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.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Reactor Safeguards Thermal-Hydraulic Phenomena Subcommittee OPEN SESSION

Docket Number: n/a

Location: Rockville, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Heather Craycraft
Official Reporter
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