aspects, that type of thing, do have effects on the analysis. So even though there may still be some lingering questions or generic comments that the staff is dealing with the PWR owners' group and things like that, we believe that what was done for Ginna is more than sufficiently conservative enough to bound any of those potential issues.

So, with that, I would like to conclude Constellation's presentation.

CHAIRMAN DENNING: Good. Well, before you leave, let me say thank you for the presentations. You certainly addressed the issues that we asked to be addressed at the last meeting, and I think you have done that very well. I would like to congratulate the presenters and thank them.

We will be providing some guidance to you on the presentations for the upcoming meeting. Obviously, we have two hours of which we will have presentations that will be much more focused than we have had in our couple of days of reviews here. We will try to get that guidance to you by tomorrow as to what our expectations are, and also to the regulatory staff, of course.

There is some duplication, obviously, that occurs in these presentations. We will probably remove some of that duplication for the presentation to the full Committee.

You will also hear we will have some talk-arounds here before we are done. Perhaps you will get some additional guidance from the individual members of the Subcommittee before we are done today. Okay?

So we will have the wrapup by the regulatory staff now.

MR. MILANO: No, sir, we don't have anything else that we would like to put on the record and stuff. Just what we were going to wrap up you have just mentioned. We were going to ask about the guidance and when to expect it in preparation for the full Committee meeting.

CHAIRMAN DENNING: Good. Again, I think we will try to get that to you tomorrow.

I would like to thank the staff, too, because I think that we did get quite a bit of enlightenment on some of the things that have been bothering us at the previous meeting, and staff's analyses were very helpful in that. Thank you.

MR. MILANO: Thank you.

CHAIRMAN DENNING: Okay, then why don't we go around the table. Jack, do you have some comments?

MR. SIEBER: Not very many. We had some questions at our meeting last month, and I think both the licensee and the staff did an excellent job of providing the answers.

One of those questions about materials was mine. That was properly answered. I think that from my standpoint any concerns that I might have had trying to guess where alloy 600 was are no longer there because they aren't in critical places.

I thought the explanation of how safety calculations are done, I think Otto and I both have been through that a few times. On the other hand, I even learned a couple of new things in the process of the presentations myself, and I thought that was well done.

MR. WALLIS: What did you use? Did you use 1.38 or 1.55 or what did you use?

(Laughter.)

MR. SIEBER: 1.55.

MR. WALLIS: You used 1.55.

MR. SIEBER: You get to pick your own number.

(Laughter.)

MR. WALLIS: Otto?

MEMBER MAYNARD: I'm trying to remember

what it was. We actually took over our own safety analysis. Again, you go back -- the real number is what the design criteria is, and then, again, you pick a number that gives you design specification margin for your field design and how much you want to use for that and how much you want to be able to use in case you find something later you didn't know about versus where you want to put your set points in your plant and how do you really want to operate your plant.

So, again, it really goes back to making sure that you meet the design criteria, and then where you put the other depends on how much flexibility you want to give to your field designer versus how much flexibility you want to give to your operator.

I forget what the number was that we used at Wolf Creek, but it was below 1.55. I don't know if it was much above 1.38. But it was in that neighborhood.

MR. SIEBER: Those safety limits are like building a box. Once you build the box, that becomes the golden rule, so to speak, and you have to operate the plant inside that box.

CHAIRMAN DENNING: And you try to make your box as small as possible.

MR. SIEBER: No, you try to make your box as big as possible.

MEMBER MAYNARD: Not necessarily. What you want to do is to give, keep yourself the ability to handle unknown or unusual situations that may come up without having to do a re-analysis every time somebody wants to change something.

So, basically, you set a box for a field designer and you set a box for other parts of the design. If you find out later that that wasn't a big enough box for your field designer, then you go to another box and you can move that around.

If you set your limit right down at the design criteria, you have no flexibility to deal with it. I think it actually creates a less safe situation.

So you actually want to have that for a couple of reasons, not just safety operation, but operational flexibility, and, again, to be able to handle any of the unknown.

MR. WALLIS: Of course, we had this conversation earlier. I can understand all that from the point of view of operation, but there isn't a measure of how much additional safety the public is getting out of this. That is what is missing. There is no link here.

MEMBER MAYNARD: The safety to the public is built into what the design criteria is in the regulations and the methodologies that are approved, not only the methodology, not only the codes, but also the way the codes have to be used, the restrictions on the application of that code.

As you have seen from a lot of these discussions, there's a lot of conservatism built into the code and into how the code has to be used and what assumptions are put into that.

That conservatism, plus the conservatism built in what the design criteria is, that is the public's safety margin. The rest of that then becomes the licensee's margin for how they want to operate.

Again, it provides the safety margin in case something comes up you really had not expected or didn't know about. You are still above your design limit.

MR. SIEBER: If you wanted to know what the margin meant in terms of safety, you would have to do it with distributions, probabilistic distributions, which deterministic rules don't really lend themselves to. So, generally, if you meet deterministic rules, you are safe enough. That is basically the way you would interpret Title 10.

MEMBER MAYNARD: And, actually, I think that you are extremely safe because it is very conservative. I think if we went to a more detailed analysis where you really tried to predict where it was, put uncertainties and stuff on it, I think that you could find that you could actually uprate these plants to a higher power. There's a lot more conservatism than what you know about.

You may find in some areas occasionally that you didn't have as much conservatism as you thought, but in the aggregate you take all the conservatisms built into all of the bounding type analyses and there's more margin there than what shows.

CHAIRMAN DENNING: Graham, anything else?

MR. WALLIS: Well, I am much more satisfied than I was before in several areas. I was not quite sure what was going on when you got these numbers and where they came from and why they were so close to limits, and so on. I think I understand much better how they were derived and why they have the form they do have.

I am much more satisfied that the licensee and Westinghouse have performed a thorough analysis. I think some of the details we saw today a lot let me know what was really behind it all that we hadn't seen before and you never get from reading the SER.

(Laughter.)

Similarly, the staff came through with explanations which are not in the SER. They are also behind the words which tend to just say the applicant did this and it's okay, which leaves completely up in the air, how did you know that?

So I feel much more satisfied today. I suppose after I have slept and dreamt a bit I might come back with another question, but I don't at the moment have a question. I am pretty satisfied. So thank you.

CHAIRMAN DENNING: Tom?

MEMBER KRESS: Well, I felt that the staff and the applicant have shown that they meet all the regulations, the rules. I didn't see any place that I thought there was glitch or a hangup. In fact, they did a good job of showing it.

I thought their analysis of the boron precipitation was highly conservative. I think they could show that they've really got a lot more time than a couple of hours. In that large break LOCA with this upper plenum injection, I really don't think that you have any boron concentrate.

CHAIRMAN DENNING: No, I don't either.

Yes, Otto?

MEMBER MAYNARD: I think the licensee has done a real good job in answering questions, which I think many went well beyond what the licensee would be required to have to answer, because our questions to the licensee and to the staff were really challenging or questioning approved methodologies, which I think is fair game, but the licensee I think did a good job of providing answers and responding, and has been responsive to our questions.

Again, I agree with Tom, I think they clearly demonstrate that they meet the regulatory requirements and that they have performed the analysis and meet all the requirements there.

I also think the staff has done a good job of demonstrating that they understand the applicant's information, that they understand the analysis. They have done some confirmatory work. So I think they have done a good job in demonstrating that they independently took a look at a number of these things and satisfied themselves that the licensee's information was accurate and representative there. So I think they have done a good.

So, overall, I think both did good.

CHAIRMAN DENNING: Very good.

Unless anybody else quickly objects, then I declare this over.

(Whereupon, at 2:09 p.m., the proceedings in the above-entitled matter were concluded.)

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON POWER UPRATES

+ + + + +

THURSDAY,

APRIL 27, 2006

+ + + + +

ROCKVILLE, MARYLAND

+ + + + +

The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 8:30 a.m., Richard S. Denning, Chairman, presiding.

COMMITTEE MEMBERS:

RICHARD S. DENNING, Chairman

THOMAS S. KRESS, Member

OTTO L. MAYNARD, Member

JOHN D. SIEBER, Member

GRAHAM B. WALLIS, Member

ACRS/ACNW STAFF:

RALPH CARUSO, Designated Federal Official

NRC STAFF:

PATRICK D. MILANO, Division of Operating Reactor Licensing

SAMUEL MIRANDA, NRR

LEONARD W. WARD, NRR

PANELISTS:

JIM DUNNE, Constellation Energy

DAVID FINK, Westinghouse

MARK FINLEY, Constellation Energy

MARK FLAHERTY, Constellation Energy

ROY GILLON, Constellation Energy

JOSH HARTZ, Westinghouse

DAVE HUEGEL, Westinghouse

JOHN KILLIMAYER, Westinghouse

CHRIS McHUGH, Westinghouse

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

8:32 a.m.

CHAIRMAN DENNING: (presiding) The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Power Uprates. I am Richard Denning, Chairman of the Subcommittee.

Subcommittee members in attendance are Tom Kress, Otto Maynard, Jack Sieber, and Graham Wallis.

The purpose of this meeting is to discuss the extended power uprate application for the R.E. Ginna Nuclear Power Plant. The Subcommittee will hear presentations by and hold discussions with representatives of the NRC staff and the Ginna licensee, Constellation Energy, regarding these matters.

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 the meeting previously published in The Federal Register on April 12th, 2006.

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.

We have not received any requests from members of the public to make oral statements or written comments.

I would make some comments. We are kind of experimenting with some revisions to this room, and some of these speakers do not transmit very well. So when you are making your presentations, please make sure you are up very close to them and speak directly into the microphone.

We will now proceed with the meeting, and I will call upon Mr. Milano of the NRC staff to begin.

MR. MILANO: Good morning. Again, my name is Patrick Milano. I am the Licensing Project Manager with responsibility for Ginna.

This morning we are going to have presentations by Mr. Sam Miranda and Dr. Len Ward of the PWR Systems Branch in the Division of Safety

Systems.

On the agenda this morning I am going to give you a brief introduction as to where things stand with the uprate application itself, and then we will cover the items that came out of the March 15th and 16th Subcommittee meeting and then go into those open items that were not in the first draft safety evaluation that was provided to you. The subsequent safety evaluation that you received on or about April 4th does have the remaining open items evaluated in it.

Just as background again, the EPU application that came in on July the 7th was preceded by three license amendment requests that are all tied directly with the license application. We have made some progress in all three. Those were the relaxed x axial offset. As you see on the slide, it is complete. The main feedwater isolation valve one we have issued and it is complete.

The revised LOCA analysis amendment, the staff's safety evaluation is complete. You will be hearing some of the information that is in it which is in today's presentation. The safety evaluation has been completed by the staff and the inputs provided, and the actual package is currently in concurrence review.

Again, we had the Subcommittee meeting on March 15th and 16th, and we are scheduled next Thursday to have the full Committee meeting with you.

Also, as part of the uprate, you recognize we have to issue an environmental assessment. That environmental assessment was published in the middle of April for comment, and the comment period ends May the 12th.

Again, the licensee plans, if we should issue the power uprate amendment and these other packages, they are planning to implement the uprate during the fall 2006 outage.

Again, in addition to hearing presentations by the licensee staff -- they are going to cover the same subject areas -- the NRC staff is going to likewise prepare presentations about what we did during the review. For the non-LOCA analysis, you are going to hear from Sam Miranda. He is basically going to talk about acceptance criteria margins and interpretation of the results of three or four different non-LOCA transients as they were reviewed for Ginna.

Dr. Ward is going to go through those items. The next two items here are those items that were not present in the first draft safety evaluation. These were the open issues or open items from the last Subcommittee meeting. He is going to go through the small break LOCA evaluation review that he did and then go into post-LOCA, long-term cooling boron precipitation.

That, basically, is all I wanted to say before turning it over to Constellation Energy for their portion of the presentation. With that, Mr. Mark Finley is the Project Manager for the uprate with Constellation, and he will be introducing his staff.

MR. FINLEY: Yes, Good morning. Mark Finley, Project Director for the power uprate at Ginna, as Mr. Milano said.

I would like to introduce Mark Flaherty, current Acting Vice President of technical areas at Constellation, to kick off the meeting for Ginna.

CHAIRMAN DENNING: Speak into that mike and let's make sure that he can hear you.

MR. FLAHERTY: Hi. I am Mark Flaherty.

CHAIRMAN DENNING: Okay, good.

MR. FLAHERTY: Here although the slide shows that I am the Acting Vice President of Technical Services, I was just transferred to the Engineering Manager of Calvert Cliffs on Monday. So with respect to the project and ACRS, whatever else, I wanted to continue supporting this project for as long as need be. So that is why I am here today.

As Pat Milano indicated, Constellation is back to discuss two topics that the Subcommittee requested further discussion from the March meeting. Those are RCS materials and non-LOCA margin. So we have presentations for both of those topics.

Secondly, there's two topics that we did not present at the last Subcommittee meeting. Those are small break LOCA and long-term cooldown. Then I will follow up with a summary conclusion once we go through the subject for presentations.

So, with that, I will turn this over to Jim Dunne who will lead us into RCS materials.

MR. DUNNE: Good morning. My name is Jim Dunne. I am an Engineering Consultant at Ginna Station. I have been at Ginna for 15 years in the Engineering Department, and for the last three years I have been the Lead Mechanical Engineer for the uprate project.

One of the open items from the meeting we had in March was a request by the ACRS to see a list of where in the reactor coolant system we have alloy 600 material or its weld equivalent, Inconel 82 or

Inconel 182, present. So the purpose of my presentation is to go over those locations.

Basically, there are four locations in the reactor coolant system where we have alloy 82 or the equivalent weld material. Three of them are in the reactor vessel. One of them is in the steam generator.

The three locations in the reactor vessel are in, basically, lower radial supports at the bottom of the reactor vessel, the bottom-mounted instrumentation welds to the reactor vessel lower head. We also have a third location which is a weld buildup on a safety injection nozzle for our upper plenum safety injection, and then in the steam generator we have alloy 600 weld material as cladding on the steam generator tube sheet.

Go back to the slide.

This is a schematic of the reactor vessel internals, showing the various components. Two of the three items in the reactor vessel are shown here. The safety injection nozzle is not shown on this schematic, but basically our safety injection nozzles are located at the same elevation as our hot and cold leg nozzles up in this area of the reactor vessel.

The other two locations, like I said

earlier, the lower radial supports, which are at the bottom of the core, basically, there are lugs welded to the reactor vessel that act as radial supports. They basically act as a keyway for keys from the core barrel that allow the core barrel to be aligned properly inside the reactor vessel.

There are four supports 90 degrees apart. The support material is alloy 600, and it is welded to the lower reactor vessel inner shell with an alloy 600 weld material.

MR. SIEBER: Have you ever examined those for cracking?

MR. DUNNE: We do a visual examination for them as part of the 10-year ISI when we do the vessel examination.

MR. SIEBER: It is hard to see though, right?

MR. DUNNE: Right. But, other than that, I don't believe there's any special inspections of that. This would be generic probably --

MR. SIEBER: It's cold.

MR. DUNNE: -- to all Westinghouse reactor vessels, would be my guess.

MR. SIEBER: It is cold down there anyway.

MR. DUNNE: Yes, the other thing is,

because it is at the downcomer, it sees cold leg temperature. Our cold leg temperature for EPU is increasing by about 8 degrees from where we are presently operating. However, the cold leg temperature at EPU will be a couple of degrees below where we operated the plant from 1970 up through 1996, when we replaced our steam generators and lowered our TF.

The second location, next slide, the second location that we have it is in the bottom-mounted instrumentation weld locations. We have 36 penetrations through the reactor vessel lower head for bottom-mounted instrumentation.

Basically, there are three areas on the bottom-mounted instrumentation where we have alloy 600 material. The nozzle itself is an alloy 600 nozzle that is machined. It is welded to the reactor vessel lower head in this area with the J-Weld, which is an Inconel 182 J-Weld material. Then the nozzle outside the reactor vessel, our nozzle, the alloy 600 nozzle is welded to a stainless steel nozzle with an Inconel 82 weld.

All three of those locations are pressure-boundary locations, and all three of them, basically, see cold leg conditions. So, as such, we don't believe they would be susceptible to any PWSCC concerns.

Next slide.

The third location in the reactor vessel where we have alloy 600 is a weld buildup on our SI nozzles. This is a plane view looking down at basically the nozzle location, the reactor vessel, the two hot and cold legs over here.

We have two SI nozzles 180 degrees apart that penetrate into the upper plenum region of the core because we are an upper plenum injection plant, like the other Westinghouse two-loop units. At the end of the SI nozzle in the reactor vessel itself internally there is a weld buildup over in this area.

Next slide, please.

So this basically shows the entire SI nozzle forging. This is the reactor vessel material here. This is the weld for the SI forging to the reactor vessel material. The SI forging itself is basically a carbon steel material with a stainless steel cladding for the nozzle itself, but at the end of it inside the reactor vessel they put in a 1-inch Inconel, I believe it is 182 weld buildup, to extend the nozzle down an inch. That was for fabrication, final fabrication, of the internals to the SI nozzle.

Then they ended up machining back on these to get the clearances they needed between the OD of the upper barrel and the SI nozzle.

MR. WALLIS: What is the SI nozzle made out of? The safe end there, what is that made out of?

MR. DUNNE: The SI nozzle is basically --

MR. WALLIS: The safe end of it.

MR. DUNNE: The safe end over here --

MR. WALLIS: Yes.

MR. DUNNE: -- is a 182 316 stainless. This weld here is not Inconel. So the only place where we have Inconel is this, which is a weld buildup. It is not pressure boundary --

MR. SIEBER: It is not load-bearing either?

MR. DUNNE: It is not load-bearing. The inside of it, basically, sees hot leg conditions or upper plenum injection conditions, which would be upper plenum pressure and upper plenum temperature. The outside portion over here and over here, because you have the upper core valve basically coming around here, basically, sees cold leg pressures and cold leg temperatures.

So there is a minimal delta P across this internal component right here because it is inside the pressure boundary. Obviously, out here this SI nozzle sees the full RCS pressure, but this portion of it is basically seeing about 30 to 40 psi delta P between the cold leg pressure and the upper plenum injection pressure. As such, it is not a highly-stressed component.

Also, because you have hot leg temperature in here and cold leg temperature out here, basically, its temperature is someplace probably close to TF. So, again, we don't believe that is susceptible to PWSCC, mainly because of the low stresses and because the temperature is relatively low and it is not really hot leg temperature.

So those are the three locations --

MR. WALLIS: It cycles in temperature a bit, doesn't it? It cycles?

MR. DUNNE: The cycles -- well, the SI nozzle for up and down, yes, that is part of the design for the reactor vessel.

MR. SIEBER: Well, ordinarily, there's no flow there, right?

MR. DUNNE: There would be no flow, yes, in here. It is a stagnant region during normal operation.

The fourth location where we have --

MR. WALLIS: Do you ever test this in some way? Do you test --

MR. DUNNE: We don't do tests to --

MR. SIEBER: Injection.

MR. DUNNE: We don't do flow tests into the reactor vessel. We do test SI flow in a recirc mode.

The fourth location where we do have cladding, basically Inconel 82 cladding, is on the steam generator tube sheet, between the bottom portion of the tube sheet. This shows the tube sheet here, and this is the primary head. Basically, the tube sheet is carbon steel. It is 25-and-a-quarter-inch thick.

The bottom portion, which has siezed the RCS conditions, basically has about a three-eighths-inch Inconel 82 clad material deposited on it. So the clad material isn't the pressure boundary material per se. It is more just to protect this carbon steel base, tube sheet base metal from the borated water.

Basically, the divider plate, in a new replacement generator this divider plate is basically a 690 material. The cladding of the primary bowl itself is a stainless steel clad material.

There's also in this little blowup here, this is the divider plate, and here is your tube sheet cladding. There is something called a seat bar buildup off the tube sheet that they use to basically build up the tube sheet so they can weld the tube sheet to the divider plate. This seat bar buildup is also Inconel 82. This weld here between the Inconel 82 material and the 690 primary divider plate is basically a 690 weld material.

During building of the replacement generators we did look at substituting a 690 clad on the tube sheet versus a 600. BNW Canada has had lots of experience with 600 clad material. They have never had any problems with it. But because of the industry concerns about 600 material in general, we evaluated going to 690 during the fabrication of the replacement generator.

There was a test program done. This cladding is basically a bead-welded material that is automatically welded to the tube sheet. So they evaluated going to a 690 wire material in lieu of the 600 material, but the testing that was done indicated that they were having problems with under-bead cracking and inter-bead cracking on the clad material. So the decision was to stay with the 600 material because of those problems with the welding.

Basically, the Ginna replacement generators and the other replacement generators that went through BNW Canada about the same time, which would be the St. Lucie replacement generators and the Duke Catawba McGuire replacement generators, all had 600 Inconel 82 clad material on their tube sheets. The Commonwealth replacement generators that BNW Canada built subsequent to ours also had 600 weld material.

After the Commonwealth, BNW was able to optimize the Inconel 690 wire chemistry and their welding process to get 690 to be an acceptable cladding material. Some of the more recent replacement generators that BNW Canada has built for U.S. utilities have gone to a 690 clad material, but at the time we were doing it they were not able to get the 690 material to work.

Basically, obviously, on the cold leg side, whichever one is the cold leg side, the cladding sees cold leg temperature; the hot leg side sees hot leg temperatures. So the cladding material will see a higher temperature than it has historically seen at Ginna. Right now we are running a T hot of around 590. Prior to replacing the steam joiners in 1996, we operated around 601-602. For a T hot with EPU we are going to be operating with around a 608-609 T hot. So we will be slightly higher there.

Historically, BNW Canada has never seen any problems with the Inconel 600 cladding in the industry. As far as we know, nobody in the industry has seen any problems with the 690 cladding on tube sheets.

The replacement generators for Commonwealth and Duke with the 600 material are operating at hot leg temperatures comparable to where Ginna will be at EPU. They have been operating for about to eight to ten years without any reported crack problems with the material. So we don't believe it is going to be an issue.

The other thing is the fabrication of the generator. Basically, the way BNW Canada fabricated the generator, they put this assembly together, welded the lower shells to the tube sheet, welded the transition cone to the lower shell, and then put that entire assembly into a heat treatment oven to do stress relieving on the pressure boundary welds. So that operation would have also acted to reduce any residual stresses from the original cladding welding on the Inconel material.

The next slide.

So, basically, in conclusion -- that's not the slide we had, but that is okay. Our conclusion is we don't believe there is any new PWSCC concerns that would arise to the Inconel alloy 600. We don't believe the alloy 600 we have in the RCS is basically going to create any new concerns due to EPU. For the lower radial support and for the bottom-mounted instrumentation, they see cold leg temperatures, so their susceptibility to PWSCC is low.

The SI nozzle weld buildup, it is not a highly-stressed component. So we don't believe it is an issue.

Then for the Inconel cladding on the tube sheet, basically, because it was stress-relieved during fabrication, it is not really a pressure boundary material. It is also the hot leg temperatures we are seeing are consistent with hot leg temperatures that other plants presently operating are seeing with the same type of cladding. Because there's been no issues in the industry on tube sheet clad problems with steam generators over the last 35 years, we believe that there are no issues with tube sheet.

MR. WALLIS: This isn't an issue for power uprate. It might be an issue for license renewal, when you are trying to extend the period of time?

MR. DUNNE: Well, this was evaluated and there is a -- basically, license renewals, which we have gone through and the NRC has approved, they looked at all the cladding material. They basically said there is no indication of cladding damage out there. Therefore, it was viewed that the uprate would not have any -- that extending the license, which would not change any conditions, just put more years on it, would not have any issue. This cladding material and tube sheet is low-flow incidency, any radiation. Again, Westinghouse's experience and BNW Canada's experience has been there have been no problems with tube sheet cladding reported in the industry.

Now for 600 material in general, the industry has a mandate to establish an alloy 600 management program, which the industry, which Ginna is part of, is going through creating an inspection program for alloy 600 going forward. So all this stuff will be reviewed as part of that program. That is how we identified, basically, the SI nozzle weld buildup, as part of just going through the weld records for the RCS just to identify where we have 600 material in the RCS.

MR. SIEBER: Do you, by any chance, know what the reactor vessel hot leg safe end to the cast piping, what the weld material is there? Is that a stainless?

MR. DUNNE: It is stainless.

MR. SIEBER: Okay. How about the pressurizer surge and spray lines?

MR. DUNNE: Stainless.

MR. SIEBER: Stainless?

MR. DUNNE: Yes.

MR. SIEBER: Okay. There are some plants where 82/182 is used.

MR. DUNNE: Right.

MR. SIEBER: But you are not one of them?

MR. DUNNE: No.

MR. SIEBER: Okay.

MR. DUNNE: And that is all I have.

MR. SIEBER: You are lucky.

MR. DUNNE: Yes.

CHAIRMAN DENNING: Do we have any other questions? Jack, are you comfortable?

Okay, thank you.

MR. SIEBER: I guess I would point out that all these cladding depositions are not pressure boundary. You can sustain a crack and have corrosion underneath, but since there's virtually oxygen in the coolant, the corrosion rate is very slow.

MR. FINLEY: Good morning. Again, Mark Finley, Project Director for the Ginna power uprate. If you recall from last time we met, in my previous life I was actually Supervisor of the Safety Analysis Group at Calvert Cliffs for several years. So I am the lucky one to present our safety analysis discussion here this morning, but I am backed up by our Westinghouse experts to help with questions.

As you recall, at the last meeting you asked about margin associated with several of the non-LOCA events. That is what we are going to talk in some detail about today, and, also, Sam Miranda, I think when I am finished, will discuss these events and perhaps others with respect to margin in the safety analysis.

I will show you the current results that are applicable now as well as the EPU results that are being reviewed by NRC. We will talk specifically about the loss of flow, loss of load, and rod withdrawal events, which were three of the more limiting events in our safety analysis.

This slide shows the current and EPU results associated with the three limiting events I just mentioned. As you can see, the EPU results in the center column there are close to the results in the righthand -- excuse me -- the acceptance criteria in the righthand column. This is the reason for the discussion today.

MR. WALLIS: These are predicted with RETRAN, is it?

MR. FINLEY: That is correct. These results, we did for the non-LOCA methodology at Ginna, we revised the methodology from LOFTRAN to RETRAN, and with respect to the core thermal-hydraulic code, changed that method from the THINC to the VIPRE code.

MR. WALLIS: Well, there's sort of two questions that are basic. One is these numbers are awfully close to the limit, and what does that mean? And the other thing is RETRAN isn't a very accurate code. You can tweak it various ways. When you get 2748.1, it would seem that the slightest tweak could make it 2749.

MR. FINLEY: Right.

MR. WALLIS: So what's implied by your saying that this is the number rather than some other number which is perhaps close to it?

MR. FINLEY: Right, right. And, actually, Gordon, temporarily go to the next slide.

We did this with the understanding of the approach that was used. We modified inputs to the analysis until we got acceptable results by the approved criteria. We didn't attempt to go any further than that and demonstrate additional margin.

That is because we understand the margins that are in our analysis and the inputs that are assumed and in the methodology, as well as margin that is above the safety limit controlled by NRC. So these results are not coincidental, as was mentioned last time.

Because of that approach --

MR. WALLIS: Deliberately tried to get to the limit, essentially?

MR. FINLEY: Well, I wouldn't term it like that. We were above the limit --

MR. WALLIS: You tested them until you got to the limit?

MR. FINLEY: We were above the limit without any changes to the inputs, and we tweaked on the --

MR. WALLIS: Pulled it down to be below though?

MR. FINLEY: That is correct.

MR. WALLIS: So it is similar. Which kind of inputs did you adjust then?

MR. FINLEY: Okay, I'll tell you what, if I can hold off on that question until I talk about the events specifically, then we can get to that.

MR. WALLIS: Sure.

MR. FINLEY: Go back one slide, Gordon. Okay, just stick with this slide.

One more comment: Current results you see in the lefthand column of the three columns there. As expected, they are somewhat higher in DNBR space than the EPU result. The trend is all, you know, it makes sense to us.

The pressure results, the same way, about eight pounds lower for the pre-EPU result, increased somewhat. We would expect that with the increased power level and decay heat.

CHAIRMAN DENNING: You're going to talk about how do you get the DNBR? What about the criterion? Where did that criterion come from?

MR. FINLEY: Yes, we will speak to where the criterion comes from here in a minute.

CHAIRMAN DENNING: Okay.

MR. FINLEY: Okay, next slide, Gordon. Actually, two slides.

With respect to the first event, this is the loss of flow and the DNBR margin, where the result was, again, close to the acceptance criteria.

Let's focus here in the middle of this slide. That is sort of the way I set up this discussion for all the events. But that is where the safety analysis limit is. Just below that you see our safety analysis result, 1.385 versus the 1.38 for the limit.

But what we are attempting to demonstrate here is sort of the range of results as you move from more realistic conditions up to the very conservative conditions.

Right underneath the safety analysis result we just modified one input to the analysis associated with the trip time delay for loss of flow. We used a conservative time in our analysis result to get the 1.385. It was 1.4 seconds.

We have done one-time testing in the past to demonstrate that result is actually less than one second, and a more typical assumption for plants in the industry is one second for other Westinghouse plants.

If you remove that margin and that trip time delay assumption, again, still using a conservative assumption that bounds actual plant performance, there's about a 3 percent change in the result, as you see, 1.42.

Now that's not a best-estimate analysis. This would still be a bounding conservative analysis. But that was one input that we could have changed even further to demonstrate additional margin.

MR. WALLIS: Now your safety analysis result is conservative in some sense? I would say that you have just mentioned one conservatism. Does it have other conservatisms in it?

MR. FINLEY: Yes, that is correct.

MR. WALLIS: You say it is a bounding result?

MR. FINLEY: That is correct, it is a bounding result. I am not going to go through all the conservatisms here.

MR. WALLIS: If there are, what do we have -- you put in some bounding assumptions. But RETRAN itself has uncertainties in it which you don't know, or you don't assess, it seems to me. So you don't really know how much uncertainty there is in the code itself. So even though you are putting in conservative assumptions, the safety analysis result is really 1.385 plus or minus something, which has to do with the inherent uncertainties in the code itself.

MR. FINLEY: Yes, to some extent, that's --

MR. WALLIS: I am curious about how big those are. If those are 5 percent, maybe it doesn't matter; you don't get beyond the design limit. But if the uncertainties in the code itself are 25 percent, then one might say, "Well, it could be that in the extreme case you could be way down to your bounding test data."

MR. FINLEY: Right, I understand.

MR. WALLIS: How to assess that?

MR. FINLEY: I understand, but our point is that these inputs are quite conservative in bounding. They more than make up for any uncertainties in the RETRAN methodology.

MR. WALLIS: That has been demonstrated somewhere?

MR. HUEGEL: In the WCAB 14882, we did -- I am sorry; this is Dave Huegel from Westinghouse.

As part of the effort to transition to RETRAN, we did do a bunch of benchmarks which compared the results to actual plant data and confirmed that the RETRAN results were consistent.

MR. WALLIS: Plus or minus what sort of --

MR. HUEGEL: The other thing is, for this event --

MR. WALLIS: Plus or minus what sort of number?

MR. HUEGEL: No, we just did comparisons to make sure that they were in line.

MR. WALLIS: Oh, you looked, you made a curve and you showed some data points that were near the curve?

MR. HUEGEL: That is right.

MR. WALLIS: There's no quantitative assessment of the uncertainty in RETRAN?

MR. HUEGEL: No, but we do know that it is conservative in terms of --

MR. WALLIS: So it is on one side of the data point? There's a bunch of data on the graph and RETRAN is above or below in some conservative way? Is that what you're saying?

MR. HUEGEL: What we are doing, what we did is we compared it to plant data and we didn't predict it on one side or the other. But what you have to do is keep in mind the transient that you are looking at.

Here we are looking at a loss-of-flow event.

MR. WALLIS: Right.

MR. HUEGEL: For the loss-of-flow event, the plant does an actual plant coast-down and confirms that the coast-down that is being predicted is conservatively bounded by what we have assumed in the safety analysis.

What is going on for this loss-of-flow event is primarily driven by the characteristics of your RCPs. The plant does confirm that the calculation of the flow coast-down is bounded by what we have assumed in the safety analysis.

Additional conservatisms that we have in the loss-of-flow event include the fact that we have skewed the reactivity that we have assumed toward the bottom of the core, so that you are not seeing any significant amount of negative reactivity until the rods are well into the core. That is another conservatism that we have within the analysis.

Another thing is, even though we have modeled the complete RCS for this particular event, as Mark is showing there, we have taken no credit for the increase in pressure, which is definitely a DNB benefit, in the calculations that we have performed.

Another thing we have assumed is frozen feedback. When you assume the effects that you have going on due to the loss of flow in the reactivity feedback, since we are modeling a point kinetics model, we get a very conservative calculation of the reactivity during this transient that is relatively quick and is over in a few seconds.

Again, as I mentioned earlier, it is primarily driven by the effects of how the RCPs are coasting down, which, again, is confirmed by the plant.

When we did a more realistic best-estimate-type calculation, we didn't do this for Ginna specifically, but we have done calculations with our RAVE methodology where we have linked the different codes, the kinetics code with our thermal-hydraulics code, and then also the VIPRE code, which does the calculations within the core. We find DNBRs that are well over two for this kind of event.

So in doing the analysis for Ginna, we have all kinds of conservatisms that we believe are backed up based upon actual test data that the plant has performed, as I mentioned, like the flow coast-down, which confirms that what we have done is conservative.

Another conservatism is in the rod drop time that we have assumed. The rod drop time is assumed based upon a very high mechanical design flow.

If you look at this particular event, what you have is a drop in the RCS flow. What you would find is your rod drop time would be much quicker, and if we were to take credit for that conservatism, we would even show a higher DNBR.

MR. WALLIS: Instead of whatever --

MR. HUEGEL: Right. You have layer upon layer upon layer of conservatism placed in the analysis.

MR. WALLIS: But say that these conservatisms somehow overwhelm the uncertainties in the thermal-hydraulic code.

MR. HUEGEL: Yes, absolutely.

MR. WALLIS: And, also, you have to put, in, to get this 1.385, you have to put in a DNB correlation --

MR. HUEGEL: Right.

MR. WALLIS: -- that has uncertainty in it as well.

MR. HUEGEL: That is correct.

MR. WALLIS: Presumably, all these things are figured into the choice of 1.38.

MR. FINLEY: And so that gets to the other side of the curve --

MR. WALLIS: There's a whole pile of stuff behind this which is difficult for us to assess without digging into it for days.

MR. HUEGEL: Understood, yes. So there's a lot of --

MR. WALLIS: If I am understanding -- I mean you're assuring us of all this stuff which sounds good, but we don't really know how to balance these things, some of which move one way and some of which move the other --

MR. HUEGEL: Understood.

MR. WALLIS: -- to be really convinced that everything you are doing is conservative. So that is the problem --

MR. FINLEY: Well, Dr. Wallis, one of the things we tried to demonstrate on this slide is the margin in the DNB testing and the data, and so forth, as well.

MR. WALLIS: Yes.

MR. FINLEY: As you see up above, up above the safety limit, there is a stackup of margin --

MR. WALLIS: Right.

MR. FINLEY: -- to address those uncertainties.

MR. WALLIS: Right.

CHAIRMAN DENNING: Are you going to

explain --

MR. FINLEY: And I will start with that.

CHAIRMAN DENNING: Go ahead. Go ahead, do that.

MR. FINLEY: I think Sam Miranda is actually going to speak more to that. But if you start sort of with the definition of critical heat flux, 1.0, of course, we have test data which is done for the particular fuel type that we are using, and there is a scatter of that data, of course.

MR. WALLIS: Well, the 1.17 reflects the DNB correlation uncertainty?

MR. FINLEY: That is correct.

MR. WALLIS: Okay.

MR. FINLEY: At a 95 percent probability with 95 percent confidence, and the applicable limit is 1.17, right?

On top of that, we have a design limit which accounts for parameter uncertainties such as temperature, pressure, flow --

MR. WALLIS: Depending on where you are on in the physical space?

MR. FINLEY: Right, some of the geometries, et cetera. So there's an additional 5 percent or so on top of that to protect for that.

MR. WALLIS: Then the thermal-hydraulic calculation uncertainties is what makes you go up to 1.38, is it?

MR. FINLEY: Help me out, if you would.

MR. WALLIS: The RETRAN uncertainties?

MR. HUEGEL: The difference between the 1.24 and 1.38 is just generic margin that we retain to account for unexpected penalties that may come up.

MR. WALLIS: There's several engineering guesses? We're not quite sure, so we'll add something on?

MR. HUEGEL: I'm not sure I would say, "guess," but --

MR. WALLIS: Well, a judgment. It is a judgment.

MR. HUEGEL: It is a judgment.

MR. WALLIS: Because other plants have different numbers.

MR. HUEGEL: Yes, that is correct.

MR. WALLIS: That is what is so mysterious about how someone arrives at 1.38 and someone else is 1.45 and --

MR. HUEGEL: Well, hopefully, it is not mysterious.

MR. WALLIS: -- someone else is 1.5, and

so on. Okay.

CHAIRMAN DENNING: A couple of other questions then.

MR. FINLEY: Yes.

CHAIRMAN DENNING: On the over-pressure, I want to make sure I understand.

MR. FINLEY: Yes.

CHAIRMAN DENNING: This is different from what -- this is primary system pressure?

MR. FINLEY: That is correct. This, of course, loss-of-flow event is a heat-up event.

CHAIRMAN DENNING: Yes.

MR. FINLEY: During the event, D average goes up, causes an insurge to the pressurizer. It compresses the bubble in the pressurizer. And even taking credit conservatively in this case for the sprays acting as they should, and so forth, the pressure goes up about 75 pounds in this transient at the time of minimum DNBR.

CHAIRMAN DENNING: And you don't take that into account in your correlation?

MR. FINLEY: That is correct.

CHAIRMAN DENNING: You just keep it at the initial pressure?

MR. FINLEY: That is correct.

CHAIRMAN DENNING: Now you could take into account or is there not a pressure dependence developed for the correlation?

MR. FINLEY: We could --

MR. HUEGEL: I think it was partly in the SER that we received, based upon how we explained the methodology, we felt that we mentioned the nominal pressure; therefore, it wouldn't be appropriate, even though it is certainly justifiable, to credit anything beyond the nominal pressure.

Certainly, as Mark explained, we see a pressure increase, and since we do see a pressure increase, we would typically assume your pressure control systems to minimize any pressure increase, like your sprays and your PORVs, but we felt, based upon what we had written up in our methodology and what was issued in the SER, we felt that we couldn't go above nominal pressure even though, again, it was perfectly justified in our minds.

CHAIRMAN DENNING: Okay. So you're saying that there are some control factors that are not allowed to be taken into account in the performance of the analysis like sprays and stuff like that?

MR. HUEGEL: No, it is just we stated we were using nominal pressure there; therefore, that's all we felt we could get away with using.

MR. FINLEY: There are items like that that we consider part of the approved methodology --

MR. HUEGEL: Right.

MR. FINLEY: -- that we would not take credit for, depending on what has been approved previously. Here I think we felt not taking credit for pressure was part of the approved method for Ginna and so we left that out.

MR. HUEGEL: Right.

MR. FINLEY: But we feel perfectly justifiable would be to take credit for that.

CHAIRMAN DENNING: Yes. Now -- I'm sorry, go ahead, Jack.

MR. SIEBER: In this particular event, though, as the coast-down is occurring, the effectiveness of sprays has gone away.

MR. HUEGEL: Sure.

MR. SIEBER: It is driven by the pump DP.

MR. FINLEY: That is correct.

MEMBER MAYNARD: But, typically --

MR. SIEBER: I mean you could actually -- well, the coast-down is what, 30 seconds or thereabouts?

MR. HUEGEL: It is a couple of seconds.

MR. SIEBER: Spray is over with before coast-down?

MR. HUEGEL: That is right.

MR. FINLEY: And we did model the spray, in determining that 75-pound increase, that was with modeling of sprays, the effect of sprayers.

CHAIRMAN DENNING: In this particular version of loss of flow is one in which, it is almost like a loss of power to the pumps where they just go into coast-down?

MR. FINLEY: Actually, this is even more severe than the typical loss of power. This, for Ginna, our limiting event is actually a grid frequency change of 5 hertz per second, which is a very, very severe grid transient, one that is worse even than the blackout that we had in 2003, where the grid actually drives the pump speed down because we are locked into the grid, okay, for a certain amount of time. It is actually a more rapid coast-down of the pumps, if you will, than the flywheel-driven coast-down would be. We actually call that a Condition 3 event for Ginna, even though we conservatively apply the Condition 2, no fuel failure criteria.

CHAIRMAN DENNING: In getting back to a point that you made about the comparisons that are made with the plant data, the plant does a similar test or has done a similar test in which it does a pump trip or something like that? And you are saying that in the prediction with RETRAN that the RETRAN results fall below the --

MR. FINLEY: Right. What we do is part of our hot functional test program. I think all plants have done this reactor coolant pump coast-down. So you get an actual data curve for --

MR. WALLIS: You don't have a back-up slide that shows that, do you?

MR. FINLEY: I don't. Sorry, Doctor.

CHAIRMAN DENNING: And that was performed a long time ago or --

MR. FINLEY: That would have been part of the initial plant startup.

CHAIRMAN DENNING: The initial plant startup?

MR. FINLEY: Hot functional testing, yes.

CHAIRMAN DENNING: But you have done the RETRAN analysis recently to demonstrate just what we heard?

MR. FINLEY: Right. But, of course, nothing really of significance would change to affect that; i.e., it is a flywheel mass really that provides the momentum and determines that coast-down rate. We have not modified --

MR. HUEGEL: But that is another conservatism, that we would reduce the inertia, even though it wouldn't apply to this event because of the frequency decay driving the pumps down, but in a complete loss of flow where the pumps are free to coast down, we reduce the inertia of the flywheel by 10 percent so that we get a conservative coast-down relative to what the plant would measure.

CHAIRMAN DENNING: And now, as far as the analysis is concerned, you start it at a slight over -- like 2 percent or 3 percent over? I mean, is this the kind of thing, over normal power?

MR. HUEGEL: Yes, all uncertainties are accounted for, but the way that we have done them is they are included in the DNB design limits. So we would have uncertainties in the power level, in pressure --

CHAIRMAN DENNING: But when you actually run it, when you run it, what power level do you use as the start?

MR. HUEGEL: It is done at nominal power.

CHAIRMAN DENNING: At nominal?

MR. HUEGEL: Yes.

CHAIRMAN DENNING: So that uncertainty was included in that --

MR. HUEGEL: Yes, that is correct.

CHAIRMAN DENNING: Now what about as things -- about during the cycle and stuff like this? Is there a point in the cycle like when the moderator coefficient is the least negative or something like that that has an impact? I am trying to get a feeling for whether it is done at the worst time in the cycle.

MR. FINLEY: Right, right. Certainly, yes. This is a heat-up event. Obviously, the least negative or positive moderator temperature coefficient would be limiting. We can't operate at full power with a positive moderator temperature coefficient. So it would be something, our most, least -- excuse me -- our least negative moderator temperature coefficient would be used early in cycle, right.

Right. So, as was said before, there are layers and layers of conservatism in each of the inputs that we take at the same time. We think that far outweighs any uncertainty in the RETRAN numerical calculation itself.

CHAIRMAN DENNING: Well, the best evidence I have heard so far is that you actually have done the work on the experiment with the plant and that the

RETRAN results fall below that level.

MR. FINLEY: Right.

MR. HUEGEL: That is correct. That is correct.

CHAIRMAN DENNING: Okay.

MR. FINLEY: We typically do that in the safety analysis for the parameters that are critical. It is done and NRC has asked to do that over time to approve the methodology.

MR. WALLIS: When you come to the full Committee I don't know if we are going to go into this again, but other Committee members may have the same curiosity that we have. So it might be good to have some back-up slides with this RETRAN compared with the real plant transient, and so on, just in case someone starts to probe.

CHAIRMAN DENNING: Well, I think let's get a little bit beyond that. I mean I would certainly like to see that.

MR. WALLIS: So we want to see it ourselves?

CHAIRMAN DENNING: Why don't we see that?

MR. WALLIS: Can we see it when, this afternoon or something, or when?

MR. HUEGEL: Do you have any of the coast-downs, Mark? I don't know.

MR. FINLEY: I will try to get it this afternoon. I don't have it at my fingertips. So we will look.

CHAIRMAN DENNING: Okay.

MR. WALLIS: Yes, maybe if we are satisfied, we can convince our colleagues to be satisfied, but that is always difficult.

MR. FINLEY: Okay, any other questions on loss of flow?

CHAIRMAN DENNING: No. Let's move on.

MR. FINLEY: Okay.

MR. WALLIS: So now we have a different issue, which is pressure.

MR. FINLEY: Okay, a different issue. This is pressure. This is a loss-of-load event. Just as the title suggests, it is a full loss of load, a turbine tripped a generator off the grid.

Again, I will start in the middle here. Our design limit or acceptance criteria for the event is 110 percent of the design pressure for the reactor coolant system. The safety analysis result was about a pound and a half below that, 2747 as compared to 2748.5.

Again, this looks close, but we need to

take it in the context of margin below and margin above, which is what this slide tries to demonstrate. For example, if we did take credit for control system functioning, i.e., steam dump operation and pressurizer spray operation, that alone would reduce the peak pressure by over 100 pounds. Similarly, if we added operation of the PORVs to that mix, that would provide another 40-pound-or-so reduction.

Probably most importantly, and why you don't see issues with these types of events in the industry, is when you get a turbine trip, we are designed, as all plants are, to get a reactor trip automatically. So there is no real delay between the time of the turbine trip and the reactor trip.

What causes the over-pressure in the analysis is a short time delay between the trip of the turbine and the trip of the reactor. There's where you have a power mismatch for a short period of time, causing additional heat and causing the pressure overshoot --

MR. WALLIS: If we were following a PRA-type analysis, you would go through this event tree and you would say, did the PORVs work or did the Pzr pressurizer spray work? And you give some probability to all those things, presumably. That would be a way you could --

MR. FINLEY: That is correct. That is correct.

MR. WALLIS: Here you are simply saying we will just assume it doesn't happen.

MR. FINLEY: Right.

MR. WALLIS: And so you give a probability of zero.

MR. FINLEY: Exactly, exactly. In fact, I discussed -- just to give a flavor for that, we have two, essentially, relays on sets of contexts which will cause a reactor trip on a turbine trip. If either one functions, you will get the reactor trip simultaneously, essentially.

I talked to our PRA folks a little about that and asked them what probability they would assign to that. He said between 99.9 and 99.99 probability of success.

So between 99.9 and 99.99 percent of the time our result is down here.

MR. HUEGEL: But it is not a safety grade function. Therefore, we can't credit in the safety analysis. So we have to rely upon the high-pressurizer pressure reactor trip to terminate the transient, even though, as Mark said, that that function, even though control grade, is highly reliable.

CHAIRMAN DENNING: At what level does the pressure trip then?

MR. FINLEY: The high-pressurizer pressure trip --

MR. HUEGEL: Yes, 2377 is the value at the plant, but the safety analysis would assume 2425 or 2435. So we have accounted for uncertainties between what the plant would be dialing in and what we were assuming in the safety analysis to account for all the instrumentation uncertainties.

MR. WALLIS: How about RETRAN here? Is RETRAN accurate to 10 percent, so we don't have to sort of add another 10 percent on this thing for some reason?

MR. HUEGEL: Well, RETRAN we found is very conservative in terms of over-predicting the pressure. Yes, it would predict a higher pressure than you would expect to see at the plant for a similar --

MR. WALLIS: It is supposed to be a realistic code.

CHAIRMAN DENNING: My experience with these codes has generally been that they predict pressure comparatively well, but what kind of evidence do you have from plant data? I mean, do you have evidence for plant data?

MR. HUEGEL: We do a lot of comparisons with these codes for load rejection tests and making sure that all the control systems are functioning as designed. We have plants out there that are full-load rejection capability plants, and in tuning the control systems we would use the LOFTRAN and RETRAN codes to make sure that we are predicting that these control systems are functioning as designed.

When we see the plant actually doing its test, we find that the results compare very favorably. But, again, that is with crediting all the different control systems, which we don't assume or credit in any of the safety analysis unless it makes the transient worse.

CHAIRMAN DENNING: Yes. As far as absolute safety is concerned here, suppose we are wrong and the pressure really is higher. Then you adjust -- you would go to the safety and the safety valves would relieve?

MR. HUEGEL: Well, the safety valves do operate in this transient.

MR. DUNNE: That is typically what terminates the transient, is when the relief valves open, but you've got to remember --

MR. HUEGEL: The reactor trip and the --

MR. DUNNE: And the reactor trip and the safety valves opening. What is happening is the peak pressure is occurring at the RCP discharge.

MR. HUEGEL: Right.

MR. DUNNE: And the pressure that the relief valves are set at is the pressurizer pressure, which is nominally around 2500. We have about a 2 to 2.5 percent uncertainty on that set point. So in the analysis base we raised the actual set point in the analysis by that 2.5 percent.

We also have a 1 percent uncertainty for loop seal drift because we have a loop seal in front of our relief valves. So you add another 1 percent on the pressure at which the safety valves will open on the pressurizer. Then there is a time delay to clear the loop seal, which is around .8 seconds or so, which there is no way to relieve --

MR. HUEGEL: Right, and there's no credit for any of the relief during that time period where the loop seal is clearing, even though you would be getting some pressure relief capability. As Jim stated, there is no credit for that in the safety analysis.

CHAIRMAN DENNING: Except if we are in an ATWS scenario which you analyze differently.

MR. DUNNE: Well, in an ATWS scenario you don't take any credit for any of that stuff. Well, you take credit for the relief valves, I think.

MR. HUEGEL: Yes, we would.

MR. WALLIS: Do you have plant data on this loss of load?

MR. FINLEY: Of course, we have experienced loss-of-load-type trips in the past.

MR. WALLIS: Yes, and you take the data and you use a realistic analysis, which would be the bottom line here using RETRAN.

MR. FINLEY: Right.

MR. WALLIS: It would be interesting to see how well you predict what really happened.

MR. FINLEY: Right. The difficulty there is you have a very benign event. This is actually the pressure at, I think, the reactor coolant pump discharge. It is low in the RCS. It is actually higher than the pressurizer pressure.

MR. WALLIS: Yes.

MR. HUEGEL: You don't even get to the point of the PORVs on the pressurizer.

MR. FINLEY: Pressurizer pressure goes up very, very little. So that data, in terms of comparison to RETRAN, wouldn't show much.

MR. WALLIS: Wouldn't show much of a challenge to RETRAN. Nothing much is happening.

MR. FINLEY: Right.

MR. WALLIS: All that is happening is in regulatory space.

MR. DUNNE: And, simplistically, you know --

MR. CARUSO: It is a challenge to RETRAN. I mean it has to calculate the physics properly.

MR. HUEGEL: That is true.

MR. CARUSO: Whatever you put in it should be able to calculate it. So if you have data for a real trip, then RETRAN should be able to calculate a real trip.

CHAIRMAN DENNING: Sure, sure.

MR. WALLIS: That would be really convincing stuff if you produced that.

MR. HUEGEL: We did have some plant comparisons in the WCAP that we submitted and was reviewed by the NRC, 14882. We chose the comparison of the RETRAN results to different plant events. I think there were some load rejections.

MR. WALLIS: Okay. Is there some key part of that that we can see at this meeting?

MR. HUEGEL: We could probably pull out the slides from that WCAP.

MR. WALLIS: Because it would be good to go away with a very convinced sort of happy feeling and not feel there are a lot of things we had better study.

MR. HUEGEL: I think the important thing to take away is that the methodology, even though we have got different DNBR limits that we are using, we still apply the same exact conservative methodology which has, as we mentioned, for example, in loss of flow, layers upon layers of conservatism. I think that is the important part.

MR. WALLIS: You sound very convincing, but then, of course, you are an advocate for your point of view.

(Laughter.)

MR. HUEGEL: Understood.

MR. FINLEY: Certainly with respect to the plant data, part of the approval process with the staff in WCAP review and approval is to provide that sort of benchmarking data.

MR. WALLIS: We have to assure ourselves that the staff at least has investigated and asked the kind of questions that occur to us.

CHAIRMAN DENNING: All right. Let's go to the next slide.

MR. FINLEY: Okay. Well, before we go to the next slide, we didn't talk, I don't think, about above the design limit, to speak to that margin.

We have for Ginna calculated, as you see here, an ASME service level C limit for hot conditions of around 3200 psig. That was done for the ATWS scenario. In fact, when we do an ATWS event, we have to meet that pressure.

That is where you would potentially start to deform components in the RCS, not likely, but potential. We wouldn't expect catastrophic failure there, but potential for bolting to stretch and that sort of thing.

So that gives you some feeling for, you know, we are not on the hairy edge in terms of this 110 percent.

MR. WALLIS: You're assuming a standard atmosphere or something when you do this? We went through this before. The difference between your psi and your psi design pressure on one of these charts is less than the variability in atmospheric pressure itself.

MR. FINLEY: Correct. We don't vary --

MR. WALLIS: You're trying to assume some kind of atmosphere --

MR. FINLEY: It's 14.7.

MR. WALLIS: Although in reality it is fluctuating up and down quite a bit.

MR. FINLEY: Okay, and the last event I wanted to speak to was the rod withdrawal at power event. This event provided results close both to the DNBR criteria --

MR. WALLIS: This is where you are even closer. This is where you are about as close as you can possibly get.

MR. FINLEY: -- and also pressure. And, again, the reason for the closeness of the result to the acceptance limit is that we reduced the -- I think in this case -- Chris, correct me if I'm wrong -- we reduced the rod speed or reactivity insertion rate, essentially, until we met this limit. That is what we established as our core design.

MR. WALLIS: How can you reduce that arbitrarily? You actually can control the insertion rate?

MR. HUEGEL: No. We make sure that we've got a conservative insertion rate. Obviously, it would bound anything that we would see at a plant.

MR. WALLIS: Make it less conservative in some way? How did you manage to change that?

MR. FINLEY: And then we incorporate that restriction into our core design.

MR. WALLIS: Make it less conservative? You justify making it less conservative? Is that what --

MR. HUEGEL: No, it is the same conservatism.

MEMBER MAYNARD: This feeds back into what your surveillance requirements would be or what set point you would have to have for certain instrumentation?

MR. HUEGEL: Exactly. The other thing is when you --

MEMBER MAYNARD: You are trying to give yourself as much of a margin --

MR. HUEGEL: When we define a safety analysis limit, keep in mind that the over temperature and over power delta T trip set points are designed to provide protection based upon the conditions that are associated with what you selected for your safety analysis limit. So it is no surprise that when you have a revised safety analysis set point, you are going to have trip set points, the OTDT and OPDT, which are designed specifically to ensure you are meeting your DNB design basis, that you are going to end up with a result that is consistent with your safety analysis limit here.

What Mark was saying is we refined the reactivity insertion rates that we looked at to make sure that we were getting the closest match to the safety analysis limit. We analyzed a whole wide range of reactivity insertion rates from like 1 pcm per second up to, say, 110 pcm per second, which covers the maximum differential rod worth you would expect to see anytime in the core design life and also associated with your maximum rod speed that you would expect to see at the plant. Combining those two, we cover the whole wide range of reactivity insertion rates.

What we just did here is refine and make sure that we picked the lowest or the exact reactivity insertion rate that gives you the closest approach to your DNBR limit. So that might have been, say, 25 pcm per second, where maybe in the previous analysis we used a more coarse comparison of reactivity insertion limits because we had more margin to the result.

MR. WALLIS: Make sure although in reality it isn't worse?

MR. FINLEY: That's correct. And then we factor that input assumption to the safety analysis into our surveillance program as well as into our core design process. So that when we design the core and we use the physics codes to validate the reactivity parameters, we do that. We do that each cycle.

MEMBER MAYNARD: They're not arbitrarily changing numbers that have no impact on something else.

MR. HUEGEL: No.

MEMBER MAYNARD: They are really defining what their surveillance requirement or their set points would be on other parameters to assure they're meeting them.

MR. WALLIS: I'm just trying to figure out if there isn't a possibility that the rod withdrawal rate somehow exceeds something that you have set to it.

MR. HUEGEL: No. The other thing is we don't limit the insertion either. I mean you have a limited amount of bank worth that you can add in terms of reactivity. What we assume in this transient is that we keep adding whatever amount of reactivity it takes us to get us up to the trip condition.

So, in reality, you may have a total bank worth say at 90 percent power of 500 pcm. That might not be enough to take you up to the trip set point that we have assumed, which is like 118 percent power. However, as part of the conservatism of the analysis, we keep adding reactivity, even though it may not truly exist, until we get to the reactor trip set point.

We do that from all different power levels, from different times in life, and for all different reactivity insertion rates. So we are analyzing hundreds and hundreds of cases to get to the reactor trip set point, when in reality for a lot of the cases you wouldn't even get there.

MR. WALLIS: Well, tell me, physically, how does this reactivity get inserted?

MR. HUEGEL: It is assumed to be inserted at a constant rate.

MR. WALLIS: It is a withdrawal of rods, right?

MR. HUEGEL: Right.

MR. FINLEY: You have to start --

MR. WALLIS: The physical withdrawal of rods? Is this something that happens inadvertently due to some glitch or is it something the operators do? Is it something that happens because of an accident or what?

MR. HUEGEL: It is considered to be a Condition 2 transient, which could be, one, a failure in your control system or, two, it could be operator error.

MR. WALLIS: So the physics limits the reactivity addition rate, doesn't it?

MR. HUEGEL: And keep in mind that --

MR. WALLIS: Doesn't it? In some way?

MR. HUEGEL: Yes.

MR. WALLIS: And so you can't so arbitrarily set it? It seems to me you are still twiddling it until you get the right number, and you can't do that. It tells you what it is going to be --

MR. FINLEY: No, no. In the core design process, by changing your core design and the worth of the rods, you can effect that reactivity addition. So we control that.

MR. WALLIS: And then you control that to be the maximum it could possibly be in the transient?

MR. FINLEY: That is correct.

MR. HUEGEL: Yes. They would have some curve. The differential rod worth varies as a function of rod position. We pick off the peak and then make sure that our --

MR. WALLIS: You make sure that it is as fast as possible then?

MR. HUEGEL: That presents an upper bound which essentially we are well beyond that differential rod worth peak in terms of the range of reactivity insertions that we would look at.

CHAIRMAN DENNING: With regard to the implied rate of withdrawal of the rod --

MR. HUEGEL: We cover a whole wide range.

CHAIRMAN DENNING: But how does that relate to the maximum, that withdrawal rate that is possible? I mean you push a button and have a rod withdrawal.

MR. HUEGEL: That's right.

CHAIRMAN DENNING: It is a certain rate of withdrawal that is implied.

MR. HUEGEL: That is right.

CHAIRMAN DENNING: And then the reactivity rate depends upon what the worth of the rod is.

What is the implied rod withdrawal rate relative to the standard? Is it --

MR. HUEGEL: Again, what this safety analysis assumes is a whole wide range of constant reactivity insertion rates in pcm per second. That implies a constant differential rod worth and a constant withdrawal rate for that given condition that we are analyzing.

Keep in mind that we analyze a whole wide range of reactivity insertion rates which conceivably would cover a whole wide range of differential rod worths and rod speeds. So we have encompassed any particular rod speed that you could have at the plant and also we have bounded any particular differential rod worth that the core design would calculate, which is confirmed on a cycle-by-cycle basis.

CHAIRMAN DENNING: What limits the rate of rod withdrawal?

MR. HUEGEL: What is the fastest -- I think it is 72 steps per minute -- or is it 66? Okay, sorry, 66 steps per minute. The maximum differential rod worth that I think we have assumed is something like 100 pcm per step.

MR. McHUGH: Yes, this is Chris McHugh from Westinghouse.

The last reload cycle, the actual calculated maximum rod worth was about 30 pcm per second. In our rod withdrawal power analyses, like Dave said, we go up over 100. So we have covered from 1 pcm per second up to 100, and on a cycle-by-cycle basis we need a maximum of about 30.

MR. HUEGEL: Thank you, Chris.

MR. CARUSO: Can you physically change the rod withdrawal speed? Or is that something that is locked into your control system design?

MR. FINLEY: Right. Not without modifying the plant and doing testing post-modification to verify the rod speed.

MR. CARUSO: But you have a current defined rod speed that is locked into the rod control logic?

MR. FINLEY: That is right. It is part and parcel to the design.

MR. WALLIS: 1.381 comes from the fastest withdrawal rate that is possible?

MR. HUEGEL: No. We have looked at a whole wide range.

MR. FINLEY: No, it is one of the intermediate --

MR. HUEGEL: Yes.

MR. WALLIS: One of the intermediate ones which is worst?

MR. HUEGEL: Yes.

MR. WALLIS: Okay. And rod ejection is something else?

MR. HUEGEL: Yes, that is a whole other beast.

MR. WALLIS: A whole other beast because, obviously, rods could go, you know, flying out under some imagined scenario.

MR. HUEGEL: Right. The other thing is I think there are also rod blocks. I think if you exceed like 3 percent, don't the rods automatically -- but that is a control grade function again, which we don't credit in the safety analysis.

CHAIRMAN DENNING: Why don't you come up to the mike? State your name, please.

MR. GILLON: I'm Roy Gillon. I am Senior Reactor Operator since 1991, current Shift Manager at Ginna.

We also have five rod stops, OT delta T, OP delta T; difference in average T, any single T average, low power, 12.8 percent, and a 20 percent drop in power also give us a rod stop.

MR. HUEGEL: And these are all well below the reactor trip set points that we are crediting on the safety analysis. We don't take credit for any of these control grade functions, which would effectively limit or make these transients very, very benign.

MR. WALLIS: I am trying to think if I'm

right now. This 1.381 comes from looking at all times in the cycle, all places where rods could be, and all rates at which they could be withdrawn? At the worst? Is that what you have done?

MR. HUEGEL: This limit is set before we even look at the transients.

MR. WALLIS: But I am just trying to make sure, are you telling me it is the worst case when you look at --

MR. HUEGEL: Yes.

MR. WALLIS: -- all times in the cycle, all places where rods could be, and all rates at which they could be withdrawn? You somehow span this whole volume of space and you look for the worst DNB situation?

MR. HUEGEL: Yes, with no credit for any of the control functions and with an infinite amount reactivity.

MR. WALLIS: So when you say 1.381, you are probably looking at the real tail-end of some probabilistic distribution of what could happen?

MR. HUEGEL: Yes.

MR. FINLEY: That's correct. Absolutely correct.

MR. WALLIS: And, in effect, you are

beyond the tail-end or you so claim to be, the real limit of the tail-end?

MR. HUEGEL: We believe that the analysis, again, is very, very conservative.

MR. DUNNE: This is Jim Dunne.

Again, what Chris McHugh said is this is the analysis that we have set up as a bounding analysis going forward for EPU. Then as part of every cycle design for the core design for that cycle, they've got to verify that their limiting condition for that cycle is, indeed, still bounded by the --

MR. WALLIS: It must be running for quite a long time to get this number.

(Laughter.)

You must be running about a third of the time you are running the reactor to predict what is going to happen next time.

MR. FINLEY: There are dozens and dozens of cases, yes.

MR. WALLIS: Right. Okay.

MR. HUEGEL: We make assumptions that, hopefully, we don't have to look at the safety analysis every cycle, but what we do confirm every cycle is that what we have assumed in the safety analysis is bounding, and as Chris McHugh stated, what we have assumed in terms of a peak reactivity insertion rate is as well above what the core designs are currently predicting.

MR. WALLIS: If you conquered some sort of fuel management program which enabled you to do this, you presumably would reduce the power or do something? You have to adjust something.

MR. HUEGEL: You would have to adjust something, but we've got so much margin here I don't think it is a problem.

MR. WALLIS: Okay.

CHAIRMAN DENNING: I think they can continue.

(Laughter.)

MR. FINLEY: Good. Next slide, Gordon.

Okay. The last slide with respect to margin here for non-LOCA events would be, again, the rod withdrawal, but this time with respect to pressure. This just demonstrates, again, if we took credit for a more realistic, yet still bounding and conservative reactivity addition rate, the peak pressure would come down nearly 200 pounds as a result, still a similar sort of bounding analysis looking at all the potential scenarios we could be in, but just taking some of the margin that is in that one assumption with respect to reactivity addition.

MR. WALLIS: So it looks as if this is what is limiting your power uprate then?

MR. FINLEY: That is correct.

MR. WALLIS: If you had a higher power uprate and you didn't twiddle a few more things, you would go over this bound?

MR. FINLEY: That is correct. These three events are the limiting events for the Ginna uprate.

CHAIRMAN DENNING: And this is actually a slightly different, it is a different -- the particular selection of input parameters that leads to this limited event is different from the selection that led to the DNB --

MR. FINLEY: That is correct. This comes from a different set of initial conditions, yes.

MR. HUEGEL: But we do cover the wide range of reactivity insertions that we talked about in the DNB space. So we still are looking at anything that we conceivably could come up with in terms of --

MR. WALLIS: When you are searching for an optimum or maximum, you have to take a lot of runs to be sure you are there?

MR. HUEGEL: It runs pretty quickly.

MR. WALLIS: So that when you take small

break LOCA, you have to take quite a lot of steps in the break size in order to get the real maximum?

MR. HUEGEL: Well, these transients are over in a few minutes. So we can run tons of cases within a half an hour. I mean this is not a problem running many, many cases. It is not a LOCA where you are looking at it for an extended period of time.

MR. WALLIS: I am just wondering if mathematically you can be sure that you are within this .4 psi in terms of having determined the maximum.

MR. HUEGEL: Well, the closer we get to the limit, obviously, the more refined we have to be in terms of what we look at in terms of reactivity insertion rate.

MR. WALLIS: But we have to get comfort from the fact that there's all this margin and all these conservative assumptions.

MR. HUEGEL: And that's what we want you to walk away with, that there is a lot of conservatism.

MR. WALLIS: About the accuracy with which you can predict this to five significant figures.

MR. HUEGEL: Exactly.

MR. FINLEY: Okay, the next slide, Gordon.

Just to summarize, once again, all of the results meet the acceptance criteria. There are various areas of margin in the methods and in the inputs. In addition, there's margin above the acceptance limits to the point of failure.

MR. WALLIS: What would make me happier, I think, in the long run would be if the margin were expressed in some quantitative way representing a measure of safety, whatever that is. Because you can talk forever about margin and say, "Well, we've got 100 psi here," but what does that really mean in terms of public safety? You have to be an engineer and you have to use judgment to say, "Well, we've got 100 psi. That sounds good."

But if you could express this margin in terms of some measure of public safety, which is 10 to the minus 10 or something, that might be much more convincing.

MR. HUEGEL: Right, and you have to also have confidence that the methodology that we are applying is robust. What we are applying here is the same that we have applied for the last 30 years.

MR. WALLIS: Then we would have to examine ASME and I would hate to get into that.

(Laughter.)

MR. FINLEY: Okay. Well, thank you. That

is all I had for the non-LOCA events.

CHAIRMAN DENNING: Very good. I think we will just go ahead.

MR. WALLIS: Very, very good. Thank you very much.

CHAIRMAN DENNING: Go ahead with the regulatory version of this.

MR. WALLIS: It's not quite a Ph.D. exam because you didn't show us equations, but we are getting there.

(Laughter.)

Now we are going to look at the staff view of all of this?

CHAIRMAN DENNING: Yes.

MR. WALLIS: To put this in perspective, I was interested enough after our last meeting on this subject, margins, to go back and read the transcript, which I very rarely do, to see what questions got answered and which questions did not. So we are really interested, at least I am very interested in this issue. I want to look at the transcript maybe from this presentation and see how well we got convinced.

MR. MIRANDA: My name is Sam Miranda. I'm a reviewer in the PWR Systems Branch. I reviewed the

Ginna power uprate application.

I have the same slides, basically, as you have seen before.

MR. WALLIS: But with now different curves on them or the same curves?

MR. MIRANDA: I have the Ginna transients I can discuss, but before that I have all the same margin and acceptance criteria slides that you have seen. Unless there are any questions, I suggest we just enter them into the record and move on.

CHAIRMAN DENNING: Okay, very good.

MR. WALLIS: Okay.

MR. MIRANDA: There is this one slide that is a little bit different. It has some different numbers on it.

MR. WALLIS: You have different numbers and then they use RETRAN instead of some other code, and so on, right.

MR. MIRANDA: So we move from seventies technology to nineties technology from LOFTRAN to RETRAN.

MR. WALLIS: So we are on the margins part here, are we?

MR. MIRANDA: Well, I am going to start with the accident analyses unless you have some questions on the margins.

CHAIRMAN DENNING: Well, I guess the only question is that change that we just had where yesterday we were looking at 1.55 and today we are looking at 1.38, and the question is, what's the smallest value that NRR will accept?

MR. WALLIS: I'm sure the industry is very interested in their answer, I'm sure.

MR. MIRANDA: That margin between the design limit and the safety analysis limit is determined by the licensee and the vendor analysis, the analysts at the vendor. It is a safety margin in the true sense. It is a contingency. It is for unexpected problems.

It is something that the staff doesn't really see. All we can judge is, do the accident analyses meet the safety analysis limit? We know there is some amount of non-zero margin between the design limit and the safety analysis limit.

MR. WALLIS: But suppose a vendor came in with 1.25 and you don't see where it came from; are you going to accept it?

MR. MIRANDA: A safety analysis limit of 1.25?

CHAIRMAN DENNING: No, the safety analysis limit is, I think, 1.2 --

MR. WALLIS: No, the safety analysis is 1.38. That is the one we are talking about.

CHAIRMAN DENNING: Oh, I thought the DNBR. Yes, let's put the margins up there again, the one that has the 1.38.

MR. WALLIS: I am a little bit puzzled. This is determined by the licensee and the vendor using methods that you don't know about?

MR. MIRANDA: We know about the correlation limit.

MR. WALLIS: Yes, that is based on a publication.

MR. MIRANDA: And we know about the design limit.

MR. WALLIS: That's based on a publication.

CHAIRMAN DENNING: Right, right.

MR. MIRANDA: Those have both been reviewed and approved by the staff.

MR. WALLIS: Right.

MR. MIRANDA: The part we don't know about is the space between the design limit and the safety analysis limit.

CHAIRMAN DENNING: Right, and Graham says, okay, suppose this is 1.25; they decide let's go for 1.25. What do you do?

MR. MIRANDA: It is a matter of judgment. If they say 1.25 and if they produce analyses that all meet that value, I don't see how we can object.

The only problem with that is if something comes up in the future, some rod bow problems or something else and they need that margin, it won't be available. Then they will have to come in and change the safety analysis limit, and that is going to require a license amendment.

MR. WALLIS: I don't understand that. I mean with 1.25, they may be predicting 1.35, and they say, well, it's a huge margin because we are predicting 1.35 and our limit is 1.25.

CHAIRMAN DENNING: Well, let me say something that I think was implied that we didn't pick up on adequately. That is this contingency element. That is, suppose during the operation of the plant there's some issue that comes up like rod bowing, and they have to then go back and say, "Oh, well, you know, we really had that extra margin in there between 1.24 and 1.38, or between 1.24 and 1.55. So we don't have to shut down the plant."

MR. WALLIS: That's what it's for?

CHAIRMAN DENNING: I have a feeling that may be what it is for?

MR. WALLIS: Is that what it is for?

CHAIRMAN DENNING: Would you respond? I wonder whether the licensee might --

MR. WALLIS: It is a very arbitrary thing.

CHAIRMAN DENNING: -- or Westinghouse might comment on that.

MR. KILLIMAYER: Hi. This is Jack Killimayer from Westinghouse, the Fuels Division.

The safety analysis limit that we use, okay, the 1.24, the design basis limit has the uncertainties rolled in and meets the 9595 criterion. When we do our analyses, we do them all to meet the higher limits, so we can build in a certain amount of margin that is shown up here.

CHAIRMAN DENNING: And the purpose of that margin is to be extra safe or is it in part or largely because you want to make sure that, if issues come up, that suddenly you're not in a position where it appears that you are beyond the design limit?

MR. KILLIMAYER: Yes to all of them. There are some known penalties that we choose to cover with DNB margins such as the rod bolt penalties. We've got a rod bolt penalty of about a percent, a percent and a half, depending on the fuel type. We cover that with the margin that we retain between the safety analysis limit and the design limit.

You do want to have some margin in all your analyses when you are going into a cycle in case something does happen when you are doing an analysis for a given reload. All our DNB analyses have an assumption on axial power shapes, and we use a bounding axial power shape, what we consider to be a bounding axial power shape, going in, and we verify that each cycle.

So if you did end up with a more limiting axial power shape, you would have margin within the safety analysis limit to address small issues like that.

MR. WALLIS: So we are talking about .14, a difference between 1.24 from 1.3, which seems to be based on something insubstantial in terms of justification. Then we quibble about the difference between 1.38 and 1.381, which is less than 1 percent of this thing which seems to be somewhat arbitrary.

CHAIRMAN DENNING: Well, you and I are quibbling; I am not sure that they are quibbling.

MR. WALLIS: Well, we are questioning, let's say.

And yet they struggle to meet this 1.38 with this huge accuracy when it seems to be itself picked out of the air, to some extent. It seems to me a strange thing, you know.

Maybe if it is 1.3 -- it really might as well be 1.37. Why not?

MEMBER MAYNARD: I didn't see that they were struggling to meet that. They were intentionally --

MR. WALLIS: Yes, they were. They deliberately tried to get right on the --

MEMBER MAYNARD: -- getting there, so that they could establish design and set point criteria.

MR. WALLIS: They deliberately tried to get to 1.381, as far as I can make out.

MR. MIRANDA: I think the difficulty there is that the safety analyses that we were looking at are not safety analyses in the strict sense. They are also sort of design analyses. They are trying to come up with, by doing these safety analyses, come up with enough operating margin, operating space, for the future as possible.

So they use, they did, for example, the rod withdrawal at power analyses over a wide range of reactivity insertion rates and other conditions such that there's no future core reload that will go outside that area. They would do that up to the very limit, up to the 1.38, to make sure that they have given themselves as much space as possible.

MR. WALLIS: But the area then doesn't set the number 1.38. They could have had a higher power uprate and done all this analysis of core reload and said, "All right, our number is 1.36 and we're happy with that."

MR. MIRANDA: Well, they could have just as easily have done that.

MR. WALLIS: Well, why don't they do that and they come in with a 10 percent power uprate?

MR. DUNNE: The power uprate, power level was picked first and then all the analyses to support it were done.

MR. WALLIS: That's right.

MR. DUNNE: We didn't do all these sets of analyses and then come say --

MR. WALLIS: Put the cart before the horse. So you assume what you want to do and then justify it.

MR. DUNNE: Well, the other thing on the power uprate is we are also limited by the balanced plant side of the plant.

(Laughter.)

So if we wanted to go higher, then we would have more modifications to make on the balanced plant side of the plant.

So, you know, you end up choosing what your power level is --

MR. WALLIS: I understand that, but we are talking about safety here. We are talking about safety.

MR. DUNNE: Right, but that's the reason why we would not have actively pursued going much higher than the number we chose.

MR. WALLIS: It seems to me there has to be a justification for 1.38 which is more than saying that the vendor and the licensee decided in some mysterious way that's what it should be.

CHAIRMAN DENNING: And that they wanted that margin.

MR. WALLIS: Right.

CHAIRMAN DENNING: I mean that seems to be the margin they want. Again, it is a value to them related to these unforeseen --

MR. WALLIS: In some unforeseen circumstances they might go down to 1.30.

CHAIRMAN DENNING: Yes, that's right.

MR. WALLIS: And then they would come to us and say, "There's no problem because it is still above 1.24."

CHAIRMAN DENNING: And then they would come up and they would say, "Well, it's no problem." I think that's what we are hearing.

MR. WALLIS: Is that what happens?

MR. MIRANDA: No, they can't -- I don't think they can do that. I mean they have set the safety analysis limit that's in the tech specs. If they come in with something less than 1.38, they would have to justify it. They would have to come in and ask for an amendment, and then the staff would review that. But anything above 1.38 --

CHAIRMAN DENNING: They're locked into that.

MR. WALLIS: There had another plant yesterday that was 1.55.

MR. MIRANDA: Yes.

MR. WALLIS: They look at this plant and they say, "Gee whiz, there's no reason we should be 1.55. Why don't we come in with 1.38 and go for a power uprate of 30 percent?" Would you let them do that?

MR. MIRANDA: Well, actually, for Beaver

Valley, that has a little bit of history behind it. They could have been below 1.55, but they had, I believe they had 1.55 in the past and they didn't need to go below 1.55. The results were acceptable at 1.55, so they just kept it. So they had more than the average margin between design limit and the safety analysis limit.

MR. WALLIS: Yes, but that's why they might use it. Why don't they use it? Why don't they capture some of that margin and go to higher power?

MR. SIEBER: Well, the higher power is limited by how many dollars you want to spend on --

MR. WALLIS: But we're talking about safety. Dollars are irrelevant.

CHAIRMAN DENNING: No, but as far as the plant is concerned, they're --

MR. WALLIS: But these numbers should have a relationship to safety. That's what we're here for, isn't it? We're not here for anything to do with dollars.

MR. FINLEY: Right, Doctor, and we meet the safety limit, right?

MR. WALLIS: Set by you, it seems to me.

MR. FINLEY: No. These limits have been reviewed by the staff and accepted. We treat them as safety limits and we demonstrate we meet them with the power level that we have chosen.

As Jim Dunne said, we chose the power level based on many parameters. These safety limits are part of that decision process.

MR. WALLIS: The 1.38 is historically what you have had in this plant, is that it?

MR. KILLIMAYER: No. This is Jack Killimayer again.

We do set the safety analysis limit. Yes, there is, in a sense, an arbitrary amount of margin that is put in. It does cover known penalties, and we do build in extra margin to cover contingencies for the future.

It is an agreed-upon number as to how much margin we retain in the DNB analysis versus where it is in operating space.

MR. HUEGEL: It is agreed upon between Westinghouse and the licensee.

MR. WALLIS: That's right.

MR. HUEGEL: We don't treat that as the license limit. The license limit would be the design limit, okay?

MR. WALLIS: The license limit is 1.24?

MR. KILLIMAYER: Right. The safety

analysis limit is essentially our -- it is like an accounting method for keeping track of DNB margin to account for penalties.

MR. WALLIS: So when the staff evaluates your submittal, do they look to see the DNB number is bigger than 1.24 or that it is bigger than 1.38?

MR. MIRANDA: We use the 1.38 value.

MR. WALLIS: You use the value, but that seems very strange because you are using something defined for the convenience of the licensee which has no relationship to public safety whatsoever.

MR. MIRANDA: Well, there is a relationship to public safety. It is a value that is greater than the design limit.

MR. WALLIS: But 1.24 has some merit in terms of a measure of public safety.

MR. MIRANDA: Yes.

MR. WALLIS: The 1.38 does not; you said, but it is bigger.

MR. SIEBER: It has more --

MR. WALLIS: But it could be 1.9. I mean it is just arbitrary.

MR. HUEGEL: But the important thing is it is greater than; the 1.38 has an important part because it was met based upon a conservative methodology. So using our conservative methodology, we are meeting the 1.38, which includes, granted, it is rather arbitrary, but some amount of DNB margin above the design limit to handle the unexpected issues that do arise, as was pointed out, the rod bow penalty, for example.

You don't want to be in a situation where you have done your safety analysis right up to the design limit; something comes up unexpected, and you're strapped and you have no room to maneuver other than telling the plant, "Well, you have to derate or something." This gives us the flexibility to address the unknown issues that we hope don't occur, but, unfortunately, do occur.

MR. WALLIS: How do you get flexibility if the staff is approving 1.38 and you go down to 1.37 because of rod bow or something?

MR. HUEGEL: Because we show that the safety analysis --

MR. WALLIS: But they wouldn't shut you down?

MR. HUEGEL: No.

MR. WALLIS: Because you're above 1.24, is that right?

MR. MIRANDA: No, they would have to

explain why they are below the safety analysis limit.

MR. HUEGEL: But we have met the design limit and the safety analysis limit, and we have said that --

MR. WALLIS: It's strange.

MR. MIRANDA: Telling us that you met the design limit does not satisfy us.

MR. WALLIS: Am I just odd? I think this is very strange.

CHAIRMAN DENNING: But it is possible they could come to you and say -- I mean it sounds like we're hearing slightly different things, but what you are saying is that is what you license them with a particular core reload, core load; that's the way they operate the plant. If they find something mid-cycle that is an issue that would say that they are in conflict with that, then the licensee comes to you and says, "We want to have some granting relaxation," right? And it would be up to NRR to say yes or no, is that right?

MR. MIRANDA: Something like that. If something comes up in the future that causes them to use up all of their 11 percent margin between the design limit and the safety analysis limit --

CHAIRMAN DENNING: Well, I'm only going to let them use up 1 percent of it. Suppose they decide that it is 1.37. You know, something has happened. Now what is the requirement on them? Do they have to now -- are they in conflict with their license and they have to either shut down the plant -- I mean they have to shut down the plant within "x" amount of time or something.

MR. SIEBER: Reduce power.

CHAIRMAN DENNING: Or reduce power? And then you would have to grant some exception to allow them to go back to power? Is that a true statement?

MR. SIEBER: They would have to justify that based on a reevaluation of the uncertainties. That is one way to do this.

CHAIRMAN DENNING: So, actually, what would probably happen --

MR. SIEBER: What they come up, the staff might or might not agree with --

CHAIRMAN DENNING: Might or might not.

MR. SIEBER: -- a new limit.

CHAIRMAN DENNING: Yes, right?

MR. SIEBER: And you would recapture some of the margin that you put in there in the first place.

MR. MIRANDA: I'm a little bit confused.

Are you talking about the safety analysis limit or the design limit?

CHAIRMAN DENNING: The safety analysis limit.

MR. SIEBER: The safety analysis limit has extra margin.

MR. MIRANDA: They need to change the safety analysis limit; they would need to come to the staff.

MR. SIEBER: You would have to agree before they could do it then?

MR. MIRANDA: Since that is in the tech specs, that is a license amendment and the staff would have to review and approve that.

MR. WALLIS: It seems to me to have nothing to do with nuclear safety. I mean if 1.24 means the public risk is 10 to the minus 5 and 1.38 means it is two times 10 to the minus 5, that is very different from its being 10 to the minus 6. Until there is some scale which tells me what we gain in public safety by having this extra margin from 1.24 to 1.38, I don't have any way to evaluate how big it should be.

MR. MIRANDA: I don't see the need for evaluating that. That is a designer's margin. That is for their use in contingencies to cover unexpected problems.

MEMBER KRESS: You are suffering under the whole problem of all the licensees in design basis space which has a relationship to safety but it is not fully quantified because you've got these design basis events that represent ranges of accidents, and they do them conservatively. You end up with margins for the design basis events.

But how to relate that to some real measure of safety, which might be a risk number, is you have to -- it is an after-the-fact thing. You can go back now and say, "We'll do a PRA and we'll see if this design is safe from the standpoint of any risk measures you have." But it is an after-the-fact calculation.

To try to relate things like how much this margin contributes to that safety is just --

MR. WALLIS: I'm really puzzled though. I mean 1.24, see, it has a basis, right? It seems to me that -- I'm trying to relate it to my experience. If we say that we are going to educate students to pass a professional engineering exam, in a professional engineering exam to be a qualified engineer, you have to get a grade of 1.24. But the student says, "Well, I want to be better than that because I want to be a better engineer. So I am going to come up and say you're going to grade me to be above 1.38," and we agree to that. But it is all just arbitrary from the student's point of view.

MEMBER KRESS: Well, sure it is.

MR. WALLIS: It is not justified by the agency.

MEMBER KRESS: It is not quite arbitrary because it is designed by space and you did it in a conservative way and you end up with a conservative --

MR. WALLIS: But the number is set by the licensee and the vendor. It is not set by the agency.

MEMBER KRESS: That's pretty much arbitrary.

MR. WALLIS: It is really peculiar to have a safety thing set by the vendor rather than the agency. But, anyway --

(Laughter.)

MEMBER MAYNARD: I think the safety thing here is the design limit. Now the closer that the safety analysis limit comes to that, the less things that they are going to be able to tolerate --

MR. WALLIS: I understand that.

MEMBER MAYNARD: -- from other things.

The higher they go, that removes operating flexibility from the plant.

It is not as much a safety issue as it is as to, how much do you want to be able to tolerate without having to go back and reanalyze and resubmit?

MR. WALLIS: They still have to resubmit though. If they come up with something which is 1.3, they have to resubmit.

MEMBER MAYNARD: But they are a lot less likely, if they started with 1.3 and that you had some rod bowing or you had some thing, they are not going to be able to absorb as much of that. So the lower they make that limit -- yes, if they do end up below that 1.38, they've got to come in.

MR. WALLIS: Right. There's a likelihood that after they come in they can go out satisfactorily?

MR. SIEBER: Yes.

MR. WALLIS: Whereas if they were closer to it, they might be more at risk of being shut down? Is that the idea?

MR. SIEBER: Well, you have to make sure that you aren't going to approach the design limit.

MEMBER MAYNARD: It is going to change other -- if they have to come in with a lower number, then it is going to change some other things in a tighter design or different set points or different limits from that aspect.

CHAIRMAN DENNING: I think another thing that we have to get perspective on, we tend to think in risk space, and these are Condition 2 and Condition 3 events. Even defeating the design limits in these cases doesn't put you in a core meltdown situation typically.

MR. WALLIS: That's right.

MEMBER KRESS: It could possibly do some fuel damage.

CHAIRMAN DENNING: It could do some fuel damage.

MEMBER KRESS: And we don't have criteria in terms of risk of fuel damage other than full core damage almost. So if we had that criteria, you might possibly be able to relate this change in the limit to how much fuel you might damage if you had a whole spectrum of events, but we don't have that, unfortunately.

MR. SIEBER: Actually, you don't do fuel damage until you hit the critical heat flux.

MEMBER KRESS: That's right. That's right. But if you did it right, these would have probability distributions. The overlap would give a probability of meeting that for all the design -- for not the design basis accident, but for the spectrum of accidents. You could end up with a probability of core damage and you could have some sort of measure. That could be a measure of safety.

We don't do that because right now it is too hard. This seems to guarantee safety this way by experience. It is a way that the staff can deal with and a way the licensee can deal with.

MR. SIEBER: It's deterministic. That is the way these things were --

MEMBER KRESS: Deterministic as opposed to --

MR. WALLIS: My problem dealing with it, because we are going to evaluate whether or not to allow a power uprate, and if one plant comes in with 1.55, this one comes in 1.38, another plant comes in with 1.3, another one comes in 1.25, and they all say, "We want the power uprate." It is clear that the one with 1.25 is probably going for a higher power uprate. So how do we decide?

MEMBER KRESS: That's a good question.

MR. WALLIS: How do we decide what is reasonable?

MEMBER KRESS: That's a good question.

MR. MIRANDA: Well, you would be putting yourself in the position of judging as to how much --

MR. WALLIS: We're asked to write a letter, right. Right.

CHAIRMAN DENNING: That's exactly where we are.

MEMBER KRESS: You had a suggestion once, Graham, that I really liked, and that is, these are calculated by some code, a thermal-hydraulics code.

MR. WALLIS: Right.

MEMBER KRESS: And if you, instead of having this number, had a distribution and you could come up with some sort of probability of exceeding your design, your actual CfA, actually correlation limit, and you have some idea --

MR. WALLIS: Where we are, yes.

MEMBER KRESS: But even there you've got a problem because, even though we have that probability, you don't know what probability is acceptable. And that is an arbitrary choice.

MR. WALLIS: But at least you know what you are doing more.

MEMBER KRESS: You know what you are doing.

MR. WALLIS: Right.

MEMBER KRESS: But not enough to base a decision on.

CHAIRMAN DENNING: Let's not redefine the whole regulatory basis.

MEMBER KRESS: No, that is not in the regulatory basis right now; that's right. So we are stuck with the judgment.

MR. SIEBER: The only way we could be certain that their number is right is for us to do these calculations, this whole series of calculations, and I don't want to do that.

(Laughter.)

CHAIRMAN DENNING: Well, thank you, Jack.

Go now to where you were going to start your presentation.

MR. MIRANDA: Okay. I was going to talk about the same three transients that Mr. Finley discussed earlier: loss of flow, which is the event that challenges that DNB ratio; the rod withdrawal at power, which, by the way, I disagree; I don't think this is a challenging analysis for the DNB ratio. Rod withdrawal at power is more of a design event in terms of testing the over temperature delta T trip to be sure it covers the --

MR. SIEBER: That's the culmination of it.

MR. MIRANDA: Yes.

And the loss of load, which is the event that is most likely to over-pressurize the RCS.

These are the results for the loss-of-flow accident. There are two cases described here. One is the frequency decay, which is the limiting event, and then there is the complete loss of flow. With both complete losses of flow, one involves tripping both reactor coolant pumps and the other is the situation where the reactor coolant flow is driven down by a frequency decay on the grid. That one produces a lower DNB ratio.

I would say that this event is governed mainly by the power-to-flow ratio. That is very important in DNB ratio. If you look at the power-to-flow ratio, if you delay the reactor trip, if you keep the power relatively high compared to the flow, which is decreasing, either because it pumps a trip or because of being driven down by frequency decay, delaying that reactor trip will cause a lower DNB ratio.

We can see, for example, here that looking at the two events, in the flow coast-down event you have the reactor trip immediately because that is the initiating event, the undervoltage condition on the power supply buses on the reactor coolant pumps. So there you have an immediate reactor trip; whereas, for the frequency decay you have to wait for the signal, for the under-frequency reactor trip signal, and that takes a little bit more than half a second.

Here we see on the bottom curve -- it is not a curve; it is a straight line. It is the flow rate responding to the frequency decay.

Then we have the under-frequency trip burn in about two seconds. Then, as the rods are falling into the core, you have reached a minimum DNB ratio about here. You see the power level is still relatively high.

This is the heat flux in the core average channel and the hot channel. This is a reminder, for one thing, that this event is analyzed with RETRAN and VIPRE. The RETRAN code will calculate the transient in terms of power level and back to coolant system pressure and temperatures and flow rate. Then that information is passed to VIPRE, which actually calculates the heat flux, and VIPRE will model a hot channel. Here we can see there is not that much difference between hot channel and average channel.

MR. WALLIS: All this is at some time in

the cycle or some extreme case or something that bounds --

MR. SIEBER: Worst.

MR. WALLIS: The worst?

MR. SIEBER: The worst. The worst time in the cycle.

MR. MIRANDA: From this curve, we see that minimum DNB ratio -- well, actually, I have another plot I can show that describes all of this.

The minimum DNB ratio will occur actually before the time that the PORVs might open. This is an illustration of that.

Here's the minimum DNB ratio occurring. If you take that up to the pressurizer pressure curve, you see that the minimum DNB ratio has been reached before the core opening set point is reached.

All of this is interesting and it is not really relevant, though, for this analysis because this pressure is information that is not passed to VIPRE as you see it here. The VIPRE code will calculate the DNB ratio based on the nominal pressure. So there is no credit taken for the pressurization.

MR. WALLIS: I think the key thing is what turns around the DNBR. It seems to be headed down and then it gets turned around rather abruptly by something.

MR. MIRANDA: The rods are fully inserted, okay.

CHAIRMAN DENNING: Heat flux. Heat flux.

MR. MIRANDA: It is the power to flow --

MR. WALLIS: It is the power that turns it around? Okay.

MR. MIRANDA: If we look at the first curve with the power levels --

MR. WALLIS: Okay, it is the power. That is where it is. The power torque falls off the cliff or it goes over -- it is not really a cliff, but it goes down the slope. Then that is what turns it around. Okay.

MR. MIRANDA: It is all a function of power-to-flow ratio.

MR. WALLIS: Okay.

MR. SIEBER: Well, the whole transient is caused because of the mismatch between the trip and seeing the actual cause, which was the loss of the coolant pump.

MR. WALLIS: So what would seem to be --

MR. SIEBER: You are producing power in a regime where the flood is decaying.

MR. WALLIS: What would seem to be

critical here would be how fast the rods drop.

MR. MIRANDA: Yes, and we had --

MR. WALLIS: Because if it is a little bit later, then this DNBR would go down below the safety analysis limit.

MR. MIRANDA: Right. That's right.

MEMBER KRESS: Why doesn't the DNBR turn around again at some longer time? Because your flow has continued to drop, but the power sort of levels off. So you expect that curve to turn over again.

MR. MIRANDA: Well, you do not produce -- you have the reactor trip. So you're not producing power anymore. The power that you see there is --

MEMBER KRESS: Decay heat.

MR. MIRANDA: Decay heat, yes. It is kind of hard to come up with --

MR. SIEBER: Well, if the flow continued going down, then even decay heat could reach the DNB.

MEMBER KRESS: The flow never really stops.

MR. SIEBER: Oh, that curve doesn't continue on down like that?

MEMBER KRESS: No, because you end up in natural circulation.

MR. MIRANDA: Natural circulation is --

MEMBER KRESS: Okay. Well, that's the explanation.

MR. WALLIS: DNBR in a close to dryout situation, high quality, the power-to-flow ratio might seem -- no, it is all liquid. It is all liquid, isn't it? It is all liquid. So it is not. No, it has nothing to do with that. Yes, it is all liquid.

I am just trying to figure out why it should be power-to-flow ratio, but that doesn't matter. It doesn't matter.

MR. MIRANDA: So this DNB ratio, the 1.385 I believe is the limiting, is the lowest DNB ratio you will find in Ginna.

MR. WALLIS: Well, you have 1.381 in another one.

MR. MIRANDA: I will talk about that when I get to the rod withdrawal at power.

MR. WALLIS: Okay.

CHAIRMAN DENNING: Okay, proceed.

MR. MIRANDA: Loss-of-load event, Ginna has done three different cases here.

MR. WALLIS: I'm sorry, I want to go back to this other one. Since everything seems to be governed very much by when the rods drop, is this a conservative analysis you are showing us about rod drop or is this a realistic analysis?

MR. MIRANDA: This is conservative.

MR. WALLIS: So the rods, where actually it says two, it is more likely to be one?

MR. FINLEY: Right. I think, Sam, if you put up your sequence of events table there?

MR. WALLIS: As rods begin to drop at two seconds; it is more likely to be one second, is that right?

MR. MIRANDA: Well, they take 2.8 seconds to drop.

MR. WALLIS: Well, they begin to drop at two. Is it more likely that they would actually drop earlier than that?

MR. FINLEY: That is correct. This is Mark Finley, Project Director for Ginna.

I mentioned in my presentation there is a 1.4-second time delay assumed between the time the frequency set point is reached --

MR. WALLIS: That is the .6 --

MR. FINLEY: -- right -- and the time the rods begin to drop. We have actually timed that in the past at less than one second. So on my slide I said, if you reduced that 1.4-second delay to one second, then you would benefit in margin.

MR. WALLIS: Yes, I was recalling what you said.

MR. FINLEY: Yes.

MR. WALLIS: And I was trying to relate it to what is being presented here.

MR. SIEBER: The rod drop speed is slow, too.

MR. FINLEY: And then the rod drop speed is tested. We have a tech spec number we have to meet for the rods to reach the bottom, and that is tested each startup.

MEMBER MAYNARD: I think I also heard Westinghouse say that they don't take much credit for the rods until they get almost to the bottom, as though all the power were being generated in the bottom there. So that is another conservatism, I believe.

MR. FINLEY: They certainly use a bounding shape in terms of the rods and the position of the rods for the negative reactivity insertion.

MR. MIRANDA: Okay, the loss-of-load case, there are actually three cases, but the important one is the RCS peak pressure case, the last one.

Ginna has looked at the loss of load in terms of DNB ratio and also in terms of secondary site over-pressurization. They are different cases.

The DNBR case is a case that is designed to produce a low DNB ratio, which means you try to keep the pressure low. To keep the pressure low, they would use the pressurizer pressure control system, pressurizer spray and PORVs. They also use the revised thermal design procedure to evaluate the DNB ratio.

For this type of an event, as a reviewer, I would look for a trip coming from the protection that is designed to protect against low thermal margin. That would be the over temperature delta T trip. That is what is happening here. The over temperature delta T trip occurs at 11.6 seconds, and then the DNB ratio reaches a minimum, again, as the rods are nearing the bottom of the core.

The case designed to look at secondary site pressure, we are not looking at DNB ratio anymore. So they are using the standard thermal design procedure, which means, for example, that they are going to use different initial conditions. They are going to use 102 percent of rated thermal power, and they are going to use temperature uncertainties on the high side.

Also, in this case they are, for peak

secondary system pressure, they are assuming no steam generator tube plugging to maximum the heat transfer from primary to secondary.

Finally, the RCS peak pressure case --

MR. WALLIS: So that's a conservative assumption?

MR. MIRANDA: Yes.

For the RCS pressure case, they are not using any pressurizer pressure control, no PORVs, no spray. They are using all the uncertainties in initial conditions in a conservative direction, high temperatures, high power, and they produce the highest pressure. For example, for a trip on the high pressurizer pressure reactor trip --

MR. WALLIS: Now, presumably, the steam generator is cooling better; the pressure is lower, isn't it? That's a different --

MR. MIRANDA: They would assume different plugging level --

MR. WALLIS: Higher secondary pressure, but what did you assume about the steam generator in the last case?

MR. MIRANDA: Maximum plugging, 10 percent plugging.

MR. WALLIS: You assume 10 percent

plugging, okay.

MR. MIRANDA: That is why in each one of these analyses you look at what parameter you are interested in --

MR. WALLIS: No, I am just interested about the steam generator in the last case because it doesn't seem to be written down here. Okay.

MR. MIRANDA: So in the first case, in the DNBR case, they have the over temperature delta T trip occurring right about here.

MR. WALLIS: We don't have that.

CHAIRMAN DENNING: It is on the third one.

MR. WALLIS: It is on the third one, okay.

MR. MIRANDA: That trip corresponds to this point. Here is your DNB ratio.

MR. WALLIS: Oh, it wiggles, unless you put the pencil mark on there.

MR. MIRANDA: Oh, the wiggle mark?

MR. WALLIS: It is your pencil mark you put on there as a wiggle, isn't it, or is it not?

MR. MIRANDA: Yes, the wiggle is due mainly to this.

MR. SIEBER: Actually, we don't have that.

MR. WALLIS: We don't have that. We don't have that, no.

MR. MIRANDA: Here we have the pressurizer pressure and you see that we have PORV opening at 2350 psi, and, in fact, it gets up to 2500, where you might begin to see the safety valves opening. Over temperature delta T trip occurs right about here.

MR. WALLIS: We don't have your first curve there for some reason.

MR. SIEBER: We don't have the last one.

MR. WALLIS: We don't have the one you just showed, the one before this.

MR. MIRANDA: The one before this? This one?

MR. WALLIS: I don't think we have that.

CHAIRMAN DENNING: No, I don't think we do.

MR. WALLIS: We don't have that.

CHAIRMAN DENNING: It is missing.

MR. WALLIS: So DNBR is sort of headed to China until the PORV opens, is it, or something? It seems to be falling off a cliff and then it levels off again.

MR. MIRANDA: Well, I don't really connect it to the PORV. It is connected to the rods providing enough negative reactivity to trip the plant.

MR. WALLIS: And that's what stops it

abruptly? Okay.

But is that wiggle something you drew on there? We don't have this figure. You drew something on there?

CHAIRMAN DENNING: Yes, that is just a marker, I think.

MR. WALLIS: It's a marker, okay. You put that on? Okay. Just don't draw on the screen, whatever you do.

(Laughter.)

Okay, so that is the figure we don't have.

CHAIRMAN DENNING: But that's okay. Proceed.

MR. WALLIS: That's okay. We have seen it.

MR. MIRANDA: So this is where the trip occurs. I mean this is where the --

MR. WALLIS: And that is, again, conservatively estimated in time and stuff?

MR. MIRANDA: The over temperature delta T trip, that is the trip that is designed to keep the DNBR above 1.3 --

MR. WALLIS: Again, you've got two second between the trip and the rods dropping? Is that this conservatism again?

MR. SIEBER: Yes.

MR. MIRANDA: Yes.

MR. WALLIS: Yes, okay.

MR. MIRANDA: That is a long time.

There is also, by the way, in the over temperature delta T trip, there is also a delay built in actually before you even reach that signal to account for loop transit time because the temperature is measured in RTDs in the hot legs and the cold legs, and it takes time to get there, something like a six-second delay.

This over temperature delta T trip is current compensated, lead line compensation to account for the time that it takes to measure the temperature versus the time to actually put the rods into the core and actually trip the plant before you reach the core limit of 1.38.

MR. WALLIS: All right. I find this extraordinarily useful. We have complained in the past many times that when you read the SER and you simply see a description of what the applicant did, and then you say the applicant meets the regulations, everything is fine, there's no indication that anything like this sort of study is behind that decision. And I think this is the first time we have really seen that this staff knows what is going on in some detail, and it has been very useful to me. So please continue.

MR. MIRANDA: This is simply the steam generator pressure, the pressurizer water volume. The limit for the steam pressure is 1209, which is right about here, 1209.

The over temperature delta T trip occurs right here.

And we also verify, since this is a Condition 2 event, that the pressurizer is not filled.

MR. WALLIS: Yes.

MR. MIRANDA: This is an 800 cubic foot pressurizer, 18.6 cubic feet for the surge line. So we see that this event would not cause any water relief for the --

MR. WALLIS: And it's getting pretty close?

MR. MIRANDA: Close, yes.

MR. WALLIS: Yes.

MR. MIRANDA: Yes, Ginna has gone about as far as they can with this uprate.

MR. SIEBER: There's still margin.

MR. WALLIS: The operator might have a little concern when he sees that headed up like that.

MR. FINLEY: Exactly, and he's got many indications that might cause him to take actions that would improve these results, but we don't take credit for that, at least not for 10 minutes.

MR. WALLIS: These are seconds on the axis here?

MR. FINLEY: Yes.

MR. WALLIS: So the 15 and 18 seconds, if this is true, this curve, he's going to be having some qualms or something. Something is going to be happening to him.

MR. MIRANDA: Well, the reactor trip takes care of that situation. As soon as you turn off the --

MR. WALLIS: If it happens, yes. Yes.

MR. MIRANDA: It starts to go down.

In this case, the steam generator peak pressure case, you see that DNB ratio is not the issue and there's lots of margin there.

MR. WALLIS: Well, as long as it turns around, right?

MR. MIRANDA: It turns around due to the trip, yes.

MR. WALLIS: Which is conservatively estimated in time.

MR. MIRANDA: This is the RCS volume for the steam side pressure case. That volume is actually much lower.

MR. WALLIS: RCS pressure?

MR. MIRANDA: RCS pressure is -- we do have core opening of 2250 --

MR. WALLIS: I guess where you said "volumes temperature," you mean the temperature increase swells up the volume? Because it is sort of related to volume, isn't it? It looks like volume.

MR. MIRANDA: This is the core opening here. Then we have safety valves opening just barely right about here, taking into account 2.5 percent pressure accumulation.

MR. WALLIS: These are all curves submitted by the applicant?

MR. MIRANDA: Yes.

MR. WALLIS: And you folks didn't do any separate predictions or running of the code or anything? I guess Westinghouse doesn't give you the code to run?

MR. MIRANDA: Actually, we ran it. We ran a case with LOFTRAN.

MR. WALLIS: They did give you LOFTRAN to run? Or you have LOFTRAN?

MR. MIRANDA: We had access to LOFTRAN at their Rockville office. We ran the loss-of-load event with LOFTRAN. LOFTRAN agrees pretty well with RETRAN.

Back in the sixties, before LOFTRAN was written, there were some tests done at some plants, including Ginna, load rejection tests. They were used to benchmark LOFTRAN. RETRAN later was used, was benchmarked against LOFTRAN, and also these tests. Those codes are available. I think they might in that RETRAN WCAP.

MR. FINLEY: They're off looking for those curves as you speak, Sam.

MR. MIRANDA: Okay. If you look at those curves, I don't think you will see a consistent conservatism where the pressure is always under-predicted or over-predicted. They are going to cross each other at several points. Probably the better measure is a statistical correlation rather than a pressure margin.

All those results were available since the sixties.

This is the last of the steam flow pressure case. We see here that the pressurizer doesn't fill and that the steam system design pressure is not exceeded, level 9 psi.

This is the peak pressure, the peak RCS pressure case. This case does not assume any operation of the pressurizer pressure control system, no PORVs, no spray. We see the DNB ratio doesn't even go below its initial value.

We were looking for peak pressure. This curve, we have the high pressure trip occurring at about five seconds, right about here.

MR. WALLIS: The rods drop later at some time, yes.

MR. MIRANDA: Yes, the rods drop, but the pressure continues to go up until the safety valves open. The safety valves are opened --

MR. WALLIS: This is stored heat in the fuel or something?

MR. MIRANDA: Yes. Yes, that's right.

MR. WALLIS: Stored heat in the fuel?

MR. MIRANDA: Yes.

CHAIRMAN DENNING: Are the PORVs still open in this one because they are not a safety grade --

MR. MIRANDA: That's right, the PORVs are considered a control system. So they are not credited to operate.

MR. WALLIS: Not allowed to open?

CHAIRMAN DENNING: Not credited, but the reality is that they would, you said? Yes.

MR. MIRANDA: This same event, the loss of load is analyzed as an ATWS event, and that is a best-estimate analysis. In that case, the PORVs would open.

MR. DUNNE: I think the point to notice on this one for peak pressure, what terminates the peak pressure is when the safety valves open. Independent of the computer program, when the safety valves on the pressurizer go open, that's when you get your peak pressure in the pressurizer and --

MR. WALLIS: So it is going to be less, so it should be less than your design because they are open?

MR. DUNNE: Right.

MR. WALLIS: And at that point it is suitable.

MR. DUNNE: Yes,

MR. WALLIS: Yes.

MR. MIRANDA: Okay, these curves verify that the pressurizer does not fill. In this case, too, the steam side pressure does not exceed its safety limit.

MR. SIEBER: What is the volume of the

pressurizer?

MR. MIRANDA: The volume of the pressurizer is 800 cubic feet.

MR. WALLIS: These maximum pressures are really determined by set point on the relief valves? Nothing else matters, does it? Or does something else matter?

CHAIRMAN DENNING: There is overshoot.

MR. WALLIS: There is overshoot?

MR. DUNNE: Yes, basically, the two things that control this one from pressure is tripping the reactor and the safety valves opening. In this event the reactor trips early, but you don't really terminate the heat up the RCS until you basically -- a little bit later in time. So you keep on pressurizing until you get to the relief valves. When the relief valve pops, they have more relief capacity than the thermal expansion of the RCS, and that terminates the transient.

MR. MIRANDA: Just to complicate things a little bit, if you were to assume the PORVs were open in this event, for example, that would delay the reactor trip because the PORVs will open at 2350 psi; the reactor trip set point is about 24-25 psi. So that PORVs opening and relieving steam at 2350 for a few seconds would delay the reactor trip for a few seconds.

MR. WALLIS: That's because they like to keep the reactor running if they possibly can?

MR. MIRANDA: Yes. They put the reactor trip between the PORVs and the safety valves. The PORVs prevent the reactor trip, and the reactor trip prevents the safety valves from opening.

CHAIRMAN DENNING: I was going to let you get through your presentation, but I think that things have gone a little bit too far for the break. So why don't we take the break now and have you come back and finish? So we will recess until 10 minutes before the hour.

(Whereupon, the foregoing matter went off the record at 10:35 a.m. and went back on the record at 10:51 a.m.)

CHAIRMAN DENNING: All right, we're going to come back in session now, please.

Proceed.

MR. MIRANDA: We had some discussion about this earlier. The licensee submittal contains three transients. The first two are examples and really are two of a series of something like 50 or 60 cases that are done for the rod withdrawal at power, basically, to map the reactor protection system area of coverage for this event in terms of reactivity insertion rates.

Now these notations that you see here are the result of some errors in the license amendment request. The first case is not a maximum case; it is a minimum reactivity feedback case.

The times of reactor trip and minimum DNBR are the times that you will see on the curve. The times were originally printed for another curve.

The same thing with the slow reactivity insertion rate, 5 pcm per second, the second case. That is a really a maximum feedback case. Those are the times of reactor trip and minimum DNBR.

These two examples of transients are taken one at a high reactivity insertion rate, one at a low reactivity insertion rate, to illustrate a transient that is protected by the high-flux trip and another one that is protected by the over temperature delta T trip.

Finally, Ginna submitted a transient to show that the rod withdrawal at power event would not violate the reactor coolant system pressure acceptance criteria.

Maybe I should mention that DNB ratio at this time. The DNBR ratio for the rod withdrawal at power that was listed at 1.381, that is not really comparable to the DNB ratio that you find from the loss-of-flow accident, the 1.385. That 1.385 value comes from VIPRE results, and the 1.381 number comes from RETRAN results. The 1.381 is really an estimate of DNB ratio based upon insensitivity of DNB ratio to changes in power, temperature, and pressure -- yes, power, temperature, and pressure all taken at a constant flow.

So that 1.381 value from RETRAN is conservatively underestimated. That value, if those same conditions of power, temperature, and pressure were to be input to VIPRE, the DNB ratio would be higher than 1.381.

MR. WALLIS: This is because RETRAN is predicting the average behavior? Is that what it is?

MR. MIRANDA: It is an estimate. RETRAN is calculating transient conditions for power, temperature, and pressure.

MR. WALLIS: But they are all average? They are all --

MR. MIRANDA: Well, no, they're not all average.

MR. WALLIS: That's total power? Okay.

MR. MIRANDA: It will calculate the

average power, but then it will also calculate pressure at various points in the reactor coolant system. It will calculate temperature --

MR. WALLIS: But it doesn't deal with hot rods and things like that?

MR. MIRANDA: Oh, no, it doesn't have that kind of resolution. That is what VIPRE is for. So it takes the average conditions and puts them into VIPRE for the DNBR evaluation.

MR. WALLIS: Why was it not put into VIPRE?

MR. MIRANDA: Why was what?

MR. WALLIS: I mean in the other case they did use VIPRE, didn't they?

MR. MIRANDA: The loss of flow, they did use VIPRE.

MR. WALLIS: Yes. So why did they not use it in this case?

MR. MIRANDA: Well, they can't do that because the DNBR estimate routine in RETRAN is all based on the core limits, and the core limits are at a constant flow rate.

MR. WALLIS: I thought last time they took the RETRAN and then fed it into VIPRE.

MR. MIRANDA: In the loss of flow they do that, yes.

MR. WALLIS: They couldn't have done it this time, too?

MR. MIRANDA: They could have done it. It would have taken longer.

MR. WALLIS: Time is of no matter when you're satisfying ACRS.

(Laughter.)

MR. MIRANDA: The limiting event is not the rod withdrawal at power; it is the loss of flow. The rod withdrawal at power has a 1.381 value.

MR. WALLIS: So you think that this is very conservative? It really should be higher than that? Okay.

MR. MIRANDA: It will be much higher than that.

Chris, did you want to say something?

MR. McHUGH: No.

MR. MIRANDA: Okay.

MR. WALLIS: Well, I think it would have been good for them to have done it and got a better number. Then we wouldn't have asked so many questions about it.

(Laughter.)

MR. MIRANDA: Well, it is a little bit

misleading because you think you are comparing apples and apples and you're not. They come from different places.

This is the rest of the sequence of events tables and the --

MR. WALLIS: Now this pressure that comes so close, is, again, this because the pressure is relieved by safety valves? Is that why?

MR. DUNNE: It's both -- the pressure is really controlled by the safety valves lifting and when the reactor trips.

MR. WALLIS: So we shouldn't be so concerned about it coming up to a limit?

MR. DUNNE: No. That's right.

MR. WALLIS: That is why the safety valves are there.

MR. DUNNE: Yes, that's why the safety valves are there, and you get full opening on the valves to get full flow and you figure out what your parameters are for --

MR. WALLIS: And you have enough valves and they are reliable and all that sort of stuff?

MR. MIRANDA: Yes, that is all conditioned on the valves relieving steam. As long as the pressurizer doesn't fill and you open the valves as designed, they release steam and they load the pressure --

MR. DUNNE: And as long as the safety valves open within the stated tolerance on them, your pressure is really limited by that, and it is not really that sensitive to the code itself.

MR. WALLIS: If this were PRA, we would be looking at the probability of those valves opening, wouldn't we? Here you just assume they do?

MR. DUNNE: Well, we actually go in and test our safety valves.

MR. WALLIS: I know that.

MR. DUNNE: We basically change out our safety valves every refueling outage. We've got two sets of safety valves.

MR. WALLIS: But for this analysis you assume they open?

MR. DUNNE: Yes.

MR. WALLIS: In this design basis accident event?

MR. SIEBER: Well, they are safety degrade, too.

CHAIRMAN DENNING: Yes, but in PRA space safety --

MR. DUNNE: They are basically the code

valves required to basically prevent over-pressurization of the --

MEMBER KRESS: Failure to open in the PRA space is like one times 10 to the minus 3.

MR. WALLIS: Okay, there is a probability though.

CHAIRMAN DENNING: I don't think on the failure to open --

MEMBER KRESS: About 10 to the minus 4 failure.

MR. WALLIS: Okay.

MR. MIRANDA: This is the transient for the first case. The high neutron flux signal is reached at about a little more than one second, and the rods begin to fall a half a second later. The rods begin to fall about here.

MR. WALLIS: Where is this?

MR. MIRANDA: The DNB ratio occurs at 2.26 seconds.

MR. WALLIS: Something we don't have, right? That's something we don't have. We don't have that upper curve.

MR. MIRANDA: You don't have this one?

CHAIRMAN DENNING: We have the lower curve but not the upper curve for some reason.

MR. MIRANDA: All right. We will copy for that.

This is the behavior in pressurizer water volume and pressure. Here we verify that the pressurizer doesn't fill. In fact, in this case the PORVs don't even open or they wouldn't open.

Since we are looking for a low DNB ratio, if the PORVs were supposed to open, if the pressure were to reach the PORV opening set point, they would open. They would be assumed to open.

This is the minimum DNB ratio occurring at 2.26 right there.

Then, as an example for low reactivity insertion rate, 5 pcm per second, this is a transient that would be protected by the over temperature delta T trip. That occurs at about 214 seconds, and you can see where that is.

MR. WALLIS: So it just slowly creeps up in power?

MR. MIRANDA: Yes. As you approach the core limit, as you approach that 1.38, the over temperature delta T trip tripped the plant.

MR. WALLIS: Would the operator do nothing all this time when it is creeping up in power?

MR. GILLON: Yes, this is Roy Gillon

again, Reactor Operator.

Yes, we are aware of 214 seconds' change in power, PPCS, our computer systems, and both observation of the control board. So this would be hard to believe that the operator wouldn't terminate this within 30 seconds.

MR. WALLIS: Before the temperature does, yes.

MR. GILLON: Right. We would see temperature increasing. We would see power increasing.

CHAIRMAN DENNING: It looks like the pressure has the water volume really increasing.

MR. WALLIS: Yes, what is this pressurizer up here?

MR. MIRANDA: The margin water level would increase since the reactor coolant system temperature is increasing, and, in fact, I have asked in the past licensees to show me a very low reactivity insertion rate because I look for this pressurizer water volume; I need to see a maximum value to be sure that it is not going to fill the pressurizer.

In real life a lot of these reactivity insertion rates are more limited than what you would see in these analyses because, on the one hand, on the high end you just don't have the differential rod worth and the rod speed to get to that 100 pcm per second. Also, on the low end or for a long transient like this, for 200-and-some seconds, chances are that you are just going to reach the end of the rod. I mean the rods are at various insertion limits. You are going to pull it out and the reactivity insertion will end, and very often without a reactor trip. You will just have a new equilibrium power level.

Here's the average temperature. You can see it looks like the pressurizer volume curve, and there's the DNB ratio slowly dropping to its minimum value where the reactor trip occurs.

These are the results. Of all of the cases that were run, something like 50 or 60 or 70 cases, at different reactivity insertion rates with maximum feedback and minimum feedback at three different power levels. So these are the results for the 100 percent power cases.

We see from this curve that the low reactivity insertion rate cases are protected by the over temperature delta T trip, and the high reactivity insertion rate cases are protected by the high flux trip. We also see what the minimum value of the DNB ratio is. These DNB ratios, again, are from RETRAN.

MR. WALLIS: So you have to have things just right to get one of these valleys? You have to have just the right reactivity insertion rate to be in the region where you get near the minimum?

MR. MIRANDA: Well, actually, these curves, there's something that is not shown on these curves. That is, when you do these cases, for example, this curve actually continues. This curve here would continue. This is the intersection. That's where they stop.

MR. WALLIS: Wait a minute. I don't understand that.

MR. MIRANDA: They do other analyses. They would do other cases. They don't know when this is going to occur, when this minimum is going to occur. They would do a whole series of cases, and there may be some cases down here that are not reported because they are covered --

MR. WALLIS: They wouldn't get there?

MR. MIRANDA: They wouldn't get there, yes.

MR. WALLIS: Okay.

MEMBER MAYNARD: But I think you're right; it takes just a very unique set of circumstances to hit one of the valleys that takes you down.

MR. McHUGH: It is Chris McHugh from Westinghouse.

We actually search for that valley. When we do our initial set of runs, we will do like 10, 20, 30, 40 pcm per second to determine where we are switching from high flux over temperature delta T, and then we do a finer mesh in between. We go down to single units, 12, 13, 14 pcm per second. So we hunt for that case.

MR. MIRANDA: That is in order to find a minimum DNB ratio.

These are the results at 60 percent power. These are not transient cases. This is a map of the minimum DNB ratio results.

MR. WALLIS: This is a lot of computation then.

MR. MIRANDA: Yes. Yes, you need a fast-running code like LOFTRAN or RETRAN. We just stack the cases one after the other, changing a single parameter like reactivity insertion rate.

MR. SIEBER: That is why you pick a number and don't do this every time. Otherwise, you would be doing it for every --

MR. FINLEY: That's right, yes.

MR. MIRANDA: And then one last case is

the pressure case. This one is at 55 pcm per second. I believe that is more realistic. That is about what you could get, right, for the Ginna?

MR. McHUGH: No, realistic value is around 30 pcm per second.

MR. MIRANDA: Thirty?

MR. McHUGH: Yes, that is the maximum that would still yield an acceptable pressurizer pressure. So we have instituted 55 pcm per second as a reload criteria and a reload limit that the core designer has to verify it is always going to be under that. The typical number is around 30.

MR. MIRANDA: So we have the reactor -- the high pressurizer pressure trip occurring in this case at about 13 seconds. Normally, if I were looking at a case of rod withdrawal at power cases, a series of cases, I would want to be sure that the protection occurs from either the high flux trip or the over temperature delta T trip because the parameter of interest is DNB ratio.

MR. WALLIS: Why does nuclear power start off so low in this plot?

MR. MIRANDA: This is an 8 percent power case.

MR. WALLIS: Oh, it's an 8 percent power?

Okay. I didn't look at it. Okay. I didn't look at the title there.

MR. MIRANDA: But since here we are looking at pressurizer pressure, the parameter of interest is pressure, and the protection comes from the high pressurizer pressure trip.

So we have the reactor trip here, and we have the PORVs opening at 2350. No, no, no. No PORVs, no PORVs in this case. This is a high pressure case; no PORVs.

So we have the reactor trip, the rods fall in two seconds later, about 15 seconds, and the safety valves open at about 2500 or a little bit higher than 2500. Then the limit is 2750, right about there.

MR. WALLIS: So the safety valves open and the pressure keeps rising for a while, and then --

MR. DUNNE: Well, I think what happens is the safety valve set pressure is actually biased up from a nominal 2500, so they really don't open up until about 2600.

MR. WALLIS: Until that peak is there.

MR. DUNNE: I think where the pressure falls is probably where the safety valves actually did open, would be my guess.

MR. WALLIS: They open pretty quickly?

MR. DUNNE: Yes.

MR. WALLIS: And they relieve pressure right away?

MR. DUNNE: They're 15 milliseconds, something like that.

MR. WALLIS: Right. So I would think the peak would be when they open.

MR. DUNNE: That's what I would expect, the peak, because, again, we biased the safety valve opening upward based tolerances on the set point and loop seal time delay and other parameters.

MR. MIRANDA: That's all I have.

CHAIRMAN DENNING: Very good. That is very helpful.

MR. WALLIS: Do you have some strange logic with all kinds of time constants in it and things that sets these response to signals and opening valves?

MR. DUNNE: I'm sorry. For the safety valves, there is no logic. It is just a spring --

MR. MIRANDA: It is spring-loaded.

MR. WALLIS: So I would think your maximum pressure would be the set pressure on the valve.

MR. DUNNE: That is correct.

MR. WALLIS: There's no control involved

at all.

MR. DUNNE: That is why there really isn't a lot of variation in what the pressure is.

MR. SIEBER: There is some uncertainty about what that set pressure --

MR. WALLIS: This is just a little bit?

MR. DUNNE: Right, yes.

MR. WALLIS: This is a little bit. But we shouldn't be surprised that the pressure is about where you set it.

MR. DUNNE: Right.

MR. SIEBER: Do you heat the loop seal at all?

MR. DUNNE: Yes, we do. We have a hot loop seal around 300 degrees.

MR. SIEBER: Keeps it from looking like a steel bullet.

MR. DUNNE: That is to protect the downstream piping from a cold water slug if the safety valves actuate.

MR. SIEBER: Three hundred degrees?

MR. DUNNE: I think it is around 300 degrees. What we have actually done is the piping from the pressurizer nozzle to the safety valve is inside the pressurizer insulation.

MR. SIEBER: Okay.

MR. WALLIS: Well, cold water slugs can be quite interesting.

MR. SIEBER: Only once.

MR. DUNNE: That's the reason why we heat them.

MR. SIEBER: Only once are they interesting.

CHAIRMAN DENNING: Okay, we are going to keep going. We are going to move ahead with the small break LOCAs now.

MR. WALLIS: I'm amazed that we're under time. We seem to have asked a lot of questions, and yet we are still within time.

CHAIRMAN DENNING: I think we got through their presentation early, quickly.

MR. FINLEY: Mark Finley again.

Two analytical areas had not yet been reviewed by NRC when we last met. So we will discuss this morning both the small break and the long-term cooling analyses, and then Len Ward from NRC will discuss the same analyses.

In terms of an agenda for this presentation, we will talk a little bit about the Ginna design and why that is helpful in the small break LOCA analysis and then shift to talk about current and EPU results for small break LOCA analysis. You will see there is a significant margin here in these results. Then delve into the long-term cooling analysis with respect to the Ginna design and then both the large break and the small break long-term cooling analysis.

First, with respect to two key aspects of the Ginna design that help in small break LOCA, we have relatively high flow, high head safety injection pumps that start to kick in around 1400 psi and capacity conservatively above 1000 gpm. In terms of the power level of Ginna, the two-loop Westinghouse-type power level, this is significant flow at high pressure, and that helps the small break result.

In addition, we have relatively high-pressure accumulators which would start to discharge at around 700 psia.

MR. WALLIS: This is injection into the upper head?

MR. FINLEY: No, the high head safety -- and I'll talk more about that -- the high head safety pumps actually inject into the cold leg.

Yes?

MR. SIEBER: You don't use them as your

normal charging pump, do you?

MR. FINLEY: No, we don't use these in our normal charging pumps.

MR. SIEBER: What do you use for charging?

MR. DUNNE: Positive displacement pumps.

MR. SIEBER: Okay, like the Navy.

MR. FINLEY: Right. And we don't take credit here in this analysis for the charging flow.

MR. HARTZ: This is Josh Hartz of Westinghouse. I'm in charge of NOTRUMP.

Westinghouse basically has two different ECCS categories, high- and low-pressure plants. The Beaver Valley cases that you saw the other day would be what we would consider a high-pressure plant because they had safety grade charging plants. The two-loop plants do not have that capability. They've got dedicated SI pumps instead.

MR. DUNNE: This is Jim Dunne.

I think the big difference is that Beaver Valley's high head safety injection pumps can pump in against RCS pressure whereas our high head pumps can't. But it gives us more flow capability at the lower pressures.

MR. SIEBER: So you have to wait. Before you can inject at all, you have to have some blowdown?

MR. DUNNE: Pressurization of the RCS, yes.

MR. HARTZ: This is true, but the SI set point is typically around 1700. So even with the very small breaks, they depressurize quite quickly and go past that. So these pumps inject very quickly into the transient.

MR. FINLEY: Okay, on this slide you see the current results and the EPU results for small break LOCA Pclad temperature. Two key points to take away from this slide:

One is the EPU result, 1167, for the limiting break size, which I believe is two inches, right, Josh? --

MR. HARTZ: That is correct.

MR. FINLEY: -- is very low, 1167, quite a bit less than the 2200.

MR. WALLIS: Using a different method than the current method, is it?

MR. FINLEY: The method is the same. Both analyses use NOTRUMP methodology.

The second key point to take away, as you already allude to, Dr. Wallis, is that the current result is actually a little higher than the EPU result. That is unexpected, but it is due to a physical phenomenon in the NOTRUMP analysis that relates to loop seal clearing, which at the time in 1994 the analysis chose to leave alone because it was still an acceptable result by far.

MR. WALLIS: The prediction using this 9595 method or is this some other sort of conservative approach? What is the method that is used?

MR. HARTZ: This is Josh Hartz.

This is not a best-estimate approach. It is an Appendix K model.

MR. WALLIS: This is an Appendix K run? Okay.

MR. HARTZ: That's correct.

MR. WALLIS: So it is pretty low for Appendix K, isn't it?

MR. FINLEY: Yes, that's the point. Very low for Appendix K. A good deal of margin on small break LOCA.

I will also point out that you see the maximum transient oxidation there, .07 for EPU, well below the limit. We also add in the pre-transient oxidation level and we control that in the reload analysis to make sure the total stays below the 17 percent.

MR. SIEBER: Now this is for the worst-case small break? What size is this?

MR. FINLEY: That's correct. This is a 2-inch break, is the worst case for Ginna.

MR. SIEBER: Did you model in quarter-inch increments or?

MR. FINLEY: We did a spectrum of analyses using the standard Westinghouse method. I believe it was the 1.5-inch, a 2-inch, and a 3-inch break.

MR. SIEBER: That's pretty gross.

MR. FINLEY: We didn't go to the quarter-inch level. I think you saw Beaver Valley did that. The reason is because we have so much margin here. Because that Pclad temperature is so low, Westinghouse hasn't seen a large variation in the Pclad temperature at this low level.

Josh, you might be able to speak to that?

MR. HARTZ: Yes. Actually, in this case we did go off and look at quarter-inch intervals just to assure ourselves that that wouldn't be the case. Because when the whole issue of break spectrum up in the Beaver Valley analysis review, we wanted to make sure that everybody was captured in that regard. So we used Ginna as a test case to kind of confirm that, and it did not show much variation in the results.

That is mainly because this is not a

boiloff -- the boiloff turbine PCT plants are the ones that are sensitive to that. Beaver Valley would fit into that category.

MR. SIEBER: So you actually did do the work?

MR. HARTZ: Yes, we did. It would not be in Ginna's SER though.

MR. FINLEY: Yes, it was not a part of the licensing report, but they did that after the fact in response to requests for additional information.

MR. SIEBER: Basically, what you are saying is you didn't find much sensitivity with regard to break size?

MR. HARTZ: No. No, not for a plant of this type.

MR. SIEBER: Okay.

MR. WALLIS: Assuming a zero break size, though, is --

MR. SIEBER: That is one of the better breaks.

MR. WALLIS: Better points, right.

(Laughter.)

When you did the large break, you did use the 9595 method?

MR. FINLEY: That's correct. The large

break was the best estimate --

MR. WALLIS: Because you got better results, presumably, than using Appendix K?

MR. FINLEY: The large break for Ginna is the limiting LOCA, and we did need the --

MR. WALLIS: Here Appendix K is okay, and it's simplest, so you just did it?

MR. SIEBER: Was your accumulator pressure always 700 or is that a change?

MR. HARTZ: No, that's -- the two-loop plants have 100 psi higher design limit than the three- and four-loop plants.

MR. SIEBER: Okay, but that is all for large break protection?

MR. HARTZ: They do give you benefit in small break space, and that is one reason why the small break results are so good in this case, is because they are jumping into the transient even sooner. Because you go into a depressurization phase --

MR. SIEBER: Right.

MR. HARTZ: And once you hit the set point of the accumulators, they deliver enough water to terminate your heatup. So, yes, in small break space they do tend to help you out, especially more in the three-loop plants where we have the safety grade charging, and the flows to mitigate the accident aren't as marginal here.

MEMBER KRESS: I don't know if you can answer this or not. If you used the transition break size, could you have a substantial increase in power and still meet the rules?

MR. HARTZ: Are you referring to the 5046(a)?

MEMBER KRESS: Yes. I know you may not be prepared to answer that, but I was just curious.

MR. HARTZ: I guess in my judgment there would probably be some other accidents waiting to get into the way of that.

MEMBER KRESS: Waiting to catch you before --

MR. HARTZ: Yes. So in LOCA space they tend to do pretty well, the two-loop plants.

MR. WALLIS: This plant is large break LOCA-limited. So if you back off a bit on the large break LOCA criteria, you might gain a bit.

MR. HARTZ: It would open some things up. It is a possibility, but I think their large break results were pretty good to begin with compared to what some other plants would be.

MR. FINLEY: Right.

Okay, so just to summarize quickly, small break, a significant amount of margin to the acceptance criteria.

MR. WALLIS: In this case the safety analysis limit is a legal one, not one specified by the vendor and the licensee.

MR. FINLEY: That is correct. That is correct.

With respect to long-term cooling, some of the key aspects of the Ginna design that come into play: again, the high head safety injection pumps. These pumps are aligned to the cold leg.

We also have low head safety injection pumps. We call them residual heat removal pumps, RHR pumps. They are aligned to the upper plenum. I will show you a diagram in a second, the same nozzles that I think Jim Dunne had on his slide earlier.

But these inject directly into the upper plenum.

MR. WALLIS: Do you understand how the water gets down into the core from there? It is a counter-current-flow situation.

MR. FINLEY: Yes, actually --

MR. WALLIS: Because it has to be lopsided or something with flow down on the outside and steam coming up in the middle or something?

MR. FINLEY: Right. In fact, in a couple of slides I will show you physically where the nozzles are with respect to the core.

MR. WALLIS: Well, you've got water up there and it has to come down here.

MR. FINLEY: That's correct. That's correct.

MR. WALLIS: It is cold water, so the steam rushing up to condense on it, and so conceivably you have a CCFL-type situation.

MR. FINLEY: Right.

Gordon, click on that slide there and let's see what we've got.

All right, this just shows --

MR. WALLIS: We can see the hole.

MR. FINLEY: -- the elevation of the nozzle there in between the hot and the cold nozzle on the reactor vessel.

Next slide, Gordon.

MR. WALLIS: Yes, as far as into the --

MR. FINLEY: And here, the plan view shows where the nozzles would inject.

MR. WALLIS: I think it makes a pool up

there, as I remember. Doesn't it make a pool up in there? It fills up. Doesn't it fill up that plenum to some extent and then it somehow drains down in preferred locations?

MR. HARTZ: Dr. Wallis, you're probably referring to the early phases of a large break transient where you could be CCFL-limited in upper plenum, yes. Yes, but in the long-term cooling situation, the steaming rates --

MR. WALLIS: Okay, yes, I'm referring to a different situation.

MR. HARTZ: Yes.

MR. FINLEY: And I'll actually in a future slide --

MR. WALLIS: Do you understand that fully, do you?

MR. HARTZ: Yes.

MR. WALLIS: Of course you're going to say yes, I know.

(Laughter.)

It was a concern of mine at one time.

MR. HARTZ: Yes, with the UPI plants and with the licensing of SECY originally, that was a big concern, to mitigate the large break transient because of the water holdup in the upper plenum.

MR. FINLEY: And I will actually speak to this mixing assumption that we make with respect to long-term cooling in this UPI injection here in a couple of slides.

MR. WALLIS: You'll come to that?

MR. FINLEY: Yes.

MR. WALLIS: Okay.

MR. FINLEY: So the point here would be we have the high head SI pumps to the cold legs, the low head SI pumps to the upper plenum, and when they are both injecting simultaneously --

MR. WALLIS: These look like hot leg injection.

MR. FINLEY: That's correct. That's correct.

MR. WALLIS: You don't have to switch it on? It just happens?

MR. FINLEY: That is correct. It just happens. They are aligned permanently this way. We verify valve lineups and locked valves, and so forth, to make sure they inject in this manner.

Okay. And just fundamentally -- and I'm sure you talked about this some with Beaver Valley -- if you have the break on the hot side, you need the injection on the cold side to get the flush through the core, and the converse.

MR. WALLIS: You've got both of them.

MR. FINLEY: Say it again?

MR. WALLIS: You've got both of them here?

MR. FINLEY: That's correct.

MR. WALLIS: You're coming from both sides?

MR. FINLEY: That's correct.

Okay. Just to walk through the large break sequence here, of course, by definition, essentially, for the break size, the RCS rapidly depressurizes to below both the high head SI and the low head SI injection points. So you get the simultaneous injection early on, and that prevents any buildup early on of boron.

As the refueling water storage tank lowers, the level lowers, at that point we switch to the recirculation mode manually. At that point we actually turn off the high head safety injection pumps.

I am sure you would ask why, but fundamentally Ginna was not designed for simultaneous injection throughout the recirculation process. In fact, early on in the large break LOCA scenario the sump temperature is higher than would support the required NPSH that is needed to run simultaneous injection for the whole course of the recirculation.

So we turn off the high head SI pumps and then turn them back on. What we have verified through this long-term cooling analysis is that we turn them back on prior to the point that we would have concentrated then to the saturation point for boron.

MR. SIEBER: How much time is that?

MR. FINLEY: And I'll get to that in the next slide.

The other point to make here -- and I will show it on the next slide in terms of a better view -- but, conservatively, we don't take credit for the upper plenum injection essentially mixing with the core volume region to prevent concentration of the boron. That is a very, very conservative assumption.

Then the operators procedurally will restart those high head safety injection pumps to again restore simultaneous injection.

Gordon, if you will go to the next slide?

In terms of the analysis that was done -- and this was in response to the NRC's staff questions. As you probably are aware, they questioned, how are we determining what the void fraction in that water in the core region is and exactly how are we calculating the two-phased level and the volume, the mixing volume. Those were good questions that we really had simplified in the past.

But in response to those questions, this time we did an analysis using the Westinghouse COBRA/TRAC method to determine what the void fraction was and take account for that, as well as what the dynamic pressures are around the loop and how that affects the two-phase level. So all that is accounted for in this concentration analysis that was done.

Gordon, why don't you click on the first one?

Here is the void fraction versus time for a large break. You can see it starts up on the order of .75, .8, and down to just under .55 for the void fraction.

And next slide, Gordon.

Sort of the converse of that is the mixing volume. This is how, with that void fraction, the volume of water changes over time for the large break. So that now is calculated explicitly with the COBRA/TRAC code.

MR. WALLIS: It is throwing away all the upper plenum injection water.

MR. FINLEY: I'll tell you what, let's

hold that thought. I will show you the control volume that we use.

MR. WALLIS: You are not taking credit for it in this volume?

MR. FINLEY: Right, we are not taking credit for any of the water coming in from the UPI up above after this point.

MR. WALLIS: So where does it go then? You just ignore it? Just ignore it?

MR. FINLEY: I will show you in a second, Doctor.

Next slide. Maybe the slide before there. There we go.

Here is a depiction of the mixing volume that is used. This is the expected condition. Actually, this was not what was used in the analysis but what would be expected would be that you would get some upper plenum injection that would then mix with this entire region, both in the core region and in the upper plenum. Because this is obviously a very turbulent region, there is a lot of boiling go on, we would expect significant mixing here. Then, of course, some amount of that is out the break.

Gordon, go to the next.

MR. WALLIS: So you are assuming the SI

flow just gets washed out in the break?

MR. FINLEY: Right, correct.

So next slide, Gordon.

What we do, very conservatively, is take this mixing volume right at the bottom of the hot leg here, and then we assume the only upper plenum injection flow that crosses the boundary is enough flow to replace the boiloff, the steam that boils off. Obviously, very conservative.

The rest of the upper plenum injection flow is assumed to go out the break, carried out the break with the steam.

MR. WALLIS: In reality, it is intercepted by all those control rod tubes and things?

MR. FINLEY: Right.

MR. WALLIS: And it drains down on them?

MR. FINLEY: The guide tubes, the rods, and so forth.

MR. WALLIS: The guide tubes and things.

MR. FINLEY: All that interference is going to cause; plus, this is not a uniform, these assemblies are not producing uniform decay heat. So you will get some hot assemblies with more steaming and cooler assemblies with less steaming. All that would tend to drive mixing across this boundary, a significant amount of mixing. But we don't take credit for that, haven't taken credit for that.

CHAIRMAN DENNING: Now I am missing some element of that, and that is, so that the amount that is going from the upper plenum injection down is matching exactly the steaming rate? Is that what is going on? Does that mean that you have no water in that period coming from the annulus? From the downcomer?

MR. FINLEY: Right, right. This particular break, this is a hot side break. This is prior to the SI pumps being started, restarted. So we have no flow coming in from the cold legs at this point in time.

MR. WALLIS: Well, you might have negative flow, wouldn't you? If you have enough pressure drop out the break, you might actually depress the level in the core, wouldn't you?

MR. FINLEY: Right. We have adequate flow here from upper plenum injection to replace the boiloff. Again, the level is calculated dynamically with that COBRA/TRAC code, so that we know exactly what the pressure drops and the manometer effect around the loop is doing to the two-phased level.

MR. WALLIS: I was just concerned about

taking too much of this safety injection out the break and produce a back pressure that actually depresses the level in the core.

MR. FINLEY: Essentially, we maintain a two-phased level in the core region, which just reflects that the pressure drops due to steam flow out the break, yes.

MR. WALLIS: All right. And SI flow?

MR. FINLEY: That is all calculated dynamically now.

MR. WALLIS: And SI flow, too, isn't it?

MR. FINLEY: Well, right now we don't have the SI flow. This is the period of time while the SI is turned off and we are calculating an increase in boron with the SI --

MR. WALLIS: So the figure doesn't apply then?

MR. FINLEY: Right. As soon as we kick the SI pumps on and then we get flow --

MR. WALLIS: Oh, I'm sorry, SI is a different thing. I mean the UPI, the UPI.

MR. DUNNE: Between low head and high head SI.

MR. FINLEY: I'm sorry. We don't have the high head SI pumps on yet in this particular diagram.

Once they are turned on, you would get the flow in the cold leg and then up through the core.

MR. WALLIS: It is the UPI flow I mean. That produces pressure to drop out at the break --

MR. FINLEY: Right.

MR. WALLIS: -- which can depress the core level, can't it?

MR. FINLEY: The steam flow and the UPI flow together would produce --

MR. WALLIS: That would depress the core level?

MR. FINLEY: Right, that produces a --

MR. WALLIS: So it reduces your mixing volume?

MR. FINLEY: That is correct. We have taken that effect into account. That is correct, yes. Yes.

CHAIRMAN DENNING: Now, as you are talking about this, this is merely the calculation of how much boron is concentrating in this period? This is not something that you are doing with a dynamic code, computer code?

MR. FINLEY: I showed you previously the input that was taken from the dynamic code COBRA/TRAC --

CHAIRMAN DENNING: Yes.

MR. FINLEY: -- that related both to void fraction and mixing volume.

CHAIRMAN DENNING: Yes.

MR. FINLEY: That was then fed into, essentially, a hand-calculation methodology that conservatively bounded that input from the COBRA/TRAC calculation.

CHAIRMAN DENNING: Yes. So you ran the COBRA/TRAC through the entire scenario?

MR. FINLEY: Yes.

CHAIRMAN DENNING: And when you did that, you had some different behavior; that is, the amount of flow that was occurring from the upper plenum injection was probably not exactly matching what is going -- I mean, isn't it possible you had some flow coming down the downcomer at that stage, even though you had UPI injection and not SI injection or is that impossible? Or was there even negative flow through the lower plenum?

MR. FINLEY: Maybe you can help me out. I'm not sure if we had any flow in the SI -- excuse me -- in the cold leg or not.

MR. FINK: This is Dave Fink from Westinghouse.

Yes, what we did was we used a dynamic code simply to adjust our mixing volume, our control volume, to account for core voiding.

CHAIRMAN DENNING: But you ran your system code through the whole scenario, right? Forgetting about what is happening with boron, you ran it through the whole --

MR. FINK: Right.

CHAIRMAN DENNING: And so, as a function of time, you have temperatures in the core; you have void fraction in the core, and this kind of stuff? Right?

MR. FINK: That is correct. Correct.

CHAIRMAN DENNING: During this period we are talking about, was there any flow in the positive direction? I mean, was there any flow in the normal direction of water coming down the downcomer and up through the core or how was it --

MR. FINK: We didn't look at --

CHAIRMAN DENNING: How did you treat it?

MR. FINK: -- those particular regions. The problem as we have it outlined here is the stagnation, the stagnant pot. So under the classic three-loop/four-loop design, the stagnant pot has always been a cold leg break with overflow out the break.

For a UPI plant for the longest time we said there is no real stagnant pot scenario, but if you look at the way we conservatively outline the control volume, you would say, yes, there could be a stagnant pot scenario. That scenario is where the UPI flow crosses the upper plenum and goes out the break.

So in our dynamic code we didn't really look at what was happening in the downcomer.

MR. WALLIS: What we are concerned with here is not when it is stagnant but when it is in reverse flow, that the flow actually comes out into the downcomer, depresses the level in the core, and decreases your mixing volume.

Is that precluded by your analysis?

MR. FINK: Well, we are looking at an equilibrium condition clearly.

MR. WALLIS: It has to go all the way around the loop?

MR. FINK: That is correct. We did spend most of the time, most of the inspection of the COBRA/TRAC runs actually looking at what happens in the core region.

I see Mark put the slide up there.

MR. FINLEY: Yes, I just pulled this from

-- actually, it is an RAI response that we haven't formally sent in yet, but we have shown it in preliminary form to the staff, to document the flow the COBRA/TRAC would calculate over what we'll call the cold sections versus the hot sections in the core, where you actually see some downward flow over the cold sections of the core and upper flow over the hot sections, as you would expect.

MR. WALLIS: Average flow rate --

MR. FINLEY: So the average flow would be --

MR. WALLIS: Is the average flow zero or is it positive or negative?

MR. FINLEY: The average flow would be negative to replace -- correct me if I'm wrong -- would be negative to replace the steam flow, the boiloff.

MR. FINK: I think the answer to the original question, we would expect virtually no flow in the downcomer and up through the lower plenum because the flow would have to -- there is nowhere for anything to go. The equilibrium level --

MR. WALLIS: Yes, but if there was a pressure drop on it, it could be pushed one way or the other, couldn't it?

MR. FINLEY: Yes, but then it is just all water head laying on top of the core region, and it will tend to communicate that effect into the cold legs, but that water will quickly fill up and seek an equilibrium throughout the whole rest of the reactor coolant system.

MR. FINK: Yes, the problem statement is an equilibrium condition.

MR. FINLEY: Right. So we don't think there would be any significant flow in that cold leg without the SI pumps, the high head SI pumps running.

MR. FINK: I think on this slide here the thing that we are most interested in is, what happens in the COBRA/TRAC models, a hot core channel, and then peripheral channels. Clearly, what we see, as evidenced in this plot here, is you get significant upward flow in the center hot channels and significant downward flow in the outer channels.

The flow that actually crosses the upper plenum in the top of the core is like an order of magnitude more than the boiloff. So that shows that you have significant circulation within the core region.

MR. WALLIS: Completely independent of the effects of the boron density, and so on?

MR. FINK: That is correct.

MR. WALLIS: Which would enhance this perhaps.

MR. FINK: Perhaps.

One other thing to take into account here, the UPI flows are very high relative to the safety injection flow rates. I mean you are down at real low pressures at this point when these pumps are injecting. The volume flow rate is very high being delivered in this situation.

We are only assuming a little fraction of it for makeup, and then everything else is just getting discarded.

MR. FINLEY: Okay, so to carry on with the analysis, we do take credit for mixing of one-half of the lower plenum. We take credit for some of that volume, and that is based on testing that has been done previously. We think that is a conservative estimate of the amount of contribution you would get from the lower plenum.

We have calculated -- click on that slide there, Gordon --

CHAIRMAN DENNING: And you base that on the BACCHUS tests?

MR. FINLEY: That's correct.

CHAIRMAN DENNING: Is that what you meant?

MR. FINLEY: That's correct. We have calculated, based on that mixing volume assumption, the time to concentrate the boron, again, using the saturation limit that is associated with atmospheric pressure, a time to reach the saturation limit of approximately six hours and 13 minutes.

MR. WALLIS: But it is really unrealistic to assume that all that upper head injection, upper plenum injection, goes out the break and doesn't -- some of it doesn't go down to the core, especially since you've got this circulation pattern and everything going on.

MR. FINLEY: That is correct.

MR. SIEBER: If you don't know what the mixing really is, you are sort of forced to make that assumption.

MR. FINLEY: Right, right. And this we will say: We have enhanced this methodology greatly in response to some of the staff's recent questions. So I am sure down the road we are going to look at taking credit for those sorts of things. But because we were resolving this on the EPU schedule, we wanted to do it conservatively.

MR. WALLIS: Well, because it can be

resolved without allowing any of the water to come down, you don't worry about it?

MR. FINLEY: Right.

MR. WALLIS: But if it couldn't be resolved, then you might do a more realistic analysis?

MR. FINLEY: That is correct. That is correct.

Now I mentioned to you with respect to sump temperature we need to have the sump temperature come down somewhat in order for the operators to restart those safety injection pumps.

If you will look at this one slide here, we have calculated that at 190 degrees we have adequate NPSH, which occurs about four hours. Again, this is for the type of an accident that would maximize sump temperature.

CHAIRMAN DENNING: In this plant how are you getting your long-term cooling for containment in the sump? Is it through sprays and a heat exchange or on sprays or what is it?

MR. FINLEY: It is RHR pumps on recirculation.

MR. DUNNE: And containment is containment air coolers.

CHAIRMAN DENNING: You have safety grade

containment in those coolers?

MR. DUNNE: Yes, we do. Basically, we have a containment spray system and a containment air cooler system. We use both of them during the injection phase of LOCA. When we go into recirc, we basically terminate containment spray, when we transition to recirc, and we just use containment air coolers to do long-term cooling containment.

CHAIRMAN DENNING: Cooling the sump is occurring by cooling through the --

MR. DUNNE: Well, the sumps basically are low head SI pumps take their suction off the sump; they pump through a heat exchanger, and then that heat exchanger then delivers low head back to the RCS. We can also piggyback our SI pumps off the low head discharge coming out of basically mobile heat exchanges.

MR. FINLEY: Right. So the point of this slide is to show that at four hours we would be able to turn back on those SI, high head SI pumps, and procedurally we are going to set that time at four-and-a-half hours to make sure we have some margin here. Even at that four-and-a-half hours, that should be well before the time to conservatively saturate the core region with boron.

Next slide.

Okay, now we will shift gears to small break, a different scenario.

CHAIRMAN DENNING: A quick question, and that is, is it possible that for this plant we are overcomplicating things? I mean, as I look at the configuration here in this scenario, I mean the feeling is it is probably not a real scenario in terms of boron concentration. I don't know what reality is.

Here we are now requiring you to turn on SI at a particular point, but maybe that is not a big issue anyway, since you're not going to need the SI.

MR. FINLEY: Right.

CHAIRMAN DENNING: For it to go on too early and you lose the SI --

MR. FINLEY: This is conservative. We have made some changes to the analysis method here that we want to cautious about. We are doing it on a constrained schedule to support the EPU.

So it does not impact safe operation in terms of doing something that is not smart. So we felt that this was the right conservative approach.

Okay, with respect to small break, here the difference, the key difference is that the RCS will depressurize below the high head SI pressure but not below the upper plenum injection pressure. Remember, I said that that is around 140 psia for the upper plenum injection point.

So there are many small break sizes which won't cause you to rapidly depressurize below that 140 psi point. So the significant difference here is we need to take credit for operator action to help that depressurization process, which is really a part of our normal LOCA response procedures. That is nothing new. Operators are going to want to depressurize to stop an unisolatable lead regardless of the boron situation. So we are just taking credit for that in the boron scenario, as I will discuss.

So for the period of time that the low head SI pumps are not injecting to the upper plenum, we do expect there will be some concentration of the boron in the core region, where you have boiloff occurring and leaving behind boron. So we would expect some concentration there.

But the operators would depressurize the plant. Again, once you depressurize to below that upper plenum injection pressure, you would get a simultaneous injection setup, both from the upper plenum and the cold legs. That would flush the core for a break on either side.

Okay, next slide.

With respect to the analysis that was done, again, we used the dynamic, in this case, NOTRUMP analysis methodology to calculate the core voiding and the mixing level, et cetera, to feed into the concentration study.

A 4-inch break was conservatively used to bound all of the small breaks in this particular study. We didn't take credit for any beneficial effect of sump additives. We have sodium hydroxide added, and that would have a beneficial effect. We did not take credit for that.

We calculated a time to reach the boric acid solubility limit of six hours and 48 minutes, assuming that the solubility limit is established based on atmospheric pressure conditions.

Gordon, if you would click on that one slide?

So here a similar curve that you saw for large break; this is for small break. As long as we initiate the upper plenum injection prior to six hours and 48 minutes, we would stop the concentration process at about 29 weight percent, and that's the limit that corresponds to the atmospheric pressure condition.

MR. WALLIS: Stopped because the UPI now flows through the core?

MR. FINLEY: That's correct. That is correct.

Okay, click on this one here, Gordon.

So it is important now for the operators to depressurize the plant prior to that six-hour-and-48 timeframe. So what we did is, again using the NOTRUMP analysis methodology and taking credit for the operator actions, conservatively taking credit for the operator actions that would occur in the EOP response, we would get below the upper plenum injection point within about five, five-and-a-half hours.

So at that point, without any further action, essentially, the upper plenum injection would kick in based on the RHR pump shutoff head.

MR. SIEBER: How do the operators depressurize the plant? What do they do?

MR. FINLEY: The first choice for the operators would be to use the steam dump system. That is not what we used here. Of course, steam dumps would require offsite power availability and condenser vacuum.

MR. SIEBER: Right.

MR. FINLEY: So what we model here is

atmospheric dump valves. So they would use the atmospheric dump valves next after the steam dumps, and if they were to fail, then we would revert to use of PORVs.

Next slide, please.

So to summarize, we feel the Ginna design is robust with respect to having the upper plenum injection point as part of the two-loop Westinghouse design.

We have significantly upgraded the analysis to address the staff concerns with respect to void fraction, mixing volume, and decay heat. I didn't mention the fact that the staff questioned the uncertainty value used on decay heat. Essentially, we used the Appendix K uncertainty for decay heat, and that will prevent boric acid precipitation based on the design and the operator response in the LOCA procedures.

Any questions?

(No response.)

Then I will turn it over to Len Ward.

CHAIRMAN DENNING: I think we will probably take our break now. Instead of doing that, we will take our break. We will take our lunch break right now, and we will pick up at 10 minutes before

1:00.

(Whereupon, the foregoing matter went off the record at 11:50 a.m. for lunch and went back on the record at 12:51 p.m.)

CHAIRMAN DENNING: I think we are ready to restart. So you can just go right ahead, please.

MR. WARD: I am basically going to talk about the same items, subjects, I did on Beaver Valley. It is just the equipment has changed; the objectives are still the same though.

So I am going to talk about, first, just quickly the ECCS design, show you a little picture on why the limiting break for a large break is different from the cold break. You know that, but I think it just helps to set up what I am going to say.

Then I will talk about large break LOCA. I am only going to talk about long-term cooling, and, of course, that is boron precipitation. You need to be able to remove decay heat for an extended period of time. It is criteria five. In order to do that, you've got to put in more water than you are boiling. Then you have to make sure the boron, the boric acid doesn't precipitate.

For small breaks, I will talk about short-term behavior. Again, that is PCT, clad oxidation.

Then I will also talk about boron precipitation for that because it is an issue for small breaks as well.

Then we can summarize with some conclusions.

Ginna is a two-loop plant. This plant is different from all the other plants in that it has an upper plenum injection system that delivers low-pressure flow through two ports into the upper plenum. Then it has cold leg injection. They call it high head safety injection. That is delivered to the cold legs.

So the operators don't have to realign HHSI. All they've got to do is make sure the pressure is low enough to get that low pressure pump on, and then they will have a flushing situation.

Now they mentioned in the large break LOCA when the RWST drains, and that takes 24 minutes for the limiting large break, they turn off the high head pump. You've got low pressure injection going in.

So for the purposes of a boron precipitation calculation, that break is going to be worse because we are going to make the assumption that it doesn't flush the core. There is water going in that keeps it covered, but we are going to assume it concentrates. We are not going to take credit for any of the circulation, if that exists. So we are going to try to do a bounding calculation there.

Before I get into the picture, I think you saw this. Here's the high head safety injection pump. It has a shutoff head of around 1400 pounds.

This is the important one. It is the low pressure. I guess they call it RHR.

This is the curve and this is how I received it. So this is what I put in the code. I think the flow really would behave this way, but we are assuming that there is no flow -- you've got to get the pressure below 140 pounds to get the system on. So for the small break where you've got to cool the plant down, that is the item we are going to be concerned with.

I think my analysis shows you are up in this range where I've got at six hours, I mean you are at 60 to 80 pounds per second. The boiloff is like 23. Remember this is a small plant. So just remember that is a key ingredient.

My cartoon here is not to scale. I am sure Sanjoy wouldn't like it, but it is simple.

This is at the wrong location, but I want to show that the UPI comes in the center line to the hot leg through two connections, and then you have hot side and high head safety injection coming into the cold legs.

So after 24 minutes in the large break, if you turn this off, the hot leg break would become limiting because there is no flow from the cold to the hot side. We are going to assume that any of the ECC coming in from the UPI doesn't flow in and mix and flush it out. We are just going to assume that it just replaces -- just keeps the core covered in concentrates. So that is why the hot leg break is going to be limiting for this plant.

MR. WALLIS: Now would you explain why the core is stagnant?

MR. WARD: Well, I can show you, explain why. The core is not really stagnant. It is boiling. Steam is rising and water is flowing down counter to it to replace the boiloff.

MR. WALLIS: Where is that flow coming in, though?

MR. WARD: If you will recall, they showed, the Ginna people showed a WCOBRA/TRAC calculation. That is their best-estimate calculation. I asked them to run that.

I will get to the reasons why. I mean

when you see when the boron starts to build up, but that is a few slides later.

What that calculation shows, the water going down the peripheral assemblies and rising up the center. So it is just sitting there circulating, replacing the water that is boiling off.

So the flow in the central part of the core is upflow, and the flow down is really cold peripheral bundles --

MR. WALLIS: If you look at the whole loop, conceivably, you could have this UPI coming in and the flow actually going up the downcomer and around.

CHAIRMAN DENNING: Well, actually, you can't.

MR. WARD: I don't see how you could get --

CHAIRMAN DENNING: We've got a hot leg break.

MR. WARD: Yes, it is a hot leg break.

CHAIRMAN DENNING: A hot leg break, right, and we are looking at large --

MR. WARD: Here's a 2-foot hole. There is a 2-foot hole right here. This is 14.7.

MR. WALLIS: Everything is the same

pressure?

MR. WARD: You've got cold side injection, and the first 24 minutes you've got forward flow. I mean everything is going to be pushed out.

MR. WALLIS: Well, that was my question. Everywhere at a certain level you get atmospheric pressure.

CHAIRMAN DENNING: Yes, and it can't go around the loops.

MR. WARD: In other words, what's on, just the UPI?

CHAIRMAN DENNING: Just the UPI is on.

MR. WARD: Okay. Well, the accumulators and HHSI pump have filled the system up. So any more water that I had in excess of the water is going to spill out the break.

MR. WALLIS: It can't push through the loop seal or something?

CHAIRMAN DENNING: No.

MR. WARD: No.

CHAIRMAN DENNING: Because you can't get over the steam generators --

MR. WARD: There's a steam generator here. It has got to flow over the steam generator to get to the loop seal. There is just a water level, there is a weir here. So it is going to sit.

CHAIRMAN DENNING: So it is really stagnant there in this case where --

MR. WARD: Unless you boil off the water -- maybe if you've got some wall heat on that side and you boil off a little bit, I think you could get some oscillations, and then that would probably promote mixing. But I don't want -- they are not going to take credit for that. I just want it to buildup -- let's try to make this the worst -- let's beat it to death. That is what I am trying to do.

These are all good questions.

MR. WALLIS: So there is no way the water can go up and spill over that loop seal until that loop seal -- is the loop seal full of water, too? Does the water level --

MR. WARD: Remember we've got a hot leg break. There's no steam binding problem here. The steam that is building up in the core, where does it go? It goes out this huge hole.

MR. WALLIS: So everything there is at atmospheric pressure?

MR. WARD: Yes, I am assuming we are at 14.7 in this guy right here, 14.7 everywhere.

MR. WALLIS: How about the other way? The other way is --

CHAIRMAN DENNING: You mean the other hot leg?

MR. WARD: Well, the other hot leg -- I mean you've got two hot legs. I mean the steam is going out that hole in the hot leg.

MR. WALLIS: So I suppose as long as it is a big break this is okay?

MR. WARD: This is a double-ended break, yes.

MR. WALLIS: Okay.

CHAIRMAN DENNING: Well, actually, we did miss the possibility of steaming going up into the steam generator, condensing in the steam generator.

MR. WARD: The path of least resistance is probably right out the side and then just flow down a hot leg, go up a bend, and then contract and get into those tubes. I think it is going to go out the hole.

CHAIRMAN DENNING: But you absolutely rely on water recirculating back into the core? Otherwise, there is no way to keep the core cool.

MR. WARD: Right. The key ingredient here is the LPSI pump, this UPI pump is putting in far more water than you are boiling.

CHAIRMAN DENNING: Yes. It can flow down

some way to get into the core.

MR. WARD: It is going to spill out that hole.

MR. WALLIS: It will fill up the vessel, won't it?

MR. WARD: Yes, sure.

MR. WALLIS: So just lower the curtain and end the play.

MR. WARD: Right. That's right.

CHAIRMAN DENNING: That is a good question.

MR. WARD: So for large breaks, what do they need to do since you turn off the high pressure pump once the RWST drains? They've got to turn it back on, and you've got to turn it back on before you would predict precipitation. It is simple.

They don't have to split the --

MR. WALLIS: But you are foolishly throwing away the other water, aren't we?

MR. WARD: Yes. But now for small breaks, the pressure -- you have to remember in the large break it gets down below 140 pounds, but for a small break you can be above 140 pounds for a long time. So what do you want it to flush the core in order to get both systems working? Remember the HPSI pumps work or that high pressure pump is working in the beginning. We need to get the pressure down so we can get that other pump from the hot side, so that if the break is on the cold or the hot side, it will just flush.

So the key ingredient there is to cool the plant down, and that is where the operator actions come in. Long-term cooling is different than short-term behavior PCT. The ECC is designed to keep the temperatures low. The operators should just verify everything is on and diagnosis. They shouldn't have to take any action.

In the long-term cooling they've got to do things. So to control boric acid, that is on the operators' shoulders. It is up to them to make it work. That is why we are focusing on this.

This being a particularly different plant, we had them do a lot of calculations. Let me talk about the large break model.

You've seen the same model in the original submittal that went back, the long-term cooling -- the large break LOCA analysis was very crude. They used a decay heat multiplier of one. They assumed the whole mixing line was full of liquid.

We didn't like that. So we said, hey, let's step back and let's do a little bit better calculation.

So they went and they did the calculation where they justified their mixing volume, took credit for the void fraction, so it is not solid liquid.

Now we are also using the same precipitation limit, 29 percent, and that is 14.7.

MEMBER KRESS: How good do we know that number?

MR. WARD: What, that?

MEMBER KRESS: Yes.

MR. WARD: How good do you know that?

CHAIRMAN DENNING: Well, for pure boric acid you know it well.

MR. WARD: I've got a curve from the boric -- from the borax company. I will just show you what it looks like.

They have measured the precipitation limit as a function of temperature. We are down here around 29 percent, 212. If you've got additives, it is up here.

So we are essentially using this. We are using the data from this.

MR. WALLIS: Is this the same borax I can buy in the supermarket?

MR. WARD: It probably is.

MEMBER KRESS: Twenty Mule Team.

MR. WARD: I think it is.

MR. WALLIS: Twenty Mule Team, yes.

MR. WARD: It is.

So you will recall this is the calculation I did, and it says, "delay" on it. You will notice that it doesn't start until 24 minutes. I will show you another curve, but if you assume the boron builds up from time zero, you are going to precipitate in four-and-a-half, 4.8 hours.

I was really confused: How are they getting this six hours and 13 minutes? I couldn't figure it out until we finally talked enough and finally he says, "Oh, wait a minute. We're not letting buildup until 24 minutes."

The reason, the logic for that is during the initial portion of the large break LOCA I have high pressure pumps on; I have a hot leg break. There's a lot of forward flow. You are depressurizing in that upper plenum. It fills up. It is probably going to concentrate within maybe the first several hundred seconds.

But once you fill that vessel up, you've got 80 pounds per second going on in one side and of the order of 80 or 90 pounds going out the other side.

So you are not going to build up boron in the first 24 minutes.

I asked them to do a calculation to prove that. They went and exercised their best estimate LOCA model, the large break LOCA code. That code has UPI models that were reviewed. It has de-entrainment on the guide tubes. It has entrainment phenomena that sweeps out drops. The droplet size distribution is based on data for spraying horizontal jet of UPI into a vertical column of guide tubes. Those models are all in there, and it's got CCFL limits. If the steam is too high, it won't let liquid go down.

So they ran that. They ran that code in an Appendix K mode.

MR. WALLIS: Let's put this in perspective. It starts off at 2400 parts per million, is that right?

MR. WARD: It starts off around, it is 3050 parts per million.

MR. WALLIS: What's that? So that's point --

MR. WARD: It is like 1.5, something like that, 1.7.

MR. WALLIS: One point five percent. It is not .3 percent.

MR. WARD: Yes, it is something like that.

MR. WALLIS: So I can't take parts per million and get percent directly.

MR. WARD: Divide by 1748. Take the ppm --

MR. WALLIS: Okay, so it is 1.5 percent or something?

MR. WARD: Right.

MR. WALLIS: And I'm going to concentrate it to 30 percent. So I've got to drive off 20 times as much water as I leave behind?

MR. WARD: Well, no, it is going to concentrate at the rate it is boiling.

MR. WALLIS: Yes, but I mean to get 29 percent, I've got to drive off 19 parts in 20 of the water. For 20 gallons, I've got to boil it down to one gallon.

MR. WARD: Yes, something like that.

MR. WALLIS: It is a humongous amount of water I've got to boil off.

MR. WARD: Sure, there is.

MR. WALLIS: I've got to start with an enormous amount of water in order to finish up with something which is the amount of water you're ending up with in the vessel, which is concentrated to this.

MR. WARD: Right, and don't forget, you know, there's a high --

MR. WALLIS: So where does all of that water come from that I've driven off?

MR. WARD: The initial water that is there, the ECC injection.

MR. WALLIS: That's nowhere near enough.

MEMBER MAYNARD: Accumulators.

MR. WARD: You are putting in 80 pounds per second in the cold side, and what's the LPSI flow?

MR. WALLIS: It is all accumulating all that time?

MR. WARD: I mean, you've got a 700-pound accumulator in there.

MR. WALLIS: And you are boiling all that off?

MR. WARD: Right. I mean you've got two huge accumulators and they just --

MR. WALLIS: So you've got plenty of water in there?

MR. WARD: -- dump tons of water in there.

MEMBER MAYNARD: You're putting a lot of water in it.

MR. WARD: I'll show you when I get to the --

MR. WALLIS: Not as much water as you finish up with that you boiled away. That is a huge amount.

MR. SIEBER: A couple of hundred thousand gallons.

MEMBER KRESS: When you boil off at atmospheric pressure --

MR. WARD: Yes.

MEMBER KRESS: -- doesn't the steam take the boron with it?

MR. WARD: It does, but we're not --

MEMBER KRESS: You are not even going to account for that?

MR. WARD: That is not credited.

MEMBER KRESS: That might take your time way out.

MR. WARD: That is right, and there's entrainment, too, that is taking that liquid and --

MEMBER KRESS: Yes, not even counting the entrainment, no.

MR. WARD: No, I'm not counting that either. I'm not. Zero.

MEMBER KRESS: Okay, so that is another conservatism there?

MR. WARD: Right, and there's 20 percent

additional power on the decay heat.

So this calculation that I did reproduces the licensee calc.

I just want to show you, well, what happens if there is no delay? This is what I was getting originally, at or around 4.8 hours. This is what was confusing me.

But look at it this way: The additives, the precipitation limit is really up here with the additives and the containment. So even if it builds up from time zero and it wasn't flushed at all, you're still going to be okay. This is still going to take, well, it is going to take a long time. This is 20 percent more decay heat. If you subtract -- if you go to 1.0, it is even going to push you out farther. That's at 14.7.

So I think it is safe to say that there is some margin in that calculation.

MR. WALLIS: As long as it doesn't boil over when it gets to about 15 percent. Suppose its properties change so that it boils over like milk boiling in a pan. At 15 percent, then you have lost it.

MR. WARD: Well, none of the tests show that. You think it is going to do that?

MR. WALLIS: You don't know that yet. I don't think anyone has done tests to that high a concentration. It is stopped at a lower concentration than that.

MR. WARD: I have seen tests that have gone up to 32 weight percent, but I can't discuss it. I've seen it. Maybe we can talk afterwards.

MR. WALLIS: Okay.

MR. WARD: So let's go to the short-term behavior and let's jump back and let's look at PCT. In the original submittal they submitted three break sizes. That is obviously not enough to identify the peak, and the peak was found to be a 2-inch break. But with a Pclad temperature of 1167, I ran that calculation and I got around 1100 degrees.

This ECC system is probably the best I have seen. I have never seen a plant with 700-pound accumulators. Those accumulators come on real early. They keep the core from uncovering.

It is really a good design in that respect. It has got very high capacity, high pressure pumps compared to the boiloff. I mean you could pump the Atlantic Ocean through this core in about 10 minutes. It is why the core doesn't uncover. If I run this at 1.0, there's going to be no uncovery for this break specter. I am going to get no heatup.

So based on the calculations that we did, and what they did, there's really no need for them to go off and spend their time looking at these non-integer break sizes when at most it might increase the PCT by what, 100 degrees. I mean they are well below 1500.

So we said, "You don't need to submit that." They went and did it anyway. But we really didn't need it.

As a mater of fact, we had them look at some larger breaks because -- and I am going to show you this in a minute -- you turn the HPSI pump off during a small break. There is no injection. Here you've boiled the system down with levels in the hot and cold leg, not something that I really like, like to see, but they've done a lot of analysis.

As a matter of fact, they looked at these larger breaks and turned the pump off for 10 minutes because they have stated that they can make that switch in five minutes and certainly within ten. When you look at all these breaks, you see a drop in the level when they turn it off but the core doesn't uncover because of the fluid above the top of the core.

Even for these larger breaks, they didn't uncover and they didn't even take credit for the UPI, only the high pressure, and it still didn't uncover. So I liked that when I saw that.

Now we did calculations with Relap also, and I am going to show you one in a minute.

MR. SIEBER: So if the UPI is the break, that side of the break, you're still okay?

MR. WARD: Yes, I'm okay.

They also looked at severed ECC lines. When you have a severed ECC line, you have one line that sees 14.7 and the other one that might see 800 pounds. So you are not going to lose half the flow. You are probably going to lose more than that. Those were not limiting also.

Now we confirmed this with a Relap5 calculation, ran the 2-inch, ran a lot of breaks. Of course, we were 1811 megawatts and 17.5 kilowatts per foot.

Again, I said we confirmed that breaks on the top of the cold leg, where you can fill the loop seal out, didn't depress the level into the core, nor did severed ECC lines become more limiting.

But the key here is you've got to reinitiate that high pressure pump within 15 minutes, and I will show you why in a minute.

One of the things that you are going to see in the calculation is I got a CHF condition again. As I mentioned before, I have been talking with Josh Hartz at Westinghouse. I think it is probably a combination, as I said before, between assumptions and differences in the code. Maybe our code is more conservative. Maybe the resistance is in the hot bundle or maybe they are a little too high.

Nevertheless, I got a 1400-degree temperature. It is maybe close to 1500. But the point is the PCT still remains well below 10 CFR 5046 limits. But we really want to understand this, and if we have to pursue it further, we will.

CHAIRMAN DENNING: Now this is where you were saying you used the Relap?

MR. WARD: Yes, this is Relap, and I am going to show you this calculation.

I am looking at a 2-inch diameter break here and turn the pump off. This is about the time the RWST drains. Turn the pump off. This is a 2-inch break, cold leg break. Turned the pump off here around 7200 seconds, and in about 15 minutes the core uncovered. In about another 15 minutes it is 2200.

So they say they can perform the action in five minutes, no later than ten. This is 1.2 times ANS. They've probably got 20 minutes if you have this break in this location.

So it is very important that the EOP be emphasized and the training be emphasized with these operators to make sure that they can do that within five to ten minutes.

MR. FINLEY: Yes, this is Mark Finley again, the Project Director for the uprate.

Len is correct, and we have emphasized this in our procedures. They have the procedures set up now to emphasize to minimize the time that these pumps are off.

But I will make the point that you see we would terminate the high head SI pumps at around two hours into this event. So this is not happening five minutes after the break occurs. So there would be time here to ensure that the operators are briefed; they understand the actions that they have to take and would turn these pumps back on.

MR. WALLIS: Why do they turn off?

MR. WARD: Because not enough net positive suction head. That is for the large break. You've got to switch it to the sump.

MR. FINLEY: Right, we are shifting from

the injection phase to the --

MR. WARD: From the RWS -- they are starting from a tank and now they have got --

MR. WALLIS: You have drained that tank; now you have got to switch to the sump? So you have to realign the intake and everything?

MR. WARD: Yes.

MR. FINLEY: Right. There's three sets of valves that have to be repositioned. We feel very confident we can do that within five minutes.

MR. DUNNE: Yes, this is Jim Dunne from Ginna.

Basically, our ops procedures, urgency procedures, basically, tell our operators to basically turn off SI and then check RCS pressure. If RCS pressure is above a certain value, then they are told to restart SI pumps. In this mode for a small break LOCA that is what they would be doing. They would turn it off.

They probably at this point in time would already know what the RCS pressure is before they go into the recirc mode. So they would probably even make an assessment as to whether they really should be turning off the SI pumps or not.

But the ELPs are based upon symptoms. So they will check the RCS pressure, and if the RCS pressure is above a certain value, they are basically instructed by procedures to restarting that SI pump.

MR. WARD: And this break, bigger breaks, and I will show you what they look like --

MR. WALLIS: How is this affected by the EPU? We are talking about power uprate.

MR. WARD: Well, it is a higher power.

MR. WALLIS: Does something change? This picture is the same now. This is what they do now, isn't it?

MR. FINLEY: That's correct.

MR. WALLIS: How does it change by the EPU. Is it a shorter time period?

MR. WARD: They probably have a shorter amount of time before the core uncovers.

MR. WALLIS: Is it really a critically shorter amount of time or how does it change?

MR. WARD: You've probably got -- what's the power increase, about 20 percent? So five minutes maybe.

MR. WALLIS: So you do have a shorter time?

MR. WARD: It is decreased by five minutes.

MR. WALLIS: Which is significant.

MR. FINLEY: Like Len said, he calculates something on the order of 20 minutes, I think, before you would start to uncover again. So that time is shortened from, say, 25 minutes to 20 minutes as a result of the EPU, something on that order. But, again, we can make these actions within about five minutes.

MR. WALLIS: And has the net positive suction head changed as well because of the EPU?

MR. WARD: I think the containment, the sprays for this have been operating for this period. You've got cold water in there. You've filled it up.

MR. FINLEY: Right. That really only applies to the large break scenario.

MR. WARD: That is the large break where you're early, you're hot, and it is probably not a good thing to do.

MR. GILLON: This is Roy Gillon, Shift Manager.

We run a scenario multiple times a year in a simulator, and we have criteria. Typically, we can get this done in five-six minutes of time. We have never had any trouble getting it done in 10 minutes.

CHAIRMAN DENNING: And is there no option considered for depressurization to assure that your pressure is low enough to have the BPI?

MR. WARD: Well, there is. I am going to get to that.

They will initiate a depressurization with both ADVs and one out, cool the plant down now. I will show you, but this is the break. A break bigger than two inches gets the UPI on it. It is a moot point.

This is probably the biggest break where you are only going to have hot side high head injection. So if it is the biggest break, this is the earliest that it would occur with the highest of K heat. So I picked this one because this is the limiting one.

CHAIRMAN DENNING: But you are showing us a case in which they have not successfully depressurized.

MR. WARD: Yes, I will show you what happened.

MR. FINLEY: Let me just clarify. There's two independent sort of issues here. This relates to not turning the SI pumps back on in a timely fashion when you switch from the injection phase to the recirc phase.

CHAIRMAN DENNING: Yes, right.

MR. FINLEY: It really doesn't relate to the pressure in the RCS.

CHAIRMAN DENNING: Well, if you had depressurized and you had the UPI on, does it make any difference?

MR. FINLEY: Well, you are correct, if we could get down below 140 psi, but this is only about two hours in. We really can't get there for all the break sizes, right.

MR. WARD: Right, and that is why this one is limiting for that case, and you're right.

MR. DUNNE: If you did depressurizing down to below the UPI cut-in pressure, you would not see that interruption at all.

MR. WARD: Now I want to talk about long-term cooling for small breaks. The analysis shows that you can borrow for long periods of time, and because it is a small break, the pressure remains above the shutoff head of that low pressure injection pump. So what do you do?

Well, you need to reduce the pressure below 140 pounds to get the UPI on, or if you can't do that, then show that it refills. I will show you what that looks like in a minute in a slide.

Now what I asked them to do is -- there were no analyses of these breaks because of this plant. I want to know which breaks will you stay in natural circulations, which ones refill, which ones don't refill, and get UPI on, so we've covered all the bases.

So they did this detailed analysis. Below two inches the UPI comes on. So they did a pretty good job and a pretty detailed analysis, looking at all these with their -- this is their Appendix K small break NOTRUMP code.

MR. WALLIS: Below two inches or above two inches? You mean above two inches?

MR. WARD: I mean above. Yes, I'm sorry, above two inches. I'm sorry. You are right.

MR. WALLIS: That was just to test us, wasn't it?

MR. WARD: Yes, that was a test, wasn't it?

Now what our audit calculation shows is that for an 01 square foot break this is a 1.5-inch; this is about 1.3 inches. I think in terms of square feet. I don't like inches. So I have got square feet here.

But in 2.8 hours this break refills, and this little larger break refills in about four hours.

Now the other thing I looked at is when I said, gee, what if I fail one of those ADVs? Well, I've got two PORVs. What does the system look like under that condition? I will show you that in a minute.

Let me show this critical break size range that I could call for small breaks. We are looking at 2 inches, 1.5, 1.3. This is RCS pressure.

Now there is a 2000-second steady state, and I didn't subtract that off, but the break opens at 2000 seconds.

Operators open both ADVs at this point and start cooling the plant down. You can see if I have a 1.3-inch break, if I refill and resubpool the system somewhere in here -- a bigger break takes a little longer. I'm out here maybe four hours. If you look at the void fraction in the core, it goes to zero for this 1.5-inch break and it will go to zero back here for this slightly smaller break.

Now if I look at a 2-inch break, I am depressurizing, but what happens is I get down below 100 pounds. So I am right in here. So the UPI is on. So I am fine.

Bigger breaks, depressurize faster. I get more and more flow. Smaller breaks will refill earlier, and you will probably repressurize up near 1400 at some point because the break is so small. So the operator will see that response.

All breaks from roughly two inches down will refill and resubpool and disperse the boric acid. Good system response.

Now I am going to say, what happens if we only have -- I'm looking at a double failure here. I just wanted to see what this looked like. This is that 1.5-inch break. I have one ADV and I am only opening up two PORVs, and I am hanging up in pressure for a while. Let's blow that up. So I am out eight hours.

Actually, what is happening is the low pressure pump is coming on here. This is about 140 pounds. I would like to see it get down around 120 pounds because now you are getting a lot of flow in there and it is flushing. It is flushing, okay, but I am out probably eight hours.

But the point is, if I have delayed the cooldown and I am coming out here and it is a slow -- it is at a high temperature, there's a high limit. It is not 29. It is 35, 40. As a matter of fact, in this case it is probably greater than 50 percent if I look at the boric acid concentration as a function of time. I am at a higher pressure. I have a lower void fraction. So it takes a while even to get to 29, but the limit is way up off the top of this page because I am over 300 degrees.

So the point here is you don't want to be crashing the pressure down if you have been boiling for a long time. So we made a point to have some discussions about changes to the EOPs, the guidance, to make sure that in order for this to be successful, you start to cool down at one hour. Caution the operators, if you have been boiling, not to crash the pressure down if you are out there eight or nine hours.

There are strict statements that do not exceed the 100-degree-per-hour cooldown limit, and that will prevent you from, say, opening the bypass and crashing the pressure down if you get power back. We don't want that to happen.

So we basically talk about emphasizing cool-down time and the equipment and the timing and the operator actions, and their attention to this event, because it is going to be controlled by them.

There are training programs that they are running their operators through. As a matter of fact, I think we are going to verify and observe and make sure that we see these things being done by the operators and they are done very effectively and very timely.

MR. FINLEY: Yes, and this is Mark Finley, again just to interject.

Like Len says, the priority is on starting the cooldown and then finishing the depressurization prior to the boron concentrating.

This really fundamentally doesn't change the operator response to a small break LOCA, however. We are not having to make any significant logic or sequence changes in the EOPs. We are doing some streamlining to minimize these times, but fundamentally the operators are going want to cool down and depressurize the plant to stop or minimize the leak.

So what we have done is put some cautionary statements in the procedure to emphasize to the operators to get the cooldown started within an hour and then to get below the UPI injection point within about five-and-a-half hours.

MR. WARD: So I guess I can summarize the review. Initially, we asked the licensee to do some more calculations because we learned the HPSI pumps, because of their design, are terminated for small breaks. There were some omissions in their long-term cooling analysis.

They did a detailed analysis to show what breaks refill, what don't, what can be cooled down, and what can be refilled if you can't flush. There was a very detailed spectrum analysis that was done with their NOTRUMP small break LOCA code to show that.

The temperatures are low for small breaks because the ECC design is very robust. They have very high pressure accumulators, 700 pounds. That terminates, prevents, precludes, basically precludes uncovery in the real world, and even in Appendix K space we're get what, 1100-1200 degrees. Good design.

Staff calculations confirm their precipitation. As a matter of fact, by doing the calculations we have found out a lot about the plant and understood better how this thing works and what is going on in the beginning of the transient as well as at the end.

It showed that boiling can last for a long time, and equipment and timing for its use is very important and needs to be emphasized again and again. I think that is a key ingredient here.

I think by this whole analysis, the

emphasis on operator actions is a positive safety thing, and it is going to be included in their training programs for their operators. The analysis that the vendor has done is going to be able to show these operators what is the signature of this, what's it going to look like, how long do we have to get down. So there's a lot of good analyses they can use there to supplement the information the operators have.

Based on the calculations that they have done, I looked at the short-term small break LOCA behavior and the long-term cooling and feel that it is a bounding calculation. It is comprehensive and it meets 10 CFR 5046.

CHAIRMAN DENNING: I have a couple of questions that I don't consider EPU questions. That relates to the modeling assumptions associated with 50 percent of the lower plenum and this kind of stuff.

MR. WARD: Right.

CHAIRMAN DENNING: The BACCHUS experiment is the principal rationale that you have --

MR. WARD: It is one of them.

CHAIRMAN DENNING: -- that are supportive of that?

MR. WARD: It is one of them. There's a

Finnish paper, and I am not sure if you remember, Ralph, or not; I think I gave you a copy of that. That shows some lower plenum mixing as well, but they have some current concerns with scaling.

I mean we have the same concerns with the BACCHUS. There's a gradient; there's a concentration gradient in the core. We are mixing everything together.

So I took the code that I developed and I predicted that if I assumed the entire lower plenum, I am too late on the precipitation. So I cut the lower plenum volume in half, and I better predicted the timing for when the top half of the core reached the limit.

MR. WALLIS: That comes from matching the BACCHUS data within a model?

MR. WARD: Yes, the boiloff. Right. I took my model and modeled that test and compared it to the boron concentration as a function of time.

CHAIRMAN DENNING: I think that we don't understand the BACCHUS experiment well enough to really understand its direct applicability in a manner like that.

MR. WARD: Okay.

CHAIRMAN DENNING: I think that one can do more mechanistic analyses of what is really happening in attempting to predict the BACCHUS experiment.

MR. WARD: Yes.

CHAIRMAN DENNING: We would like to see some effort done there.

You know, earlier we had some recommendations related toward looking at what happens as you get closer to precipitation.

MR. WARD: I agree.

CHAIRMAN DENNING: I understand there's some work that is going to happen there.

MR. WARD: Right.

CHAIRMAN DENNING: We would like to see a little more.

MR. WARD: We will gladly share that with you. I mean, for example, what I would like to see is break the core up into 10 regions and model the gradient. That is a more sophisticated calculation, but --

CHAIRMAN DENNING: Yes, I think you can do that calculation --

MR. WARD: Yes, that can be done. That can be done.

CHAIRMAN DENNING: -- in a mechanistic way.

MR. WARD: Yes, I think it can be done. I agree with you.

This generalized letter with the concerns in it about how the vendors have been doing calculations, that is one of the issues in there.

This one, this average concentration, show me that that -- make it bounding or do a detailed calculation. Show me what it is. What does it really look like?

MR. WALLIS: Wasn't there some kind of critical thing in BACCHUS where after it got a certain difference it turned over or something?

MR. WARD: Yes. They are putting in cold water. Once the concentration in the core and upper plenum exceeded the density in the lower plenum, then it started to mix.

MR. WALLIS: And then it turned over. It is a turning-over criteria.

MR. WARD: Then it turned over, yes. You can look at the Finnish test and you will see the same thing. It occurs at a different time. It is at a different temperature.

But there are a lot of questions, and the owners' group are addressing them right now.

MR. WALLIS: You have a half. If you had

something like a third, this would change the time when they have to take action?

MR. WARD: Sure, absolutely. Sure. Lower plenum is probably worth three or four hours on pre-set time.

MR. WALLIS: I think this is a little bit tenuous, this determination of just what the time is when they have to take action.

MR. WARD: Well, remember the limit is more like 40 percent. If you threw out the lower plenum, you've got 15-16 hours.

CHAIRMAN DENNING: Well, we hear you, but we would like to see a little more to make us understand what is really going on.

MR. WARD: All I am saying is there is a margin there, and they are doing analyses to address all these issues. We don't have all the answers right now, but we are going to get them.

MR. WALLIS: There's a research program in RES that is addressing this?

MR. WARD: Well, no, but --

MR. WALLIS: Is it Westinghouse? Who is addressing it?

MR. WARD: The owners' group.

MR. WALLIS: The owners' group.

MR. WARD: The letter went out to all of the vendors and utilities who do calculations, asking them -- well, there was a list of concerns on how they do their calculations. We wanted to get them on the same page. There are a lot of questions about justification for their model; what happens when you've got debris going in there; what happens when you add cold water. That is in there, too.

There's probably two pages of issues that I see is going to require some experiments to --

MR. WALLIS: What will concern me is if, as a result of this new research, you have to radically revise your view of boron precipitation.

MR. WARD: Boy, I hope that doesn't happen.

MR. WALLIS: I know.

MR. WARD: I know. Well, I can't stand here and say, "Boy, that's not going to happen." I can't. That's why we asked the questions.

MR. WALLIS: Well, think of all the surprises you got with the sumps. Surprises do happen.

MR. WARD: That's right. Well, I suspect there's going to be a few surprises here.

MR. WALLIS: We will shine the spotlight

on you in a while. Okay.

MR. FINK: If I can say something? It is Dave Fink of Westinghouse.

I heard something up here, if you will forgive me. The WAD program has been mentioned a few times here.

Recently, the NRC sent a letter to the PWR owners' group stating the staff's principal boric acid precipitation methodology concerns. The PWR owners' group is in the process of preparing a response to this letter.

I happen to be the lead, the Westinghouse lead on that program, so I know a little about it.

It is important to emphasize that the methodology concerns raised by the NRC in their letter have been addressed for Beaver Valley and Ginna for the uprates, as we discussed over the past few days.

As suggested by the staff, in the owners' group response to the NRC letter we use insights from these analyses, that is, as performed for Waterford, Beaver Valley, and Ginna, to show that from the plants represented by the owners' group that existing calculations are conservative and that existing emergency procedures will prevent boric acid precipitation after a LOCA.

While the upcoming owners' group response to the staff's letter addresses the principal methodology concerns, there are many other tougher questions that the staff and the Committee have raised regarding mixing phenomena in the reactor vessel and regarding boric acid solutions in general.

These questions are the subject of ongoing GSI-191 programs and also a longer-term owners' group boric acid precipitation methodology program. The objective of this latter program is to answer the questions that can be answered and, probably more importantly, to show that the methodologies such as those used for Waterford and Beaver Valley and Ginna are adequate to ensure the safe operation of the plants and to demonstrate compliance with all regulations.

The owners' group intends to meet with the staff in the near future to discuss this program, the specific objectives of this program, and the long-term solutions to these questions and these problems.

CHAIRMAN DENNING: Thank you for that.

I think we are done now with the presentations, and I think we are just into some wrapups.

MR. FINLEY: Yes, Dr. Denning?

CHAIRMAN DENNING: Please.

MR. FINLEY: There is one open question from this morning. We do have some data with respect to the question about RETRAN uncertainties. So we would like to show you that data.

CHAIRMAN DENNING: Please do that.

MR. FINLEY: Okay.

MR. HUEGEL: My name is Dave Huegel. I am from Westinghouse.

One of the things that was being discussed this morning was the loss-of-flow event. What we have here is I just put together a plot where the blue line -- and I picked out points as best I could of what the flow coast-down was as measured at the Ginna plant.

This is a normalized curve and it is based upon whatever the actual flow that was being measured at the plant. Keep in mind they do have a tech spec which identifies the minimum measured flow that the plant has to meet and verify going into a cycle that they are above that flow rate.

The minimum flow rate that we assume in the safety analysis is the flow that we were doing the DNB calcs and lower than what the plant has to ensure that it is meeting.

What you have here in the purple line,

that is the complete loss-of-flow event where the coast-down that is caused by the complete loss-of-flow event, where the pumps are allowed to coast down freely.

Probably the biggest difference between these two curves is, as I mentioned this morning, the fact that in the safety analysis we take off 10 percent from the pump inertia, and we do in the safety analysis model all of the pump characteristics, the homologous curves, so that we have captured in the RETRAN model an accurate representation of what the plant or the pump models are.

Another thing that I mentioned in the loss-of-flow analysis, when we assume the rods are dropping into the core, that is based upon a confirmation that the plant performs based upon full RCS flow conditions. As you can see, during the coast-down you are going to be at a degraded flow condition, and we would expect that the rods would fall into the core even faster.

Another thing that we do is in the modeling of the reactivity that is inserted in our point kinetics model, as I mentioned, it is assumed that there was a xenon transient in effect where your reactivity is pushed towards the bottom of the core, and that is what we assume for the addition of the reactivity as the rods are falling into the core.

Yet, at the same time when we do the DNB analysis we would assume a shape that is closer to a shape that has an AFD axial flux difference closer to zero, which would be limiting for DNB-type calculations.

So, at the same time, you would have a reactivity shape where your axial power shape is skewed towards the bottom of the core. Yet, at the same time we are assuming a DNB axial power shape that is skewed more closer to the top of the core. So that is an additional conservatism that we have within the analysis.

The results that are represented this morning were for the under-frequency decay case. The way that the pumps operate is they operate off of the frequency on the grid. So if you have a change in frequency, it affects how the pumps are operating. Fluctuations in voltage typically don't affect the pump speed that much.

What we have here is a case where we have assumed a very conservative 5 hertz per second decay in the pump coast-down. Now one of the features at a typical Westinghouse plant, and it also applies to

Ginna, is that as soon as you hit the under-frequency set point, then your trip breakers would, your pump breakers would open, and the pumps would be free to coast down.

So that at some point in here the pumps in reality would begin to follow the line closer to what you would see in the purple line, actually the blue line. Yet, we have assumed in the analysis that the pumps are dragged all the way down to essentially a zero condition at 12 seconds.

So this is just to show you the comparison and to tell you that we did do a comparison of what the actual plant data would be versus what we have assumed in a safety analysis.

MR. WALLIS: There is no plant data per se here?

MR. HUEGEL: Well, the blue is the plant data.

MR. WALLIS: It is plant data?

MR. HUEGEL: Yes.

MR. WALLIS: Okay. I wasn't quite sure --

MR. HUEGEL: I'm sorry, yes.

MR. WALLIS: -- if it was your prediction from realistic or it is the plant. Oh, it is actually the data? Okay.

MR. HUEGEL: Yes, that is actually the data, yes.

MR. WALLIS: It is a line through the data or does the data have a big scallop --

MR. HUEGEL: I was just given a plot from the UFSAR, and I was picking off points as best I could. I apologize; I didn't do a super job there with the blue line.

MR. WALLIS: Which is one transient. There's no bouncing around?

MR. HUEGEL: No. If there was any bouncing around, it would probably be to detect noise. I mean we do see, if you look at, for example, your hot leg temperatures due to the RTDs being where they are, you do see noise in your hot leg signals which presents a problem for like the over temperature delta T, which has a lead lag function. If you have a spike in your T-hot which affects your TAV, you get a spurious spike on your margin of the OTDT, which isn't real, yet presents a problem in terms of ensuring a plant margin when you are just in a steady-state condition.

MR. WALLIS: This is graph paper.

(Laughter.)

MR. FINLEY: That is the curve from the

UFSAR and shows the two-pump coast-down alpha and bravo flow.

MR. WALLIS: This is measured?

MR. HUEGEL: Correct, that is measured.

MR. FINLEY: Correct. That was part of the hot functional testing when Ginna initially started up. Dave just transcribed that data to the plot you see on top, the blue.

MR. WALLIS: Oh, okay.

MR. HUEGEL: I am due for an eye exam. So I apologize.

CHAIRMAN DENNING: Now are you going to show other characteristics then of the --

MR. HUEGEL: Yes, yes.

CHAIRMAN DENNING: Go ahead.

MR. HUEGEL: Were there any questions?

CHAIRMAN DENNING: I understand that, yes.

MR. HUEGEL: This is a comparison of the RETRAN that we just recently completed. This was for the Ringhals 3 plant. It is a plant in Sweden where we did some comparisons against plant data.

We don't have any, other than what I was just showing you with the flow coast-down for Ginna, but here is a comparison, if you can see that.

CHAIRMAN DENNING: It looks like you cut

off the top. What are they?

MR. HUEGEL: I'm sorry. That is the nuclear power transient.

This is for a power load decrease, and the hash line in here is the plant data, and the red line is what the RETRAN model is doing.

MR. WALLIS: After being adjusted?

MR. HUEGEL: Yes, keep in mind that the RETRAN model, we are using a point kinetics model. So as your rod control system is moving in and out, we have some differential rod data, but the fact that we are using frozen feedback and a point kinetics model, we did have to make adjustments to that differential rod worth. Once we did, we got a close match with the nuclear power.

MR. WALLIS: Are you fitting the data or are you making a real comparison?

MR. HUEGEL: Well, this, actually, on the nuclear power, you would say it is more like fitting the data. Then the question is, how is the RCS responding to the transient once you have done a comparison or a fit of the nuclear power?

This here is your vessel TL. The plant data is the black hash line, and your red line is the RETRAN predicted --

MR. WALLIS: You have used invisible ink for the RETRAN base somehow?

(Laughter.)

MR. HUEGEL: Actually, it's in there.

CHAIRMAN DENNING: It's in there. Yes, I see it.

MR. WALLIS: It is sort of visible.

MR. HUEGEL: But this is a comparison where we have the rod control system turned on. We have the steam dumps model. We also have your pressurizer pressure control and level control all turned on. So all these kinds of different control systems that certainly we don't credit when we perform a safety analysis.

CHAIRMAN DENNING: And that is a pretty fine scale, actually. I mean things are a little bit tight --

MR. HUEGEL: Yes. Granted, it is.

Here is a plot just showing response of the RETRAN model to the pressurizer level. Again, given the scale, I think it is tracking the results rather well.

Here's the pressurizer pressure transient, again, the red being the RETRAN results and the hash line being the plant data. So it is showing a fairly good match of this transient where you are getting fairly substantially large changes in the nuclear power and other parameters.

This is the coolant flow, the RCS coolant flow, the loop steam flow, steam header pressure.

MR. WALLIS: Wait, wait.

MR. HUEGEL: Do you want to go back and look?

MR. WALLIS: So when we look at these, we see a sort of agreement, but there's a difference, too. So we don't quite know how to interpret this when you show us a plot of a prediction of a transient, how much we should allow for RETRAN uncertainties around that prediction, because we know there are some, as you can see here.

MR. HUEGEL: Sure.

MR. WALLIS: We don't quite know how to translate what you show us here to what you showed us earlier today.

MR. HUEGEL: Again, I would look at the scale and say that, yes, it looks like a big change, but if you look --

MR. WALLIS: This is a proportionate change or is it a certain error and a certain number of bars?

MR. HUEGEL: I think it is more a function of the units that were selected. I mean I only have 70 units a bar here.

The other thing, as I was mentioning this morning, the other important point is we do make very conservative assumptions in the analysis in not crediting the different control systems, which gives us what we believe a very conservative analysis.

When we do a comparison, for example, to flow coast-down, we do see that we are predicting a very conservative coast-down.

MR. WALLIS: In this case the actual pressure is significantly above the RETRAN phase. The change in pressure is also significantly bigger.

MR. HUEGEL: Keep in mind this is the steam header pressure.

MR. WALLIS: Right.

MR. HUEGEL: We are most concerned in looking at the steam pressure and the steam generator conditions, not necessarily what is going on down in the steam header. So the question is -- in most plants you do have different runs between where your steam generators are located and then your piping to where they are all headered together. So it could have been the assumption that is made in terms of what piping was selected, because I don't really care what is going on at the header.

My concern is what is going on in the steam generator and between the steam generator to where the safety valves are connected. What's the delta P between those two points? What happens down at the header is not really a big concern.

CHAIRMAN DENNING: Why don't you go find another curve that is more appropriate than on the pressure.

MR. HUEGEL: Well, the good plot I thought was on the pressurizer pressure where we did actually have a good comparison of what the plant was indicating in terms of a pressure versus what RETRAN was showing the pressure was.

CHAIRMAN DENNING: Yes.

MR. HUEGEL: Obviously, the peak pressure is one of the parameters of concern in the non-LOCA analysis that we do look at.

MR. SIEBER: Probably if you started your scale at zero, it would appear to have much greater correlation.

MR. HUEGEL: Yes. There's all different ways of manipulating the data. That would be one of them.

(Laughter.)

MR. SIEBER: And it is apparent.

CHAIRMAN DENNING: Is there anything else you were going to show us then?

MR. HUEGEL: If that is good enough --

CHAIRMAN DENNING: Yes, excellent.

MR. WALLIS: It is very interesting. It is, however, qualitative, isn't it? So we don't quite know how to look at its effect in some sense.

MR. HUEGEL: Well, I still feel very strongly that the methodology that we are using for performing the analysis is very conservative and does a good job of ensuring that the plant is safe.

If I look back, like I was talking about with the rod withdrawal at power, we analyze a whole wide range of cases and go all the way to the condition of trip. I know from my discussions with plants that they have problems just at normal operating conditions because of the noise in the channels and the hot legs, of having margin to the trip, and that is without any transient going out at all.

Yet, here I am running my transients and going up to power levels of 120-130 percent, which is where I have the trip set points because I have accounted for all the safety analysis uncertainties. In the case of an OTDT K-1, the uncertainty is on the order of 15 percent. So I've got my safety analysis that is showing I've got a nice, smooth plot of here's what TAV is doing. Yet, at the plant it is bouncing all around, and with the lead lag compensation, it is trying to compensate for the difference between indicated and actual conditions. I am running into problems trying to ensure the plants have adequate margin just for normal operating conditions.

Then if you go out, say, for example, a loss of loss in feedwater event, that is a heat-up event. Well, if you were to ask a plant when they have a loss in feedwater event, it is a problem in terms of maintaining shutdown margin because they get so much cooling because of the aux. feedwater.

Yet, we would assume a turbine-driven failure. We assume one of the two motor-driven has failed and is at a minimum condition. So that we would analyze it in safety space; it is heatup event long term. But if you look at the plan, it is a cool-down problem.

So I feel very comfortable that the methodology that we are applying in these different events is conservative and robust and ensures that the plants are operating in a safe manner.

CHAIRMAN DENNING: Thank you.

MR. HUEGEL: Thank you.

CHAIRMAN DENNING: Okay, let us now move into our wrapup.

MR. FINLEY: If we perhaps could summarize, Mark Flaherty would just give a conclusion from our side.

MR. WALLIS: Well, I have a question. I was just looking here at this solubility of borax versus temperature. Do you have also some sort of a curve of the boiling point versus the degree of borax dissolved in the concentration? Is there a boiling point elevation due to concentration as well, a curve like that you could give us to take away?

CHAIRMAN DENNING: Yes, also if you have density, too, because --

MR. WALLIS: Density, too, because all those things are related, yes.

CHAIRMAN DENNING: -- I had some trouble getting the density's function on concentration.

MR. WALLIS: If we want to look at BACCHUS with some intelligence, we need to have that sort of stuff.

MR. FINLEY: I'm not sure this is what you are looking for.

MR. WALLIS: That is solubility. I was looking for boiling point. Presumably, as you dissolve more borax, the point goes up, does it?

MR. FINLEY: I don't have the boron point.

MR. FINK: This is Dave Fink.

Mark, go back to that plot you just had up there.

MR. WALLIS: There is a boiling point. It says, "Boric acid solution boiling point, 218," but that must be at some concentration.

MR. FINK: That is at the atmospheric solubility limit, that is correct.

MR. WALLIS: That is at 30.

MR. FINK: Correct.

MR. WALLIS: So it hasn't changed very much then. I presume it is coming up from 212 to 218, as you have added up to 30 percent by weight.

MR. FINK: That is correct.

MR. WALLIS: So it hasn't changed that much. Okay, thank you.

MR. FLAHERTY: In conclusion, Constellation came back today really to discuss four topics. Two of them were bring-backs.

For the first one, dealing with alloy 600 material and PWSCC, we believe that we proved that it is not a concern with respect to uprate.

The other bring-back item dealt with the margin. Obviously, we have had lots of discussion about margin. I believe that what we attempted to show you today was that there's margin in many different aspects with how we do things. This includes inputs, assumptions of keeping RCS pressure at nominal value even though it increases, and not crediting that for DNB; looking at reactor trip at 1.4 seconds versus less than 1 second; doing some analysis at 102 percent power; looking at steam generator plugging from 0 to 10 percent, depending on which is worse case. So that is one aspect for inputs.

We just discussed again some of the code. The code has been benchmarked somewhat against real plant data.

There's also margin and safety analysis limits where we do assume penalties in looking at margin with that.

Finally, even the design limits, even though there's, for instance, a limit of 3200 pounds for RCS pressure from ASME code, that is just at the point at which you have an increased probability of causing additional damage. So there is additional margin even beyond that.

So, in sum, there's lots of different sources of margin within the analysis.

With respect to the two new topics we discussed today for small break LOCA and long-term cooling, we did demonstrate that we do have acceptable results. I would like to reiterate that the analyses that were done were very conservative from the standpoint of looking at things even from decay heat of 120 percent. This decay heat, that adds -- that affects the analysis in many ways with respect to what we believe would actually occur during a real event.

To put this in perspective somewhat, with the higher decay heat, you are going to have increased steaming and, therefore, increased pressure inside containment. So this will increase the need for containment spray.

But, in all honesty, if you look at just normal decay heat with reduced, relatively reduced steaming effects, so, therefore, containment pressure would be reduced; hence, containment spray by procedure would be looked to be terminated in an earlier standpoint, extending out the period of time in which operators would look to go on to recirculation.

So a lot of these conservative aspects, that type of thing, do have effects on the analysis. So even though there may still be some lingering questions or generic comments that the staff is dealing with the PWR owners' group and things like that, we believe that what was done for Ginna is more than sufficiently conservative enough to bound any of those potential issues.

So, with that, I would like to conclude Constellation's presentation.

CHAIRMAN DENNING: Good. Well, before you leave, let me say thank you for the presentations. You certainly addressed the issues that we asked to be addressed at the last meeting, and I think you have done that very well. I would like to congratulate the presenters and thank them.

We will be providing some guidance to you on the presentations for the upcoming meeting. Obviously, we have two hours of which we will have presentations that will be much more focused than we have had in our couple of days of reviews here. We will try to get that guidance to you by tomorrow as to what our expectations are, and also to the regulatory staff, of course.

There is some duplication, obviously, that occurs in these presentations. We will probably remove some of that duplication for the presentation to the full Committee.

You will also hear we will have some talk-arounds here before we are done. Perhaps you will get some additional guidance from the individual members of the Subcommittee before we are done today. Okay?

So we will have the wrapup by the regulatory staff now.

MR. MILANO: No, sir, we don't have anything else that we would like to put on the record and stuff. Just what we were going to wrap up you have just mentioned. We were going to ask about the guidance and when to expect it in preparation for the full Committee meeting.

CHAIRMAN DENNING: Good. Again, I think we will try to get that to you tomorrow.

I would like to thank the staff, too, because I think that we did get quite a bit of enlightenment on some of the things that have been bothering us at the previous meeting, and staff's analyses were very helpful in that. Thank you.

MR. MILANO: Thank you.

CHAIRMAN DENNING: Okay, then why don't we go around the table. Jack, do you have some comments?

MR. SIEBER: Not very many. We had some questions at our meeting last month, and I think both the licensee and the staff did an excellent job of providing the answers.

One of those questions about materials was mine. That was properly answered. I think that from my standpoint any concerns that I might have had trying to guess where alloy 600 was are no longer there because they aren't in critical places.

I thought the explanation of how safety calculations are done, I think Otto and I both have been through that a few times. On the other hand, I even learned a couple of new things in the process of the presentations myself, and I thought that was well done.

MR. WALLIS: What did you use? Did you use 1.38 or 1.55 or what did you use?

(Laughter.)

MR. SIEBER: 1.55.

MR. WALLIS: You used 1.55.

MR. SIEBER: You get to pick your own number.

(Laughter.)

MR. WALLIS: Otto?

MEMBER MAYNARD: I'm trying to remember

what it was. We actually took over our own safety analysis. Again, you go back -- the real number is what the design criteria is, and then, again, you pick a number that gives you design specification margin for your field design and how much you want to use for that and how much you want to be able to use in case you find something later you didn't know about versus where you want to put your set points in your plant and how do you really want to operate your plant.

So, again, it really goes back to making sure that you meet the design criteria, and then where you put the other depends on how much flexibility you want to give to your field designer versus how much flexibility you want to give to your operator.

I forget what the number was that we used at Wolf Creek, but it was below 1.55. I don't know if it was much above 1.38. But it was in that neighborhood.

MR. SIEBER: Those safety limits are like building a box. Once you build the box, that becomes the golden rule, so to speak, and you have to operate the plant inside that box.

CHAIRMAN DENNING: And you try to make your box as small as possible.

MR. SIEBER: No, you try to make your box as big as possible.

MEMBER MAYNARD: Not necessarily. What you want to do is to give, keep yourself the ability to handle unknown or unusual situations that may come up without having to do a re-analysis every time somebody wants to change something.

So, basically, you set a box for a field designer and you set a box for other parts of the design. If you find out later that that wasn't a big enough box for your field designer, then you go to another box and you can move that around.

If you set your limit right down at the design criteria, you have no flexibility to deal with it. I think it actually creates a less safe situation.

So you actually want to have that for a couple of reasons, not just safety operation, but operational flexibility, and, again, to be able to handle any of the unknown.

MR. WALLIS: Of course, we had this conversation earlier. I can understand all that from the point of view of operation, but there isn't a measure of how much additional safety the public is getting out of this. That is what is missing. There is no link here.

MEMBER MAYNARD: The safety to the public is built into what the design criteria is in the regulations and the methodologies that are approved, not only the methodology, not only the codes, but also the way the codes have to be used, the restrictions on the application of that code.

As you have seen from a lot of these discussions, there's a lot of conservatism built into the code and into how the code has to be used and what assumptions are put into that.

That conservatism, plus the conservatism built in what the design criteria is, that is the public's safety margin. The rest of that then becomes the licensee's margin for how they want to operate.

Again, it provides the safety margin in case something comes up you really had not expected or didn't know about. You are still above your design limit.

MR. SIEBER: If you wanted to know what the margin meant in terms of safety, you would have to do it with distributions, probabilistic distributions, which deterministic rules don't really lend themselves to. So, generally, if you meet deterministic rules, you are safe enough. That is basically the way you would interpret Title 10.

MEMBER MAYNARD: And, actually, I think that you are extremely safe because it is very conservative. I think if we went to a more detailed analysis where you really tried to predict where it was, put uncertainties and stuff on it, I think that you could find that you could actually uprate these plants to a higher power. There's a lot more conservatism than what you know about.

You may find in some areas occasionally that you didn't have as much conservatism as you thought, but in the aggregate you take all the conservatisms built into all of the bounding type analyses and there's more margin there than what shows.

CHAIRMAN DENNING: Graham, anything else?

MR. WALLIS: Well, I am much more satisfied than I was before in several areas. I was not quite sure what was going on when you got these numbers and where they came from and why they were so close to limits, and so on. I think I understand much better how they were derived and why they have the form they do have.

I am much more satisfied that the licensee and Westinghouse have performed a thorough analysis. I think some of the details we saw today a lot let me know what was really behind it all that we hadn't seen before and you never get from reading the SER.

(Laughter.)

Similarly, the staff came through with explanations which are not in the SER. They are also behind the words which tend to just say the applicant did this and it's okay, which leaves completely up in the air, how did you know that?

So I feel much more satisfied today. I suppose after I have slept and dreamt a bit I might come back with another question, but I don't at the moment have a question. I am pretty satisfied. So thank you.

CHAIRMAN DENNING: Tom?

MEMBER KRESS: Well, I felt that the staff and the applicant have shown that they meet all the regulations, the rules. I didn't see any place that I thought there was glitch or a hangup. In fact, they did a good job of showing it.

I thought their analysis of the boron precipitation was highly conservative. I think they could show that they've really got a lot more time than a couple of hours. In that large break LOCA with this upper plenum injection, I really don't think that you have any boron concentrate.

CHAIRMAN DENNING: No, I don't either.

Yes, Otto?

MEMBER MAYNARD: I think the licensee has done a real good job in answering questions, which I think many went well beyond what the licensee would be required to have to answer, because our questions to the licensee and to the staff were really challenging or questioning approved methodologies, which I think is fair game, but the licensee I think did a good job of providing answers and responding, and has been responsive to our questions.

Again, I agree with Tom, I think they clearly demonstrate that they meet the regulatory requirements and that they have performed the analysis and meet all the requirements there.

I also think the staff has done a good job of demonstrating that they understand the applicant's information, that they understand the analysis. They have done some confirmatory work. So I think they have done a good job in demonstrating that they independently took a look at a number of these things and satisfied themselves that the licensee's information was accurate and representative there. So I think they have done a good.

So, overall, I think both did good.

CHAIRMAN DENNING: Very good.

Unless anybody else quickly objects, then I declare this over.

(Whereupon, at 2:09 p.m., the proceedings in the above-entitled matter were concluded.)