# Official Transcript of Proceedings

# NUCLEAR REGULATORY COMMISSION

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

MEETING OF THE SUBCOMMITTEE ON

THERMAL-HYDRAULIC PHENOMENA

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WEDNESDAY,

MARCH 19, 2003

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The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Graham Wallis, Chairman, presiding.

PRESENT:

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| GRAHAM B. WALLIS | Chairman |
| SANJOY BANERJEE | ACRS Consultant |
| THOMAS S. KRESS | ACRS |
| DANA A. POWERS | ACRS |
| VICTOR H. RANSOM | ACRS Member |
| JOHN D. SIEBER | ACRS |
| MICHAEL R. SNODDERLY | ACRS Staff |

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8:34 a.m.

CHAIRMAN WALLIS: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Thermal-Hydraulic Phenomena. I am Graham Wallis, Chairman of the Subcommittee. Subcommittee members in attendance are Tom Kress, Victor Ransom, and Jack Sieber, as well as our contractor Sanjoy Banerjee.

The purpose of this meeting is to discuss thermal-hydraulic issues associated with design certification of the AP1000 reactor design. The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee. Medhat El-Zeftway is the designated federal official and Mike Snodderly is the cognizant ACRS staff engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the motives of this meeting previously published in the Federal Register on March 5, 2003. 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 received no other written comments or request for time to make oral statements from members of the public regarding today's meeting.

This is the second in a series of meetings to support a future full committee meeting on the staff's draft safety evaluation report on the AP1000. The first meeting was to review the AP1000 PRA.

Before we get started, I would like to state that what I hope to see happen at this meeting is the focus on technical issues which may need resolution and understanding. Not a lot of other material.

In particular, I would like to see how the various formerly correlations and so on that have been pulled out of the literature and applied to this system, what the evidence is that they actually apply because we all know in two phase flow you can pull something from one area and try to use it in another and it may be that the geometry and the conditions are so different that you have to validate it very carefully and that's what I would like to see happen.

We will now proceed with the meeting. I

call upon Mr. John Segala of the Office for Nuclear Reactor Regulation to begin.

DR. SEGALA: Thank you. Can you all hear me okay?

CHAIRMAN WALLIS: I think it's most important that the transcriber hear you.

DR. SEGALA: I'm John Segala. I'm a new project manager for the AP1000 design certification review. Larry Burkhart, who was the previous PM, has left NRC to go work for the State Department. We now have a team of project managers to handle the design certification review to get our draft safety evaluation report out.

I'm going to discuss a little bit about the background. You are all probably very familiar with that, as well as a summary of the preapplication review. I'll talk about -- give a brief overview of what transpired during that review. A discussion or summary of where we are in the design certification review.

I'll talk a little bit about the status of the application issues that were identified during the preapplication review. And discuss a little bit about some follow-on issues. The way I define follow-on issues are issues that weren't identified during the preapplication review that could possibly be an open item in the DSCR report, the Draft Safety Evaluation Report.

As you are aware, the AP600 was certified in December of '99. Westinghouse expressed interest in applying for the AP1000 design certification using much of the AP600 design. Westinghouse and NRC agreed on a three-phased approach. The first two phases were during the preapplication review. The preapplication review is completed.

Phase I is the scoping review where we identified key review issues. Phase II we focused on four issues, acceptability of the DACR, design acceptance criteria, the acceptability of certain exemptions, and the applicability of the AP600 analysis codes and test program to the AP1000. We are currently in Phase III which is the design certification review and I'll discuss a little bit of that.

In terms of an overview of the preapplication review, I just wanted to highlight some key meetings that we had. We briefed the ACRS on Phase II. There was a joint future plant design in the Thermal-Hydraulic Phenomena Subcommittee in February.

We had a full committee meeting the beginning of March. Based on the full committee meeting the ACRS issued a letter to the NRC on March 14th and they agreed in general with the staff's conclusions regarding the preapplication review.

Following that the NRC issued a letter to Westinghouse on March 25th where we reviewed the analysis codes and test programs for the AP600 and determined in general that they applied to the AP1000. However, we identified some exceptions to that which was the six issues that were brought out in that letter. I'll briefly discuss the status of those issues in a couple more slides.

Before I get to that, I just wanted to go over the summary of design certification. Westinghouse submitted their design certification application in March of 2002. The NRC staff reviewed and issued 714 RAIs. Westinghouse responded to the RAIs by December 2nd.

In the 714 we issued recently -- that includes the five additional ones we issued just recently. NRC staff reviewed the Westinghouse responses and we provided comments to Westinghouse.

CHAIRMAN WALLIS: I have a comment on these RAIs. We got hundreds of RAIs. If you look through them, some of them look very minor and some look very serious. It would be useful if you had green RAIs and white and red and orange or something, or some classification so we could say these ones are important and these ones really are very minor. Other ones you have to resolve. Otherwise, there is some real safety issue.

DR. SEGALA: I think those would be the ones that would be open in the Draft Safety Evaluation Report but that doesn't necessarily help you doing your review. I have a slide coming up that gives you an overview of the RAIs, not necessarily the thermal-hydraulic but the whole picture.

Following the conference calls and meetings, Westinghouse issued revised responses and as of February 28th we sent a letter to Westinghouse identifying 188 unresolved RAIs which we are working with Westinghouse to try to provide our comments on those so Westinghouse can provide responses.

I just wanted to point out that the staff has not finished their review and are still in the process of doing reviews so we haven't made any final conclusions yet on the acceptability of the AP1000 design certification.

This is the overview slide. Some key

things is you could look at reactor systems as 187 RAIs and PRA has 99 RAIs. You focus in on where was the staff asking most of their questions. In the reactor systems arena we had 48 that were dealt with the analysis codes and test program and about 48 that dealt with the Chapter 15 analysis.

Getting back to the preapplication issues that were identified, and I'm just going to give you a little status of those issues. We had the liquid entrainment in the upper plenum or hot leg during ADS-4 actuation. This is one of our more significant issues.

Following the preapplication review Westinghouse submitted WCAP 15833. The staff reviewed that and we issued 48 RAIs on that. Fourteen of those came from NRR and 34 came from research. A lot of discussions and conference calls and RAI responses.

We have about 6 RAIs that are unresolved in the sense that they may become open items in the DSER. We just issued yesterday a letter to Westinghouse requesting new test data to support justification of the modeling of the entrainment process during a small break loca.

CHAIRMAN WALLIS: Does this include the level swell?

DR. SEGALA: I think so, yeah.

CHAIRMAN WALLIS: Because these are all contributors to carrying liquid out of the vessel.

DR. SEGALA: Yes.

CHAIRMAN WALLIS: And you have to get the level swell right as well as the entrainment presumably. They affect each other. It swells more it gets into the hot leg and can be entrained.

DR. SEGALA: Tomorrow we're going to have Steve Bajorek from research. He's going to go into this issue in a lot of detail.

CHAIRMAN WALLIS: We're not going to discuss this one today?

DR. SEGALA: No.

MR. CORLETTI: This is Mike Corletti from Westinghouse. Our presentation this afternoon will be dealing with the entrainment issue.

CHAIRMAN WALLIS: So you will be doing it?

MR. CORLETTI: Yes, this afternoon. I think Dr. Bajorek will be speaking to it tomorrow. It's the major focus of this meeting, I think.

DR. SEGALA: The next issue, potential steam voids in the RCS following main steamline break. Initially in the preapplication phase Westinghouse didn't provide a main streamline break analysis.

They provided that in the DCD or their design certification document. The staff issued an RAI on the ability of LOFTRAN to evaluate steam voids. Westinghouse provided a response to that and the analysis showed that there were no steam voids. Walt Jensen is going to discuss this issue this afternoon.

The nonconservative boiling heat transfer correlation and NOTRUMP at high heat fluxes in the passive RHR heat exchanger. The staff issued an RAI on this. Westinghouse provided a response taking a 50 percent reduction in the passive RHR heat exchange or heat transfer area. Based on that, this issue is also considered resolved and Walt Jensen will discuss this in more detail as well.

The potential boron precipitation in the vessel during long term cooling. The staff issued an RAI on this. Westinghouse provided a response and the staff needed more additional information. I believe that Westinghouse has responded to this item. The staff has not had a chance to review that yet due to the timing.

Concern for core uncovery during small-break LOCA and performing complete break spectrum. The staff issued an RAI on this. Westinghouse responded and additional break sizes were analyzed and no core uncovery was -- it was shown that no core uncovery had happened. This one is considered resolved.

Except for the first one all the way down to this one are going to be discussed by Walt Jensen this afternoon.

MEMBER RANSOM: Can I ask a quick question? Why is boron deposition in the vessel of concern?

DR. SEGALA: I think --

DR. LOIS: This is Lambrose Lois of the Apple Systems Branch. There is so much water in the vessel. When the long term cooling phase initiates, the potential is that only steam can exit the ADS-4 so, therefore, the boron keeps concentrating. If you assume that the water is cycled only once, then you have enormous amount of concentration which will solidify and block the circulation of water.

MEMBER RANSOM: It seems like if you have solid boron in the core, it must mean you have a saturated mixture.

DR. LOIS: You do. The theoretical maximum is about 60,000 ppm in the water and 35,000 is the precipitation limit for those temperatures.

MEMBER RANSOM: At maximum concentration

why would it matter? I'm not sure I understand why this is so significant.

DR. LOIS: Because when it exceeds 35,000 ppm the remaining will precipitate.

MEMBER RANSOM: Yeah, but to me that would say you still have the maximum concentration in the liquid and so the --

DR. LOIS: But the amount that precipitates will block the circulation.

CHAIRMAN WALLIS: It will block circulation of the circuit. That's what we're worried about.

MEMBER RANSOM: Oh, that's what you're worried about. I see.

DR. SEGALA: The last item, use of the approved WGOTHIC containment evaluation model to address large scale test shortcomings. We didn't issue any RAIs on this. This was addressed in the design certification document. Westinghouse developed a conservative model and the staff finds this acceptable. Ed Throm is going to discuss this this afternoon.

CHAIRMAN WALLIS: Is Westinghouse going to discuss that?

DR. SEGALA: I think so, yes. The follow-on issues, again, are those issues that weren't identified during the preapplication phase. We issued about 48 RAIs related to Chapter 15 analysis, both LOCA and non-LOCA. This is just a sampling of items. Westinghouse responded satisfactorily to all except for these few.

The feedwater line break analysis to identify limiting case. Is the double-ended rupture a limiting break for the feedwater line break event. Tech spec required flow to support adequate flow mixing in the RCS. The safety analysis assumes RCS dilution, volume, well mixed during the boron delusion event, and is the tech spec minimum flow adequate for the well mixing assumption.

The ATWS analysis to identify the limiting case Westinghouse needs to perform analysis of all applicable non-LOCA transients to identify the limiting ATWS case.

All these issues the staff feels that when they review Westinghouse's responses, they think that these will probably be acceptable and won't be open items. We just wanted to give you a feel for some other areas that we were looking at beyond the preapplication phase.

CHAIRMAN WALLIS: The last one could be

important, the last bullet. What happened was that Westinghouse did some ATWS analysis and it was not extensive enough. Is that it?

DR. SEGALA: I think so. Summer?

DR. SUN: This is Summer Sun and I'm a reactor system grange. ATWS analysis as presented in DCDs based on a limited case which is loss of normal feedwater. The basis for selecting limited cases is based on AP600 sensitivity study and, based on that, identify that loss of normal feedwater.

The staff asked them to extend their sensitivity study for AP1000 and it confirmed that the loss of normal feedwater is still limited and we are still waiting for the Westinghouse response on this RAI.

CHAIRMAN WALLIS: Thank you.

DR. SEGALA: Okay. That concludes my discussion this morning. The last slide I'm going to discuss tomorrow afternoon. That will be sort of the concluding summary remarks of where we plan to go in the future.

CHAIRMAN WALLIS: Thank you very much.

Mike, it looks as if you're on next.

MR. CORLETTI: Yes.

CHAIRMAN WALLIS: You're keeping the best for last? You're not going to talk about liquid entrainment?

MR. CORLETTI: We are keeping the best for last, or else we would probably never get through the easy ones, I think.

CHAIRMAN WALLIS: That's a photograph of a real AP1000?

MEMBER SIEBER: Yes.

MR. CORLETTI: Good morning. It's a pleasure to be here today. We're going to be talking -- seeing John's presentation there's a lot of similarities in the slides that I've prepared. I will go through them but I think maybe I'll just try to put where we see the issue.

CHAIRMAN WALLIS: Move right into the tentacle issues.

MR. CORLETTI: I think these objectives are pretty much in mind with what you're looking for. If you will, just let me go over where we see our scheduled objectives. We have provided our DCD application and we've gone back and forth on these RAIs.

We are now going through our RAI responses that the staff found they would like additional information. We're in that process of trying to revise our RAIs and provide supplemental information.

We are trying to do that this month time frame to support the staff doing the -- issuing the DSER in June. Our goal is really to address all the open items in this DSER to the extent that we can and to have as few of those going out of the DSER as we can.

CHAIRMAN WALLIS: That looks a little tight to me. I mean, if NRC is going to issue this on 6/16, and they will probably be late, then we have to read it and analyze it and then beginning of July we have to write a letter. Is this realistic?

DR. SEGALA: This is John Segala with NRC. I think we believe this is an aggressive schedule but we are putting as much resources towards it to try to achieve that.

CHAIRMAN WALLIS: If you could do it in 5/16, or even 6/1. Give us some time to study this. We want to avoid having to study it a week before we have to make a decision.

DR. SEGALA: I think the draft sections can be made available to the ACRS at an earlier time.

CHAIRMAN WALLIS: Some of us have vacations in June.

MEMBER SIEBER: We don't have a meeting in

August.

CHAIRMAN WALLIS: Maybe that's all planned ahead of time. If there isn't enough time, then we won't be able to write the letter until August.

MEMBER SIEBER: There is no meeting in August.

CHAIRMAN WALLIS: There is no meeting in August. It would be September.

MEMBER KRESS: Or probably July.

CHAIRMAN WALLIS: It says July here but if they don't give us time, if the DSER comes too late, we won't be ready to write a letter in June. That's what I'm saying.

MEMBER KRESS: I see.

MR. CORLETTI: I think it's a good point and we'll see what we can do to facilitate that.

Okay. This was useful then putting up this schedule slide I think.

CHAIRMAN WALLIS: Yes.

MR. CORLETTI: Some of the future meetings, as we said, we had a PRA Subcommittee. This is the Thermal-Hydraulic Subcommittee. We're talking about an AP1000 subcommittee meeting or meetings. I guess we are still working on that.

These are some of the additional issues

that have been identified. I think we should come back to this at the end of the two days here to see are there additional items that we want to -- that comes out of this committee that we would want to discuss in these future meetings.

CHAIRMAN WALLIS: The last one is interesting, the man-machine interface. I'm not sure how we'll resolve it.

MR. CORLETTI: For design certification essentially it's really not resolved under design certification. AP1000 similar to AP600 in the other certified designs approved this as a future design acceptance criteria.

CHAIRMAN WALLIS: Are you using fewer operators than the existing reactor?

MEMBER KRESS: I think that was one of our major issues, was how many operators that you're talking about.

MR. CORLETTI: We are not using fewer operators than what is allowed by the regulations, although we have goals. We design it with those sort of objectives. As far as our licensing commitments, it is not.

MEMBER KRESS: That wouldn't be an issue then in that case.

DR. CUMMINS: This is Ed Cummins. The utility requirements document for passive plants said that the passive plants should be able to be operated by a single operator and a single supervisor. We support that what is in the design certification is a process to determine the required number of operators, not a determination of the number of operators. It's really a deferral of a process to the COL stage.

MR. CORLETTI: Okay. John went over this. I think the part I would add will be that in this phased approach for license, the emphasis of the precertification review really was the applicability of the tests that were performed for AP600 to AP1000. Were those tests suitable for stealing purposes to be sufficient for AP1000 licensing.

Following that, were the safety analysis codes that were validated to those tests also applicable to AP1000 design certification. We wanted to address that early because we see that as a -- it can be a significant issue and can delay the overall schedule so we wanted to have some certainty going into licensing AP1000 that we were on solid foundation there.

I think the results of that generally were yes, the tests were applicable. We did significant scaling studies. We're going to talk a little bit more about those this afternoon. We did scaling studies of the AP600 test and showed how they were applicable, or not, to AP1000. Identified that most of them were applicable. The one issue that did come out of that was the liquid entrainment issue.

MEMBER KRESS: Are you going to do any more on supporting the range of pie groups that designates applicability?

MR. CORLETTI: We at Westinghouse have not done anything more on that. I think that was identified in the --

MEMBER KRESS: It was a comment to the staff.

MR. CORLETTI: Yeah, from the ACRS letter on the pre-cert, the pre-certification review. I think it was a comment really not for AP1000.

MEMBER KRESS: It was for the staff to get ready for all future scaling type events. You're right. I remember.

CHAIRMAN WALLIS: The assumption seemed to be that if the pipe group was within a factor of 2 everything was fine. This seemed to be an article of faith that a factor of 2 is okay but there is no real evidence that 2 is better than --

MR. CORLETTI: Or 200. Right. What was the right number. It was the accepted practice and we continue to use it on 600.

CHAIRMAN WALLIS: It was not rebuffed. I don't know if it's accepted practice. You made the argument, I think, and then it wasn't challenged. Isn't that more description than to say it was accepted practice?

MR. CORLETTI: Maybe.

DR. CUMMINS: Ed Cummins. In the certification I believe the NRC and their consultants used a factor of 3 and we got certification so we sort of felt that there was some informal acceptance of a factor of 2. Though I think we understand your comments that the justification of that was not really provided.

CHAIRMAN WALLIS: I'd be in real trouble if I were evaluating the flight of a golf ball and I said the Reynolds number was six times 10 to the fifth and it actually turned out to be 12 time 10 to the fifth, I would have a complete different answer for sure.

MEMBER KRESS: Different phenomenon.

CHAIRMAN WALLIS: Different phenomenon. That's a simple case. Golf presumably is a simple thing.

MR. CORLETTI: You've never seen me hit a golf ball.

MEMBER RANSOM: Well, it seems to me there are two issues actually and the similarity argument that you don't often hear but one is qualitative similarity meaning the same phenomenon are basically there. That generally is this rule from, say, a half to two. You are relatively assured that the same phenomenon are governing.

Then the other aspect is quantitative similarity which means you must have some measure of just how close it really is. It seems like the argument here is stuck back on the qualitative similarity. I don't know that they have really answered this question of how quantitatively similar are the events.

MEMBER KRESS: The other issue is a lot of times the phenomena is not governed by a single pie group. It may be the composite of them and each one of them -- if each one of them is on the low side you're not sure how to add them up, how each contributes to the phenomenon.

MR. CORLETTI: The key factor in the integral system performance, he had all these competing phenomena and you try to design your facility that every one of them to be one. It's impossible to get them all at one.

DR. BANERJEE: Are you going to visit this issue of pie groups at all in this presentation?

MR. CORLETTI: No, not really.

DR. BANERJEE: So we're going to take it as a given that this sort of analysis was okay?

MR. CORLETTI: Yeah. I think this committee reviewed that analysis as did the NRC under the precertification review. We had not planned on reopening that issue of the scaling. We are going to focus it on the entrainment. We will talk about some scaling aspects of entrainment.

MEMBER KRESS: Sanjoy, our thinking on that was that the ECCS provisions in AP1000 are so robust that you almost always keep the core covered. The calculation for that using the codes does depend on this pie groups. But the experiments that they relied on also showed that you almost always kept it covered. There's just no way to uncover the core. We intuitively thought that the process was acceptable based on that.

CHAIRMAN WALLIS: What was the pie group you used for enjoy intuition?

DR. BANERJEE: There will be some pie groups then that will come up in the entrainment studies. Right?

MR. CORLETTI: Yes.

DR. BANERJEE: So we can discuss it at that point because I guess that is the most critical issue on core level.

MR. CORLETTI: Not really the most critical issue on core level but it is the last remaining issue that we're discussing. I think there's -- we feel that it's not the most critical issue.

DR. BANERJEE: If I remember the AP600 top-down scaling, it really ended up being a group which dominated that determined outflow from the ADS-4 system and the friction in the line leading in. There was sort of a balance. If you didn't get enough, or got too much outflow, you couldn't get the flow in because of the friction in the line. I'm just thinking back now. This was like five or six years ago.

MR. CORLETTI: I think we're going to have a very detailed discussion of the phenomena involved in the IRWST injection phase and the ADS-4 phase. I think we're going to be able to adequately address --

CHAIRMAN WALLIS: We'll ask these questions when you get to that point.

MR. SNODDERLY: Chairman Wallace, if I could make a suggestion. This is Mike Snodderly. I would like to -- maybe we could ask John Segala and the staff if tomorrow maybe if Steve Bajorek could give us an update of how the staff plans to respond to the ACRS's letter because we did ask specifically how they were going to consider modeling of pie groups in the future and maybe they can give us the status of that and that may be the more appropriate time if Steve Bajorek briefs us tomorrow morning. John, would that be possible? You can get back to us later on that.

DR. BAJOREK: Yeah. Dr. Kress, what I'm planning to do, yes, in tomorrow's meeting where we have the point where RES is going to talk about its future actions, I have a few overheads where I would like to talk about the scaling and how we want to try to address this issue of .5 to 2.0. I'm not sure it's going to resolve the issue but I want to present out thoughts on it and some of the things that we might want to do in that area.

MEMBER KRESS: Thank you.

MR. CORLETTI: Okay. This slide is just really telling you what was included in our application. Our DCD application includes what traditionally is called the Final Safety Analysis Report for an Operating Plan, or Standard Safety Analysis Report.

Also included the complete PRA, the plant specific PRA for AP1000 including the technical specifications for the plant. I have 20 topical reports. I think our number's above that. I didn't update that. You have probably been seeing the flow of topical reports across your desk.

CHAIRMAN WALLIS: Are you giving the staff your codes to run?

MR. CORLETTI: In the pre-certification review we had this issue and we have agreed that the codes that were approved for AP600 and AP1000, we didn't see the need to do that at that time. We did say that additional codes that we would develop --

CHAIRMAN WALLIS: I thought there was a new negotiation which occurred between you and the NRC.

MR. CORLETTI: That is right. What we said is new codes that we developed for AP1000, new applications, we would make those available.

CHAIRMAN WALLIS: Do you get on the source code?

MR. CORLETTI: Yes. We have not done it under this review, Dr. Wallis, but Westinghouse would make it available.

CHAIRMAN WALLIS: You're going through here sort of the things that you've done and given the staff so I just wanted to know --

MR. CORLETTI: The things we didn't do? Yes.

CHAIRMAN WALLIS: How much in the way of codes you gave the staff or intend to give the staff. Ideally I think our position is you should make everything open and they should be able to run your codes. Have you reached that point yet?

MR. CORLETTI: I think fundamentally we don't have a problem with that.

CHAIRMAN WALLIS: You don't?

MR. CORLETTI: I think the issue really was --

CHAIRMAN WALLIS: It's just that the legal people have a problem with it?

MR. CORLETTI: No. No. I think so. I think the issue is that under this review we had already approved the codes so we weren't reopening that code review and we had completed that under the precertification review. Any new codes that we would develop for this application we would share with the staff and for them to run that. I think that is consistent with what other vendors were doing as well.

CHAIRMAN WALLIS: I guess what I'm getting at is sort of the issue of public confidences reinforce tremendously if they can run your codes and get the same answers that you get.

MR. CORLETTI: Yes. Public confidence is also instilled when they can get the same answers with very independent codes.

CHAIRMAN WALLIS: That is also true.

MR. CORLETTI: Which they have done as part of this review. I think --

CHAIRMAN WALLIS: I guess we will ask the staff about that when we get to them.

MR. CORLETTI: There is also an ACRS letter on the pre-certification review. I think in general you endorse the findings in the precertification review from the staff in Westinghouse's contention.

This just really summarizes our position on the codes in coming out of the pre-certification review. Basically we agreed the AP1000 introduces no new phenomena compared to AP600. The separate effects and integral test were acceptably scaled.

The issue of upper-plenum entrainment, and we're going to talk a lot more about this today, we do believe that while it was interesting, we believe that it was a local effect that was somewhat self-limiting but I think we're going to talk in detail further about that.

We believe that additional code validation, or additional testing, was not required. We thought we would be able to resolve this issue by sensitivity calculations and analysis and we're going to be talking about that.

MEMBER KRESS: Does this apply to WGOTHIC also?

MR. CORLETTI: No. This was really the COBRA/TRAC, Westinghouse COBRA/TRAC sensitivity studies that we did.

DR. CARUSO: Dr. Wallis, this is Ralph Caruso from the staff. I would like to correct maybe misimpression that may have been left by the previous discussion about staff access to the Westinghouse computer codes. The staff does not have any Westinghouse computer codes in house at this point. None of them. And has not as far as I'm aware.

MEMBER KRESS: Have you requested them?

DR. CARUSO: They have been requested and they have not been provided to us. There is currently under negotiation an agreement with Westinghouse for them to provide the codes to us but that agreement has not yet been finalized.

CHAIRMAN WALLIS: So they will not be available by the time you're making the decisions.

MR. CORLETTI: Dr. Wallis, Ralph, forgive me for interrupting. They were not requested as part of the AP1000 design certification review by the staff.

DR. CARUSO: I would disagree, Dr. Corletti, because they were requested.

MR. CORLETTI: Mr. Corletti.

DR. CARUSO: Mr. Corletti. Excuse me. They were requested by the staff but because of management decisions, that request was not followed through on.

MEMBER KRESS: There was no official request then.

DR. CARUSO: There was a request made but it was not followed through.

MEMBER KRESS: Okay.

CHAIRMAN WALLIS: There was a letter sent?

MR. CORLETTI: This is during the precertification review we discussed this and that was the request.

CHAIRMAN WALLIS: I knew that for some time there's been misnegotiation going on. I thought it had been resolved. It's still being done?

MR. CORLETTI: I believe it has been signed by RCEO. I thought we had actually signed it.

DR. CARUSO: The agreement has not been finalized

CHAIRMAN WALLIS: Okay.

DR. CUMMINS: This is Ed Cummins. In the verification review we thought the codes were already approved in AP600 and we believe that position was endorsed. In the circumstance where the codes are already approved, then we argued that it was not necessary for the staff to re-review or reapprove the codes for application of AP1000.

CHAIRMAN WALLIS: It's not necessary for you to hide your codes. There should be no reason why it can't be open. That's the thing. I mean, the fact that you can argue that you have enough basis is no reason to -- there must be some other reason involved in order to not supply code. Presumably the only argument you have there is some kind of commercial value to the code.

DR. CUMMINS: Right. I think the Westinghouse company and the staff are coming to agreement on that.

CHAIRMAN WALLIS: There is no safety issue which is helped by your not providing the code. It can only do good to provide the code to the staff in terms of public safety. There's no way I can see that public safety is enhanced by you not providing a code. If you have a good argument, then please make it, but I don't think there's anyway the public safety is enhanced. It has to be commercial safety or Westinghouse or something that's at stake.

DR. CUMMINS: -- Westinghouse is being resolved independently of this review by Westinghouse and the staff. I think it is essentially resolved. In the public management sense Westinghouse paid for the review and we don't think --

CHAIRMAN WALLIS: Okay. Should we move on now?

MEMBER KRESS: We didn't view our request as a new review of these codes. It was a new use of them actually where we were that staff could actually exercise them and look at them. We didn't intend for it to be a full review and reapproval.

CHAIRMAN WALLIS: Well, the rescue phase -- we've got to move out of this, but when you do supply the codes, then the staff does run them. If something gets revealed then, then it might come back to haunt you.

MR. CORLETTI: It was not an issue of --

CHAIRMAN WALLIS: Okay.

MR. CORLETTI: This was from the letter, and I think we talked about this quite a bit. This was from the NRC's letter on the pre-certification review. Really talking that the separate effects interval test programs are appropriate for use in support of the analysis. The analysis codes validated for the design could be extended to that of the AP1000. This is from the staff letter from the pre-certification review.

The plant response during ADS-4 operation was raised. This is essentially the issue on the treatment of upper plenum and hot leg entrainment. That issue needed to be dealt with during the design certification.

MEMBER KRESS: There was a question about the NOTRUMP momentum model or non-momentum model and how you dealt with that.

MR. CORLETTI: Right. We are going to

have -- I have a slide on that, but also in our NOTRUMP presentation later this morning we are going to address that as well.

MEMBER RANSOM: Maybe you could give me an answer to a quick question.

MR. CORLETTI: Sure.

MEMBER RANSOM: I wonder why do you have to have this NOTRUMP code when you've got Westinghouse COBRA/TRAC which is presumably more sophisticated. It seems like it just makes issues.

MR. CORLETTI: The NOTRUMP code is our license to approve small-break LOCA. The COBRA/TRAC code that we applied, the supplemental calculation of a COBRA/TRAC code really was a focused calculation at the lower pressure phase of ADR-4 IRWST injection. The COBRA/TRAC code has not been validated over that entire range of the condition for small break.

This is a summary of the WCAP 15833 which was our attempt at resolving the entrainment issue. We are going to have about two hours of presentation this afternoon. I believe that's the one you have there.

CHAIRMAN WALLIS: That's this one here?

MR. CORLETTI: Yes, sir.

CHAIRMAN WALLIS: That seems to me to be

a lot of -- what I was saying when I started this meeting, a lot of stuff brought out of the literature, but I didn't see much in the way of validation of anything. I didn't see comparisons with data in here.

MR. CORLETTI: There actually is a section where we compared the predictions of the COBRA/TRAC calculation to the tests performed.

CHAIRMAN WALLIS: Maybe you could point those out later on today or tomorrow.

MR. CORLETTI: Okay. And we did a series of sensitivity studies really aimed at trying to range the -- see the effects on increasing the magnitude of entrainment, both hot leg and upper plenum entrainment. Really what we're trying to see can we see a sensitivity to the overall plant performance? I think in our conclusions we were not able to see appreciable difference in overall plant performance.

CHAIRMAN WALLIS: So what you're going to do is show us that even if you don't have a very good model, let's take some extremes of a lot of entrainment or not much and it doesn't make much difference because the level drops to some point and doesn't go any further. Is that what you're going to show us?

MR. CORLETTI: Essentially. I think

another aspects of that is the models in COBRA/TRAC have very high -- already COBRA/TRAC had very high entrainment rates with the correlations that they have. We're going to talk about the basis for those models in COBRA/TRAC that we use and the analytical basis for that this afternoon.

CHAIRMAN WALLIS: If I recall, you did a sensitivity around the Kataoka-Ishii model. Right?

MR. CORLETTI: Yes.

CHAIRMAN WALLIS: And you're going to talk about how well that model applies?

MR. CORLETTI: Yes. We're going to talk about that model and the models that are actually in COBRA/TRAC and compare them.

CHAIRMAN WALLIS: And you have some comparisons with data to show those models are applicable?

MR. CORLETTI: We have some comparisons with data. The staff has not found those comparisons, I believe, to be sufficient. We will discuss --

CHAIRMAN WALLIS: Just comparing COBRA/TRAC to Kataoka-Ishii is not the same thing as validating it against data.

MR. CORLETTI: Yes.

DR. BANERJEE: Also, there was a concern, if I recall, with the AP600 that the OSU facility was probably the least well-scaled, particularly with regard to poor height. At least in the top-down scaling study that was done, there was concern about that. Have you compared this data, say, something like Rosa 4?

MR. CORLETTI: I don't think we agree that it was the least scaled facility. I think for high pressure phase of the transient the SPES facility in Italy was the best scaled probably. The lower pressure phase of the transient OSU was probably the best scale. We did in the pre-certification review in our scaling report we showed comparisons to SPES, OSU, and Rosa as well.

DR. BANERJEE: I was involved with Idaho study and our conclusion -- this is memory, which might be wrong, was that SPES had a problem with heat loss and it was, we thought, not all that typical. APEX and the OSU facility there was this problem with height that we were concerned about. The facility that was closest to being well scaled was the Rosa facility.

MR. CORLETTI: Was this a public NUREG that you did or was this something that you did for the --

DR. BANERJEE: I think so.

MR. CORLETTI: Okay.

DR. BAJOREK: No. This was a NUREG that you had done for the AP600.

DR. BANERJEE: AP600. Yes.

DR. BAJOREK: I think our conclusions when we did the independent scaling for AP1000 were more or less consistent with that. We think that SPES was okay for the high pressure periods leading up to ADS-4, venting. APEX was good for the long-term cooling in the period following that. Rosa may have been the best overall, but SPES was a little bit better for the early periods, APEX for the late periods.

DR. BANERJEE: I agree, Steve, but the issue of the lowest core level when that was reached, Rosa was the best scaled. That also was published as a paper in Nuclear Engineering and Design and given as a keynote paper at Nuretz which was held at Kyoto so it's public information.

MR. CORLETTI: In our scaling report that we did for AP1000 we did compare, I believe, some of the key pie groups of Rosa to AP1000 as well from the Rosa facility. We had independent scaling done by actually INEL. They had done the scaling before for AP600.

They went and did the scaling for AP1000 and provided that in our scaling report. I think it showed Rosa was adequately scaled for AP1000 when you look at the pie groups. So what we confirmed is that the confirmatory conclusions that the staff was able to get from Rosa could also be applied to AP1000.

DR. BANERJEE: Well, I think it's okay if you show us comparisons with data about whatever correlation you are using.

CHAIRMAN WALLIS: This is going to happen tomorrow?

MR. CORLETTI: Tomorrow we're going to talk about the scaling of --

CHAIRMAN WALLIS: And we are going to resolve it tomorrow?

MR. CORLETTI: That's a good objective for this meeting.

DR. BANERJEE: The core height has changed. Right?

MR. CORLETTI: Yes, it has.

DR. BANERJEE: So one of the concerns was the core height scale and that was what as the problem, I think, with OSU.

CHAIRMAN WALLIS: OSU is actually going to be here tomorrow.

MR. CORLETTI: Yes, they are.

DR. BAJOREK: I don't think we would use anything in APEX for what goes on in the core.

MR. CORLETTI: Right.

DR. BAJOREK: It wasn't scale for that. It's too short. In particular, I think they use like 1-inch diameter rods --

DR. BANERJEE: It was not the best scaling. Absolutely. That issue of what you are using to validate this quote needs to be resolved.

MR. CORLETTI: Okay. I think John spoke to this. I think Walt is going to speak to this as well so I don't need to belabor this issue. There was an issue with the LOFTRAN. I think it really wasn't -- I mean, the issue was and I think the staff concern was we know LOFTRAN is the transient analysis code.

You've been using it for a very long time. We know it is generally a single-phase code that allows two phases in the pressurizer and allows two phases in the upper head but it's typically not a two-phase code. The worry was the large steam generators that we were going to with the AP1000.

On the main steamline break would the depressurization be so significant that you would lose subcooling and then have questions about whether the code was adequate for that.

We did not provide the final analysis during the pre-certification review. We did provide that as part of the DCB analysis. I believe the item is resolved. I think we've provided those results to the staff.

There were several issues in the pre-cert review on the NOTRUMP. One was the heat transfer model. We have a fairly detailed presentation of that showing comparison plots of the heat transfer model in NOTRUMP compared to the heat transfer correlation we developed for AP600 and AP1000 based on the test.

This was another issue from the pre-cert which the staff had really not reviewed keyed-up methodology under AP600 for core uncovery cases because we didn't really have core uncovery. The issue was if you have significant core uncovery we would want to revisit this issue, if we had sufficient core uncovery for AP1000.

Our results for AP1000 are very similar. Any Gagnon is going to be presenting that this afternoon. There was one case that we didn't really have uncovery but we had very, very high voids which in our analysis we did a conservative -- assumed above a certain level, I believe it was over 90 percent, we would assume it would be uncovered and we did a very conservative adiabatic heat-up of the fuel rod to show that even in that case the PCT is well under the limits.

CHAIRMAN WALLIS: It has to stop. If you have a heat-up, something has to turn it around. What turns it around? The refilling of the --

MR. CORLETTI: Yes. It was a blow-down uncovery so it was a very short blow-down uncovery.

CHAIRMAN WALLIS: Instead of having that sort of V where the level goes down and comes up again, it actually goes down, uncovers, and then covers up again.

MR. CORLETTI: Yes, sir. Yes, sir.

The momentum flux model in NOTRUMP was also an issue. The issue there was the methodology that was employed for AP600 acceptable. I think the staff found that it would be. Westinghouse committed to do a supplemental calculation with WCOBRA/TRAC which is a more sophisticated computer code to show a relative comparison to assess the impact of our methodology. That is also in that WCAP 15833. We are going to talk about that later.

With regards to GOTHIC, I think here for AP1000 as we reviewed in the pre-cert review, the approved methodology for AP600 was found to be acceptable provided once we did the final analysis the important scaling numbers were still within the range.

We had done preliminary analysis during the pre-cert review and showed that we were in the key range of the important Rayleigh numbers and Grashof numbers. Rick Wright is going to be speaking to that. Generally we showed that with our final DCD analysis we were still within our scaling basis for --

CHAIRMAN WALLIS: Didn't you also do analysis of CFD type?

MR. CORLETTI: Yes, we did. Under the pre-cert review we showed the mixing characteristics with CFD analysis.

MEMBER RANSOM: Was WGOTHIC coupled with the COBRA/TRAC code or NOTRUMP?

MR. CORLETTI: Manually coupled but it's not linked. We do not have them linked. We do take the mass of energies either from COBRA/TRAC or other calculations and feed them into the GOTHIC containment model.

MEMBER RANSOM: As the calculation proceeds?

MR. CORLETTI: No, they are not linked. It's not a link.

MEMBER RANSOM: Independent calculation?

MR. CORLETTI: Yes.

MEMBER RANSOM: How can you do that then because I thought the source of energy --

MR. CORLETTI: Reiterate. We do them a couple of times essentially. You're right, it's not a link.

This issues was also -- I think Lambrose is going to speak to this. This really wasn't a code issue per se for the pre-cert. It was really a safety analysis results issue. As John mentioned, in PWRs, and especially for cold-leg breaks in PWRs the long-term boiling in the core, it's postulated that this goes on for a very, very long that boron could precipitate in the vessel and could impact core cooling if it finds a blockage of the core cooling.

We did a series of calculations and analysis where we actually calculate the long-term boron concentration in the sump and in the core. We take the output from our COBRA/TRAC LOCA analysis to get the steam qualities in that calculation. What we showed for our base case was peak boron concentration of 5,500 ppm in the core. The boron solubility limit, as Lambrose said, is about 3,500 PPM at that temperature so we are very far away from that.

We also do sensitivity studies to see what could the quality -- we range the quality and see the sensitivity. The RAI response asked us to do a different COBRA/TRAC analysis that was maybe select the way the assumptions that you made to minimize entrainment. We did that with a calculation COBRA/TRAC with a very high containment back pressure. All of the valves opened to minimize the velocities to see if that could have an impact on that.

I think John had this as unresolved, I think, yesterday or the day before. We are hopeful that our additional RAI response will resolve the issue. I think Lambrose hasn't seen that.

CHAIRMAN WALLIS: What are your sources of water? Your IRWST and your CMTs, do they all have the same boron concentration?

MR. CORLETTI: No, they don't. The IRWST is borated to about 2,500 ppm. It's refueling water. The CMTs and the accumulators are at the higher boron concentration. This is the system arrangement that you have long-term core cooling in the AP1000. You'll see that you're steaming out the ADS-4 and you're recirculation flow back through the containment recirc lines. Your team is concentrating. Your steam is condensing on the containment shell.

CHAIRMAN WALLIS: As long as it all recirculates there can't be a problem.

MR. CORLETTI: That's right.

CHAIRMAN WALLIS: Where else would it go? You would have to have a leak in the containment or something.

MR. CORLETTI: Yes. The worry is -- I mean, there are worries that --

CHAIRMAN WALLIS: Holding up in parts of the structure or something?

MR. CORLETTI: Are you deluding in the sump. Are you concentrating in the core. You can do these -- we do these transfer kind of calculations to track long-term with the range of the concentrations. Operating plants do this as well. They do these sorts of calculations.

In an operating plant they actually have procedures that in 24 hours they switch connections from the RA jar pumps to back flush water through the core. I think a lot of them are going away from that because in the PRA risk significant it's hard to show this is a real issue.

CHAIRMAN WALLIS: Now, you have this recirculation screen. I think we visited that with AP600 but this has become a big issue with PWRs and debris.

MR. CORLETTI: Yes. The staff has asked us questions on that. We have answered those. Essentially with our passive plant we have very, very low velocities to the screen. In addition, we have taken out all fibrous insulation that contributes to this sump blockage.

We have metal reflective insulation. We are very robust in that area in regards to our sump performance. I'm waiting for the staff to tell us. These were late RAIs so we haven't really discussed them but we think our answers are --

CHAIRMAN WALLIS: Didn't you put something above the screen?

MR. CORLETTI: Yes, a plate above the screen to prevent things from falling on the screen and blocking the screen.

MEMBER KRESS: Your ADS-4 valves that aim into the containment, have you aimed those in a way that --

MR. CORLETTI: Yes, away from anything that could damage. They are actually in the loop compartment or the steam generator compartment.

MEMBER KRESS: Not a lot of stuff in there.

MR. CORLETTI: That's right. Because we are designed to flood, we have to be very careful about where we put our safety related instrumentation and things that are located below the flood level have to be designed to be able to flood.

MEMBER RANSOM: Mike, could you explain for me again how the containment basically deborates water and it comes back into the IRWST and then drains back into the core. How do you prevent deborating core?

MR. CORLETTI: That's another issue. You have to do your calculation with everything skewed the other way. I think that is the mechanism that occurs. I think when you do the calculations you don't see that it -- because of all the born that we start with, you --

MEMBER RANSOM: How does boron get mixed back with this deborated water?

MEMBER KRESS: I would say it works the other way, Vic. At relatively high pressures when you blow off the steam you're enriching the water in boron and the steam is less rich in boron. It will carry some with it. Then when it condenses the boron may get left behind but it would take a long time at relatively high pressures. At low pressures you might be able to have a problem where the steam would carry more boron with it.

MEMBER RANSOM: I'm worried the other way around. The steam basically condensing on the containment deborates the water.

CHAIRMAN WALLIS: But then where is the boron? It's probably in the core.

MR. CORLETTI: That's right.

MEMBER RANSOM: How does it get back?

MEMBER KRESS: The steam leaves. That's only at relatively high pressures. As the pressure gets lower and lower you will take out more boron with the steam. If you're at low pressure and long-term cooling, there is a possibility of you carrying a significant amount of boron with the steam and then it ending up on the containment wall. I think that is the issue you worry about. It would have to be relatively low pressures for that to happen.

MEMBER RANSOM: Also it seems like the boron could wind up in that pool of water surrounding the reactor but not draining back into the reactor director.

MEMBER KRESS: That may be. That's a question of distribution.

MR. CORLETTI: But have we discussed it

enough, or have we answered your question?

MEMBER RANSOM: You've done calculations to assure that you do not get a boron pollution?

MR. CORLETTI: Or a boron -- that's right. We look at it in both -- we have selected the assumptions in both ways to show that either you do not concentrate or if you do it the other way, you do not dilute.

MEMBER RANSOM: Is NRR satisfied with that?

MR. CORLETTI: I think Lambrose is going to speak to that this afternoon, or this morning.

Lambrose?

DR. LOIS: This is Lambrose Lois again from Reactor Systems. The deboration of the vessel is accomplished by expelling water out of the vessel. In the long-term cooling phase, of course, you have steam going out as the steam takes some water out with it. If that's the case, then there's no problem. If the blast of the steam is so small, it will not be able to carry water with it into the containment and then there's a problem.

MEMBER RANSOM: The issue I was concerned with is the deborated water is being returned to the reactor.

DR. LOIS: Yes, it is, because it continuously circulates through the core for the cooling.

MEMBER RANSOM: So it would seem like you have a net loss of boron from the core.

DR. LOIS: Not unless you expel water from the vessel.

MEMBER RANSOM: Right.

CHAIRMAN WALLIS: Are we going to address that later on?

MR. CORLETTI: We are not planning on a detailed presentation on this issue. We can arrange for that in the future if you would like. We could show you curves from our calculations where we did that. That is essentially what we do. We look at both the potential for delusion in the core and the potential for --

CHAIRMAN WALLIS: I don't think the staff has reviewed all this so we're going to have to stop.

MEMBER KRESS: The sorry about delusion is whether the core can go critical again. I don't see much potential for that.

MR. CORLETTI: That's right. If you've thrown in so much more at the beginning of the event that you really --

MEMBER KRESS: It would take a long time.

MR. CORLETTI: You can't get there.

CHAIRMAN WALLIS: You're concentrating boron really.

MEMBER SIEBER: Concentrating.

MEMBER KRESS: Except in the long-term cooling when you've got the pressure down you are deluding then. The steam will carry a significant amount of boron.

CHAIRMAN WALLIS: But if you leave it behind somewhere else.

MEMBER KRESS: And it will just stay on the containment walls.

MR. CORLETTI: But in your containment -- but you remember we had 500,000 gallons of borated water here which is in the sump now which is mixing with the condensation that returns.

MEMBER KRESS: I haven't done the calculations but my feeling is that it would take a long time to get down to boron concentration you would be worried about.

DR. BANERJEE: With regard to the recirculation screens, we were shown some results that even a very small amount of fiber causes a problem because you get this effect of filtration which catches the small particles. To handle that some rather clever screen designs have been --

MR. CORLETTI: Improved screen designs. Right.

DR. BANERJEE: Have you looked at this because this seemed like a real issue when we looked at this problem.

MR. CORLETTI: We have looked at those screen designs and we could the screen designs that would significantly increase the surface area given the same kind of footprint. It's not in our base design but we could use that. I think the staff is reviewing it.

When you look at the amount of fibrous material we have in our AP1000 because of the elimination of that sort of insulation, and you also look at the very low velocities we have here, the approach velocities to the screen, when you categorize is this an issue or not for AP1000, it didn't appear to us that it was. I think we wouldn't have a problem going to implementing the advance -- the one that I've seen, at least, which is --

DR. BANERJEE: What they do is they have velocities parallel to the walls rather than normal to them. They take care of the problem by design.

CHAIRMAN WALLIS: It has to go through eventually, though.

DR. BANERJEE: Well, also there was this issue that could you separate -- at least have redundancy in the screens having two which is geometrically at different locations.

MR. CORLETTI: This is maybe an issue that if we want to talk about this more in our next meeting, we could show you drawings, the results of our calculations that we've done. I think it is an industry issue that's getting a lot of attention and I think we can show you what we've done. I think we've tried to address this with design.

DR. BANERJEE: You have an opportunity to take care of the problem before it occurs.

MR. CORLETTI: Yes.

MEMBER KRESS: And I think our committee -- I don't mean to speak for them but in one of the letters we expressed the opinion that an increased surface area screen is not a good fix if that's what you're talking about because it takes so little of this insulation to block even a large screen that we thought it wasn't the best kind of fix anyway.

MR. CORLETTI: We thought eliminating the fibrous insulation was the best thing.

MEMBER KRESS: That would be a good way to do it.

MR. CORLETTI: That was what we did.

CHAIRMAN WALLIS: I think our concern was with the paint fragments all coming down and floating around. They are sort of like leaves. That kind of material is very bad for screens, too. It doesn't take many sheets of thin stuff to block up a screen.

MR. CORLETTI: We have looked at the paint that we do use, the paint where they would be susceptible to blow-down forces. We've taken care to make sure we have the safety related paint that is required. I think these non-safety paints are an issue. They assume they all fall off.

CHAIRMAN WALLIS: Unsafety paint.

MR. CORLETTI: Or non-safety painters.

CHAIRMAN WALLIS: Okay. We should perhaps move on.

MR. CORLETTI: This is more of the same --

CHAIRMAN WALLIS: This is what you told us already.

MR. CORLETTI: -- of what you heard from John. We are trying to resolve the issues. I think John said a couple of these might be open. We are going to keep working hard to resolve as many of them as we can. I think staff has been accepting our input there.

I thought this was useful. It is hard, Dr. Wallis. To do a presentation of the RAIs is to get a sense of what were the important ones and what were the unimportant ones. I'll just touch on some of the key ones that we had. If this committee is interested, before I leave we can get you the numbers and point you to the ones that talked about these issues.

We did increased spectrum for the small-break LOCA. We did more complete spectrum on what we presented to the staff. We also did additional shut-down accident analysis. We did a loss of cooling accident initiated in a low power mode without the accumulators to see the robustness of the design for shutdown. Long-term operation of the passive RHR to show it is capable of cooling the plant long term.

ATWS analysis. This plant has a very robust design with regard to ATWS. One of the measures of acceptance is unfavorable exposure time. The amount of core light time where if you would have an ATWS you would actually exceed the reactor cooling system service level C pressure.

The acceptance criterias have that to be a very low number. For AP1000 we were essentially zero, I think, before we submitted our last RAI. We are not at zero at this time. We were 99 percent.

I think Summer Sun wanted us to do additional analysis to show not only did we do the worst case, but also can we meet the essentially zero UET. We are preparing that response. The staff does not have that but I believe our response will resolve that issue.

We did significant amount of PRA success criteria analysis. We discussed a lot of that at the PRA subcommittee meeting. The staff had asked for a multiple steam generator tube rupture analysis as well and we provided that in our RAI response.

And, finally, the low-temperature overpressure analysis. This demonstrates that your cold temperature, your Appendix G pressure limits on the reactor vessel are not exceeded.

CHAIRMAN WALLIS: This multiple steam-generated tube rupture, is this based on simply assuming that several will break or is it talking about the mechanisms whereby the next one?

MR. CORLETTI: It assumes that multiple ones on an area break and then --

CHAIRMAN WALLIS: That just assumes so

many break. You don't discuss how they might break simultaneously.

MR. CORLETTI: No, we don't. We don't postulate that. We didn't postulate that as far as to that analysis. Well, that summarizes my presentation.

CHAIRMAN WALLIS: You are way ahead of time here. I'm wondering if we could move up one of the next -- do one of the presenters have a presentation that might take half an hour?

MR. CORLETTI: The next presentation is the large-break LOCA analysis.

CHAIRMAN WALLIS: Would that take half an hour? We could then take a break after that.

MR. CORLETTI: I think that's fine.

CHAIRMAN WALLIS: Okay. Let's do that.

MR. CORLETTI: Okay. Very good. Thank you.

CHAIRMAN WALLIS: Thank you very much.

DR. KEMPER: Can you hear me all right?

MEMBER KRESS: The question is how advanced are you?

DR. KEMPER: Well, you may have your own judgment on that coming up. Am I able to be heard by everybody? Okay. I am Bob Kemper from the LOCA group at Westinghouse and I wanted to go over somewhat briefly with you the large-break LOCA analysis that we've done for AP1000.

For AP600 what we did was begin with the approved large-break LOCA best estimate technology using WCOBRA/TRAC.  That had been approved for the three and four loop Westinghouse operating plants. Then reviewed that in the context of the AP600 design.

We concluded that basically it was acceptable to use it.  The main things that showed up as different in a PIRT investigation were direct vessel injection feature of the AP600, now AP1000 design.  In the work that we did for AP600 we did some simulations of a CCTF and a UPTF test to demonstrate code capability for phenomena associated with direct vessel injection during a large-break LOCA.

Ultimately, the AP600 methodology was reviewed and approved for that purpose in the NUREG shown.  For AP1000 we are just building on that and using the same methodology to analyze a plant which is basically the same in design as AP600.  As part of the approval, the NRC identified some limitations on the methodology which we followed during the AP1000 analysis.

A number of these are carryovers from the three and four loop model approval concerning natures of distributions and ranges and that sort of thing that are generic. There are a couple specific for the advanced plant design including consideration of the effect of core makeup tank or PRHR on the results obtained during the large break analysis.

To accommodate that we did a case eliminating the CMT which doesn't really play a factor of much significance in large-break LOCA, and also eliminating the PRHR from the model. In either case, the results were actually less limiting.

Large break is really very similar in this plant to the conventional plant. You have your doubled-ended cold leg rupture and the accumulators are the thing providing the inventory necessary to refill the vessel and recover the core.

This is one of the transients that we did during the large-break LOCA best estimate methodology. It calls for doing a series of like 14 global model cases in which we are varying parameters such as the discharge coefficient of the break and the resistance of the broken nozzle. There are some simplifications that we placed into the advanced plan analysis.

CHAIRMAN WALLIS: Could you show where the CMTs come in on this figure?

DR. KEMPER: The CMTs actually --

CHAIRMAN WALLIS: Or the accumulators. Just show some of the key events here.

DR. KEMPER: Okay. The CMTs come on when you get an S signal which is maybe four seconds into the event.

CHAIRMAN WALLIS: So they are pretty early.

DR. KEMPER: They come on very early and inject for several seconds of time. The depressurization is so great that you then hit the accumulator set point. Once the accumulators begin to inject the CMTs shut off.

CHAIRMAN WALLIS: When are the accumulators coming on here?

DR. KEMPER: It would be about 10 seconds. Roughly 10 seconds.

CHAIRMAN WALLIS: So this heat up between 50 and 120, this is a an adiabatic heat-up?

DR. KEMPER: No, this is -- well, part of the time.

CHAIRMAN WALLIS: It's an uncovered core.

DR. KEMPER: The core is in essentially adiabatic heat-up --

CHAIRMAN WALLIS: Uncovered.

DR. KEMPER: -- until maybe 70 seconds or so on this scale. Then we begin to -- you refill the lower plenum.

CHAIRMAN WALLIS: But it's still on that track. It's pretty linear. There's about 70 seconds of linear heat-up and then 100 seconds of linear cool down. It looks like a very simple picture.

DR. KEMPER: This is actually more simple than a lot of large break transients. You have the big break, uncover the core. The AP1000 is equipped with capability to drain the upper head liquid very effectively into the upper plenum. A lot of Westinghouse plants don't have wholes in the upper support plate that permit this draining to occur. That enables you to get a very good cooling from 1,600 odd degrees down to 1,200 or so.

CHAIRMAN WALLIS: This is lightly the small-break LOCA in reverse. In one case you're worried about uncovering the core as they're coming down on some curve. Then you turn around and go up again. It doesn't quite uncover.

Here you go up on some heat-up and something has to turn it around. It's pretty key that you predict that turnover right. If it went on for another 30 seconds instead of turning around, you would be up in the danger zone. It's pretty important that you predict the turnaround right.

DR. BANERJEE: what's the pressure when the turnaround is occurring?

DR. KEMPER: Somewhere below 1,000 PSI. It would be going into the time when the accumulators are going to begin injecting.

CHAIRMAN WALLIS: So it's the accumulators that turn around?

DR. KEMPER: No.

CHAIRMAN WALLIS: They have already come on.

DR. KEMPER: The accumulators really turn around the second peak there when they provided enough water --

CHAIRMAN WALLIS: That's what I mean, the second peak.

DR. KEMPER: -- to refill the vessel. The initial blow-down heat-up is turned around by the blow-down cooling.

CHAIRMAN WALLIS: That's the second one. It's the accumulators. It's the balance of water coming in from the accumulators that turns it around at this elevation.

DR. KEMPER: That's correct.

CHAIRMAN WALLIS: Okay. So that's what it is.

DR. KEMPER: You essentially have to have enough accumulator and water injected to fill the downcomer and refill the core.

CHAIRMAN WALLIS: That is a pretty predictable thing. You've got a valve accumulated with pressure and you can predict with a lot of confidence how rapidly that water comes out of that accumulator.

It's not like some of these later events where you're balancing hydrostatic terms here, there, and everywhere and a little more uncertain about just what the flows are going to be. The accumulator flow is pretty certain.

DR. KEMPER: You've got to get the pressure right.

CHAIRMAN WALLIS: Right. You've got to get the pressure right.

DR. BANERJEE: That's why it would be interesting to see what the pressure was like.

DR. KEMPER: Well, the pressure by 30 seconds is down to containment pressure.

CHAIRMAN WALLIS: So it's down to nothing really and the accumulators are --

DR. BANERJEE: When do the accumulators

come on then?

DR. KEMPER: Roughly about 10 seconds into the transient when you hit 600, 700 PSIA set point.

DR. BANERJEE: So what's the pressure at 125 seconds or something when the turnaround occurs? The second turnaround.

DR. KEMPER: The second turnaround.

CHAIRMAN WALLIS: It's containment.

DR. KEMPER: Containment plus pressure.

CHAIRMAN WALLIS: So is that the accumulators turning it around or something else?

DR. KEMPER: It's the accumulators

CHAIRMAN WALLIS: Just filling up the vessel. That's all it's doing. It would seem to be a pretty predictable thing. You depressurize and the water is squirting in and it's filling up and the simple analysis will probably get you that one.

DR. BANERJEE: So it's taking 120 seconds or something.

CHAIRMAN WALLIS: Just to fill up the vessel.

DR. KEMPER: Fill the core high enough that you get good enough cooling.

CHAIRMAN WALLIS: The physical things happening are pretty simple. It's not as if it's subject to a lot of uncertainties in the modeling. Do a hand calculation. Sanjoy could do it overnight and get the same answer.

DR. BANERJEE: If I knew the answer.

DR. KEMPER: I would agree this is more straightforward than some of the other events.

CHAIRMAN WALLIS: Right.

MEMBER KRESS: Is this COBRA/TRAC?

DR. KEMPER: This is COBRA/TRAC.

DR. BANERJEE: So it's not due to IRWST water or anything like that?

DR. KEMPER: No. Accumulators are still injecting.

CHAIRMAN WALLIS: The passive aspects have nothing to do with this transient really. This is just like the classical PWR.

DR. KEMPER: Only the ultimate classic passive system, the accumulator.

CHAIRMAN WALLIS: But that was there before.

DR. KEMPER: Yes. That's nothing new here. The result of the calculation is --

CHAIRMAN WALLIS: This is so reassuring that you have ADS-4 to create a large-break LOCA in the other transients.

DR. KEMPER: That is true. I mean, LOCA will become a large break eventually.

CHAIRMAN WALLIS: This suggestion was made, it seems to me, about 30 years ago that since we no longer analyze large-break LOCAs, let's make everything into a large-break LOCA. Now it's finally going to happen.

DR. BANERJEE: The BWS followed that.

DR. KEMPER: I don't necessarily recall that. I do recall it being considered blow a hole in the hot leg to enable the venting.

CHAIRMAN WALLIS: This is a best estimate calculation?

DR. KEMPER: This is a best estimate calculation.

CHAIRMAN WALLIS: A realistic calculation.

DR. KEMPER: So then we proceed to consider the uncertainties and identify peak cladding temperature of the 50th percentile and at the 95th percentile. We need to meet the 10 CFR 50.46 regulatory requirements here for PCT as well as the cladding oxidation both local and core-wide. The 0.73 percent could also be called your core-wide oxidation or hydrogen generation number.

CHAIRMAN WALLIS: That's the 95th

percentile as well?

DR. KEMPER: That is done according to the methodology that was approved for the three and four loop plants. It's really based on a calculation that exceeds a WCOBRA/TRAC transient whose cladding temperatures exceed the 95th percentile PCT value. Then there's a methodology that uses these elevated temperatures to identify what the cladding oxidation is.

MEMBER RANSOM: Were these results generated using a nonparametric statistical approach?

DR. KEMPER: It uses a response surface methodology.

MEMBER RANSOM: You then go back and sample, I guess.

DR. KEMPER: No. It's done by generating response surfaces from varying model parameters. Then based on identified distributions identifying what the 50 percent values are.

CHAIRMAN WALLIS: Since you're just filling a vessel from an accumulator, it would seem the uncertainty should be pretty small. Is it the uncertainty in the heat transfer coefficient that gives you this number?

DR. KEMPER: Part of the methodology is to look at things like ride internal pressure, heat transfer coefficient on the fuel rod, and things such as this which would be --

CHAIRMAN WALLIS: In fact, the rods cool before the level gets to them and you're going to pull that in there so it's the whole reflood basis of assumptions comes into play.

DR. KEMPER: That's right.

MEMBER SIEBER: Maybe you can help me understand a little bit physically what's going on. The clad temperature is a LOCA phenomenon.

DR. KEMPER: That's correct.

MEMBER SIEBER: You're looking for the hot rod and the point on the hot rod where the power history was the highest, which is assuming parabolic would be somewhere in the middle. On the other hand, during a reflood you have a level that's changing the location of where that hot spot is. Do your codes actually look at the fact that the hot spot physically moves?

DR. KEMPER: In doing one of these analysis we keep track along the length of the rod where the highest or hottest point is at any particular point in time.

MEMBER SIEBER: Okay. That will change

the effect of -- the shape of the curve that you showed on slide 26. That probably has greater effect on what the slope and consistency of that curve is than just how many gallons you're pumping in or pushing in.

DR. KEMPER: That's also a function of the paper shape that you're assuming. What we've identified for this plant is a top skewed shape which is the most limiting and that's what is analyzed here. One simplification that we introduced for the advanced plan analyses is to do some shape studies and use the bounding shape. In our conventional plan analyses we sample power distributions and consider a variety of them and the uncertainty methodology.

CHAIRMAN WALLIS: Sample in an aleatory way the time after refueling. The power profile changes. Do you sample that in an aleatory way?

DR. KEMPER: Well, the sampling in the three and four loop methodology is from a number of shapes. It's not necessarily tied to a given burnup. That methodology does assume maximum start energy so it's early in life.

CHAIRMAN WALLIS: So if you change your fuel management scheme for this right after you built it, you would have to redo all the stuff?

DR. KEMPER: We would have to, I think, verify the shape that we looked at is bounded. That's our intent here. That is another reason to go with what we believe to be a bounding shape so that we're not -- we don't have a distribution based on shapes that ultimately might --

CHAIRMAN WALLIS: So there's an additional conservatism in this then?

DR. KEMPER: Yes, definitely.

CHAIRMAN WALLIS: Besides this 95th percentile is the fact that you've used some kind of what you think is a bounded shape for the power profile.

DR. KEMPER: Definitely.

MEMBER SIEBER: And there's other factors there, too, because you have to make an allowance for misaligned broads and tilts and things like that which is also built into that calculation

DR. BANERJEE: How much volume do accumulators have compared to the volume of the vessel?

DR. KEMPER: Accumulators are 2000 cubic foot tanks and the water level nominal level is 1,700 cubic feet. With two of them there's like 3,400 cubic feet and that's certainly larger than the lower part of the vessel and the core.

DR. BANERJEE: So that would cover the core, 3,400 cubic feet?

DR. KEMPER: Yeah. I'm thinking maybe there is 1,200 or 1,500 cubic feet to the hot leg elevations in the reactor vessel.

DR. BANERJEE: And roughly how many cubic feet do you lose out of a cold leg break?

DR. KEMPER: The initial part of the transient while your accumulator is injecting and the pressure is still high you do have bypass and lose most all of the accumulator water at that point in time. I would guess that it's less than 20 percent of the total water in the accumulators for this plant that would bypass during this point.

CHAIRMAN WALLIS: Well, this shouldn't be an issue. This is an old PWR analysis and there's nothing new about AP1000 presumably.

DR. KEMPER: Yeah. That's --

CHAIRMAN WALLIS: Except the CMTs don't have a role.

DR. KEMPER: CMTs really come on the first few seconds but then it's all accumulators and they don't contribute.

So for AP1000 we have performed a large-break LOCA analysis as we are required to according to the regulations and followed the restrictions that had been identified by the staff both in terms of those carried over from the methodology as a whole for Westinghouse best estimate large-break LOCA. And also the AP600 restrictions from the SER issued for that plant design.

CHAIRMAN WALLIS: So you do this calculated 95th percentile. Presumably there's some PCTs you calculate which are higher than that. Is that your disparities to get to a distribution and then cut it off at the 95th percentile?

Or are you going to say the nonparametric method is to say we'll calculate a lot of things and make sure we've got a 95 percent confidence that we've got at least something in 95th percentile and then we use that value and it may be above or -- it may be way up above the bound of the 95th percentile if you did billions of calculations, but at least it's a number you can use.

DR. KEMPER: Well, this methodology uses response surfaces and sampling to identify that. I believe some of my colleagues are going to be speaking with you hopefully soon about the approach you're indicating that we are aware one of our competitors has adopted. That doesn't apply to AP1000.

MEMBER KRESS: With the response surface at 95 percentile means that 5 percent of the results are above that number.

DR. KEMPER: Five percent are --

CHAIRMAN WALLIS: That's right. So you do calculate some numbers higher than 2,124.

DR. KEMPER: Yes.

CHAIRMAN WALLIS: What's the highest one you calculated then?

DR. KEMPER: Really our codes aren't set up to necessarily identify them.

CHAIRMAN WALLIS: They have a loop that says if it gets over 2,200 --

DR. KEMPER: No, no. They print out the 95th percentile value when you're doing your Monte Carlo sampling. I'm not --

CHAIRMAN WALLIS: I guess someone else might address that. It's a generic problem with these codes and it's a problem with the realistic code approach is what are you going to accept as being good enough statistically.

MEMBER KRESS: I think it's already been decided.

CHAIRMAN WALLIS: I think it's already

been decided so we can't do anything about it. I think this committee might want to revisit that in the future generically for all reactors. It's all been approved so we can't do anything about it.

DR. KEMPER: I won't argue with that.

MEMBER KRESS: The point is you want to keep the core cool and this is a conservative number anyway to keep the core cool.

DR. KEMPER: So in the way of RAIs our initial presentation of this material was, I'll call it, rather sparse consistent with some of what the operating plants had been providing for their three and four loop methodology. They wanted significantly more information so we provided that.

Another request was to continue running the large grade beyond the time at PCT out beyond the point the accumulators are empty and you have the CMTs now providing the injection. They are the source of injection until such time that you reach the low level in the CMT tank to permit IRWST to come on. We performed that analysis and the injection is adequate to maintain the core quenched.

CHAIRMAN WALLIS: Maybe this is a good time to take a break before you move on to the next topic.

DR. KEMPER: If you think so.

CHAIRMAN WALLIS: I think it is. We'll take a break. We'll come back at 10:30 which will bring us back to our original schedule. Okay. We'll take a break until 10:30.

(Whereupon, at 10:14 a.m. off the record until 10:33 a.m.)

CHAIRMAN WALLIS: Okay. Let's get started again.

DR. KEMPER: Bob Kemper speaking again.

DR. CARUSO: Bob, before you start. Dr. Wallis, I had one other piece of information to add to my last comment about code availability. I determined there is one Westinghouse code that the staff does have in house. It's called the Map 5 code. It was submitted by an operating reactor licensee to support a change in containment licensing basis and we do have a copy of the source code in-house. That's the only one I'm aware of at this point.

CHAIRMAN WALLIS: This is not the code that --

DR. CARUSO: I believe it's the Map 5 version of that code, yes.

CHAIRMAN WALLIS: The one that we had considerable questions about?

DR. CARUSO: I believe so, yes.

CHAIRMAN WALLIS: In terms of the handling of the mixing?

DR. CARUSO: Yes, I believe so.

CHAIRMAN WALLIS: And you never came back to us with an improved explanation?

DR. CARUSO: I don't want to go there.

DR. THROM: Dr Wallis, Ed Throm with the staff. I'm in the group Plant Systems that will be looking at that Map 5.

CHAIRMAN WALLIS: We need to look at Map 5 again in this committee.

DR. THROM: Yeah, but we do have -- just to address the issue of code availability, we do have the code. We have the source term and we may exercise it as necessary.

CHAIRMAN WALLIS: Okay. But presumably this licensee is intending to use it.

DR. THROM: Yes, and we are still very early in the stages of the review.

CHAIRMAN WALLIS: We need to see that code again. I think our staff will follow up on that.

DR. THROM: Yes. Thank you.

CHAIRMAN WALLIS: Sorry to interrupt.

DR. KEMPER: All right. I'm going to

proceed to talk about long-term cooling analysis that we performed for AP1000. As a large break what we're doing is applying a methodology that had been developed and approved for use on AP600. This is another analysis that uses our WCOBRA/TRAC code in a very much less detailed nodalization and approached than is used for the large-break LOCA event.

In the approval for AP600 the staff did identify some limitations for the application. We have adhered to those doing the AP1000 analysis. Nodalization is the same as it was before so it is still consistent with the validation calculations that we did to support the application on AP600.

CHAIRMAN WALLIS: Did you do sensitivity studies on nodalization? The idea that you could fix a nodalization on OSU and then use it for this large scale device is quite a step, if that's what you're doing.

DR. KEMPER: That's indeed what was done for AP600 that we are doing here. In doing OSU they did investigate some things regarding to the code. I honestly don't recall if they were noting sensitivity studies as a part of that.

MEMBER RANSOM: You mean you have a longer vessel. You didn't use any different nodalization in the vessel than the AP600?

DR. KEMPER: The vessel below the hot legs is approximately the same length. The core is now 14 feet instead of 12 feet. No additional node was used for that.

MEMBER RANSOM: I thought there was a longer distance between the top of the core and the bottom of the hot leg.

DR. KEMPER: I believe that dimension is the same.

MEMBER RANSOM: That's the same?

DR. KEMPER: Yeah. I think that was the only thing we wanted to definitely preserve for this. We took some volume out of the lower plenum to accommodate the additional two feet of active fuel length.

MEMBER RANSOM: How many nodes are used between the top of the core and the bottom of the hot leg?

DR. KEMPER: This is a very coarse model so there are two nodes within the core and one node within the upper plenum range. These transients are very long duration with slowly changing phenomena. The idea here was to come up and validate a simple model that we could use and make it feasible to do in computer running time space.

MEMBER KRESS: Refresh my memory on the window mode. What you did was took a window in time and looked at the transient and then extrapolated that to some other window in time?

DR. KEMPER: Okay. Window mode was another thing that was implemented, again, because these transients are so long. The idea of the window mode is to focus on the time of most interest or the time of lowest capability of the system.

Typically that has been when the IRWST is drained down to the point that sump injection or containment recirculation of water begins. This can be for some breaks well out there in time.

The window mode methodology is developed and validated against OSU to look at that point of time that you're interested in, specified boundary conditions for the containment pressure, the levels in the IRWST and/or sump, and temperatures associated with the liquid present there.

Then start with those boundary conditions and begin initializing the reactor vessel and primary system with a set of identified initial conditions that were deemed reasonable. Now, this is a boundary value type of problem.

The way we've approached it is you run the code for a period of time just until it settles out to its determine condition and overrides the initial condition that you specified. Then you proceed to analyze the transient.

CHAIRMAN WALLIS: This is long-term cooling?
This is when you have already filled the sump?

DR. KEMPER: This is --

CHAIRMAN WALLIS: The picture we just saw that Mike showed a picture of water everywhere around the reactor. Is that what is meant by long-term cooling?

DR. KEMPER: That would be during long-term cooling.

CHAIRMAN WALLIS: So nothing exciting is going to happen. Is it?

DR. KEMPER: Probably not if you have your --

CHAIRMAN WALLIS: You've got water everywhere and it's higher than the core and its got access to the core. The question might be can you get rid of the heat to the environment?

DR. KEMPER: It should be very benign given that you have properly sized ADS stage 4 valves pass the safety system.

CHAIRMAN WALLIS: That's all over. Isn't it?

DR. KEMPER: Still your ADS-4 --

CHAIRMAN WALLIS: Yeah, but it's open. It's a huge opening.

DR. KEMPER: It's been open for an extended period of time. You're draining water from the IRWST and/or recirculating it from containment.

CHAIRMAN WALLIS: And the velocities at that valve are relatively modest, aren't they, by then? Or are we still dealing with fairly high flow rates out of ADS-4?

DR. KEMPER: Well, the velocities at the time of sump injection, which was the picture Mike showed, depending on your assumptions regarding single failure of the ADS-4 valve that we would need to assume.

CHAIRMAN WALLIS: It can't be very high. It would have a big pressure drop and then you wouldn't get the water in so there must be a very low pressure drop for that valve. Or is it a few feet of water or something?

DR. KEMPER: No. It's a PSI or two. It's not large.

CHAIRMAN WALLIS: A few feet of water?

DR. KEMPER: Yeah.

DR. BANERJEE: Did the OSU tests show any oscillations in the long term?

DR. KEMPER: There were some.

DR. BANERJEE: Could you explain those?

DR. KEMPER: Well, a lot of effort went into explaining those. I was not part of that. I don't think they were considered safety significant. I know there was something that went on in the core makeup tanks in one of the tests but I'm not really familiar with all of what was determined from that.

MR. CORLETTI: Perhaps tomorrow Dr. Reyes from Morgan State will be here as will some of the other people that we have involved with the test program and we could revisit some of that question as far as the oscillatory behavior.

CHAIRMAN WALLIS: So do you think that behavior was peculiar to that facility or would it be expected?

MR. CORLETTI: Not necessarily. I think it's characterized by flood of water and filling up the system and then burping out of the ADS valves and filling the system and burping out. It is an oscillatory behavior that you can see. We even see some of that in our calculations for the plant as well.

DR. BAJOREK: Bob, weren't those due to condensation on the outside shell of the CMT in the tests and it was condensation inside the CMT that was stagnating the flow periodically and that was feeding back on the oscillation.

MR. CORLETTI: I think that was associated with one set of oscillations. I think there may have been some other things going on, too. I never personally looked into that to a great extent.

DR. BANERJEE: And your codes model this for this calculation?

MR. CORLETTI: The codes will show behavior of a slug of liquid enters the ADS stage 4 and then you pressurize the system. Once that passes the pressure drops back. That type behavior is observed in the code level.

CHAIRMAN WALLIS: Are you discussing this now because this was some point of issue with the staff?

MR. CORLETTI: No. Just basically that we've --

DR. CUMMINS: This is Ed Cummins. Maybe I could give a context to long-term cooling. The safety issue or the safety significant question is that as the IRWST empties and transitions to injection to the sump you have less driving head and is the driving head sufficient at that stage to cool the core with your ADS flow.

We had to prove that to the staff and we used this analysis to prove it to the staff. The sequence was modeled in the OSU test and we benchmarked the codes against the OSU test and then predicted the plant.

DR. BANERJEE: I guess the issue is whether a bubble short of grows and there's liquid held up in the hot leg in the ADS system which has to be pushed out. Then this bottle sort of vents and then starts that process again. Is there something like that happening or is it just the CMT sucking? There was that, too, wasn't there, that the CMT motivated?

DR. KEMPER: Slugging of portions of liquid flow through the ADS-4 valves were observed in the tests but it was never to an extent that you were doing anything significant to the water inventory.

CHAIRMAN WALLIS: I think your argument has to be that if you get water into the ADS-4 line, there must be water above the core and there is no problem.

DR. KEMPER: Exactly.

DR. BANERJEE: Well, it could be a growing steam bubble. You see, you've got stuff there in the hot leg ADS and you could be growing this until it breaks through and vents and then it starts again. That's why I'm asking what is the phenomena? If the phenomena was something different, that's fine, but what was the basis of this phenomena? Did they see oscillations in core temperature?

DR. KEMPER: No, I don't think so, not in any of the -- nothing significant in my recollection occurred in the way of inventory in the vessel during anything that was causing these small fluctuations in pressure.

DR. BANERJEE: Anyway, maybe tomorrow he could just briefly address it.

MR. CORLETTI: Dr. Wallis, one question you asked is when are we presenting this. This is one of the safety analysis that is in the Chapter 15 of our DCD and we were providing it.

CHAIRMAN WALLIS: But I thought we were going to go over today the areas where there might be some tentacle problems we had to think about.

MR. CORLETTI: The only issue that was

raised by the staff during the pre-cert review was in addition to this window mode that we talked about where we looked at key windows, they had asked could we do a continuous transient calculation from the beginning of the transient and we did do that.

Some of the transients are so slow. We can't run a 30-day transient that way but we did run some that are -- we did run a limiting one in that way and provided that also to the staff. That was, I think, the one issue from the pre-cert review.

CHAIRMAN WALLIS: What you're predicting is that you've got this sump full of water, water flows into the core, there's a two-phase flow in the core but what comes out into the ADS-4 line is steam.

MR. CORLETTI: It's a mixture, yes.

CHAIRMAN WALLIS: It is a mixture?

MR. CORLETTI: Yes.

CHAIRMAN WALLIS: So maybe you have to get the two-phase flow right.

MR. CORLETTI: Yes, I believe you do.

DR. BANERJEE: It's not steady.

CHAIRMAN WALLIS: It's not steady.

MR. CORLETTI: In the oscillatory behavior that you are referring to here on these tests was kind of a filling and venting kind of a long-term operation. The other oscillations about the core makeup tanks and condensing in the core makeup tanks, that's a very early time. The core makeup tanks before they start to inject --

CHAIRMAN WALLIS: If the core were to be drying, you wouldn't get two-phase flow out the ADS-4 line. Would you?

MR. CORLETTI: That is correct. When we do have water in the hot leg, we think this is a good sign.

CHAIRMAN WALLIS: So if you analyzed that case where you began to dry-out and showed that you were okay then, maybe you wouldn't need to do all the other work.

MR. CORLETTI: That would be a bounding calculation. Yes, I agree with that.

CHAIRMAN WALLIS: I think that would be more convincing to us if you could show a couple of pictures about what's happening and say, "We make this bounding calculation and it can't be worse than that and everything is okay," we can believe it. But when it's sort of just words like this, we don't quite know what we're looking at.

MR. CORLETTI: Maybe, Bob, if you continue.

MR. SNODDERLY: I'm sorry. Mike, before we continue, Dr. Wallis, when we developed the agenda I thought it would be useful for Westinghouse to just give you an overview of the large-break LOCA, small-break LOCA and the containment analyses. What we accomplished in bullet No. 3 was really what the major open issues that were identified during the preapplication process except for liquid entrainment which we're going to do later and to try to give some idea --

CHAIRMAN WALLIS: Four is just a review of some of the major safety analysis results which were not subject to RAIs.

MR. SNODDERLY: They were subject to RAIs and there may be some open items that will be covered, but I think what I wanted to try to do here was just to provide you an overview of those analyses.

CHAIRMAN WALLIS: I think it would always help this committee if instead of getting this presentation which says, "We did this and everything is fine," if you could sort of give a better picture of, "We had to consider these phenomena and this is what happened. The RAI analyses were secure," and so on.

DR. BANERJEE: Well, in particular with

this the collapsed liquid level is six to eight feet or eight and a half. That's about 50 percent of the core roughly. Oh, sorry. You were going to come to that?

DR. KEMPER: Yeah. Well, we can come to that.

CHAIRMAN WALLIS: Can you just keep going until you get that picture?

MR. SNODDERLY: I'm sorry, Bob. Before we move on, I hate to interrupt again, Graham, but could we go back to assist me in my notes. Dr. Banerjee's question, I don't know if we clearly answered that when he asked can COBRA/TRAC model the oscillations that were seen at the OSU test. It wasn't clear to me whether they could or they were.

DR. KEMPER: Yeah, WCOBRA/TRAC models oscillations are comparable to those that occurred in the test with regard to ADS-4 liquid and steam flow.

MR. SNODDERLY: Thank you. Bob.

DR. BANERJEE: And that's documented in some report?

DR. KEMPER: Yeah, there's a large inch-thick WCAP about Oregon State University.

DR. BANERJEE: Do you know that number?

DR. KEMPER: 14776 Rev. 4. Okay. So the

one thing with condenses and the time since AP600 we did have the capability to run a DEDVI line break case from the end of the NOTRUMP run onward out into the sump injection phase and did not at this point have to use window mode to do this. This is one of the results from that run. As Dr. Banerjee noted, it's core collapsed liquid level.

CHAIRMAN WALLIS: What is times zero on this plot?

DR. KEMPER: Times zero on this would be the end of the NOTRUMP run which would be 4,000 second maybe.

MR. CORLETTI: After IRWST injection.

DR. KEMPER: Once the IRWST is on and has established itself as a consistent source of input of water to the vessel.

DR. BANERJEE: But this level is calculated by COBRA/TRAC?

DR. KEMPER: Yes. This is COBRA/TRAC result for collapsed liquid level.

DR. BANERJEE: It's a 3-D calculation?

DR. KEMPER: No, it's essentially a 1-D calculation.

DR. BANERJEE: And this is with the two nodes or something or more nodes?

DR. KEMPER: Two nodes in the core.

DR. BANERJEE: If I remember, you have some large bundle tests, right? Fourteen-foot bundle tests and 12-foot bundle tests, G2, G1?

DR. KEMPER: There's blow-down heat transfer.

DR. BANERJEE: No, I'm saying boil-up collapse liquid level. When do you get dry-out? At what sort of collapsed liquid levels?

DR. KEMPER: Dry-out was not observed during this phase in any of the AP600 tests at OSU.

DR. BANERJEE: No, no. OSU is not the same height. This is a 14-foot bundle. Do you have tests showing first that you are getting the right collapsed liquid levels and, second, getting about 50 percent there that you don't have dry-out? The numbers are around 30 or 40 percent that you get dry-out with collapsed liquid levels.

CHAIRMAN WALLIS: You're meaning the level of the two-phase mixture which you're looking for.

DR. BANERJEE: Well, that's where it goes to. If you have about 30 percent collapsed liquid level, for sure you get dry-out. At about 40 percent the jury is out. Maybe you do get dry-out.

CHAIRMAN WALLIS: You get two-phase swell that you wet the top of the core.

DR. BANERJEE: And that's a function of the height of the core. Actually, it's quite easy to show analytically.

CHAIRMAN WALLIS: There is a situation here where if you had too little heat produced, you might be worse off because you might actually not so much swell so you might actually dry-out the top.

MEMBER KRESS: Is the 14-foot the top of the active core?

DR. KEMPER: That's right.

MEMBER KRESS: So your two-node break line is at the seven-foot level?

DR. KEMPER: That's right. This is the overall collapse level which is at like 60 percent.

CHAIRMAN WALLIS: It's a pretty crude model with two nodes and you're trying to predict this level.

DR. BANERJEE: And also what's the validation of this? If you get things a little bit wrong in terms of flow resistance this could be dropping so there's an issue of sensitivity as well.

DR. KEMPER: There were some sensitivities looked at in the WCAP that I mentioned earlier concerning the OSU predictions in terms of implementation of this methodology.

DR. BANERJEE: Remind me of the height of the OSU core.

DR. KEMPER: It's three feet. Quarter scale of 12-foot core so three feet.

DR. BANERJEE: Do you feel that those experiments were really applicable to a 14-foot core?

DR. KEMPER: I would say no. Less so than to a 12-foot core. You're still looking at a height-scaled facility no matter.

DR. BANERJEE: You have experiments which are with the 14-foot core and a 12-foot core, don't you, with bundles?

DR. KEMPER: I'm not really familiar with 14-foot core.

DR. BANERJEE: Only 12-foot cores? One of these big bundle experiments for level swell, how many feet were they?

DR. KEMPER: They are all older experiments. My recollection of the height would be 12-foot.

DR. BANERJEE: Okay. So let's say 12-foot. That's closer than three-foot. How do the -- what did you find in these level swell experiments? I mean, how much collapsed liquid level was required to give no dry-out?

DR. KEMPER: I don't really know the answer to that.

CHAIRMAN WALLIS: But you're still having two-phase flow-out the ADS-4.

DR. KEMPER: Yes.

CHAIRMAN WALLIS: So what you're predicting is that this two-phase flow go all the way through the core and above it and that's what cooling it.

DR. KEMPER: Right. You have two-phase flow.

CHAIRMAN WALLIS: The reason this level is so low is because you're making a lot of steam in the core.

DR. KEMPER: Yeah. You still have significant decay heat.

DR. BANERJEE: Also whether it's steady is important or not. I mean, in the long term. Do you just have a steady boiling with flow out or are you getting some sort of jogging phenomena which is going back and forth?

If there is jogging, how much further down is it going and what is the period? I think those are things which your code can probably calculate but it has to be validated against some database which is representative. A three-foot core maybe tells you something but it's not the same as a 14-foot core.

CHAIRMAN WALLIS: Don't you have to get the bubble rise velocity or the interfacial drag or something right to do this? Isn't that the key thing you have to get right to get the level swell?

DR. KEMPER: The interfacial drag would be what you're looking at to get the level predictions good in the vessel in the upper plenum, yes.

MEMBER RANSOM: Two analogies don't have enough detail really, I would think. I was going to ask you does the fuel also only have two axial nodes, the core?

DR. KEMPER: That's right.

MEMBER RANSOM: So you have lumped the upper region and the lower region into relatively low power type situation whereas you're missing the point of the highest power where you're more likely to get dry-out.

MEMBER SIEBER: That's true.

MEMBER RANSOM: I really don't know that you could look at these kind of calculations and draw any conclusion about whether or not you have seen dry-out.

CHAIRMAN WALLIS: The dry-out that we're talking about, I think, is not DMB type dry-out. It's just simply the water doesn't get to the top of the core. Is that what you're talking about?

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: There isn't water at the top level of the core so it's just steam cooling and that might take off. I would think this is a simple problem and the staff could do some of its own checking calculations or something. Well, it has. You want to ask the staff? Is this a problem here? Are we spending too much time with it?

DR. BAJOREK: I think it's a valid question. I do kind of question having only two axial nodes in the core for this. Now, I think in answer to the question, yeah, we have looked at this type of phenomena.

Generally what you find by looking at tests like Oakridge, the G2, which is a 14-foot bundle, G1 and, I believe, Theta which I haven't looked at, is that at lower-type pressures your level swell, which can take like a ratio between your two-phase level and your collapse level, is about a two with some uncertainty.

That would say in these calculations you would start expecting dry-out at the top if you got below about seven feet. Now, it appears that there's enough level in this one that you're probably not drying out.

Maybe not by a large amount but, there again, the way that is nodalized you may be cheating yourself of some liquid because you aren't getting a good axial discretion in the void fraction. I mean, this is saying that you're getting basically a 40 percent void fraction on average in the core.

My guess is without looking at the plots, we're seeing something that's on the bottom cell of .1 void fraction with something fairly high. That is going across too many flow patterns for COBRA/TRAC to really do a good job on.

DR. BANERJEE: Just another question. Does COBRA/TRAC have a drift flux model built in to get the level swell or how does it do it?

DR. KEMPER: Well, this is in the COBRA vessels where you have representations of interfacial drag. It's not a drift flux model.

DR. BANERJEE: Two fluid?

DR. KEMPER: Two fluid.

DR. BANERJEE: Then how does it get the level swell right? Two fluid models are notoriously bad at getting level swell right as far as I know. How does it get the level right?

DR. KEMPER: Well, again, in the WCAP by reference that shows that for this application that was done right. The COBRA/TRAC expert will be here this afternoon if you would like to pursue that further.

DR. BANERJEE: Who is the expert?

DR. KEMPER: Dr. Ohkawa.

CHAIRMAN WALLIS: I think it would be good if they could show more detail than just this curve. This doesn't show us much.

DR. BANERJEE: Really what is the basis for the belief in this?

MEMBER RANSOM: Well, it would be helpful to see the void fraction profile, too. At least the voids in the three different nodes that they have above the core and the two core.

CHAIRMAN WALLIS: But they are very crude models.

MEMBER RANSOM: Oh, extremely.

CHAIRMAN WALLIS: If you're worried about whether it's six or seven or eight feet, I would think the model is too crude to really distinguish that.

MEMBER RANSOM: Well, standard approach in

1-D codes use about a one-foot node, you know, so you would have like 12 to 14 nodes.

CHAIRMAN WALLIS: Doesn't your window do that? Doesn't your window mode use more number nodes or not?

DR. KEMPER: No.

CHAIRMAN WALLIS: It's so easy to do, though. Just take a look at it at one point there and calculate as it it were steady state. It should be easy.

DR. BANERJEE: Well, the only thing I don't know is whether, first of all, this is right. Even if it is off by a little bit it doesn't really matter. If it gets to six foot, for example, then you are going to be drying out a significant part of the top of the core if it's not eight but six.

CHAIRMAN WALLIS: It must depend on the power level. If there's no power at the six-foot level, it's dried out the rest of the core.

MEMBER SIEBER: It doesn't make any difference.

CHAIRMAN WALLIS: If it's a very low power level, then the bottom is just water and the top is dry and heating up.

DR. KEMPER: Yeah.

CHAIRMAN WALLIS: It's primarily worse off than it was a long time in the future and you just don't have enough power to keep the level of the swell up there. It's all tied up to the ADS-4 flow rate, pressure drop through that and how much that depressures.

The whole picture isn't here at all. Where do we go from here? I think someone has to look into it in more detail because we're not getting enough detailed answers here to be reassured. I don't think we're going to get them right now.

MEMBER RANSOM: The other part that is helpful is the hear transfer mode in each of these nodes here.

DR. BANERJEE: This is almost a steady state calculation. Right? Virtually. There's going to be a calculation which is almost possible to do by hand.

MEMBER KRESS: Hot water bottle with a line feeding in.

DR. BANERJEE: As long as you know the resistance.

MEMBER KRESS: Resistance coming in and going out.

DR. BANERJEE: The only thing is if you do get this oscillatory mode of chugging, that needs to be dealt with, too, to either explain it or something.

MEMBER KRESS: You would have to have momentum equations so you couldn't do that by hand very easy I don't think.

DR. BANERJEE: Very slow probably.

MEMBER KRESS: Yeah.

DR. BANERJEE: You might be able to do a number of steady states.

MEMBER KRESS: You might.

CHAIRMAN WALLIS: Where do we go from here? Are you guys going to come up with something that is more convincing today or tomorrow or are we going to ask the staff to look into this with more detail? I don't see why you just don't do a 20-node model. Stay the same would be trivial to do it. There's not much going on. What's the problem?

DR. KEMPER: Well, the original problem was computer resources for a very long transient. That has changed over the years.

CHAIRMAN WALLIS: There's a big pool of water and it's coming in through DVI line right into a vessel and it's essentially sort of one dimensional flow up through the vessel.

DR. BANERJEE: It may not be so one

dimensional.

CHAIRMAN WALLIS: Is it wet at the top or not?

MR. CORLETTI: Dr. Wallis, this is Mike Corletti from Westinghouse. We'll be talking a lot about WCOBRA/TRAC this afternoon and the models in the code. Not on this code but in the other that we have that we've done our supplemental calculation. I think that maybe we can bring some questions there about how this COBRA/TRAC really handled that. Then we can go from there to see whether we need a future action after we discuss that this afternoon.

CHAIRMAN WALLIS: I don't think looking at the models in the code is going to help very much. You have to look at what they are predicting in more detail.

MR. CORLETTI: In our DCD we have quite a bit more plots than this plot here. Also we can get with Bob and see what is the best way to present that.

CHAIRMAN WALLIS: Maybe you could give Dr. Ballenger a homework problem where you can tell him some of these levels and the power level and the resistance to the ADS-4 valve and he can come back tomorrow and say it's okay or it's not.

MR. CORLETTI: I can give you the

resistances.

CHAIRMAN WALLIS: It's not a complicated problem. That's why it's really puzzling me why you are saying you are limited by computer resources. I could do this overnight it seems to me with a PC.

DR. BANERJEE: You should get an analytical solution.

CHAIRMAN WALLIS: Analytical solution. Okay.

MEMBER RANSOM: Another question that I think would be interesting to ask is when they resist crude nodalization and one-dimensional in a multi-dimensional code, do they use the same constitutive package for the interface drag?

The floor regimes are quite different that you must use from a 1-D representation versus a multi-dimensional that uses the radio distribution across the core. Is it the same constitutive package then that is being used in COBRA/TRAC for both of these calculations?

DR. KEMPER: Again, that would be a question -- if you want to pursue it, I think you need an expert.

CHAIRMAN WALLIS: Well, suppose we told you we don't think a two-node model is good enough.

Show us something more detailed. Wouldn't that be a fair position for us to take? It doesn't have to be transient. You have quasi-steady state at 2,000 and 8,000 second or something. So where's the details with the quasi-steady state model? Should be able to do it in a couple of days.

DR. CUMMINS: This is Ed Cummins. I think the question is can you use another tool to predict this result. It doesn't have to be COBRA/TRAC. It can be Dr. Banerjee's or Westinghouse's hand calculation. We can handle that kind of a question.

CHAIRMAN WALLIS: But not with two nodes. Two nodes seem much too crude for this problem. Did the staff let them get away with two nodes?

Steve, did you let these guys get away with a two-node model for this thing?

DR. BAJOREK: Well, I want to hesitate because I don't really feel I should be answering your question, but I think the answer is no, you should not be using a two-node core for something like this.

The reason being, and I think Katsu can give you more explanation this afternoon, is that in subroutine interfere where these interfacial drag correlations are done, it will break up this core or this process into several discrete flow patterns.

Embedded in those interfacial drag correlations are RAMS that go between delta alphas of about .2 to .4.

Now, I think just by doing a mental calculation on the delta void, what the code has done in this is it has jumped over a couple of these flow patterns. You've gone from bubbly up to annular and you've mushed out everything else in between. The interfacial drag in those regimes are drastically different. Much higher down on the bubbly and the slug than it is up in the annular.

I think by not having the nodalization there, you've missed several of the bubbly slug churn pattern which would probably give you or retain a higher froth and more liquid in the core. I think that's what you would wind up seeing in a more detailed calculation.

Secondly, I think maybe what you might want to think about are, I think, some of the simulations that were done with the G2 and I think G1 and Oakridge to kind of show that COBRA-TRAC does kind of model level swell. I think there is a basis there but I think you're going to have to pull that out.

DR. KEMPER: Yeah. Well, as Dr. Bajorek mentioned, we have these simulations. Now, this is a no core uncovery situation so it's a different condition from some of these tests that were core uncovery tests literally.

CHAIRMAN WALLIS: It seems to me we have some new RAIs here and somehow or the other these 700 RAIs didn't pick up these matters that we're discussing now.

DR. BAJOREK: I think 440 164 alludes to that.

DR. KEMPER: Well, this is long-term cooling as opposed to --

CHAIRMAN WALLIS: This is long-term cooling. Did you discuss the long-term cooling issue with the RAI's? Did you have this sort of discussion with them that we're having now about the two nodes?

DR. BAJOREK: I guess you would have to ask the NRR reviewer who was doing the long-term cooling.

CHAIRMAN WALLIS: Apparently not.

DR. LOIS: This is Lambert Lois, Reactor Systems. Within that question of the two-node solution that they proposed, we do have some outstanding questions regarding the model that they used generally in the long-term cooling.

CHAIRMAN WALLIS: We just have two samples. We have large-break LOCA and long-term cooling that we've looked at a little bit today. One of them looks as if we have some important questions about. From a sample of two, one of them we've got important questions about. If we had a sample of 50, I wonder how many we would have important questions about.

DR. BANERJEE: Just as a matter of information, what is the velocity of the steam and the hot leg and through the ADS valves? This is almost atmospheric pressure.

DR. KEMPER: Right. Now, the velocity depends on how many valves you are presuming to have open and what conditions. It would be on the order of 100 feet per second. For a single failure case you could be maybe size 300 feet per second at some point steering this transient.

CHAIRMAN WALLIS: This is in the hot leg?

DR. KEMPER: This is in the ADS-4 line.

DR. BANERJEE: And the hot leg?

DR. KEMPER: The hot leg would be lower because of its significantly higher area.

CHAIRMAN WALLIS: It's a lot less. What's the area ratio?

DR. KEMPER: Let's see. Maybe a factor --

DR. BANERJEE: Two to one. Let's say 50

to 100.

DR. KEMPER: Two and a quarter to one.

CHAIRMAN WALLIS: And what's the velocity coming out of the core?

DR. BANERJEE: Top of the core.

CHAIRMAN WALLIS: Steam velocity coming out of the core.

DR. KEMPER: There you have a wide area.

CHAIRMAN WALLIS: Is it one or 10 feet a second? Twenty? .1?

DR. KEMPER: Order of 10 feet per second I would say.

CHAIRMAN WALLIS: So it's reasonably significant. It's above the bubble rise quite a bit. It's probably carrying over liquid. If it's carrying over liquid, then double the core is wet. It seems to me all these answers ought to be there just like this without having to dig for them.

DR. BANERJEE: But, as Graham says, if the velocities are at 10, 15, 20 feet per second, you are probably getting droplets coming out.

CHAIRMAN WALLIS: No problem at all. You may have no problem. But you may have a problem at 2,000 seconds or something, or 20,000 seconds. When the power level has gone down, you don't have enough velocity to carry the liquid up. This long-term cooling may be a problem several days later. I don't know. Did you pursue this out much longer in time?

DR. KEMPER: We've got a calculation that was done at 28 days, 30 days.

CHAIRMAN WALLIS: Is this okay 100 days in time?

DR. KEMPER: The 38-day calculation assumes low levels as well within the containment so it's a minimum driving head type of situation. That case was adequate.

DR. BANERJEE: Why is it lower?

MR. CORLETTI: In 30 days we assumed that there are passive leaks inside the containment and that the water actually falls to a lower level. It's what we call a wall-to-wall flooding case where we assume that all the compartments flood to an even level and that's a reduced level compared to the design basis case earlier.

CHAIRMAN WALLIS: When I looked at this figure, what concerned me right from the start is you've got a collapsed level that starts out at something like 7.5 and it slowly goes up to something over 8. Then at around 8,000 seconds it starts to wiggle more and come down. You wouldn't expect that, would you?

DR. KEMPER: That's when you have reached the point of minimum driving head. The IRWST is emptied to its level where you begin self-recirculation. That's a low-driving head situation and you begin to have warmer water come that's been from the sump. Instead of the highly sump cool water from the IRWST, you are now having warmer water being injected into the sump.

CHAIRMAN WALLIS: So it makes more bubbles, makes more steam, makes more voids.

DR. KEMPER: You have a little more voiding in the core.

CHAIRMAN WALLIS: This curve really should be continued out until the level is 14.

MR. CORLETTI: In the 30-day case, the wall-to-wall flooding case actually shows collapsed level of about 14 feet.

CHAIRMAN WALLIS: Thirty days?

MR. CORLETTI: Yes. We do this window mode for some time periods in between so this shows that we're --

CHAIRMAN WALLIS: And it never goes -- does it steadily go up from eight to 14 or does it go down part of the time?

MR. CORLETTI: This is the window modes that we're doing. I believe if you look at the windows that we did --

Bob, do you have an answer to that trend? Is that the trend? Or did we do enough windows in that trend?

DR. KEMPER: I would expect that to be the trend but there's no specific calculation done for AP1000 under that.

CHAIRMAN WALLIS: To be reassured about the safety of this thing, I would like sort of more positive answers.

MR. CORLETTI: Yes. I understand.

CHAIRMAN WALLIS: I thought you guys had it all sewed up.

MR. CORLETTI: This methodology is the methodology we did use on AP600 and that we reviewed during the pre-cert.

CHAIRMAN WALLIS: I don't really care about methodology. I just care about convincing arguments that this thing isn't going to have any problems.

MR. CORLETTI: Okay. I understand.

CHAIRMAN WALLIS: That's all I care about. I only care about approved methodology and all that stuff.

MR. CORLETTI: I misunderstood your question. I think --

CHAIRMAN WALLIS: You can always take refuge in approved methodology. If I'm not convinced that it's going to work, it doesn't help me.

DR. BANERJEE: But what is approved for a 12-foot core at a lower par density and different floor regimes may not be approved for this.

CHAIRMAN WALLIS: Okay. So this is going to be resolved before you come before the full committee? Maybe we have to have another subcommittee meeting. Maybe there is something we're just missing here.

MR. SNODDERLY: Graham, I think the meaning is objective in the sense that we want to try to identify issues so that in the summer when we write this letter on this draft SER the staff will have identified certain open items.

We would confirm and say that they have identified the proper open items or issues or they haven't. This may be an example of an area where the staff is inadequately -- not the staff but the models used for long-term cooling are inadequate. Maybe we should use more than two nodes.

These types of comments I would see going in to the letter where the staff would either have to come back and say, "No, we don't think that rises to the level of an open item," and they will have to convince us.

CHAIRMAN WALLIS: What concerns me is that we were supposed to focus on sort of the tentacle issues that the staff had problems with with the RAIs and all of that. We weren't supposed to discuss this at all but I understand that Mike said, "Why don't you go and review some of these other things." By this sort of randomly picking this, we seem to have run into some major problems already. We wouldn't have seen it at all unless you happened to present it.

MR. SNODDERLY: What I would like to suggest is with the time we have we could spend a half an hour on the small-break LOCA analysis and then a half hour on the containment analysis.

CHAIRMAN WALLIS: I think we should move onto something else having identified this as a problem. Let's move on and see what we get on the next one. But we're not reviewing everything by any means. This is just a few things. Maybe we should look at the next presentation and see what comes up with that. Are you ready for another one?

DR. GAGNON: Good morning. Can you hear me fine? My name is Andre Gagnon and I'm here to talk about the AP1000 small-break LOCA analyses and NOTRUMP.

First of all, we're going to look at some of the open items from the pre-certification review. Not all of these were specifically identified by the NRC but were issues that the ACRS also had related to NOTRUMP. The first one is the ADS-4 momentum flux issue. The second issue is the existence of upper plenum and hot leg entrainment.

The third issue is the passive RHR heat transfer model, what we are doing to account for nonconservatism in the nuclear boiling correlation. Next is the noncondensable -- treatment of noncondensable gases in the modeling for the simulations. The last issue is the treatment of core uncovery.

ADS-4 momentum flux. The NOTRUMP model itself does not contain the detailed momentum flux model. It does not -- it has the standard evaluation model package which does not deal with changes in area and density. For most instances for small-break LOCA the velocities are low enough --

CHAIRMAN WALLIS: This can't be true in

the break. The break is all momentum flux so it must be only in the pipe that you do it. In the valve it's all momentum flux.

DR. GAGNON: And that's treated as critical flow models.

CHAIRMAN WALLIS: So your concern is that the valve is big enough compared with the pipe that the flow velocity in the pipe is big enough that there is momentum flux in the pipe which needs to be considered in comparison with the other pressure drop terms.

DR. GAGNON: And of the paths where this is an issue, ADS-4 was shown to be the biggest issue. ADS-1 to 3 was shown to have a relatively minor effect but ADS-4 was shown to have a relatively large effect. Now, to deal with this --

CHAIRMAN WALLIS: I'm really puzzled. For 40 years or something we've been neglecting these momentum flux terms or something, and now all of a sudden we have to worry about them. It seems to be so easy to get them right the first time.

DR. GAGNON: Anyway, continuing. To address the ADS-4 issue in the AP600 program, what was done was to utilize an IRWST level penalty. That was a penalty based on scaling arguments and --

CHAIRMAN WALLIS: That's a very round about way to do it.

DR. GAGNON: Yes, it was.

CHAIRMAN WALLIS: Because momentum flux is wrong in the pipe so you change the level in the tank?

MEMBER KRESS: Gave you a bigger driving edge.

DR. GAGNON: That's what was done originally for AP600. What was done as part of the latter ACRS reviews, if you remember, Dr. Kress, was that we developed the stand-alone momentum flux model to model the ADS-4 flow path from the hot leg to determine the effect.

We then compared that to the IRWST level penalty to show that they were comparable. But what we are doing in terms of AP1000 is to utilize that same methodology which is the detailed ADS-4 momentum flux model to generate a resistance adjustment for the ADS-4 flow to more directly attack the problem which is ADS-4 pressure

CHAIRMAN WALLIS: Is this a problem in the long-term cooling or is this a problem in earlier part of the transient?

DR. GAGNON: This is a problem particularly from the transition from sonic to subsonic.

CHAIRMAN WALLIS: So it's an earlier part of the transient.

DR. GAGNON: Yes.

CHAIRMAN WALLIS: Through the ADS-4 valves?

DR. GAGNON: Yes.

DR. BANERJEE: And is that based on steam flow or two-phase flow?

CHAIRMAN WALLIS: Two-phase flow.

DR. BANERJEE: Two-phase flow? High quality?

DR. GAGNON: We actually have run the model from low quality to high quality and then do a regression to get a fit and an adjustment factor. The adjustment factor that we came up for AP1000 was approximately 70 percent increase.

CHAIRMAN WALLIS: I'm not quite sure. The way that people often argue about momentum flux is, "If I get it wrong, it doesn't matter because the momentum flux that comes out of one node goes into the next one. If I get too little here, I pick it up in the next one." It also works out in the end. If you model momentum flux in the pressure drop in one node and you take that momentum flux to the next one, you can get that momentum flux back.

DR. GAGNON: But what this is is there's a single flow link with fluid node that represents the ADS-4 pipe and then we model the squib valve which is a break link so there's nothing downstream.

CHAIRMAN WALLIS: But the momentum flux that you have in the pipe, the pressure drop in the pipe, you can pick up again in the valve if you take the incoming random flux into the valve as part of the momentum balance for the valve.

DR. BANERJEE: It depends how they do the critical flow calculations.

CHAIRMAN WALLIS: Take the incoming momentum flux --

DR. GAGNON: -- based on stagnation empathy.

CHAIRMAN WALLIS: Stagnation empathy? It's not stagnate, though. Is it?

MEMBER RANSOM: Well, is NOTRUMP like a tube and tank type of model in which the flow from node to node is really a quasi-study phenomena? You know, ignore inertia and momentum flux and just consider resistance type formula?

DR. GAGNON: Yes. I believe that's true.

MEMBER RANSOM: So you don't even have the acceleration effects in this kind of model, let alone the momentum flux part.

CHAIRMAN WALLIS: I'm not sure I want to see another momentum equation.

DR. BANERJEE: But this is a drift flux model.

CHAIRMAN WALLIS: It's a drift flux model?

DR. GAGNON: Yes.

CHAIRMAN WALLIS: For EDS pipe at high velocity?

DR. BANERJEE: No, no. You are modeling that separately. Right?

MEMBER RANSOM: This is modeling the slip between the phases with drift flux model.

CHAIRMAN WALLIS: The drift flux model doesn't apply to this sort of situation.

DR. GAGNON: Not at the valve, no.

CHAIRMAN WALLIS: Not even in the pipe.

DR. BANERJEE: At the core it does.

CHAIRMAN WALLIS: Yeah, in the core it might but pipe? This is the pressure drop in the ADS-4 pipeline you're talking about?

DR. GAGNON: Yes.

DR. BANERJEE: They have a separate model.

CHAIRMAN WALLIS: They have a drift flux

model for that?

DR. BANERJEE: They have a separate model.

CHAIRMAN WALLIS: This is all, I suppose, old stuff. This is what you did for AP600?

DR. GAGNON: Yes. What is in the DCD for AP600 is the IRWST level penalty cases. We also ran the ADS-4 resistance change cases to show that they were comparable. But for AP1000 we went with directly attacking the problem rather than changing something --

CHAIRMAN WALLIS: This is all written up in some document somewhere?

DR. GAGNON: They are in RAIs.

CHAIRMAN WALLIS: This is all in the RAIs? What is the actual number?

DR. GAGNON: This is an AP600 RAI which is RAI 447.

CHAIRMAN WALLIS: Does that give equations and things or is it just talk?

DR. GAGNON: Yes.

CHAIRMAN WALLIS: It gives equations. Okay. The staff accepted these? Did the staff accept these?

DR. JENSEN: Yes, the staff has accepted this.

CHAIRMAN WALLIS: Okay. So if I looked at these momentum equations I would accept them, too?

DR. JENSEN: I think so. It's back out from the momentum equation what an equivalent resistance would be.

DR. GAGNON: This was developed by Mike Young in the AP600 and was reviewed by the ACRS.

CHAIRMAN WALLIS: We can't review everything in detail, though.

DR. GAGNON: I don't remember for sure but I believe Dr. Schrock --

CHAIRMAN WALLIS: So momentum flux is treated as an equivalent resistance in some way.

DR. BANERJEE: I guess you're integrating over the whole pipe. Right?

DR. GAGNON: Yes. The detailed momentum flux model has approximately 440 cells simulating the entire piping down through the squid valves.

DR. BANERJEE: Dr. Watson fatal. That's not good. And so that adjustment would change depending on the length of the pipe and so on.

DR. GAGNON: Yes. Yes. And this is specifically for AP1000.

CHAIRMAN WALLIS: Is this just a theoretical calculation or is it related in some way to evidence or data?

DR. GAGNON: It was compared to data for OSU.

DR. BANERJEE: I thought they got slugging.

DR. GAGNON: Well, we're talking early phase.

CHAIRMAN WALLIS: Okay. So there's a comparison and it shows that your new model is better than the old model?

DR. GAGNON: Yes.

MEMBER RANSOM: This could have been accepted by NRR and have been benchmarked against some of the scales, some of the classical blow-down experiments and basically accepted as a licensing tool?

DR. JENSEN: Yes. The code was compared to experimental data primarily in the AP600 review. It was benchmarked against a wide range of data, SPES, OSU, and the staff accepted the code. Then we looked at the application of the code to AP1000 and had some additional questions. Yes, we believe the code has been appropriately benchmarked against experimental data with the exception of the entrainment coming off the ADS-4.

CHAIRMAN WALLIS: This code that was benchmarked had no momentum flux terms in it?

DR. JENSEN: No. At one time it did have momentum flux long ago and Westinghouse for some reason took the momentum flux terms out for the purpose of reviewing the advanced plants probably because of low-pressure problems.

CHAIRMAN WALLIS: What's been benchmarked, this one with the momentum flux terms put back in again?

DR. JENSEN: The one that was benchmarked against SPES and OSU is the same code that is used for AP1000. It does not have momentum flux.

CHAIRMAN WALLIS: So what assurance do we have that these momentum flux calculations or corrections are valid?

MR. CORLETTI: I think if we go over the presentation it will answer that.

CHAIRMAN WALLIS: You'll get to that? Okay. You don't see comparisons with data in here, though. I'm just leafing through the slides.

DR. KEMPER: For the ADS-4 momentum flux?

CHAIRMAN WALLIS: Going back to AP600 we had a lot of questions about the code and the more we looked at the code, the more we said gee whiz. But then what eventually convinced the bulk of the committee was the evidence, the comparison with SPES and so on, saying that, yes, there's all these hocus pocus in the theory but it works. That's what you need to do here, too.

MR. CORLETTI: Our overall approach was we were going to also benchmark this NOTRUMP against COBRA/TRAC and show COBRA/TRAC to the test, which I think Andy is going to get to next.

CHAIRMAN WALLIS: Those figures show very different protections by the two codes.

MR. CORLETTI: That's where were are next.

DR. BANERJEE: So there was a point made that the momentum flux terms were taken out because you had problems at low pressure?

DR. GAGNON: I don't believe that's the case. I believe at one time -- this is all from memory so don't take this as gospel. Where the code used to run was on the CDC when you had small core memory, large core memory and it was a space issue. At that time the momentum flux models weren't being used. They took them out and they have never been reintroduced.

DR. BANERJEE: But with low pressure there's a large change in volume going from water to steam. Acceleration has to be somewhat important more at low pressure than at high pressure so what is the logic of taking it out at low pressure when it was in at high pressure?

DR. GAGNON: It's been out for a year.

MR. CORLETTI: That predates --

DR. GAGNON: It's not a recent removal.

CHAIRMAN WALLIS: Sometimes it gives yo problems. Momentum flux sometimes gives you nonphysical oscillations.

DR. BANERJEE: It's a nonlinear term.

DR. GAGNON: Anyway, in order to support the missing pieces of NOTRUMP, which is basically the ADS-4 momentum flux, or to supplement what we're doing with NOTRUMP which is using that detailed ADS-4 momentum flux model to calculate a resistance, we proposed to perform supplementary calculations with the COBRA/TRAC code for the ADS-4 and IRWST initiation phase.

That code does contain the momentum flux terms in the momentum equation. It also contains upper plenum and hot leg entrainment models which NOTRUMP does not. It also contains horizontal flow models, flow regime.

DR. BANERJEE: Was this a 3-D model you

used it with or not for the core in the upper plenum?

DR. GAGNON: Yeah. I believe the core upper plenum is 3-D, yes.

DR. BANERJEE: Or is it the three-node model that we talked about before?

DR. BANERJEE: No, no, no. This is in the ADS-4 IRWST phase. Right?

DR. GAGNON: Right.

DR. BANERJEE: So you had a detailed model in COBRA/TRAC for this?

DR. GAGNON: I'll let Dr. Kemper answer that question.

CHAIRMAN WALLIS: So NOTRUMP has no hot leg entrainment models. It just assumes that whatever quality goes in comes out or something like that?

DR. GAGNON: What is modeled in NOTRUMP is the pipe diameter that is attached to the hot leg is extended into the hot leg by that pipe diameter so it's rather arbitrary.

CHAIRMAN WALLIS: There's no change in quality or anything. Whatever comes along hot leg goes out there.

DR. GAGNON: Whatever comes along. It's a circular contact so whatever mixture is in there will determine what the flow quality will be as a function of level.

CHAIRMAN WALLIS: Now you think you have a more physical model than WCOBRA/TRAC?

DR. GAGNON: That's correct. We will be discussing that more in detail.

DR. BANERJEE: You're going to tell us the model in COBRA/TRAC later?

DR. GAGNON: That will be this afternoon, yes.

DR. BANERJEE: So how many nodes did you have for the core in rough terms?

MEMBER RANSOM: In the COBRA/TRAC model for this ADS-4 initiation phase there are four nodes in the core for this application.

DR. BANERJEE: And then the upper plenum?

DR. JENSEN: The upper plenum we actually looked at sensitivity to having one node there and three nodes there. The sensitivity studies we performed have three nodes in the upper plenum region.

DR. BANERJEE: Okay. And was this all 1-D or did you do some 3-D?

DR. JENSEN: There's no radial representation in here.

CHAIRMAN WALLIS: Okay. Please go on.

DR. GAGNON: The idea with the

supplementary COBRA/TRAC calculation was to demonstrate that the adjusted NOTRUMP model provides a conservative prediction of the IRWST injection phase. As part of, I believe -- is that WCAP 15883? Is that the entrainment?

COBRA/TRAC comparisons to the NOTRUMP models or NOTRUMP simulations were performed for the DEDVI line break inadvertent ADS case. What was shown was that COBRA/TRAC predicts a much higher entrainment rate through the ADS-4 flow pass than is predicted by NOTRUMP. We have some curves that will be demonstrated here shortly.

COBRA/TRAC also depressurizes much more rapidly basically because of NOTRUMP's break flow blending model that restricts flow as it approaches subsign.

CHAIRMAN WALLIS: How does it predict much greater entrainment? I thought that NOTRUMP sort of assumed what goes in comes out so there's no mechanism for de-entrainment.

DR. GAGNON: Only when it gets into contact with the pipe elevation. As the mixture level stays below, the contact elevation --

CHAIRMAN WALLIS: Gives no entrainment at all. Okay. Until you take this pipe and stick it in a little bit further.

DR. GAGNON: Stick it in kind of like circular sideways so as the mixture level approaches the contact point, it begins to entrain or carry out liquid.

DR. BANERJEE: Now, I just want to understand the physics of this. When you take steam out of a break, the pressure goes down quicker. Right?

DR. GAGNON: Um-hum.

DR. BANERJEE: You take liquid out, it takes the mass out and keeps the pressure out. So if COBRA/TRAC takes out liquid and NOTRUMP doesn't, why does COBRA/TRAC depressurize more rapidly?

MEMBER RANSOM: COBRA/TRAC actually takes out roughly the same amount of steam but it's taking out a lot of liquid with it.

CHAIRMAN WALLIS: That should keep the pressure up because the two-phase pressure drops most greater than for the steam alone.

DR. JENSEN: Well, this goes back to NOTRUMP has a very conservative modeling of the flow rate through the ADS-4 flow paths. Andy mentioned a blending model which is known to be highly conservative.

DR. BANERJEE: But didn't you adjust the NOTRUMP to a more realistic model by modeling this ADS line in more detail?

DR. GAGNON: Yes.

DR. BANERJEE: So what you were doing is you were looking at -- you mapped an inlet quality where you varied it from low to high quality. That, I presume, was the inlet of this ADS-4 line.

DR. GAGNON: Yes.

DR. BANERJEE: And then you lumped this thing into some gross behavior based on what the inlet quality was?

DR. GAGNON: Correct.

DR. BANERJEE: So for that line, at least, you had a realistic model. I don't understand the blending here. Where is the blending coming in?

DR. GAGNON: At the choke point which is at the valve. As it transitions from sonic to subsonic there is a splind fit that takes it to the orifice equation.

DR. BANERJEE: Right. I mean, both Graham's and my point is that if you have two-phase flow, you should get a bigger pressure drop in general. Therefore, I don't understand why. The physics doesn't work for me.

CHAIRMAN WALLIS: It doesn't matter. It's an approved code.

DR. BANERJEE: Even if it is approved, I still have to understand the physics. Why does COBRA/TRAC depressurize more rapidly? It doesn't make any sense to me.

DR. GAGNON: I have no --

MEMBER RANSOM: COBRA/TRAC does have the physics and the representation of the hot legs and the ADS-4 flow paths. With that modeling, the steam flow rate predicted is comparable to that, or exceeds that of NOTRUMP enabling you to depressurize. You also are taking the liquid out in the COBRA/TRAC analysis with its entrainment modeling.

DR. BANERJEE: Right. So let's go back again. Either COBRA/TRAC has more steam coming out than NOTRUMP, in which case it is understandable why depressurization should be more rapid. Or there is some mechanism operating that I, for one, don't understand. So does COBRA/TRAC take out more steam?

DR. JENSEN: Well, the steam flows are about equivalent.

DR. BANERJEE: Then why should it depressurize more rapidly? If you just do a mass balance around the system with the pressure, just lump the whole thing together, it should take out the same amount of steam to a first approximation and the pressure should stay the same.

DR. JENSEN: Now, I think one thing is we are moving energy from the system and the liquid as well.

DR. BANERJEE: Right.

DR. JENSEN: Because this is saturated liquid and there is a significant energy removal occurring with the liquid.

DR. BANERJEE: Okay. That could explain part of it, but then the pressure drop also should go up because you're removing the liquid. So the back pressure -- I just don't understand why this should depressurize.

CHAIRMAN WALLIS: Comparable steam flows and when you put water in, the pressure has got to be higher.

DR. BANERJEE: Right.

MR. CORLETTI: This is Mike Corletti. The resistance in the NOTRUMP ADS-4 line has been artificially increased so you don't have this increased resistance in the COBRA/TRAC calculation. We have the actual resistance. In the NOTRUMP calculation we have an increased resistance in the

ADS-4 line.

CHAIRMAN WALLIS: I think it's the ADS-4 valve.

MEMBER RANSOM: Well, if you look ahead to the plot -- you've got a plot, I think, in the next slide.

CHAIRMAN WALLIS: Maybe we should move and look at the plot.

MEMBER RANSOM: It shows them depressurizing at about the same rate but there's a transition which occurs at a different point which I assume must be the transition from sonic flow to subcritical.

CHAIRMAN WALLIS: Can you explain these curves here? Why are there three curves or four curves?

DR. GAGNON: Well, this is just the low pressure side.

CHAIRMAN WALLIS: It's magnified?

DR. GAGNON: Yes.

MEMBER RANSOM: One reads to the right and the other reads to the left.

DR. GAGNON: There's an overlay. This is the high pressure phase and this is the low pressure.

CHAIRMAN WALLIS: One only stops at --

DR. GAGNON: One starts at ADS-4.

MEMBER RANSOM: This is what you call a window calculation, I guess.

DR. GAGNON: Right.

DR. BANERJEE: Again, I missed the explanation. Could you please repeat it? What is blocked here?

DR. GAGNON: This side represents the high pressure phase, this curve here. I should label left side. This is the low-pressure phase and you can see -- I mean, there's an overlap of NOTRUMP and COBRA/TRAC. This is NOTRUMP and this is COBRA/TRAC.

CHAIRMAN WALLIS: Starting COBRA/TRAC at 500 seconds.

DR. GAGNON: At ADS-4.

CHAIRMAN WALLIS: It looks as if they are doing the same thing until --

MEMBER RANSOM: They are just transitioning to a different point.

CHAIRMAN WALLIS: Oh, I see. So they are pretty close we would say until --

MEMBER RANSOM: Yeah. They are pretty close until you get to the region where the NOTRUMP blending transition model kicks in.

DR. BANERJEE: That keeps the pressure

higher?

DR. GAGNON: Um-hum.

DR. BANERJEE: So that's the top curve?

DR. GAGNON: That's the top curve, yes.

DR. BANERJEE: That's the NOTRUMP.

MEMBER RANSOM: I guess is that a transition from choking to unchoke flow, I guess. That knee and the curve.

CHAIRMAN WALLIS: Is that what it is?

DR. GAGNON: Yeah, it has to be.

DR. BANERJEE: So why does the transition occur lower in COBRA/TRAC since you are actually using a detailed model for the ADS-4 line and NOTRUMP? The methodology you explained was you --

DR. GAGNON: Adjust the NOTRUMP, yes.

DR. BANERJEE: You have a very detailed -- how many nodes did you say, 18 nodes or 100 nodes?

DR. GAGNON: 440.

DR. BANERJEE: 440.

DR. GAGNON: 400 and some odd.

DR. BANERJEE: So that's a good calculation we think. Right? Why is it different from COBRA/TRAC?

DR. KEMPER: Well, I think, isn't it the blending model, Andy?

DR. GAGNON: The blending model is only on for a short duration and then it's transitioned.

DR. KEMPER: Maybe it hasn't been brought out. In doing the COBRA/TRAC calculation here we are trying to get -- what we've done is try to get a handle on the better estimate of the performance of the system and using the entrainment modeling present in that code.

NOTRUMP is intended to be a conservative calculation for licensing purposes so what Andy has for his resistances are bounding resistances according to the plant design parameters. The COBRA/TRAC calculation is based on expected or nominal resistance in the ADS-4 flow paths. That might explain the question you raised before about pressure.

CHAIRMAN WALLIS: Can we move on to some of the other predictions here? We've spent forever on this one. I think we need the whole picture.

DR. BANERJEE: Then we can come back to this.

CHAIRMAN WALLIS: Then we can come back to this one if you want to. We can't spend all day. This one is so dramatic you're losing a lot more water than the other case.

DR. GAGNON: That's correct.

CHAIRMAN WALLIS: The number we're losing here, the difference is something like 50,000 pounds and 800 seconds, the difference between the two predictions.

DR. GAGNON: Right. Roughly 50,000 pounds out of that path.

CHAIRMAN WALLIS: Then if we look at the core inventory, the one after this one --

DR. GAGNON: This is the other slide.

CHAIRMAN WALLIS: This is the other slide which, again, talks about tens of thousands of pounds of water difference. Yet, when you come to the vessel inventory, it does make much difference. What happened to this 50,000 pounds of water we lost with COBRA/TRAC? Where did it come from? Did it all get injected or something? Did more get injected?

DR. GAGNON: Well, yeah. It's getting IRWST injection much sooner than NOTRUMP is.

CHAIRMAN WALLIS: Ah. So another 30,000 pounds injected from somewhere but balance the extra we lost.

DR. GAGNON: Yes.

DR. BANERJEE: But then it becomes very critical to get the pressure right. Right?

CHAIRMAN WALLIS: Yes. If you had lost

all that water and then your pressure hadn't come down, you would be in real trouble.

DR. GAGNON: Right. It would be very uncovered.

CHAIRMAN WALLIS: So if we're saying that by losing that water the pressure should have stayed up. Then you would be in real trouble. I mean, if you're losing more water, generally a valve get blocked by the water so you lose less steam. It's hard to believe. It doesn't make sense in terms of our appreciation of the physics.

DR. BANERJEE: So you're back to that old curve.

CHAIRMAN WALLIS: Here we have another thing we don't understand.

DR. GAGNON: Is the break flow model going to be explained this afternoon? The COBRA/TRAC break flow model?

DR. KEMPER: That may be. You can maybe help me with some of this, Sandy. That may indeed be possible. This transient is actually in critical flow for the large majority of the COBRA/TRAC transient up until the time when you get -- certainly up to the time in which IRWST injection begins to occur.

The modeling there in COBRA/TRAC is, I'll call it, a small break LOCA type of critical flow model in which upstream conditions are used to identify the critical flow through a restriction such as an ADS-4 valve. That's the COBRA/TRAC critical flow model. NOTRUMP has its critical flow model and then it goes into this blending model and takes it from there.

DR. BANERJEE: When does the blending model operate in this? I don't fully get the idea of what the blending model is but is that written up somewhere?

DR. GAGNON: Yes. It's in WCAP 14807 Rev. 5.

DR. BANERJEE: 14?

DR. GAGNON: 14807 Rev. 5. It's section 2.13.

CHAIRMAN WALLIS: I don't know if we need to look at all these models. It's just the fact that if you've got all this extra water going out, the same amount of steam flow, you've got to have more pressure draw. You're saying there was something so artificial about NOTRUMP that we should really forget about it and just believe this other one, WCT.

DR. BANERJEE: I don't think you should reach that conclusion because they have made a very detailed model of the ADS-4 line from what you've said for NOTRUMP.

DR. GAGNON: Well, it's adjusted based on a detailed model.

DR. BANERJEE: But what is different between COBRA/TRAC and NOTRUMP would be quality at the entrance to that line. Right? Because, in fact, since you've got a detailed model for the ADS-4 line in NOTRUMP, it's probably doing better than COBRA/TRAC because COBRA/TRAC probably doesn't have a detailed model with 140 or 440 nodes. Right?

DR. GAGNON: I am not aware --

DR. BANERJEE: What am I to sort of conclude from this? That the model with your 440 nodes is probably pretty good. Right?

DR. GAGNON: Yes, I would have to say so.

DR. BANERJEE: Probably better than the COBRA/TRAC model. Yes or no?

DR. GAGNON: For that modeling I would have to say I would think it has to be.

DR. BANERJEE: Right. So the only issue then is the quality right of the inlet or not. Now, if there is more entrainment, which means that COBRA/TRAC has higher quality coming in -- lower quality coming in, you would expect a pressure drop and less steam to flow out.

Therefore, what is puzzling about the whole thing is why are you getting lower pressure in COBRA/TRAC and that's the whole thing that's allowing IRWST to come in earlier. The increased mass loss makes sense because of entrainment. You are feeding it with a lower quality of the inlet. But what doesn't make sense is why the pressure comes down faster. That's really the issue.

CHAIRMAN WALLIS: If we jump ahead to your slide 44, it's even more critical here. The pressure with one prediction at 2,000 is something like 400 psi. The other one is down to 100. Tremendous difference.

DR. GAGNON: Now, this is the right-hand scale. This is --

CHAIRMAN WALLIS: I'm sorry. Those two there. Okay. That's a big pressure compared with what the IRWST had so that's important. The pressure of psi there is -- oh, there's a false origin.

DR. BANERJEE: It's 30 and 25 or something.

CHAIRMAN WALLIS: It's a false origin. That's what confuses me.

DR. GAGNON: Yes.

CHAIRMAN WALLIS:  Okay.  Why did you do that?

DR. BANERJEE:  But still 5 psi is a few feet of water.  Right?

CHAIRMAN WALLIS:  So we're talking about 25 versus 35 or something.  But then if I look at the integrated water flow, though, and, say, figure 323 which goes with this, I've got a huge amount of water coming out with WCT and almost nothing with NOTRUMP.

DR. GAGNON:  Correct.  This is for the inadvertent EDS case.

CHAIRMAN WALLIS: These are both realistic codes?

DR. GAGNON:  Well, it was intended to be Appendix K based.  It's not best estimate.

CHAIRMAN WALLIS:  But it doesn't matter here.  They are both trying the model of physics.

DR. BANERJEE:  I guess the thing is very delicate.  If the pressure doesn't come down fast enough and you get water out, you hang up the pressure and then the IRWST didn't come in.  That was the balance that I remember was the issue in AP600 as well.

When we did some hand calculations we just used a homogeneous model for the discharge and it still didn't give a large -- you can do this problem by hand. In fact, we did it by hand just to make sure. It didn't give a large time of core unrecovery or anything.

It just went back if I remember that. Here it should be possible to do the same thing. The homogeneous equilibrium outflow is a very conservative outflow. It will tend to keep the pressure up and lose mass.

DR. GAGNON: That's what's being done with that detailed momentum flex model that uses the HCM model.

DR. BANERJEE: Right. In which case that is a believable sort of bound, if you like. What happens in that case? Does the core uncover?

DR. GAGNON: No, the core does not uncover.

DR. BANERJEE: I see.

CHAIRMAN WALLIS: Is that the NOTRUMP?

DR. GAGNON: It calibrates that factor.

DR. BANERJEE: I see.

DR. GAGNON: IRWST injection is delayed. There's a veritable injection gap between CMT and IRWST but it is smaller than what was predicted for AP600. The AP1000 design has shortened that injection gap period.

CHAIRMAN WALLIS: Usually you show you make different assumptions. Yes, the pressure stays up more or you lose more mass but the actual core uncovery is okay. That's one thing I've seen in the past. Here you seem to have a problem where you're losing mass after keeping the pressure up.

DR. GAGNON: The pressure is coming down in this case.

CHAIRMAN WALLIS: In this case it's okay then. Okay. That's right. One is compensating for the other and we're saying how can that be because of the characteristics of the two-phrase flow through the valve. That's right.

DR. GAGNON: And to --

CHAIRMAN WALLIS: This is a key part. The key part of AP600 and AP1000, the whole key part of this passive system is you've got to depressurize the IRWST. You've got to depressurize without losing too much mass. The whole key to the operation of the system.

DR. GAGNON: Correct.

CHAIRMAN WALLIS: It looks here as if you've got such tremendous changes when you change the codes that we wonder how much reliance we can put on the results.

MR. CORLETTI: There are other differences in the calculations as well as just the differences in the codes. Maybe one was done with a conservative decay versus the 79 decay heat.

DR. KEMPER: No.

MR. CORLETTI: Are they the same?

DR. KEMPER: They both have decay heat. The ADS-4 resistance is a nominal number in COBRA/TRAC, whereas it's bounded in NOTRUMP. I think the main difference is probably the WCOBRA/TRAC prediction. It's critical flow or choke flow all the way until IRWST injection occurs. The choke flow model is really the main actor in terms of the WCOBRA/TRAC prediction.

CHAIRMAN WALLIS: Do we have any staff prediction to put on this plot? Has the staff made an independent calculation of some of these transients?

DR. JENSEN: Yes. The staff has calculated a lot with these transients. I didn't bring a plot of the pressure versus time but in general RELAP will depressurize faster than NOTRUMP and the IRWST injection occurs then much earlier than NOTRUMP predicts.

CHAIRMAN WALLIS: So RELAP probably loses more water but gets the pressure down so IRWST comes on.

DR. JENSEN: Well, the way I see this, Dr. Wallis, the way it seems to me the pressure coming down quickly causes the IRWST to inject earlier putting more water in the core. The water then flows to the core and out into the upper plenum and out of the hot legs and out of the ADS-4.

Because the IRWST flow is greater, then this causes more water to be, in effect, pumped with the ADS-4. With the IRWST it's the driving force giving the water and that's the reason there's more water in the ADS-4 for WCOBRA/TRAC than there is for NOTRUMP because there's just more water there.

CHAIRMAN WALLIS: As long as you've got pressure down enough so that the IRWST is injecting.

DR. JENSEN: Yes, sir. That's important.

CHAIRMAN WALLIS: What is that pressure level where it begins to inject? Can we put that somehow on these figures?

DR. GAGNON: For -- I don't remember what it is. I think it's around 28 psi.

CHAIRMAN WALLIS: 28?

DR. GAGNON: I believe.

CHAIRMAN WALLIS: So it's right between

these two predictions here. One of them is predicting that you get a lot IRWST. The other one is predicting that you get none of it.

DR. GAGNON: Until much later in time.

CHAIRMAN WALLIS: Until much later. It seems to me this is a case where I would think that the staff would have to run a lot of its own calculations because there's so much lack of certainty here.

This is really a case where the staff ought to be running your codes since it appears you can tweak the codes by putting in various assumptions about whatever mixing or how long the pipe is that you stick in from the side and so on.

DR. BANERJEE: Let me ask the question about the NOTRUMP. The 440 node calculation for the ADS-4 line, was that assuming homogenous flow in that line?

DR. GAGNON: Yes. They also looked at the impact of slip and homogeneous was determined to be the most restricted.

CHAIRMAN WALLIS: Well, I should include as a member of the technically informed public, I've got three calculations. I've got WCT, I've got NOTRUMP, and I've got RELAP. It's clear the difficulty modeling with physics because they all predict quite different things in this particular time period. Yet, the answer in terms of vessel inventory and core uncovery is sort of the same.

If I had three codes which are all very poor approximations to the real physics, yet the answers say it's safe, does that give me a good feeling or not? I would like to have a code which is a good approximation of real physics really if I'm going to make a decision. I'm not quite sure where I am and these three codes have very different predictions.

DR. BANERJEE: Well, one way would be to keep the pressure from NOTRUMP and the mass loss from --

CHAIRMAN WALLIS: You could do that. You could probably put in enough assumptions to make that happen. You could take the worse case from everything. Take the worse part of the RELAP code, too, and use that and still show that the mass inventory is okay.

DR. GAGNON: We actually sensitivities in AP600 where we played around with that contact diameter to have entrainment anytime there was a level in the hot leg.

CHAIRMAN WALLIS: I've followed AP600 not from this committee but from outside and what concerned me was that the mass vessel inventory curves evolved over time as the codes were -- something was done with the codes so the thing did better and better as time went on.

DR. BANERJEE: Did you do any comparisons with ROSA at this phase?

DR. GAGNON: No.

DR. BANERJEE: Because in the AP600, I don't know if Westinghouse did any comparisons, but AP600 the ROSA results were really the best scaled for this phase between IRWST and ADS-4. If you didn't do any, did the staff do any which were relevant to this calculation?

DR. GAGNON: They did that. I believe they benchmarked.

DR. BANERJEE: Did you benchmark things against ROSA for this case?

DR. JENSEN: For AP600 the staff did benchmark RELAP against ROSA so we did.

DR. BANERJEE: But the problem if I recall, was that RELAP went into some vicious oscillations and nothing useful came out of it. Am I right or wrong on that?

DR. JENSEN: I looked at those reports. I don't remember any vicious oscillations but I think the code ran.

DR. BANERJEE: What happened was that there was an oscillation due to the low pressure and there was a vaporization flip-flop that was going on which didn't allow a stable calculation or, if there was one, it was hard to believe. Maybe Steve could comment or somebody else on this.

DR. BAJOREK: That was well before my time with the staff. Over the break I can go up. There is an adequacy report that was done for RELAP in comparison to numerous experiments. I think it was SPES, ROSA, and APEX. I can find some of that out but I don't remember.

CHAIRMAN WALLIS: Let's look back at the big picture here. With the old PWRs we had more active systems working. This is supposed to be a better design because it's now passive. Nature takes care of it. Yet, I don't think with the old PWRs you've got such tremendous differences in predictions depending on which code you use.

It seems to me there's some uncertainty in modeling the physics. It's becoming much more important with these passive designs so that going to a passive design buys you something. Then the gravity is always going to be switched off. You've now got to be much more careful about how you analyze what's happening.

DR. BANERJEE: In general with emergency relief systems of this type, which is essentially what you have, lack of vapor disengagement gives you the worse scenario. Now, this is basically like a chemical plant so it behaves the same way. If you don't disengage the vapor, you get the worse pressure because --

CHAIRMAN WALLIS: You take out a lot of mass.

DR. BANERJEE: Yeah, take out a lot of mass.

CHAIRMAN WALLIS: Keep the pressure up.

DR. BANERJEE: NOTRUMP is more or less doing that.

CHAIRMAN WALLIS: And homogeneous models were even worse. There is some HCM in homogeneous, you said.

DR. BANERJEE: So that --

CHAIRMAN WALLIS: It's worse except it has this anomaly about water flow.

DR. BANERJEE: That has to be resolved.

Assuming that can be resolved, that sort of gives you a bound of pressure.

CHAIRMAN WALLIS: Well, it seems to me that the arguments have to be better presented then.

MEMBER KRESS: Am I correct in remembering that your Chapter 15 analysis did not invoke any of your active systems?

DR. GAGNON: That's correct.

MEMBER KRESS: It was all just passive. In reality you would have active systems that you would turn on under these circumstances?

DR. GAGNON: Yes, we did.

MEMBER KRESS: Just not taking credit.

DR. GAGNON: We don't take credit for those. We look at those in the PRA.

MEMBER KRESS: They are part of the PRA because it's reality and PRAs are supposed to be reality.

DR. GAGNON: They are designed to complement. Actually, sometimes they are the first level of defense, or core makeup tanks which are high pressure injection. We have makeup pumps that are very much like the high-head injection pumps.

CHAIRMAN WALLIS: Where are we in your presentation? I see in the overall schedule it says large-break LOCA, small-break LOCA and containment, and then we go to lunch. There seems to be a lot more stuff.

DR. GAGNON: There's a lot of simulations of comparisons between AP600 and AP1000 for various simulations such as 2-inch cold leg break D, DVI, and 10-inch cold leg break.

CHAIRMAN WALLIS: That was supposed to have been gone through this morning?

MR. CORLETTI: This is information that's in the Chapter 15 of the DCD. I don't know that we have a specific issue with it. We were providing it for your information.

CHAIRMAN WALLIS: I just wondered for anything in particular we ought to focus on in that.

DR. GAGNON: I don't believe there is anything.

CHAIRMAN WALLIS: I think the thing that concerns me is you've got two things we've focused on and we had a lot of questions about them. Do you folks have anything else that we are likely to have a lot of questions?

DR. BANERJEE: Noncondensables.

CHAIRMAN WALLIS: Well, AP600 results are presumably going to look like AP1000. Is there any place where there is a significant difference?

DR. GAGNON: No.

CHAIRMAN WALLIS: ADS stage 4 integrated flows?

DR. GAGNON: ADS-4's size is considerably larger for AP1000.

CHAIRMAN WALLIS: All right.

DR. GAGNON: You would expect it to behave --

CHAIRMAN WALLIS: Expect it to be different.

DR. GAGNON: Therefore, IRWST is actually coming on earlier for AP1000 than it did for --

CHAIRMAN WALLIS: Injection line mass flows there's a bigger pipe?

DR. GAGNON: That's correct. The resistances have been -- resistances and line sizes have been changed.

CHAIRMAN WALLIS: So all of those things are what you would expect.

DR. GAGNON: Correct.

MR. CORLETTI: Perhaps when the staff makes their presentation if any issues come out of that in covering this subject area, we could come back to this. I think in general we see this as issues that aren't -- we don't see issues here and this is pretty much what we first presented in the DCD.

CHAIRMAN WALLIS: So this package I'm looking through here, is this what you intended to go through this morning?

DR. GAGNON: Yes, sir.

CHAIRMAN WALLIS: That's it?

DR. GAGNON: Yes, sir.

CHAIRMAN WALLIS: Then there will be another package this afternoon?

DR. GAGNON: Yes, sir.

CHAIRMAN WALLIS: Okay. Can you just sort of flip through this by, say, 12:30 or something so we can then go to lunch then or is it best to take a break now? Maybe it's best to take a break now. We can come back and flip through this ourselves and decide if we want to ask you anything about anything else.

DR. BANERJEE: I just have one question. If you normalize the ADS outflows by power do they look about the same? This plant is roughly 1,100 megawatts electric versus 600 megawatts for the other plant. Do the ADS outflows look about the same in that ratio?

MR. CORLETTI: Yeah, we have done

comparisons with the ADS-4 a size larger than AP600 on the power basis.

CHAIRMAN WALLIS: It looks about the same ratio.

MR. CORLETTI: The area of the power ratio, I think, is larger for AP1000.

DR. GAGNON: That's described in the --

CHAIRMAN WALLIS: And the same with the DVI line flow rate? Is that about the same ratio there? It looks like it if I could find it again. The injection line mass flow.

MR. CORLETTI: There's a difference between the size of the pipes and the actual performance in the transient. When we sized the pipes, we tried to size them larger on the power basis. In transient behavior it doesn't always -- it's not always the same because pictures are different, temperatures are different.

DR. BANERJEE: So that's for the DVI line. What about the piping and the resistances after the core? How do they scale to the ADS-4 line?

DR. GAGNON: From like the top of the core into the hot leg?

DR. BANERJEE: Through the hot leg to the ADS-4.

MR. CORLETTI: We have higher velocities in our hot leg. We did not change the hot leg diameter so it is the same

DR. GAGNON: The upper internals --

MR. CORLETTI: The upper internals tend to be the same. Part of the reason why the entrainment issue is the steam velocity is higher for AP1000 than AP600.

DR. BANERJEE: By a factor of 2 roughly there.

MR. CORLETTI: 1.75.

CHAIRMAN WALLIS: So it's a higher power level but when we look at something like system mass inventory, we should be thinking is it about the same vessel?

MR. CORLETTI: The vessel is larger because we made --

CHAIRMAN WALLIS: So you would expect the mass inventory to be higher. In fact, it's lower.

DR. GAGNON: AP1000 should be higher.

CHAIRMAN WALLIS: Well, not in, say, slide 69. The AP600 system inventory is higher.

DR. BANERJEE: So the vessel volume is dropped to the same. Is that it?

MEMBER KRESS: Yeah, but the steam

generator is bigger.

MR. CORLETTI: Pressurizers.

DR. GAGNON: Right. This system inventory curve is more than just --

CHAIRMAN WALLIS: Slide 69. Even so, you would expect 1000 to have more water in it.

MEMBER KRESS: Look at times zero.

DR. GAGNON: Times zero AP1000 does have more water.

CHAIRMAN WALLIS: Yeah, but between 1,000 and 3,000 it has less water.

MEMBER KRESS: Yeah, but that's the dynamic.

CHAIRMAN WALLIS: Well, I think we're going to take a lunch break and we'll come back and ask questions about this. We probably need to hear something about containment from you after lunch since there were some questions raised about that by one of our members. I don't know if he's going to be here or not.

MR. CORLETTI: I was told he wasn't going to be here.

CHAIRMAN WALLIS: I fear he's vanished.

MR. CORLETTI: We have the answers to his questions in our presentation material.

CHAIRMAN WALLIS: Anyway, I think we are saturated with what we've been doing this morning and it's time to take a break. Take a break until 1:15 and then we'll continue.

(Whereupon, at 12:21 p.m. off the record for lunch to reconvene at 1:15 p.m.)

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

1:20 p.m.

CHAIRMAN WALLIS: Are there any questions? I wasn't sure I could find anything here which was an issue that we need to spend some time with, unless my colleagues want to pick up anything between where we left off and Slide 82. We have a little time to look at these if you haven't done so before. Any questions we want to raise on any of these matters or can we go right to the containment?

Of course, no new phenomena were observed, because you put no new phenomena into the analysis. It's really not a very good conclusion.

DR. CUMMINS: I think sometimes -- calculate flow regimes that are suggestive of phenomena.

CHAIRMAN WALLIS: What you really mean is no new sort of events. Phenomena, to me, means slug flow or any other flow or condensation. They are the same phenomena. They are assumed. It's just that there are no new surprises in the outputs from the code.

DR. CORLETTI: That's true.

CHAIRMAN WALLIS: So can we move on then?

DR. CORLETTI: Yes. Our next speaker is

Rick Wright. He is going to talk about the containment analysis.

CHAIRMAN WALLIS: We will start with Slide 84, and we will continue at this pace, since you've done 36 slides in five minutes.

DR. WRIGHT: Good afternoon. My name is Rick Wright, and I work for Passive Plant Engineering on the AP-1000. Before that, I worked on AP-600, and I am going to talk about the containment analysis work.

From the pre-certification review, the open item we had was that Westinghouse needs to perform containment analysis with evaluation model, appropriate bonding conditions to ensure that the mass and heat transfer correlations remain valid for the AP-1000 design.

As a result of this, we issued these two reports. One was the AP-1000 containment evaluation model, and then the DCD analysis, which shows how the analysis was done.

CHAIRMAN WALLIS: How can you show that mass and heat transfer correlations remained valid by doing an evaluation model?

DR. WRIGHT: Okay. What we can do, and I'll show a Vu-Graph a little bit later on, is to take a look at the range dimensionless parameters that were studied in the test program and show that the analysis results give dimensionless parameters that are within the range of the test data.

CHAIRMAN WALLIS: Next?

DR. WRIGHT: Okay. This is the noding diagram -- would you say it looked like a milk churn -- for the AP-600. Basically, the differences between AP-600 and the AP-1000: The containment diameter is the same. The height has been has been increased about 25 1/2 feet, and the change in the nodalization was to add one extra layer of nodes and to increase the air flow paths on the outside by one node on the downcomer side and on the riser side.

CHAIRMAN WALLIS: Now these are cylindrically symmetric?

DR. WRIGHT: Cylindrically symmetric, that's right. It's an actually symmetric model. The nodalization -- this is done with lump parameter nodes, which are -- Basically, they are nodes with flow paths. Okay?

GOTHIC has the capability of doing distributed parameter or, when we did the sensitivity studies in AP-600, we found that the results were similar between the lump parameter and the distributed parameter, and the lump parameter is a lot more efficient to run a lot of cases with. So, basically, what we did was to do the lump parameter for both AP-600 and AP-1000.

CHAIRMAN WALLIS: If I have a plume or something in here, I don't really see why the flow path should have any axis symmetry. I think it might well be a turnover with the flow going up one side and coming down the other.

DR. WRIGHT: I'm sorry, Dr. Wallis. I misunderstood. This is not actually a symmetric model. It's a three-dimensional model, but --

CHAIRMAN WALLIS: So there are nodes in the other dimensions?

DR. WRIGHT: That's exactly right. This is just looking at it in 2-D. But if you look at it, it is symmetric the way the nodalization is. But if you have, you know, your releases from this node here --

CHAIRMAN WALLIS: So there are 12 pieces of pie or something?

DR. WRIGHT: Basically, yes, that's exactly right.

MEMBER RANSOM: How many circumferential nodes are there then?

DR. WRIGHT: I think this one has eight. I'm sorry, I'm messing you up here. What should I be hitting when I do this? Okay, good.

Yes, there's eight circumferential nodes. Basically, what these are corresponding to is that there are wet and dry sections on the outside of the containment wall. So we have an equal number of wet and dry nodes that are connected to nodes on the inside of the containment wall.

So, basically, by going with eight, we have four and four all the way around. So you get a certain amount of, you know, this symmetry from that.

MEMBER RANSOM: What do you mean, eight wet and dry? You mean that the fall over the outside doesn't cover the entire --

DR. WRIGHT: That's right. There is the provision for putting on water at different flow rates. For very high flow rates, you can get up to 90 percent. Well, we credit 90 percent, but actually the test showed 100 percent coverage. At lower flow rates, you get less coverage.

The result of that is we have to have the capability in the code to model both the dry heat transfer and the wet heat transfer on the outside of the containment shell.

Anyway, the next step is to get the bonding conditions right, and to do that we calculated the mass and energy releases for both the main steam line break and for the LOCA. The main steam line break -- we used a code called LOFTRAN to look at double-ended steam line ruptures at different power levels, and we found that 30 percent was the limiting power level, if you looked at the integrated energy out of the steam line.

The LOCA releases are calculated for both the double-ended hotleg break and double-ended cold leg break, and assumed at 101 percent power. The methodology is the same as what is described in WCAP-15846.

Due to the larger RCS and steam generator volumes, the energy was released at a different rate. It takes a little longer to release the energy from the RCS metal and the steam generator than it did for AP-600. So probably the only difference between the methodology for the LOCA M&Es is this change in timing for the release of the energy from the steam generators and from the RCS metal.

As a check, we did a comparison to WCOBRA/TRAC where we ran WCOBRA/TRAC out to see what the mass and energy releases would be, and we are significantly higher in the methods that we used to get the LOCA mass and energy releases than what COBRA/TRAC says.

For the LOCA, which is really the only time that the water that is put on the outside of containment becomes important -- okay? -- for the LOCA calculations, we also do an iterative approach to determine what the evaporation limited PCS flow is. This is the flow that is put on the top of the containment dome, and it flows down the outside.

There was some concern that, if you put too much water on, then the water that runs off the bottom will take away heat. So to be conservative, what we did was to run a calculation, put all the water on from the design of the passive containment cooling system tank, and got the answers, used the wet evaporative heat flux to come up with what the maximum amount of water that could be evaporated is.

In the case at the beginning of the event where you are putting on the most water, a lot of times it's a lot less water that can be evaporated than what you are putting on. In other words, a lot more of it is coming off the bottom.

So we do an iteration to change the water application rate to only put on enough water so that everything is evaporated by the time it gets to the bottom. What it results in us having to do is a couple of these WGOTHIC runs, and that was part of the design certification process for AP-600.

There was some question as to whether or not the code could handle water running off. So in order to take that out of the play, we conservatively reduced the amount of water we applied to only that that is going to be evaporated.

Now late in the transient after the peak is reached for the LOCA, we cut the water down. For the longer term, there's less water, and it is accomplished by standpipes in the tank at the top of the containment.

For the case where there is less water being put on, generally we don't have to throw any water away, because it all evaporates by the time it gets to the bottom.

CHAIRMAN WALLIS: This is only for a large break LOCA?

DR. WRIGHT: That's correct. This is the only time it really comes into play. The other events are more of an adiabatic flow-down, and the peak is reached very early and then drops off.

MEMBER RANSOM: Well, what is done on the actual situation? How is the water distribution controlled?

DR. WRIGHT: Basically, there is a big tank on top of the shield building, and inside that tank are standpipes. When you first open the isolation valve, the maximum amount of water, both from the head of the water inside the tank and the fact that all the standpipes are contributing, is dumped on top of the containment.

Now that goes on for about the first 5,000 seconds, I think it is. I'll have to look and see. I haven't looked at the PCS for a while. But after that time, you have already reached your peak.

Usually for the peak pressure we find it is about anywhere from 1,000 to 1500 seconds after the initiation of a large cold leg break. Okay? Then we cut down the amount of water we have to -- What happens is the water comes down. Obviously, your head drops off, but then you start to uncover these standpipes, and you get less flow. So you get these step changes that occur.

MEMBER RANSOM: So this is all pre-programmed then?

DR. WRIGHT: Yes.

MEMBER RANSOM: Just one valve that you

open .

DR. WRIGHT: You don't do anything. You just walk away. There's provisions after so many days to be able to pipe water back up to the top so that you can, you know, cool indefinitely.

CHAIRMAN WALLIS: So this comes on sometime after the LOCA is initiated?

DR. WRIGHT: That's right. There is a high pressure signal in containment, and that causes a valve to -- isolation valve to open, and it just pretty much does it all by itself.

CHAIRMAN WALLIS: That's what is happening here, something like this peak here?

DR. WRIGHT: Yes. That's exactly right. In this particular case, this is the containment response for the main steam line break. What we found when we did the tests for AP-600 was that there was a time delay between when we got the signal and when we got fully developed flow on the outside of the containment.

Very conservatively, we take that entire time delay and say there is no water at all until we get to the point where we know we have fully developed flow on the outside.

So for the case of steam line break, the peak occurs before you even get a real effect from the water cooling on the outside. So the thing that really mitigates the steam line break is the containment volume and the heat sinks inside containment.

When we did the design for AP-1000, we used the team line break as the limiting case and basically designed the volume to give us the --

CHAIRMAN WALLIS: So what turns this around?

DR. WRIGHT: What turns this around is the fact that there is only so much energy that you can release. The decay heat doesn't come out during the main steam line break. Basically, what happens is you get the blowdown of the secondary side and the steam generators. When that is gone, really there is nothing else left, and you have the decay off.

Now this rate of decay is determined by the water put on the outside. If you didn't have water on the outside, it would still decay, but it would come down at less of a slope.

CHAIRMAN WALLIS: So you don't really need this water on the outside --

DR. WRIGHT: Not for the steam line break. We've done calculations that show that for a steam line break and for the hotleg break that we can use air cooling, and it works just fine.

The more interesting one is cold leg break, and for this case what you find is that there is a small peak associated with the initial blowdown. You can't see it really well because of the -- You can just see a little bit of a job right there in the log scale, but the second peak is the one that occurs from the release of energy in the primary system, both the steam generator energy and the energy from the RCS metal mask.

For this case, we wind up with a peak pressure of 55.4 psig and, if you compare it to the steam line, the steam line is the limiting case.

Like I said, we ran the same case with the WCOBRA/TRAC M&Es, and we found that the peak was far lower for those M&Es. So we think the methodology we are using for the mass and energy releases for the LOCA is very, very conservative.

CHAIRMAN WALLIS: Now what is the mechanism of heat transfer to this water that is flowing down the outside? Is it actually boiling or is it evaporating?

DR. WRIGHT: No, it's evaporating. The temperature of the shell where it's wetted is always below 212.

CHAIRMAN WALLIS: Below the boiling point.

DR. WRIGHT: Yes.

CHAIRMAN WALLIS: Atmospheric pressure.

DR. WRIGHT: That's right. This is the containment response for the hotleg break, and it looks a lot like the -- You can see, the time scale is very, very short here. We reached the peak just as the blowdown occurs, and then it's just a long decay-off after.

DR. BANERJEE: The evaporation goes into an air stream?

DR. WRIGHT: That's right. That's right. Basically, what happens is that there is a buoyancy induced flow there coming up this annulus just from the fact it's being heated up, and also it's gaining water vapor from the evaporation, and the combination of the air cooling on the places where there is no water film and the evaporation, which is primarily the biggest source of heat removal -- those two things combine to give you the total energy that is dumped to the environment.

DR. BANERJEE; So how do you calculate the evaporation rate? Is that based on a mass transfer coefficient?

DR. WRIGHT: Yes. It's a Reynolds analogy. I have the correlations. One of the things Dr. Powers asked us to bring were the heat transfer correlations that are used in the annulus for the air flow, and then what is used as a Reynolds analogy to come up with what the mass transfer is. So --

In answering your question earlier, these are the dimensionless parameters that were -- tried to scale these to get the correct test condition, so that when we were able to compare the test results to the WGOTHIC results back in AP-600, we were able to cover off the range of dimensionless parameters.

You can see that, for the Reynolds number/Grashof number Prandtl number, we were within the range of the best data that we used for --

CHAIRMAN WALLIS: Now what do you mean by riser and downcomer in this context?

DR. WRIGHT: Okay. There is basically -- The way this works, the inlets are around the top of the building. So the air actually comes in here, goes down a downcomer portion which is -- Really, it's not heated, but it is heated. In other words, there's heat transfer coming across radially.

CHAIRMAN WALLIS: So this is just for the air side.

DR. WRIGHT: That's right.

CHAIRMAN WALLIS: It's not talking about what's happening inside the --

DR. WRIGHT: That's exactly right. Basically --

CHAIRMAN WALLIS: -- talking about the inside containment?

DR. WRIGHT: Inside containment, we pretty much rely on the WGOTHIC correlations that are fairly well known. There's the condensation -- Really, the dominant mechanism for heat transfer on the inside is condensation of steam along the wall, and GOTHIC has the -- if I can remember, the Chen correlation, I think, is what we use. I forget offhand, Dr. Wallis, but --

CHAIRMAN WALLIS: Well, I assume you set up some circulation that is really -- It's really the Reynolds number that comes --

DR. WRIGHT: That's right.

CHAIRMAN WALLIS: -- the heat transfer, not the Grashof number.

DR. WRIGHT: Well, in a sense it is a natural circulation problem, but since the -- you know, it's a big building.

CHAIRMAN WALLIS: No, but it's just the

actual circulation of the -- the velocity of the air itself, not the local boundary air in the Grashof number that governs.

DR. WRIGHT: That's right. That's exactly right.

CHAIRMAN WALLIS: The Grashof number may get it started, but then --

DR. WRIGHT: Then it goes. Yes, as a matter of fact, when we did the testing for the AP-600, obviously, we didn't build a building that was 300 feet high. So we had to put a fan in the top of the air flow path in order to draw the air up at what we knew would be prototypic velocities, because there is just no way you could get that with natural convection.

CHAIRMAN WALLIS: It's a big chimney. What sort of velocity did you get?

DR. WRIGHT: On the order of about 12 feet per second inside the annulus. The outside is fairly wide, but the inside annulus is 12 inches. That's the distance between the air baffle and the containment shell. So I think, you know, all the calculations that we did and all the testing that we looked at, it was about 12 feet per second.

CHAIRMAN WALLIS: So this isn't enough to produce significant drag on the water?

DR. WRIGHT: No, I don't think that they saw much of that in the tests that were done. We did the test with a plexiglass baffle. So you could actually stand there and watch and see what was happening to the water film, and it didn't seem like it did much -- It didn't strip off very much.

DR. BANERJEE: So most of the heat is really going into the evaporated water.

DR. WRIGHT: Yes.

DR. BANERJEE: To the latent heat of vaporization.

DR. WRIGHT: Yes, exactly. That's exactly right.

DR. BANERJEE: Otherwise, the velocity is too low.

DR. WRIGHT: Right. The velocity is too low. AS a matter of fact, if we -- We did calculations where we assumed that we didn't have the water available, and we get much higher flows, much higher velocities.

MEMBER KRESS: So is your annulus partition in the circumferential direction at all?

DR. WRIGHT: No, not at all.

MEMBER KRESS: It's an annulus all the way around?

DR. WRIGHT:  It's an annulus all the way around.  There are supports that provide the stand-off between the baffle and the containment, but there is no partitions *per se*.

MEMBER KRESS:  So if you had it dry, and you built out these high air velocities, you might have some more trouble putting the water in, because it would blow away as you try to get into the annulus.

DR. WRIGHT:  Well, actually, the way it works is that the water is applied right on the very top of the dome.

MEMBER KRESS:  The water is already there.

DR. WRIGHT:  There's a big bucket on top of the dome, and it's allowed to fill up that bucket and overflow, and at two points around the top of the dome there are weirs that redistribute the water, because if you pour all the water on one side, it may all just go down one side.  So they have these weirs set up to redistribute the water.

So by the time you get past the spring-line of the dome, it's fairly uniform distribution.

MEMBER KRESS:  But the air is going all the time.

DR. WRIGHT:  The air would be going all

the time, except that -- I think his question was, if you had a situation where you couldn't get the water on and you got the air go on really, really good --

MEMBER KRESS: His question is the air is going all the time, but the containment is dry. So I suppose they go up there pretty fast.

DR. WRIGHT: Well, no. Let's consider the case where you have a -- an accident occurs. You have a break inside containment. Okay? You have a high pressure signal. That opens up your water. Nothing is heated up yet. I mean, the --

MEMBER KRESS: Containment is pretty cold.

DR. WRIGHT: It's cold. It's cold, and nothing has actually happened. So you basically have, you know, a good flow of water going on, and then slowly the temperature of the containment shell comes up until you get to the point where --

CHAIRMAN WALLIS: I was just asking a hypothetical question. If you had it dry and turned the water on later, you might have more trouble getting it to flow down.

DR. WRIGHT: I would say that might be true, except for the way they put the water on. They have a pipe that comes straight down into a bucket on the very top. So what you are saying is true. The air would be coming up through this annulus pretty quick and would basically bypass the top of the dome and go straight up into the chimney.

CHAIRMAN WALLIS: It might make a pool.

DR. BANERJEE: You would have a CCFL on a grand scale.

DR. WRIGHT: Grand scale, that's right. The other thing, too, is you --

MEMBER KRESS: Three-dimensional effects. That's why I asked the question.

DR. WRIGHT: I wouldn't want to borrow it.

CHAIRMAN WALLIS: So if I drive by an AP-1000 and I see a big steam plume coming off the top, I know it's had a LOCA?

DR. WRIGHT: I would say you're probably right. Yeah, the tests that were run were pretty impressive when they would turn these thing s--

CHAIRMAN WALLIS: This figure on the front here have a big steam plume?

DR. WRIGHT: That's the cooling tower.

DR. BANERJEE; If the water didn't go on, is there some calculations to see if the air could remove all the heat?

DR. WRIGHT: Yes. What we found in AP-600 with air-only cooling, we were -- Obviously, we can't stay within the design pressure of containment, but we were able to stay within the ultimate yield strength of containment. So basically make the case that, for no water, we could still survive.

I don't know -- How did it end up, Mike, with AP-1000? I know it was touchy.

DR. CORLETTI: You should have stopped one sentence before that. That was good. That was a good answer.

DR. BANERJEE: He knew what the next question would be.

DR. CORLETTI: Seriously, that was what we showed in our PRA analysis. The way they do this, there's like a one percent probability that it would be exceeded, but it was, by and large, shown that it was --

DR. BANERJEE: For the AP-1000, which is -- what? -- less surface area for units of power, you would exceed the yield strength probably. Right?

DR. WRIGHT: I think that's what the calculation showed, yes, but it takes a long time.

DR. CORLETTI: WE have a thicker containment shell and higher design pressure.

DR. WRIGHT: Yes, the design pressure is higher, but I think the combination of the design pressure being higher wasn't quite enough. I mean, you've got to go out, you know, hours, 150 hours or something like that, before you creep up to the pressure where, you know, you would break the containment.

MEMBER SIEBER: What outside air temperature and humidity did you assume?

DR. WRIGHT: Well, on our design calculations, we assumed like 120 degrees, 120 degrees for the air temperature, 120 degrees for the water in the top of the --

MEMBER SIEBER: That's pretty hot.

DR. WRIGHT: That's hot.

MEMBER SIEBER: It's Texas.

DR. BANERJEE: So is it limited by the heat removal capacity of the air or the heat transfer coefficient?

DR. WRIGHT: That's a good question. I honestly don't know offhand. I'd have to look at --

CHAIRMAN WALLIS: I would think you would be limited by the air flow rate, just to carry it away.

DR. WRIGHT: Yes, that could very well --

CHAIRMAN WALLIS: It's a huge area for heat transfer.

DR. WRIGHT: Yes.

DR. BANERJEE: It's not obvious.

DR. WRIGHT: It's not. It's not. That's a good question.

MEMBER KRESS: You don't have any provisions to vent the containment, do you?

DR. CORLETTI: We do have provisions in our severe accident management strategies.

MEMBER KRESS: You have to open up a valve or something?

DR. CORLETTI: Yes.

DR. WRIGHT: As a design basis guy, I'm not allowed.

MEMBER KRESS: No, no, I understand. I understand.

CHAIRMAN WALLIS: How big is the annulus space the air goes through?

DR. WRIGHT: The annulus space is 12 inches wide.

CHAIRMAN WALLIS: Twelve inches wide?

DR. WRIGHT: One hundred thirty-five feet is the containment diameter.

CHAIRMAN WALLIS: So it's just 12 inches?

DR. WRIGHT: Twelve inches.

CHAIRMAN WALLIS: How much does the

containment swell when you pressurize it?

DR. WRIGHT: I don't know. Mike?

DR. CORLETTI; It's about one inch.

CHAIRMAN WALLIS: One inch?

DR. BANERJEE: The heat capacity of air.

CHAIRMAN WALLIS: But it's a big L over D. So it's going to be pretty much an equilibrium heat exchanger.

DR. WRIGHT: Okay.

MEMBER KRESS: Do you worry about this annulus being offset a little bit so it's narrower on one side than it is on the other?

DR. WRIGHT: I don't think at those low air velocities it would make all that much difference. I think, you know, it's still dominated by the evaporation. I guess if you could get it down to like one inch on one side and 23 inches on the other side, maybe that could be, you know, a limit. But personally, I've never done a calculation to see, but --

MEMBER KRESS: To see if the offset would affect the heat transfer much?

DR. WRIGHT: I don't really -- I can't see how it would.

CHAIRMAN WALLIS: So it's the buoyancy of the air that gets this flow going?

DR. WRIGHT: Yes, that's right.

CHAIRMAN WALLIS: And if you have Texas air at 120, and then you've got water which is, for some reason, not so hot, you could actually have the inside colder than the outside. If you had enough water cooling the -- for a while, you would have a slide backwards.

DR. WRIGHT: Well, in that case, though, you would still be heating the water.

CHAIRMAN WALLIS: Oh, you're heating the water, yes, but the air flow --

DR. WRIGHT: Right, but at some point that would turn around. It just depends on -- When you first turn the water on, the shell is still cold.

CHAIRMAN WALLIS: So if you turn the water on, the air flow probably goes the other way. The water drags it down, and it goes the other way.

DR. WRIGHT: It probably could, yes. Yes. But what you have happen is that the water -- I mean, gravity is going to make the water go downhill all the time. So we haven't been having a problem there. Eventually, it should be self-compensating, because as the containment shell heats up, the air is going to heat up, and it's going to get the air flow started right about the time you need to start taking heat away.

CHAIRMAN WALLIS: If it gets stagnant, it's going to get even hotter. So it's going to have more buoyancy.

DR. WRIGHT: That's right. That's right.

DR. BANERJEE: What's the maximum temperature rise in the air with the water there?

DR. WRIGHT: With the water there?

DR. BANERJEE: Yes, the evaporation.

DR. WRIGHT: I don't know offhand, but I know for a fact it never comes out, you know --

DR. BANERJEE: It's not huge, right?

DR. WRIGHT: No, it's not very high.

DR. BANERJEE: Because otherwise your velocities would be greater.

DR. WRIGHT: Be too high, that's right.

CHAIRMAN WALLIS: So does it get saturated when it comes out, the air?

DR. WRIGHT: The air? It gets saturated from the standpoint of relative humidity, yes.

DR. BANERJEE: Whatever temperature it's at.

DR. WRIGHT: Whatever it's at is 100 percent.

DR. BANERJEE: Have you done tests on this?

DR. WRIGHT: Yes.

DR. BANERJEE: At what scale were these tests done?

DR. WRIGHT: The scale for AP600 was 1/12th. Okay? That was a 1-12 volume.

DR. BANERJEE: The height?

DR. WRIGHT: The height was -- you're going to make me think now. The height -- I know, looking at the test facility, the height was maybe about 35-40 feet. So when you looked at that compared with the -- this is, what, 220 feet. That's why we had to use the fans in order to get the air flow up.

That particular test was the more prototypic. The other test we did was tall and thin. This one was to look at both inside and outside containment phenomena. So we made it prototypic from a L over D, height over diameter ratio, but not from a -- couldn't make it full height.

Actually did some testing at full height to look at the water distribution system, but that was unheated. So that's not --

DR. BANERJEE: Now did you do tall and thin, as you said --

DR. WRIGHT: Yes.

DR. BANERJEE: -- with heating?

DR. WRIGHT: Yes.

DR. BANERJEE: And what did that show? Did the air velocities come out to be what you calculated?

DR. WRIGHT: Once again, we used -- It wasn't tall enough to be full height. So we had to use a fan on the top in order to get --

DR. BANERJEE: How tall was it?

DR. WRIGHT: Oh, maybe about the same height as the other one. It was about 40 feet at the most.

DR. BANERJEE: All you really need is a sector -- right? -- a little segment two feet wide with a flat wall. You don't need a curved wall, because this is like the earth.

DR. WRIGHT: Right.

DR. BANERJEE: So your experiment was you let water down a heated flat wall of some sort?

DR. WRIGHT: We did a lab scale experiment that was a heated flat plate. That was the first one we did, and then the second one we did was this long, thin, but it was full -- One of the things we wanted to do was do some steam distribution. It was just a better way to look at that.

Then the last test we did, we called it our large scale test. This was to look at the heat sinks inside containment, how they affect these. We ran some transient tests with that facility. We did a number of things. We did some dry tests with that facility to see what the -- We actually did some tests without the fan on, just to see what the flow rates -- or what we could get.

CHAIRMAN WALLIS: Now did all these things agree with the theory? Did all the data agree with the theory?

DR. WRIGHT: Yes.

CHAIRMAN WALLIS: I would think it's a pretty simple problem.

DR. WRIGHT: It's pretty simple.

CHAIRMAN WALLIS: As long as you get your heat and mass transfer coefficients right.

DR. WRIGHT: Right. That's exactly right. I will show you -- I mean, they come right out of Holeman's heat transfer book, you know. We use -- Sorry if anybody else has a heat transfer book that I didn't use.

CHAIRMAN WALLIS: But it's just initial number versus --

DR. WRIGHT: That's right, initial number -- It's a round number, depending --

CHAIRMAN WALLIS: The mass transfer is the Stanton number or whatever.

DR. WRIGHT: That's exactly right. So it's pretty straightforward. As a matter of fact, Dr. Powers asked us some questions about the correlations. These last two slides really talk to his questions.

He asked about the air cooling annulus. Basically, like I said before, we use what we call stacks, wet and dry. All they are, are volumes connected by flow paths with friction and form losses to correspond to the inlet, the outlet. There's a turning vein at the bottom of this thing, you know, the chimney and what-not.

The flow characteristics for the flow path were determined from test data. We set up a 1/6 scale, 14 degree segment, and did the -- you know, come up with what the losses were, and then we increased those 30 percent for AP600. So the same losses were used in AP1000 with the exception of the fact that we have a longer flow path. So we have more --

CHAIRMAN WALLIS: Did you use a smooth wall for the water-air interface?

DR. WRIGHT: No. What we used was a -- We used a prototypic wall where we used the paint, and we used the worst --

CHAIRMAN WALLIS: No. I mean the surface of the water. See, the water as it comes down the wall forms waves. That will increase the friction, presumably, on the air.

DR. WRIGHT: No. This particular experiment was done dry.

CHAIRMAN WALLIS: Well, the theory assumes a smooth interface?

DR. WRIGHT: It had a -- whatever the manufactured --

CHAIRMAN WALLIS: Smooth on the water-air interface.

DR. WRIGHT: Yes, I think it does.

CHAIRMAN WALLIS: It assumes a smooth interface.

DR. WRIGHT: Well, what we use is a -- We have increased the losses arbitrarily by 30 percent to account for any of the uncertainties that we don't know.

DR. BANERJEE: But you have experiments. Right?

DR. WRIGHT: We have experiments, but we

don't measure what the air flow is. You know what I Mean?

DR. BANERJEE: You didn't have anemometer to measure that?

DR. WRIGHT: We had different places where we could take the air velocity, and we had the fan telling us what the CFMs were, but it wasn't really set up to do the sort of thing you're doing.

DR. BANERJEE: You didn't measure any pressures in the top and the bottom?

DR. WRIGHT: We didn't use those tests to do the loss coefficients.

CHAIRMAN WALLIS: What a pity. What did you do? What did you use?

DR. BANERJEE: Do you have the data?

DR. WRIGHT: You probably could back something out of that.

DR. BANERJEE: What did you measure?

DR. WRIGHT: What we were measuring mostly was the conditions -- Well, for the large scale test, we measured the conditions inside. Okay? And we were looking at temperatures and pressures inside, and we had thermocouples all over the place to see what the distribution was of the noncondensable gases inside.

CHAIRMAN WALLIS: That's not what we are

talking about here.

DR. WRIGHT: No.

CHAIRMAN WALLIS: We're talking about the --

DR. WRIGHT: We're talking about the outside.

CHAIRMAN WALLIS: -- the friction between the water and the air.

DR. WRIGHT: Yes. I don't think we have anything that would be up to your --

CHAIRMAN WALLIS: Well, I don't know if it's up to my standards or not. I just want to know, is it relevant for this problem?

DR. WRIGHT: But I think so. I think that the tests that were done to get the loss coefficients, if you increased it, you know, by 30 percent, you probably cover over anything that you would get from waviness on the outside. I don't know.

DR. BANERJEE: It depends on the Reynolds number, because if it's a fully rough wall with waves on it, you might have a friction factor of .005 or something. If it was a smooth wall, it would actually go down to .001.

CHAIRMAN WALLIS: If you have a smooth water surface, it would go down, but then when you develop waves on the water surface, go up again. And you probably will have waves.

DR. WRIGHT: I think you would have to, yes.

CHAIRMAN WALLIS: Go to anyplace where they have large sheets of water flowing down a wall, you get waves.

DR. BANERJEE: The wall at the airport in Zurich, you see it.

DR. WRIGHT: We have movies of the tests that were done, and you can see -- what is it, laminar waves coming down the outside of this thing. Doesn't seem to be -- Of course, we didn't have the fan running. So I don't know how affected by the air flow it would be.

CHAIRMAN WALLIS: Probably not very much, because it's a low velocity.

DR. WRIGHT: Right. That's exactly right.

MEMBER RANSOM: I wonder if you wouldn't get some entrainment.

CHAIRMAN WALLIS: I think the velocities are so low.

DR. BANERJEE: It's too low. It's evaporating, isn't it? The reason it's low is the evaporation is keeping the air cooled. So there isn't much of a temperature difference --

DR. WRIGHT: To really bring it up. That's exactly right.

DR. BANERJEE: So you get a relatively low velocity.

DR. WRIGHT: If you have the -- If the water is off, it goes fast, a lot higher.

CHAIRMAN WALLIS: Someone has put up here -- I mean H-3 should be H-4, and H-4 should be H-3.

DR. WRIGHT: Oh, that's my fault. You're right. That's bad cutting and pasting.

DR. BANERJEE: I assume that's -- McAdams is for turbulent-free convection. Right?

DR. WRIGHT: That's for turbulent-free. It must be. Yes, you're right.

DR. BANERJEE: If it's not, then -- because the Grashof number is fairly high.

DR. WRIGHT: Yes, the Grashof number is real high.

CHAIRMAN WALLIS: Typically, you've got a third, and the dimension disappears from the correlation.

DR. WRIGHT: I took the dimension out. Yes, I've got these wrong.

CHAIRMAN WALLIS: I think forced is going to go, isn't it?

DR. BANERJEE: Where is the link scale in your Grashof number?

DR. WRIGHT: Was it the link scale? I took it out.

CHAIRMAN WALLIS: It disappears. It disappears because you have L-cubed --

DR. WRIGHT: Yes, it's L-cubed in the Grashof number and L in the neutral number.

CHAIRMAN WALLIS: This number disappears.

DR. WRIGHT: Anyway, this is simple heat transfer 101.

CHAIRMAN WALLIS: That's assuming there is no effect of the water.

DR. WRIGHT: That's right.

MEMBER KRESS: Now, you take this same Colburn forced convection equation and use the Reynolds analogy to get the evaporation rate?

DR. WRIGHT: Exactly right, yes.

DR. BANERJEE: Of course, that is not for a rough wall.

MEMBER KRESS: That's for smooth wall, yes. That's well developed flow, turbulent, smooth wall?

DR. WRIGHT: Right.

CHAIRMAN WALLIS: And what's the water? The water is a uniform temperature assumed to be?

DR. WRIGHT: It is a applied at a uniform temperature. There is -- When you are at hot conditions it takes a certain amount of flow, you know, distance traveled in order to go from subcooled to saturated or close to where --

CHAIRMAN WALLIS: Well, do you calculate the water surface temperature?

DR. WRIGHT: The water surface temperature is calculated, yes. It's part of --

CHAIRMAN WALLIS: Because it's evaporatively cooled.

DR. WRIGHT: That's right.

CHAIRMAN WALLIS: It's going to be quite a lot less than the wall temperature.

DR. WRIGHT: Right. That's right.

CHAIRMAN WALLIS: So what do you do with the falling film? Are you going to show us a picture of how you analyzed the falling film?

DR. WRIGHT: I can't, because that's my last slide.

CHAIRMAN WALLIS: These are trivial, but calculating the mass transfer and the actual temperature of the interface may be trickier.

DR. WRIGHT: Okay. The way the film is done is that you have discrete axial -- it's not axial, really; it's two-dimension, but you can think about it in terms of falling down the side from the dome down to the bottom.

Basically, they take what goes into the node from above at whatever temperature it's at, adds the heat transfer coming out of the wall at that particular time for that time step, and based on that and whatever the correlations are, you wind up evaporating so much of that water. So that by the time you get to the next step, you put in that much less water into the next step.

CHAIRMAN WALLIS: How much you evaporate is a mass transfer phenomenon. It depends on the temperature of the interface.

DR. WRIGHT: Right.

CHAIRMAN WALLIS: Do you calculate a temperature of the interface somehow?

DR. WRIGHT: Yes. It's calculated as the code is going through its --

CHAIRMAN WALLIS: And this has all been checked by the staff, and they gave you an A for the analysis?

DR. WRIGHT: We got our design

certification for AP600 based on this. Yes. Some of the things I showed you before about how we had to go through the iteration to reduce the water flow were from comments from the staff asking, you know, is your code able to account for the fact that you have this water that you are not using. How are you going to convince us that that water is not somehow taking away more heat than we think it is and, rather than do that, we'd say, well, we'll just rerun the case after an iteration and take that water away, so everything we put on gets evaporated.

MEMBER KRESS: Now as the flow goes up through the annulus, it is picking up more and more of water and getting more and more saturated. At some point the mass transfer due to evaporation will cut off.

DR. WRIGHT: That's right.

MEMBER KRESS: Now you deal with that by dividing the annulus into --

DR. WRIGHT: You're right, into axial nodes, yes. That's right.

CHAIRMAN WALLIS: It doesn't cut off unless -- It just warms up more then.

DR. WRIGHT: It warms up more. You wind up getting --

MEMBER KRESS: Could change the saturation.

DR. WRIGHT: You'll get the saturation.

MEMBER KRESS: It reaches saturation and will cut off there.

DR. WRIGHT: But it goes the other way, too. I mean, the hottest part of the containment is at the bottom, you know.

MEMBER KRESS: Now where does -- I'm not familiar with how you combine free and forced conviction by using this cubed and one-third law. Where does that come from?

DR. WRIGHT: That was one of the -- during AP600 -- I'm talking. Wasn't me. This was something that came out of the literature for how to -- and basically, this only comes into play when you are close to the transition between forced and free convection.

I think what Dr. Wallis was saying is true. I mean, when you get further out, when you get a well developed situation, you are basically dominated, and this will make sure you are dominated by the forced convection in this equation.

MEMBER KRESS: There probably is an empirical relationship rather than based on fundamentals. Let's ask Sanjoy. In the mixed region where you have both forced and free, where does that equation come from?

DR. BANERJEE: I don't know.

DR. WRIGHT: We found a paper. I can get that for you, if you'd like.

CHAIRMAN WALLIS: I don't think it matters.

MEMBER KRESS: It probably doesn't matter, because you are in forced convection most of the time effectively.

MEMBER RANSOM: A more conservative approach is just take the maximum flow.

MEMBER KRESS: Well, that would be one way, or to take the minimum.

CHAIRMAN WALLIS: Take the bigger one, and forget about H-3. Just take H-4.

DR. BANERJEE: The three one, if I'm right, is for a nonbounded flow.

DR. WRIGHT: Just on the outside of a building without any wall.

CHAIRMAN WALLIS: It's not really very appropriate.

DR. BANERJEE: Not appropriate.

DR. WRIGHT: Well, it depends on whether

the size of the annulus is big compared with the boundary layer.

DR. BANERJEE: In this case, it's not likely to be, because you have just a 12 inch annulus, and this is like --

DR. WRIGHT: His thermal boundary layer keeps getting bigger.

CHAIRMAN WALLIS: Well, I don't know if we are going to make anymore progress here. If we were going to dig into this, we would have to look at all the details of your heat and mass transfer. I don't know if we want to do that or not.

DR. BANERJEE: The more interesting case is the evaporation case and how you do that calculation.

CHAIRMAN WALLIS: Oh, yes, it is. Yes, it is.

MEMBER RANSOM: Well, they should use a driving potential as just the vapor pressure of the water film to the partial pressure of water vapor on the air flow.

MEMBER KRESS: If they were using a Reynolds analogy, that's what you would do.

MEMBER RANSOM: And you can't go any further than saturating the air stream.

DR. BANERJEE: Well, the issue really is that the water, I guess, can get to the wall temperature.

DR. WRIGHT: The wall temperature, in theory. I mean, the evaporation should cool it. So you'll have a radiant across the film.

DR. BANERJEE: So how did you do that calculation? That was what we were discussing.

CHAIRMAN WALLIS: You mean the temperature distribution on the film?

DR. BANERJEE: Even less detailed than that. I mean, did you just do a one-dimensional calculation?

DR. WRIGHT: Yes. It's a one-dimensional radial calculation to find out what the temperature distribution is. Use the film surface temperature to drive the -- you know, get the thermodynamic properties to do the mass transfer and heat transfer calculations.

MEMBER KRESS: Then you would have to iterate on that.

DR. WRIGHT: No.

MEMBER KRESS: You wouldn't?

DR. WRIGHT: No. I don't think the --

DR. BANERJEE: Is it written up somewhere?

DR. WRIGHT:  It is.  IT is.  It's in this WCAP that describes the --

DR. BANERJEE:  What number?

DR. WRIGHT:  I have it right here.

CHAIRMAN WALLIS:  Shall we assign Dr. Banerjee to review it?  We don't all need to do it.

DR. BANERJEE:  We need a very deep review.

DR. WRIGHT:  It's on the first page here. It's WCAP-15846.

MEMBER RANSOM:  Is it possible for us to get copies of that?

CHAIRMAN WALLIS:  We can get all this.  We could spend a whole lot of time reviewing all of this.

MEMBER RANSOM:  I'd just like to take a look at it at home.

DR. WRIGHT:  1-5-8-4-6, and then --

CHAIRMAN WALLIS:  So we've got to make sure that Dr. Banerjee has a copy, and he can give us the evaluation.  Sounded like the analysis of a cooling tower.

DR. BANERJEE:  We have it.  Mike has this?

DR. WRIGHT:  Mike should have that, yes.

CHAIRMAN WALLIS:  Mike will get him a copy.

DR. WRIGHT:  Okay.  That's all I have.  If there's anymore questions? Thank you very much.

CHAIRMAN WALLIS: It sounds reasonable, but without the details, we can't really give it an evaluation.

MEMBER SIEBER: But it is not complex either.

CHAIRMAN WALLIS: Shouldn't be complex, but who knows?

MEMBER SIEBER: Yes, you never do.

CHAIRMAN WALLIS: Depends what they actually did with it. Let's move on to the next. We are going to move back to the staff now. Is that the plan? Maybe the staff can catch us up on a bit of time, but it's not a requirement.

DR. SEGALA: This is John Segala from NRC. Our first speaker is going to be Walt Jensen. He is going to discuss some of the pre-application issues.

CHAIRMAN WALLIS: Can we concentrate on the technical matters rather than a lot of history?

DR. JENSEN: Yes, sir. That's what we'll do. I didn't think you would be very interested in that after discussions this morning.

Before I start, I did look up the qualification for the qualification runs during AP600 on RELAP5 against the ROSA test, and that's in INEL report 96/0400. They concluded -- I looked at this during lunch, and they looked at a double-ended DB air line break, among others, and they declare they did a pretty good job. So I would just like to add that before I start.

Okay. I am going to talk about how we closed some of the at least challenging open issues from the preapplication review, and I am not going to talk about entrainment nor containment. So, basically, with LOFTRAN and NOTRUMP are what I looked at, LOFTRAN being the transient analysis code which we also do steam line breaks with, and NOTRUMP being the small break LOCA code.

Briefly, what our review consisted of: We looked at the review for AP600, the major differences between AP600 and AP1000, looked at the scaling which we asked Research to help us with. We reviewed the user standards for preparing the input, and we performed independent audit calculations with RELAP.

CHAIRMAN WALLIS: Now those would be interesting.

DR. JENSEN: We will get to that. LOFTRAN -- this is the issue with the steam line break, and we are concerned about voids in the reactor coolant system, and LOFTRAN has a homogeneous model. So it avoids the current and the coolant system. They would not collect and block natural circulation flow, and so the code really wouldn't be appropriate for looking at conditions whether loops became saturated.

Westinghouse did the calculation. The loops remained subcooled. The CMT did not begin to drain. So that issue was closed.

CHAIRMAN WALLIS: How does RELAP handle something like entrainment into the ADS fall line?

DR. JENSEN: It has a flow regime map. It calculates entrainment in the core. It uses, I Think, a Zuber drift flux model with which it backs out interphasial drag coefficients, and it then passes that entrained liquid into the upper plenum and then out the hotlegs.

RELAP pretty much showed the hotlegs to be in an annular mist flow regime. So it just was carrying everything out the ADS4. So that's what RELAP would do.

MEMBER RANSOM: Yes, with one exception, that if you were to predict stratified flow in that leg, why then there is a model in it for entrainment or, depending on whether it is on the top of the leg or the bottom or the side, it will either pull vapor through or, in the case of ADS-4, I guess entrained liquid, provided you are predicting stratified flow to exist.

DR. JENSEN: That's true.

MEMBER RANSOM: If that's annular flow, like you're saying, why then the mixture will go out the break.

CHAIRMAN WALLIS: Well, if it doesn't go out the break, where does it go. And if it's coming into the hotleg and some of the liquid doesn't go out the break, where does it go? It comes back out the hotleg again?

DR. JENSEN: In what I was looking at, the flow -- all the liquid on the hot let went out ADS-4 with a lower velocity than the steam flow in the hotleg.

CHAIRMAN WALLIS: But none of it was de-entrained in the hotleg at all?

DR. JENSEN: As Dr. Ransom says, there was some stratification. We assumed that there was a single failure in one of the ADS-4 valves -- Westinghouse did -- and in the side it only had one ADS coming off the hotleg. There was some stratification on that side.

CHAIRMAN WALLIS: I guess we get to ask someone else about this entrainment, because if you have de-entrainment in the hotleg, then presumably the water has to run back into the vessel in countercurrent flow along the bottom of the hotleg. Is that the water that doesn't go out the break that does that?

DR. JENSEN: I would presume it does. Like I say, most of the water went out the ADS.

CHAIRMAN WALLIS: I don't know that RELAP would model that then. RELAP may be carrying everything out the break, because it has to because of the way the code is set up.

DR. BANERJEE: But it can handle countercurrent flow. Right?

MEMBER RANSOM: It can handle what?

DR. BANERJEE: Countercurrent flow.

MEMBER RANSOM: Oh, yes, countercurrent liquid vapor flow.

CHAIRMAN WALLIS: But does it have a mechanism for de-entraining into that countercurrent flow?

MEMBER RANSOM: I think that would only occur if you are in stratified flow. In stratified flow, then you have to have a void --

CHAIRMAN WALLIS: How did you get into stratified flow? You've got to get the water coming from somewhere to get stratified flow.

DR. JENSEN: The velocities were very low, I believe.

CHAIRMAN WALLIS: Coming in as droplets, they got to settle out somehow.

DR. BANERJEE; Has to de-entrain.

CHAIRMAN WALLIS: Well, someone is going to tell us all about what happens in this entrainment/de-entrainment? Maybe Westinghouse is going to tell us.

DR. JENSEN: And Dr. Bajorek is going to talk about it, I guess, tomorrow.

CHAIRMAN WALLIS: Okay.

DR. BANERJEE: Coming back to this drift-flux correlation being used to back out the interphasial drag, I don't remember if Zuber's correlation had some change in the drift velocity with flow regime from the bubbly to the churn.

CHAIRMAN WALLIS: I think it does. It does.

DR. BANERJEE: It does, doesn't it. So how does it handle that?

DR. JENSEN: I don't know. I don't know the answer to that.

MEMBER RANSOM: I can give you a clue, I

guess. It only uses it in the core on the vertical regime, and it uses the EPRI correlation to back it out, and horizontal components, you do not use the drift-flux.

DR. BANERJEE: Right. I realize that. So it doesn't use the Zuber then. It uses -- That makes more sense.

CHAIRMAN WALLIS: Chexelle-Larouche or something.

DR. BANERJEE: That at least doesn't take account of flow regimes.

MEMBER RANSOM: No. It's all embedded within it.

DR. BANERJEE: All embedded within it.

MEMBER RANSOM: Right. I think it's a full range.

DR. JENSEN: Well, let's move on to NOTRUMP then.

CHAIRMAN WALLIS: You see, we get all these slides of bullets and words. We almost never get a slide which shows a picture of what happens anywhere.

DR. JENSEN: I'm coming to that.

CHAIRMAN WALLIS: Okay. Well, it's just going to be outputs from codes. It's not going to be here's what happened.

DR. JENSEN: Well, hopefully, it will be what -- It's what the code says will happen.

CHAIRMAN WALLIS: Yes, but then I still don't get a picture about is the code representing this countercurrent flow or this de-entrainment or this -- I need some sort of a picture of the vessel on the pipe and saying, now where does the water go, and in what form is it. Maybe we'll get to that sometime. That would help a lot anyway.

It may be RELAP -- We all know that these codes can predict something, but it may well be that they are based on a physics which isn't what is actually happening. That's one of the major concerns that I think we have.

DR. JENSEN: We are looking forward to seeing some of the new OSU test data.

CHAIRMAN WALLIS: That's the same problem we have with them. They have a theory which is based on the particulars of a conceptual cranial model which has nothing to do with what we see in the picture of the flow regime. So that's the same kind of problem there.

DR. JENSEN: We would agree that RELAP isn't any better benchmarked as far as predicting entrainment out of ADS-4 than the Westinghouse codes are.

DR. BANERJEE: But suppose you've got steam flowing with some drops along the hotleg. Now the ADS-4 line is at an angle to this. The steam turns the corner, and the water keeps going straight. Does the code take that into account?

I mean, Graham is looking for a de-entrainment mechanism.

CHAIRMAN WALLIS: It goes up, and it goes up and comes back from the steam generator.

DR. BANERJEE: Right, and it comes as a slug.

CHAIRMAN WALLIS: It may come back as a slug, yes.

DR. BANERJEE: And then what happens?

DR. JENSEN: I didn't see any slugs. Like I say, it was mostly annular mist.

CHAIRMAN WALLIS: This was in the theory, not in the reality. We saw slugs, though, at OSU. So --

DR. BANERJEE: But what relation does this have to these OSU experiments?

DR. JENSEN: I think the part that you are mostly concerned with is the latter part of the analysis where the flow in ADS-4 becomes subsonic, and in the earlier parts perhaps we don't have that much of a problem. But basically, I would like to show you what we've predicted, and this is all tentative on tests shown at OSU. But this is what we have now, and the code has been benchmarked against data from the AP600 test, and it did a pretty good job.

We don't think that that data is really completely applicable to AP1000, but that's still open.

MEMBER RANSOM: Well, I guess in your defense, you are modeling AP1000. You are not modeling the APEX facility, I guess. Right?

DR. JENSEN: That's true. We have not modeled the revised APEX facility with RELAP, of course.

MEMBER RANSOM: It would be interesting to see what you get in that event. Maybe they will talk about that tomorrow. I don't know.

CHAIRMAN WALLIS: Well, I think we might agree --

DR. BAJOREK: Well, let me -- I'm sorry. Well, let me try to clarify just a little bit. You are talking about getting an annular mist in RELAP. What you focused on was the double-ended DVI line case.

In that case, you have high water levels in the hotleg for a relatively brief period of time. Then when the ADS-4 does open, everything flushes out and, because of the low water level in the inner vessel, droplets which are entrained come out at high velocity. Most of that is swept immediately out the ADS-4.

Now I think the mechanism in the code for de-entrainment really comes from the phase separation model with the branch line. It is going to use the model by Schrock to take a look at the gas flow going up into that branch line, and it's going to say, hey, only give me so much water. Anything else is going to be left behind.

That will stay there until the level comes up, and that model were to entrain enough to satisfy that correlation.

Now I think what Dr. Wallis is going to point out very clearly tomorrow when we start looking at the mechanisms of hotleg entrainment is that these codes, be it RELAP or anything else we would want throw at it, really isn't picking up this new type of flow pattern that's seen in the hotleg where we get not really a horizontal stratified flow but some oscillatory slugging that is feeding the entrainment. But what RELAP is predicting right now, and I think what Walt is trying to point out in his flow patter, one, is indicative of a DBI line case where we don't have a real level in that hotleg for very much of the time and that what is left is benchmarked on how much the Schrock correlation or the phase separation model allows it to take out at any one time. There's not much there in those simulations.

MEMBER RANSOM: Even a Schrock correlation probably is only going to differentiate if you have stratifying.

DR. BAJOREK; Not always. Now I would have to go back and look at the flow pattern map, because what these maps tend to do is assume that it would be all, let's say, the Schrock correlation in a horizontal stratified regime. However, it will take part of that and ramp it into the other regimes.

So by imposing that correlation, you are also affecting what goes on in annular mist and in some of these others. So there is a very close relationship between what it's trying to entrain or de-entrain and these flow patterns.

CHAIRMAN WALLIS: Tomorrow someone is going to actually show photographs and draw pictures of where the liquid -- where the steam is in this hotleg and how we predict the various flows of the two phases in the various parts or are we just going to get words again?

DR. BAJOREK: No, I actually have a movie that I can show you, if you would like. But what we would like to try to do tomorrow is talk about the mechanisms and how the staff has tried to bound what may be going on at this branch line, and see its effect on the inner vessel mixture level.

DR. BANERJEE: Steve, did you see any oscillations in the discharge in ROSA?

DR. BAJOREK: Yes. I checked that, and the adequacy report that I took a look at, and only briefly, did characterize ROSA as being fair looking at these oscillations. There were fairly significant oscillations late in the small break and into the long term cooling.

In APEX there were oscillations that were relatively high frequency, and the concern was these high frequencies weren't being picked up by the data acquisition system. So it tended to be a little bit smoother. But there were some fairly significant oscillations in ROSA.

DR. BANERJEE: I seem to remember that.

Right. Clearly, RELAP didn't pick that up.

CHAIRMAN WALLIS: Well, I think Westinghouse is going to say it doesn't make any difference. Once the level goes below the hotleg, it doesn't matter. Isn't that going to be the approach?

DR. BANERJEE: You don't know that.

CHAIRMAN WALLIS: They don't know that, because we haven't got a picture of what happens yet.

DR. JENSEN: Dr. Banerjee is correct. In these runs we did not show any oscillations in the hotleg flow with RELAP. This was a very short interval. We didn't run it out very much past the time that the IRWST started to inject, but in the time we did run it there weren't any oscillations.

CHAIRMAN WALLIS: And the key question here is going to be, once the level goes below the hotleg, once there is sort of two-phase level in the vessel, if there is such a thing, goes below the hotleg, then the method of getting liquid through the ADS fall line has to be droplet entrainment from the vessel.

The question has to be: Do all the drops that get entrained in the vessel go out the ADS fall line or do some of them get de-entrained or keep going straight and come back along the floor of the hotleg and go back to the vessel again?

I've never really seen that explained. Is someone going to explain that to me sometime?

DR. JENSEN: I hope you will get your answer. I hope you will get it tomorrow, perhaps this afternoon.

CHAIRMAN WALLIS: You guys are the experts, though. You're the guys who have been examining this with a microscope.

DR. JENSEN: I could look and see what RELAP predicted, but I don't think --

CHAIRMAN WALLIS: It doesn't help me. Okay. What's the reality? I don't know.

DR. JENSEN: All right. Well, this slide just says that entrainment is unresolved.

CHAIRMAN WALLIS: That sounds like a good conclusion.

DR. JENSEN: But the PRHR heat transfer issue is resolved with Westinghouse.

CHAIRMAN WALLIS: This is by them being conservative enough, you accepted it?

DR. JENSEN: Yes, and they compared it with --

CHAIRMAN WALLIS: With data.

DR. JENSEN: Indirectly with data. This

one says we asked them to run a number of different break sizes, and particularly hotleg sizes, because the hotleg is located at a lower elevation than the cold legs, and they had not run those at first. So we asked them to go back and look at the hotleg.

That doesn't show any core uncovery either. So there wasn't any need to do any heat-up calculations except for the one that they did for the ten-inch break where they got the high void fraction during the early blowdown when they got flow stagnation for this ten-inch cold leg break.

Now this is -- There's some data here that's kind of a jumble, but these are all the audit calculations that the staff ran. This blip here in the purple is the ten-inch break. This is the early flow stagnation. Westinghouse assumed adiabatic heating during this time, and they calculated a temperature of 1300-and-something.

RELAP didn't calculate any core uncovery. The break that --

DR. BANERJEE: Which is which again?

DR. JENSEN: Can you not read that? The purple is the ten-inch break. The black at the bottom is the double-ended DB out-line break. RELAP says this is the worst case.

CHAIRMAN WALLIS: IT predicts a core level of 30 percent at times?

DR. JENSEN: It looks like about 30 percent, and it blips down like at 28 percent. Somebody might say, well, isn't -- this is pretty highly voided. Yes, it is pretty highly voided, right. When they benchmarked RELAP against some blowdown tests, a FLECHT SECET test, it blew more water out of the test facility than the data showed. The void fractions were higher, and hey concluded that the interphasial drag coefficients were too high in RELAP, which is probably true.

DR. BANERJEE: So does this mean there is core uncovery then, that 30 percent level?

DR. JENSEN: It might. But, remember, RELAP has blown out too much water. It has blown this water with the same models, with the same interphasial drag.

CHAIRMAN WALLIS: So this is an extreme case, but is it predicting core uncovery?

DR. JENSEN: No, sir, it's not. I'm going to show you --

MEMBER RANSOM: That would be the question. Is there any heat-up of the core?

CHAIRMAN WALLIS: Even with this extreme

case of RELAP, there is still no --

DR. JENSEN: No core uncovery and no core heat-up.

DR. BANERJEE: So the 30 percent between, sa, 1000 and 1500 seconds or something is the black line?

DR. JENSEN: The black line.

DR. BANERJEE: That doesn't lead to -- and it hangs around below 40 percent for a long, long time. Right? A few thousand seconds?

DR. JENSEN: Yes. This time is between ADS-4 actuation and IRWST injection. This is the minimum in all these curves.

DR. BANERJEE: Right. Okay, so the black line then is below 40 percent from about 1000 seconds to, as far as my eye can see, 3000-odd seconds.

DR. JENSEN: Yes.

CHAIRMAN WALLIS: There's still enough water there?

DR. BANERJEE: How does it -- I mean, if you base that on a level swell, that 30 percent would give you dryout of the top.

CHAIRMAN WALLIS: A very high void fraction, I think.

MEMBER RANSOM: Well, how many nodes were in the core?

DR. JENSEN: There were nine, nine in the RELAP core.

MEMBER RANSOM: And this is just collapse. So the water presumably is somehow distributed.

DR. BANERJEE: But when you do bundle experiments with collapsed level below about 50 percent with a 14-foot height, 12-foot height, you dry out the top of the bundle.

MEMBER RANSOM: I don't know.

DR. BANERJEE: I think so. Can you answer that?

DR. JENSEN: I can just say it was benchmarked against FLECHT SECET, and it worked pretty well with a little bit higher voided than the test was.

DR. BANERJEE: g1, g2?

DR. JENSEN: RELAP, to my knowledge, wasn't benchmarked against those. NOTRUMP was against the g2 test.

DR. BANERJEE: What did those tests show for a collapsed liquid level of 50 percent and less? I'm just talking about the experiments. Forget RELAP.

DR. JENSEN: I don't know.

DR. BANERJEE: I think they showed dryout.

Maybe somebody else can answer that question. So when you've got a collapsed liquid level of 40 percent, that would suggest you've got dryout. I mean, I'm not saying whether RELAP is conservative or not conservative. You keep going back to that. I don't know if it is or not. I'm just asking.

DR. JENSEN: What I would say is that it's entrained too much liquid. It's carried this liquid out of the system, out at the ADS-4, and this liquid is lost. Had there been a lower amount of entrainment, a lower drag between the phases, the --

CHAIRMAN WALLIS: But it's also nonconservative now in the pool swell, because if it's got too much entrainment, too much drag, it's carrying up some of this liquid higher than it should and, therefore, it's wetting the top of the core in a way that shouldn't happen, if it were more realistic and it's interfacial drag.

So it's got -- It works both ways. You carry out too much, but then you carry up too much.

DR. BANERJEE: You cool too much at the top.

CHAIRMAN WALLIS: You cook too much. So it's not clear that it is conservative.

MEMBER RANSOM: These temperatures you

show in the back -- are those the maximum hottest point in the core?

DR. JENSEN: Yes. These are for the double-ended DVI line break.

CHAIRMAN WALLIS: Well, where is this void fraction? This says core void fraction. Is this at the top or where?

DR. JENSEN: At the top. The dark line is at the top, and the lighter line is in the middle.

DR. BANERJEE: Which ones do they correspond to here?

DR. JENSEN: This is the double-ended DVI line break.

CHAIRMAN WALLIS: So it's carrying off a lot of water to the top, although the level is really very low. It's still able to carry it up.

DR. JENSEN: That's what the code says. We think this is the worst case. Fortunately, this is the one of the first --

CHAIRMAN WALLIS: If you took the RELAP collapsed level and some other interphasial drag model which was not so conservative, you might well find it dried out.

MEMBER RANSOM: Well, actually, his last slide shows it is drying out. A little between 2000 and 1500 seconds you are getting momentary dryouts in RELAP, and you can see that in the void fractions here, too. They are going to one, basically.

DR. JENSEN: You can see it also in the core heat-up where, when it dries out, it gets these little whiskers. This is the next slide. I have two peak clad temperatures. We have a hot rod in RELAP with a little higher peaking factor.

This hot rod, however, I must say, is located in an average coolant channel. So it doesn't have its own channel, but it gives the effect of -- It shows you what a little higher heat flux might do.

CHAIRMAN WALLIS: Tiny little blips. Now what would happen if you brought in Westinghouse's calculations on top of your RELAP's? That would give us some kind of a -- something to compare with.

DR. JENSEN: If I had Westinghouse's code, I could have run it, and then I could have applied that data and put it on top of RELAP.

CHAIRMAN WALLIS: But didn't they do calculations of the same transient?

DR. JENSEN: Yes, they did, and if they would show you the void fractions that they calculated using NOTRUMP, they would look very much like the ones I have with RELAP.

CHAIRMAN WALLIS: Did they calculate at the level, the percent core level that you showed us, the purple curve?

DR. BANERJEE: And the black one.

CHAIRMAN WALLIS: What would theirs look like for the purple and the black curves?
I don't have an equivalent curve from Westinghouse.

CHAIRMAN WALLIS: Didn't they have -- Didn't you guys have a percent core level for DVI line breaks? Could you get us that now or tomorrow?

DR. BANERJEE: Is it in the package?

CHAIRMAN WALLIS: Where do we look in the packet?

MEMBER RANSOM: Which slide?

DR. GAGNON: This is Andy Gagnon. For the DVI line break, two-phase mixture level is on Slide 75.

CHAIRMAN WALLIS: That's one of those we skipped over.

DR. GAGNON: Yes.

CHAIRMAN WALLIS: Well, I'm glad we came back to it.

DR. GAGNON: It was at 14.7 psi containment and --

CHAIRMAN WALLIS: The mixture level is 26 feet?

DR. BANERJEE: What does that mean in terms of core level, though?

DR. GAGNON: That means it's up in the upper plenum. The core is covered, not a collapsed level.

CHAIRMAN WALLIS: It's not a collapsed. Do you have a collapsed level?

DR. GAGNON: No, I don't have that here.

DR. BANERJEE: So if you go back to the collapsed level there, at 30 percent -- sorry, below 40 percent for 2000 seconds roughly, will you see the same thing?

DR. GAGNON: I would have to look.

CHAIRMAN WALLIS: If this mixture level is up in the upper plenum like this, then there's a disengagement and it is all vapor above that. Is that right?

DR. GAGNON: Yes. That's correct.

CHAIRMAN WALLIS: So these are droplets that are bouncing around in the upper plenum or what is it? What is in the upper plenum between 20 and 26 feet?

DR. GAGNON: Between 20 and 26 feet? It is actually a lower void fraction --

CHAIRMAN WALLIS: It's like a fluidized bell of droplets, and then it's just disengaged above that? Is that what --

DR. GAGNON: It is phase separation.

CHAIRMAN WALLIS: So it's droplets.

DR. CARUSO: The void fraction is less than one.

CHAIRMAN WALLIS: Yes, but is it droplets. What do I envisage is happening here? There's the core. Then there's a whole lot of droplets bouncing around above it, and above that there is a region where there are no droplets, and it's steam.

DR. GAGNON: Steam.

CHAIRMAN WALLIS: Is that what this means? I don't really care what the code predicts. I want to get some idea of what is reality here. So is that your interpretation of this?

DR. GAGNON: Yes. It's actually a lower void fraction -- and NOTRUMP predicts a lower void fraction in the upper plenum than is in the top of the core.

CHAIRMAN WALLIS: I don't understand how a code does this. The only reason these droplets are there is because presumably they have some velocity at the bottom, and they've got a trajectory, and they go up and then they turn around or something, isn't it? Are they suspended up in there like a fluidized bed or what? How do they disengage? How do you get a level like this? Is that where the hotleg is? Is that why --

DR. GAGNON: That is actually the hotleg elevation there.

CHAIRMAN WALLIS: So you are decreeing by the way you nodalize that they can't get above there. It's not physics.

MEMBER RANSOM: Excuse me. Where is the hotleg? What elevation on that plot?

DR. BANERJEE: What's the top of the core?

CHAIRMAN WALLIS: The dotted line.

MEMBER RANSOM: That's about 19.

CHAIRMAN WALLIS: So we've got seven feet or something two-phase, and above that it's dry, just steam above that? That's where the hotleg is?

DR. WRIGHT: Six feet above the top of the core.

CHAIRMAN WALLIS: The hotleg is -- The bottom of the hotleg is six feet above? This is the middle of the hotleg or something like that.

MEMBER RANSOM: Where? Where is it?

CHAIRMAN WALLIS: So that's why there is

a level there.

MEMBER RANSOM: Is that the answer? It's about 26 feet now?

DR. BANERJEE: It depends on how they stratify. Does your code allow stratification in vertical nodes?

DR. GAGNON: Yes, it does.

DR. BANERJEE: Okay. So that probably is the hotleg then.

DR. GAGNON: Yes.

CHAIRMAN WALLIS: And what is your velocity? We didn't seem to see if it's possible to have droplets up there or it's just an artifact of the code.

DR. BANERJEE: I don't think so, if that is the hotleg and it allows vertical stratification.

CHAIRMAN WALLIS: But then there is nothing allowed above that. Above that is just a dead space of steam, presumably.

DR. BANERJEE: All the steam goes out to the hotleg.

CHAIRMAN WALLIS: So this doesn't help us. It doesn't help us to compare with RELAP. But you could ask for that, couldn't you? Can you show us that tomorrow? Can you show us something that would compare with the purple and black curves?

DR. CORLETTI: We could show -- Andy, do we have the void fraction available?

CHAIRMAN WALLIS: To compare with what we see here. Presumably, the staff compared it with what you had. Maybe not.

DR. CORLETTI: I think we have the void fraction.

CHAIRMAN WALLIS: Do you have this?

DR. CORLETTI: I don't believe -- Do we have core collapse level. I do not believe that we have core collapse level with us.

CHAIRMAN WALLIS: Can't you get someone to FAX it to you?

DR. CORLETTI: We can try that, yes.

CHAIRMAN WALLIS: Will you please get someone to FAX it to you?

DR. CORLETTI: Yes.

CHAIRMAN WALLIS: And will you show it to us tomorrow? Did you say yes?

DR. CORLETTI: Yes.

DR. JENSEN: I'll give you a copy of my curve, Mike.

DR. BANERJEE: Now before you move on from this curve, I want to go back to this issue of collapsed liquid level. There are experiments which are available at these pressures with bundles with different collapsed liquid levels.

Now they are under conditions which are very similar to this. So did you take a look and see whether they got dryout at the top or not?

DR. JENSEN: I looked to see how Westinghouse's code compared to the experiments, and Westinghouse benchmarked NOTRUMP against their 14-foot g2 tests, and they were conservative. They dried out sooner than the data did.

DR. BANERJEE: Right, but NOTRUMP is also showing a liquid level above the core for this collapsed liquid level or whatever.

DR. JENSEN: And they have a very high void fraction similar to RELAP.

DR. BANERJEE: Now I guess we will have to resolve this tomorrow when they show their void fraction curves, but if I remember, with the 30-40 percent coverage, there was significant dryout at the top of the bundle. Maybe we can ask Westinghouse. When they had 30 to 40 percent collapsed liquid level, was there dryout at the top of the bundle? Somebody?

DR. CORLETTI: We will have to get an answer to that. Are you asking from the tests when we validated NOTRUMP? Yes. See, I don't believe we know that here, but we can get you that information.

DR. BAJOREK: Well, I think you can look at the g2 and your g1 experiments and get an answer at low pressure. At higher pressures, from like the Oak Ridge tests, a collapsed level around 30 or 40 percent would have had uncovery at the top. Now those are at higher pressure.

At lower pressure you would expect more frothing, a little bit higher. I think the appropriate place to look at g1, g2 in the FAVA series of tests to try to get a handle on that level swell.

Now from FLECHT SECET, which isn't directly applicable, because they are reflood experiments, but they were done at low pressure, if you take a look at those tests, the level swell, two-phase level over collapsed level, was about 1.5, 1.6, 1.7. That ratio, based on the 30 or 40 percent, would suggest that there would be some uncovery at the top.

So at the very least, it is got to be pretty close to the point of core uncovery, and I think all of the codes are showing that. RELAP with a void 90 percent. I think NOTRUMP we saw at one point voids 90 percent or greater. COBRA/TRAC likewise -- I think they were 90-95 percent.

Now we have put our an RAI asking for some clarification on these high voids and how they relate to the nodalization and the radial discretion in the core. Even though your average cells across this large core are at 90-95 percent, can you rule out the possibility of having localized regions like the hot assembly at 1.0 and heating up, while others are at a lower void fraction?

CHAIRMAN WALLIS: If we look at Figure Slide 43 that they gave us, this says DE DVI, double-ended DVI line break, and some vessel mass inventory in pounds mass, and it starts off with around 160,000. It's presumably a full vessel. Then it goes down to about 80,000, presumably a half-empty vessel.

This would seem to be the same transient that you show us in your purple curve. Is that true?

DR. JENSEN: I don't have that.

CHAIRMAN WALLIS: This thing here that we saw this morning.

DR. JENSEN: So this is atmospheric back pressure.

CHAIRMAN WALLIS: Right. So there is something different about the back pressure.

DR. WRIGHT: A little bit.

CHAIRMAN WALLIS: Doesn't make much

difference?

DR. WRIGHT: A little positive effect of back pressure.

CHAIRMAN WALLIS: So what I conclude is that RELAP is predicting a lot less liquid in the vessel than they are predicting, and it is predicting a minimum occurring after 1000 seconds, whereas theirs seem to have settled down after about 600 or something.

There's a big difference between your purple curve and Figure 316 Westinghouse, which is really the same thing, I think. It's the same plot.

DR. JENSEN: The purple curve is a 10-inch break.

CHAIRMAN WALLIS: Which is the DVI line?

DR. JENSEN: The black one.

CHAIRMAN WALLIS: The black? Well, that's the same thing. It's worse.

DR. BANERJEE: It is, in fact, staying at the low inventory for a longer time. Right? I guess, if you believe the inventory, which may be wrong, what that means in terms of uncovery or dryout at the top really needs to be understood more clearly. The calculations you have here are probably very sensitive to what heat transfer correlation has been used and so on.

So with a void fraction of over 90 percent or 95 percent, as you have here, it's not so clear that your temperatures will be so benign as you show in the next slide.

CHAIRMAN WALLIS: I just wonder how you make a decision when you see your -- You run RELAP as a check, you know, as independent check by the staff on RELAP, and you find you are predicting that you got about half as much water in there as Westinghouse is predicting.

Now do you do with that? How do you use this to make a regulatory decision?

DR. JENSEN: We felt that -- At least, I felt that we were getting about the same results as Westinghouse, because we looked at their void fractions in the core, and they were about the same as we were calculating.

Then we looked at Westinghouse's analysis of the level swell test, and they did a pretty good job. They did noding studies in the core. They ran up to --

CHAIRMAN WALLIS: So their analysis was much better than yours. Is that what you concluded?

DR. JENSEN: Well, I'm saying that, bottom line, it's the same. They both showed the core to be covered.

CHAIRMAN WALLIS: Yes, but you've got two analyses which indeed tell us so different, and they both show everything is all right. I wonder --

MEMBER RANSOM: I don't think they are two different views. If you look at the inventory, they have 50 percent with COBRA/TRAC, and he's getting 40 percent with --

CHAIRMAN WALLIS: He's getting 28 percent as a minimum, he said there.

DR. JENSEN: This is just the core. We've got -- We have water in the lower plenum. We've got water in the downcomer. I think maybe -- I don't know we can draw any conclusions about what's in the vessel, but I --

CHAIRMAN WALLIS: No, I'm just saying, here is what looks like a key parameter from two different codes, which is --

DR. BANERJEE: Well, you don't know that that is the same.

CHAIRMAN WALLIS: That code is this one.

DR. GAGNON: Excuse me, Dr. Wallis. The vessel inventory that you see from NOTRUMP there is total vessel. In other words, that includes downcomer, lower plenum, not just the core.

CHAIRMAN WALLIS: But when it's halfway, isn't it about halfway through the core?

DR. GAGNON: There is still considerable level in the downcomer.

CHAIRMAN WALLIS: Yes, but it's halfway in the downcomer, too. So --

DR. BANERJEE: What does that mean in terms of core inventory? Is it 50 percent or 30 percent?

DR. GAGNON: I got to look at that.

DR. CUMMINS: I don't think we think they are similar, we are comparing similar measurements, and I think that we'll try to get some similar measurement by tomorrow.

CHAIRMAN WALLIS: Okay. And everything may become clear.

DR. CUMMINS: Yes.

CHAIRMAN WALLIS: But I just wonder, what's the rationale -- If it turns out they are very different, you've got two codes that predict very different vessel mass inventories, and yet the conclusion is that the heat transfer is fine at the top of the core.

Now what should we do with that

information? Should we say everything is fine or should we say, well, you know, one of these key parameters is way off in the prediction. Two codes are predicting two very different things. We're not satisfied.

DR. JENSEN: Actually, our philosophy is to base our decisions on what the applicant's code is calculating unless we see something that looks vastly different. I haven't looked at the core collapsed level from Westinghouse. Perhaps I should have, but I did look at the things I did look at. It looked fairly similar.

DR. CORLETTI: I guess one comment I would just like to introduce -- Walt, in your calculations of heat-up, I mean, do you see anything approaching PCP, any regulatory limits as far as core heat-up, and maybe is that worth mentioning here in that regard?

DR. JENSEN: You can. This is the highest temperature that I calculated.

DR. BANERJEE: That depends on what heat transfer model you use. At 95 percent void, it's not clear that that should be the temperature. I mean, it just depends on what factor you put in.

DR. CARUSO: Well, Dr. Banerjee, I think one of the points that's been left out of here -- When we make these decisions, we look at the calculations, the code that is developed by Westinghouse. We consider the assessment work. We consider the code that we have and how it has been assessed, and our code, RELAP5, has been assessed against a large number of these experiments, and the heat transfer packages that Walt is using are ones that have been determined to be appropriate for these conditions.

That's why he uses that code. Now he doesn't redo his assessment every time he uses it. He is using a code that was assessed for AP600, and his professional judgment is that the conditions are similar enough that he can continue to use it.

He hasn't gone back and redone all of his assessment work. Westinghouse is trying to make the same case for their codes for AP1000. What we are doing in the regulatory space is making a judgment based on the work he did for AP600 -- he was one of the principal analysts for AP600. So he looked at a lot of the codes for AP600. He looked at how they were applied He looked at how they were assessed, the test data they were assessed against, how well they did against that test data.

He considered all of that, and he considered then that his code was assessed against other data, was assessed against some of the same data. He considered his professional judgment in analyzing a lot of reactors over -- how many years, Walt, 30 years? -- 30 years, and he made a decision.

DR. BANERJEE: Right. Let me ask -- There is this set of data which is very close to these -- There is experimental data which is very close to these conditions where I asked a straightforward question, did they show dryout or not.

DR. CARUSO: And the answer is we don't know, because we don't have that, you know, right at the top of our head about whether they showed dryout. What he looked at was void fraction, and that's what he considers in his judgment to be the important parameter to consider. So he looked at void fraction.

We can maybe find those experiments and determine what the temperatures were, yes, but the question you asked was, well, what do we do, how do we make these decisions. This is how we make the decisions.

CHAIRMAN WALLIS: I'm trying to think of some analogy, because this is a strange world of nuclear safety, and so any other situation I can think of where I've got an analogy -- and I only give you one, because it's all I can think of at the moment.

We are analyzing something like the Brooklyn Bridge, and we are going to say how many people can stand on it at one time without it breaking. There's two elements. One analysis says, oh, I predict that it's safe. I predict that the cable stretched by one percent, but the deck is stiff, and so the whole thing only goes down by 10 feet, and it doesn't break.

Another analyst comes along and says, oh, I've done a different analysis, and my prediction is that the cables actually stretch by five percent, but I've got a compensating error somewhere else in the deck stiffness which predicts that the bridge only goes down by eight feet, and it doesn't break.

Now is this a basis for making a decision?

DR. CARUSO: Well, it's interesting you bring this up, because I just finished reading a book about the Brooklyn Bridge, and there were actually technical disagreements about that exact subject, about whether it would hold up.

What they did was they went out and they measured it as it was being built, and you can measure it.

CHAIRMAN WALLIS: But you can't do that with these reactors.

DR. CARUSO: That's the problem with these reactors. You can't -- Luckily, we don't have any real data.

CHAIRMAN WALLIS: If I wanted to be really secure, I would want to see the two codes predict the same key parameters.

DR. CARUSO: And that's why you have the code --

CHAIRMAN WALLIS: Like this level.

DR. CARUSO: That's why you have the codes assessed against test data. Test data in the ROSA facility, test data in the SPES facility, and that's why we have an Office of Research to go off and do this sort of assessment work for us, and some very smart people at laboratories and universities --

CHAIRMAN WALLIS: Well, models of the Brooklyn Bridge being tested in the lab. I still have to face the fact that two competent people using competent codes predict something very different about the details of what happens.

DR. CARUSO: I'm not sure they are that different. That's the point I'm trying to make. I'm not sure they are actually that different, because as Walt said, he looked at void fractions, and the void fractions that he saw were reasonably close.

CHAIRMAN WALLIS: Yes, but we are going to see this tomorrow.

DR. CARUSO: We need to put this all on the same plot, because they have given us some RAIs with COBRA/TRAC on there, and if I recall, the collapsed levels were on the order of 30 or 40 percent.

CHAIRMAN WALLIS: But it was 30 percent? Okay. We are going to see that then. We are going to see that tomorrow. They are going to save us --

MEMBER RANSOM: I think part of the problem here is you see a very incomplete picture. You know, you can't just look at, say, collapsed core level and draw any conclusion. You need to know what the void fraction distribution looks like, what the heat transfer coefficients are in the different parts, in order to come to any conclusion.

CHAIRMAN WALLIS: I can draw a lot of conclusions. If RELAP predicts 30 percent and NOTRUMP predicts 70 percent, there's a major difference in the amount of water in there.

MEMBER RANSOM: Well, I guess I'm not sure they do, but that's quite a bit.

CHAIRMAN WALLIS: Then I have to somehow rationalize my acceptance of this kind of level of uncertainty about a major problem.

DR. BANERJEE: Maybe what we need tomorrow is a comparison of what you see here and, say COBRA/TRAC which, as you say, may or may nota be different -- it may be very similar -- and the void fractions and the temperatures, and ideally, what actual experiments with a 14-foot core showed, because those experiments have been done.

DR. JENSEN: Westinghouse did present those in their NOTRUMP topical for AP600, and they did a pretty good job.

DR. BANERJEE: Well, NOTRUMP may be a little bit off, because in the sense that you are showing a 30 percent level here to 40 percent, which is what Steve says COBRA/TRAC is showing. My impression from looking at the NOTRUMP results are that they are showing a higher level, but that's just an impression until we see that in general. Okay, tomorrow we will know exactly.

In any case, we have experiments at 30 percent, 40 percent collapsed liquid level. So there is not that much ambiguity here. We actually can see what the temperatures were.

CHAIRMAN WALLIS: Did APEX ever get so low in collapsed level?

DR. BANERJEE: APEX is a tiny little code.

CHAIRMAN WALLIS: Yes, but it's one of these. It's supposed to mode AP600. I don't remember it getting -- Well, most of these transients here get down to about 30 percent. All your colors get down to about 30 percent or so.

DR. JENSEN: Yes, that's the interesting part. No matter where the break is and what size it is, the result is always about the same.

CHAIRMAN WALLIS: Well, I think in APEX -- I'm just going from memory. I don't remember this happening so much, that all the transients went down to -- I don't think they went down to such a low level.

DR. JENSEN: Again, it's possible RELAP is not quite right here. It has too much drag between the phases, and I'm not here to say it is.

CHAIRMAN WALLIS: Well, we should probably move on.

DR. JENSEN: Well, let me flash my last slide up here very quickly.

DR. CUMMINS: This is Eric Cummins. It seems to me that the slide we have not paid attention to is the slide that's there where the highest temperature is the temperature at the start of the accident, and all temperatures of the core are lower than that after the start of the accident. That should be fairly comforting, we think.

CHAIRMAN WALLIS: This one looks comforting here.

DR. BANERJEE: If it agrees with experiment.

DR. JENSEN: That's important. And this is my last slide. Well, it is really Lambrose's slide, and it says that the -- One additional issue we raised during the preapplication review was we were worried about the boron precipitation in the core during the long term cooling, and this would be because perhaps there would be separation in the reactor system, and the steam would be transferred out of the ADS over the long term, and water not flow to the vessel.

Since there is no hotleg injection in the AP1000, as there is in operating plants, we are worried about long term boron precipitation in the core, and we are awaiting some additional information from Westinghouse to resolve this issue.

CHAIRMAN WALLIS: So if you take all the boron that was originally in all the water and put it in the core and take the core with the amount of water that you think is in there, then you get way above this 35,000 pounds per million or whatever it is ?

DR. LOIS: Yes, sir. About 69,000.

CHAIRMAN WALLIS: So the limiting case is obviously bad.

DR. LOIS: Yes, sir. However, this morning I was informed that some more information has been provided to us, but I didn't have a chance to look at it.

CHAIRMAN WALLIS: I would think that, as you keep putting water in with boron in it, you keep taking steam out without boron in it. Eventually, all the boron is going to end up in the core.

DR. LOIS: It did. It did.

CHAIRMAN WALLIS: And it's bound to precipitate.

DR. LOIS: That's right. Exactly.

CHAIRMAN WALLIS: You go on distilling long enough, it's bound to happen.

DR. LOIS: And the only way to avoid that is to expel some --

CHAIRMAN WALLIS: Carry out some water.

DR. LOIS: Carry out some water. Precisely.

CHAIRMAN WALLIS: Which is hard to do when you are in long term cooling. There isn't much flow of steam.

DR. LOIS: Well, that is correct, and one of the initial statements in the first submittal was that this state of long term cooling can go on forever. I asked Westinghouse to determine the point where the functions of long term cooling, as described in the initial stage, are no longer valid.

For example, the extremely low ADS steam velocity -- Unfortunately, I don't have a response yet to that question.

MEMBER KRESS: When you are boiling away forever on long term cooling, you actually don't concentrate the boron. You take it out with the steam. I think you guys better rethink that and go back and look at your distillation calculations.

The boron will actually go out with the steam at low pressures. It's a function of pressure.

DR. LOIS: Yes, you're absolutely right. The pressure level was very low, and from what I read in the properties of boron, it seems that it does precipitate, crystallizes in the bottom, and really you don't have to take -- to have the entire amount of boron into the vessel. A portion of that will start doing damage, and beyond that is irrelevant.

CHAIRMAN WALLIS: So there is boron vapor coming out with the steam?

MEMBER KRESS: Yes.

DR. LOIS: But by that time, the boron that was crystallized already had done damage.

MEMBER KRESS: Yes. It's a matter of how long are you at high pressure and how are you at low pressure,

DR, LOIS: From there on, it's irrelevant.

MEMBER KRESS: It's not just the carryout of the liquid. It could come out with the steam. That was my point.

CHAIRMAN WALLIS: So this is still an unresolved issue then?

DR. LOIS: For the time being, yes, until I have a chance to look at the additional information which was provided today.

CHAIRMAN WALLIS: So Westinghouse is providing you with information today?

DR. LOIS: Yes, sir. Well, it arrived this morning. I didn't have a chance to look at it.

CHAIRMAN WALLIS: Are they going to present it to us tomorrow?

DR. LOIS: If I have an opportunity to look at it, we may.

DR. CORLETTI: Dr. Wallis, this is some of the information that I presented this morning in regards to the boron precipitation.

CHAIRMAN WALLIS: Not much detail.

DR. CORLETTI: No. I think -- We think we have resolved it with the calculations and analysis that we've done, and I think, if we want to get into the details --

CHAIRMAN WALLIS: If we were to have another Thermal Hydraulics Subcommittee meeting on AP1000, this could be one of the things we could take up.

DR. CORLETTI: Yes.

CHAIRMAN WALLIS: All right. And we may, after today and tomorrow, decide we have enough issues, we want to have another meeting with you.

DR. BANERJEE: You know, I saw some -- and I think many of us saw some calculations supporting the use of RELAP5, a version of it, for this PTS analysis which was compared to the AP600 and ROSA and stuff.

Is this the same version of RELAP5 you are using?

DR. JENSEN: This is the latest version of RELAP5.

DR. BANERJEE: They had some number gamma something. I can't remember.

DR. JENSEN: This is beyond gamma. This is 3.3 as released, I think it was last -- about a year ago, last spring.

DR. BAJOREK: It's 3.2.2 and 3.3 account for some relatively minor updates. I think, for all practical purposes, the PTS version of this and the 3.3 are about the same.

MEMBER RANSOM: I know they've only got one guy working on it. So there's very little change going on.

DR. JENSEN: That concludes my talk, unless there are any questions.

CHAIRMAN WALLIS: Well, let's move on to the next one. There's another staff presentation, I understand.

DR. SEGALA: Yes. Our next speaker is Ed Throm from Plant Systems Branch, talking about WGOTHIC.

MR. THROM: Good afternoon. As pointed out, my name is Ed Throm. I'm with the Plant Systems Branch. We are reviewing the WGOTHIC application to the AP1000.

I was also the reviewer who reviewed

WGOTHIC for the AP600. So I've been involved with the program for many, many years.

In consideration of time, on this second slide here, this is information you have seen before. This is basically a track record of documentation. GOTHIC is used for DBAs. The WCAP has been mentioned. Our initial evaluation was presented in the NUREG.

Basically, what we have done in containment space is developed a conservative evaluation model and things that are done in the modeling to address the lump parameter network, circulation stratification. The PCS flow in heat and mass transfers have all been done in very conservative fashion.

CHAIRMAN WALLIS: What is PCS again?

MR. THROM: The passive containment cooling system.

CHAIRMAN WALLIS: It's the part, though, on the outside in the air?

MR. THROM: Yes. It's the water coming down to cool the situation.

This has already been done before. During Phase II we looked at the difference in the AP600, AP1000, and basically determined there were no new phenomena that needed to be incorporated into any of the analytical models.

As Westinghouse presented earlier, when you look at the dimensionless numbers and look at the mass and heat transfer correlations, you find out that when you are doing the calculation, you are using the correlations within the ranges for which there is test data that demonstrates they are applicable.

The open issue was we really wanted to see the analysis done consistent with the evaluation model and all of those components that we had determined were applicable to the AP600. The initial calculation Westinghouse did back in December 2001, different nodalization, different assumptions -- it would have been very difficult for us to kind of revisit the whole review and relook at the potential to redo nodalization studies on how many climes, which is what they call their heat transfer package, would be necessary to conclude that we still understood the way the code was behaving and modeling the system to the extent that we could feel comfortable that we were having a conservative evaluation.

MEMBER RANSOM: What is the evaporative flow model?

MR. THROM: Westinghouse talked about that.

MEMBER RANSOM: That was the iteration they talked about?

MR. THROM: This is the iteration, right. There was a question early on. It dealt with some of the characteristics of what the film might be. There were some potential concerns with the numerics in the code about what would happen with the excess water.

So when they do the analysis, they only credit that amount of water that can actually be evaporated so it becomes an iterative calculation. If the code is calculating water coming off the bottom clime, they will go back and redo the analysis with a lower water flow rate, such that over the course of the transient there is none of this excess water to contend with, either from the potential numerical issue with the code or it addresses some of the concerns in whether the film has a little bit of waviness to it. It kind of compensates for the correlation that is being used.

So that was how we kind of resolved that issue.

So the bottom lien is they are doing the analysis the way we expected to see it done. The calculations are based on the approved methodologies, and the mass and energies are being calculated consistent with the standard review plan, which is a very conservative method for treating containment.

They have incorporated in that WCAP-15846 a Section 14 which describes the methodology for the way the mass and energies are calculated. They do show the comparison to WGOTHIC, and you can basically determine that, when containment performance analyses are done, the mass and energies release the containment at a very high rate over a much shorter period of time and, of course, you get less impact from the heat structures as you do the calculation. So it is very conservative.

MEMBER KRESS: What would you have done if margin to the time pressure turned out to be slightly above the 60?

DR. THROM: If it became slightly above, we would be in a negotiation somewhere. Right now, the acceptance criteria is basically below. One could argue in the legal perspective --

MEMBER KRESS: There is no required margin?

DR. THROM: No. Basically, if you look at the standard review plan and basically the interpretation of the Commission's requirements, it is less than at the operating license stage.

So what is built into the AP600, AP1000 program is what we call ITAC, initial test and acceptance criteria program, which means they will go in when the plants are built. They will actually dump the PCCS water and verify that the flow coverage they are using in the analysis are correct, that the flow rates are correct. They will verify that all the heat structures that they are taking credit for in the analysis are there, and the PCS will be periodically checked to make sure that it is performing.

MEMBER KRESS: That's all under the ITAC?

DR. THROM: That is -- Yes. Again, this is -- Normally, if I were doing a construction permit, I would be looking for about a ten percent margin at this particular stage. So in order to make sure that the as-built is okay, and these calculations are representative of the as-built, we have the ITAC part of the new Part 52 licensing which says we identify all of those system features and components that are important to our understanding of the licensing basis, and they are validated prior to operation.

CHAIRMAN WALLIS: Now what is CONTAIN2 calculations?

DR. THROM: I'm going to get to those.

CHAIRMAN WALLIS: You did that.

DR. THROM: Yes.

MEMBER RANSOM: Could I ask you one other question about WGOTHIC, though. Do they have a carryover factor for how much water is entrained and carried out without being evaporated?

DR. THROM: In --

MEMBER RANSOM: And here I'm thinking this thing is no different than any cooling tower, and we all know that you get water -- some carryover invariably in a cooling tower.

DR. THROM: I don't think they do, but I don't think we've really looked at that as far as -- I don't think we envision any real entrainment of droplets into the air stream.

MEMBER RANSOM: What would prevent entrainment in a case like that when you drop water down a cooling tower and you get entrainment?

DR. BROWN: I would think a cooling tower normally has got a fan at the top and --

MEMBER RANSOM: No, I'm thinking natural draft, you know, parabolic type.

DR. BROWN: If you look at our velocities, if you look at like some of the scaling numbers, the velocities and things are very low. I don't think they are anywhere near the type of thing you're thinking of with a cooling tower.

MEMBER RANSOM: No, you got a similar height.

DR. BROWN: I know, but we still have -- With the path going down there, I think that, if you look at the annulus there and you actually calculate the velocities, they are not really that --

DR. THROM: Two things. The bucket that is above the containment is in the chimney area,and the chimney is huge.

DR. BROWN: It's huge. It's very, very big.

DR. THROM: The velocities are very, very low.

DR. BROWN: Right.

DR. THROM: You fill a bucket. The bucket is very close to the containment. So it's not like we are trying to dump water down the sides. It's being distributed through a weir system to run down the sides.

MEMBER RANSOM: Running over the sides, right?

DR. THROM: Right, and there is a distribution system to do that. As I indicated earlier this morning, you want to make sure that you are getting a relatively uniform and good distribution of the water.

So it's not really like dropping it into this updraft.

MEMBER RANSOM: Well, I think you are, as a matter of fact, and even in a cooling tower you don't want to entrain water. I mean, you would rather recover all the water, because that's what you're after, is to cool off the water by evaporation and use it in the condenser.

So entrainment there hurts just as much as it would in a case like this, but I would be very curious to know what the entrainment is like in a structure of that type compared to this one, which is assuming no entrainment.

MEMBER KRESS: I'm not sure entrainment hurts you in this case.

MEMBER RANSOM: It sure as hell does.

MEMBER KRESS: Well, small droplets, you're going to get the heat transfer between the droplets and the air before it ever gets carried out the top, and you want to cool down the air. Unless it gets carried out the top --

MEMBER RANSOM: Well, that's what I would assume.

CHAIRMAN WALLIS: You don't want to cool down the air. You want to cool down the shell.

MEMBER KRESS: Well, if you cool down the air, then the heat transfer between the air and the shell is enhanced. You cool down the shell.

DR. BROWN: Dr. Kress, you will remember from AP600, the other thing to keep in mind, that typically our peak pressure occurs when you look at that relative to where we really need the PCS, that really the majority of the heat and mass transfer is really typically done on the internal heat sinks and the volume.

Those are still a lot of the predominant, and the PCS is really helping us to keep the pressure down, once we get it down there, keeping it long term to stay down there. We are not really relying upon it to turn over the peak pressure.

So when you put it in that context, you realize that how large of an annulus space that this really is, and those velocities, you realize that it is really not addressing the problem with looking at peak or design pressure. It's really more of an issue of how much you allow the pressure to recover after you have turned it over.

MEMBER RANSOM: Well, even if you had

carryover, it is only going to affect how long before you are going to have to resupply some water.

DR. BROWN: Well, admittedly, we waste a lot of water in this when we initially deluge and dump it over there. Really, the problem again, like in the internal, is really more of a problem of excess water rather than not enough water.

CHAIRMAN WALLIS: So CONTAIN calculations were things that you ran?

DR. THROM: Yes. Actually --

CHAIRMAN WALLIS: In this case, you got the same answer that Westinghouse did.

DR. THROM: Yes, which --

DR. CUMMINS: I think in the other case also.

CHAIRMAN WALLIS: Which is my expectation.

DR. THROM: Yes. Put the overhead up. I was hoping to have the LOCA evaluation done by today, but we couldn't get it done. So I only have the main steam line break.

For the reference, when we talk about the tier two information, that's the current analysis that Westinghouse says this calculation, when we indicate with bias. If you remember, last year almost a year ago, when we were doing our scoping calculations with containment, at that point we were not adding in a lot of the conservative features like reducing the mass and heat transfer multipliers and turning off heat sinks below the deck to compensate for issues on circulation, stratification.

So the Office of Research has been assisting us with this effort, and these are their calculations.

CHAIRMAN WALLIS: These are sort of bounding. The realistic CONTAIN would be lower than this, if you didn't turn off those heat sinks and all that.

DR. THROM: Yes. Yes. In containment you do three things essentially when you look at the Westinghouse model. Number one, you would have very conservative mass and energies, and then actually the second part is all -- The second part is the initial conditions that you assume for the calculation are done to maximize the prediction of pressure.

You look at a high initial internal pressure, initially a high temperature. You look at high temperatures for the PCCS water and the air flow. So that there tends to be a conservative aspect, but that is used to demonstrate that your limiting conditions for operation are meeting your design base.

In other words, when they say I can operate containment up to 120 degrees, this analysis shows that I have a high reliability or confidence that the containment pressure will not exceed the design basis.

CHAIRMAN WALLIS: So this is a conservative type approach. If you had done a realistic analysis, you might well get something much lower, but you would then need to have uncertainty bounds and you would have to evaluate --

DR. THROM: Right, which we typically don't. Based on what I have seen to date, my guesstimation is that the conservative aspects of the mass and heat transfer multipliers, turning off heat sinks is worth two psi.

CHAIRMAN WALLIS: That's not much.

DR. THROM: No. No. If you also look at the initial conditions, if you run the case with a more nominal expected environmental and containment conditions, you would probably get about another two psi.

When you look at the mass and energy, I think Westinghouse has an analysis. I don't remember if the analysis is in Chapter 14, but if you look at realistic mass and energy, you almost - - you'll walk away from the situation.

That's where I believe most of the conservatism is, and that is been the stay of the licensing framework for the last 40 years, is basically the mass and energy is done in a very conservative manner.

As a matter of fact, during the AP600 I researched at an analysis where they coupled CONTAIN to RELAP5, and basically what you see for that situation where there is an importance in the coupling, the second peak in the performance of the AP plants is very dominated by what we do in the evaluation model.

CHAIRMAN WALLIS: I think we saw this eons ago, it seems now.

DR. THROM: Yes.

CHAIRMAN WALLIS: And this was reassuring, that when you did couple these codes, you got a considerably lower pressure.

DR. THROM: Yes. Yes. And the reason we have this effort from Research assisting us is because there is an effort at Research to start looking at coupling. I think they are going to try and couple TRACM with CONTAIN for the AP1000. It's something they are doing.

So we are benefitting. They benefit from the work we started by getting most of the AP1000 containment model done when we were doing the preapplication review. Now they are kind of paying us back in kind by assisting us with making sure the deck is of quality and it will eventually be used in this program that they have to look at future capabilities to do coupled calculations.

CHAIRMAN WALLIS: It looks as if there isn't a problem with containment. There is not a problem with containment.

DR. THROM: No.

CHAIRMAN WALLIS: And we could probably, on a good note, take a break.

DR. THROM: Yes.

MEMBER KRESS: What happens at 1000 to turn it around?

DR. THROM: That's when the generators dried out. There's no more mass and energy going in. Now the heat structures are able to start condensing the steam.

CHAIRMAN WALLIS: The source is switched off.

DR. THROM: Your source is switched off,

yes.

CHAIRMAN WALLIS: Can we take a break then until 3:30, and we will move back to the Westinghouse presentation after that.

(Whereupon, the foregoing matter went off the record at 3:22 p.m. and went back on the record at 3:37 p.m.)